Transitory Income Changes and Consumption Smoothing: Evidence from Mexico

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Abstract

We test if 3,534 beneficiaries of PROSPERA, Mexico's cash transfer program, smooth food consumption before and after the date of the transfer receipt, and if consumption smoothing is costly. The transfer is an anticipated and transitory income shock and, thus, the PIH predicts that consumption should be smooth before and after its receipt. We find that food consumption does not change the days before and after the transfer date and we find no evidence that households bear costs to smooth consumption. The transfer's cost of access, which encompasses participants' distaste for using debit cards and costly ATM withdrawals, may help time-inconsistent and less experienced debit card holders smooth consumption.

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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 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 2006, 2009; Mogues 2011). This behavior may cause households to forgo future higher consumption in exchange for smoother current consumption. Second, income fluctuations may 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 2022).

This paper tests whether households smooth consumption and if it is costly to do so before and after receiving the PROSPERA cash transfer, an anticipated and transitory income shock. PROSPERA, formerly known as PROGRESA/*Oportunidades*, was Mexico's flagship anti-poverty program. The transfer was paid every two months at dates which recipients knew in advance and varied between households. It was deposited into savings accounts linked to debit cards. The average transfer in our sample is 1,604 *pesos*, slightly larger than regular weekly income (the modal income payment frequency). Therefore,

weekly income doubles when households receive the transfer.[1]

We randomly varied the timing of the interview for 3,534 peri-urban households, such that half are surveyed the week before receiving the transfer, an approach pioneered by Stephens (2003) and later adopted by others in this "payday" literature. Thus, we can compare household consumption before and after the transfer date. The random timing of the survey ensures that there are no systematic differences between the before and after groups. Moreover, the variation in the day the transfer occurs 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 measured food consumption on the previous day. We also collected data on finances, asset sales, employment and income, mental health, preventative healthy habits, cognition, and preferences to test whether there is a transfer date effect on these families of outcomes.

This study has two main findings. The first finding is that food consumption does not change before or after the transfer date: the estimated transfer day effect on food consumption is small and statistically insignificant. The confidence interval rules out a daily consumption drop of more than 0.01%. That is, even very small drops in consumption before the transfer date are highly unlikely. We find that food consumption does not vary around the transfer date also for time-inconsistent recipients, as well as households whose transfers account for more than 20% of total income, although estimates suggest that impatient households may not fully smooth consumption.

The second finding is that there is no evidence that smoothing consumption around transfer dates is costly. Households do not sell their assets, there are minimal increases in adult labor supply, and no changes in child labor. This result suggests that food consumption does not decline before the transfer date because households are not running out of money towards the end of the pay cycle. Moreover, we do not find any reduction in cognitive performance, mental health, healthy habits, or any changes in time preferences.

[1]The transfer corresponds to USD 79 at 2022 exchange rates.

This finding is consistent with the hypothesis that this transitory income fluctuation has no hidden cognitive or health costs.

The difference between our results and studies that find a "payday effect" may partly be explained by differences in measurement, the timing of the transfer date, and transfer access costs. First, we measure food consumption, while other studies (e.g., Stephens 2006, Gelman et al. 2014, Carvalho et al. 2016, and Olafsson and Pagel 2018) consider expenditures: drops in expenditures do not necessarily reflect changes in consumption (Aguiar and Hurst 2005). Second, since the date on which the transfer occurs is uniformly distributed throughout the month between and within locality, we avoid price effects (Hastings and Washington 2010) and spurious correlations between the timing of recurrent expenses and the transfer date, which would make consumption *seem* lower before the date of the transfer (Gelman et al. 2014). Third, participants' preferences and ATM withdrawal charges make access to PROSPERA transfers costly. The transfer is deposited into a savings account. Although ATMs are generally easy to access, free ATM withdrawals are limited. Moreover, although several stores accept debit card payments, many households prefer not to make purchases using their debit cards. Lastly, the median household lives 6.2 kilometers away from the nearest bank branch. This combination of preferences and costs may prevent some households from consuming more when their transitory income is higher, possibly helping some households resist temptation. Consistent with this hypothesis, we find that total food consumption is between 13 and 15% lower 4 to 1 days before the transfer date for households living at least a thirty-minute walk from the nearest bank branch, if their transfer recipient is time inconsistent and a less experienced debit card holder. This result suggests that the cost associated with accessing the transfer may help some time-inconsistent transfer recipients smooth consumption. This finding is conceptually consistent with Huffman and Barenstein (2004), Shapiro (2005), and Mastrobuoni and Weinberg (2009), whose work shows that failure to smooth consumption is driven by or consistent with hyperbolic discounting. However,

this evidence is suggestive at best. In addition, while transfer access costs might help beneficiaries with time-inconsistent preferences, they may be welfare-reducing for other households without bank branches or ATMs nearby. Finally, the existence of these costs may simply delay lumpy consumption for some households.

We also explore additional mechanisms that lead to consumption smoothing around the transfer date. We rule out network effects, intrahousehold bargaining, and daily income earners.

This study makes several contributions to the literature. First, it focuses on Mexico, a middle-income country. This adds to the literature that primarily tests the PIH in industrialized countries. Second, it considers the validity of this hypothesis for anti-poverty program recipients, a socioeconomically vulnerable population. The PIH may be less likely to hold, or consumption smoothing costlier to achieve, for this population because of limited access to the financial sector. Third, besides considering whether the PIH holds, this study also explores potential mechanisms and whether smoothing consumption is costly. Fourth, the study contributes to the scarcity literature by showing that large income fluctuations do not always cause cognitive impairment, consistent with Carvalho et al. (2016).

2 Setting and Data

PROSPERA, formerly known as PROGRESA/*Oportunidades*, is one of the largest and best-known cash transfer programs. It began 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 received transfers every other month on pre-specified dates. Recipients had a calendar with all the dates on which transfers occurred, so that they knew each transfer date in advance. The transfer dates varied across participants and depended on the program rollout in

each locality and the timing of enrollment in the program. Roll-out occurred gradually over time across Mexico. Therefore, we do not expect a systematic relationship between transfer dates and household characteristics.[2] This is important for identification, as will be discussed below.

Between January 25, 2016, and February 25, 2016, we surveyed 3,534 PROSPERA beneficiaries from 52 peri-urban localities in six states, collecting socioeconomic individual and household data.[3] For budgetary purposes, we surveyed PROSPERA beneficiaries in six central states of Mexico: Distrito Federal, Hidalgo, Estado de México, Michoacán, Morelos, and Veracruz. We further restricted the localities to include between 120 and 500 families enrolled in the PROSPERA program to ensure that we had a sufficiently large sample of households per locality. The selected areas and households are representative of this subpopulation.

Our sample has food consumption and income levels similar to the overall population of urban beneficiaries of PROSPERA, whose daily food expenditure is 77 pesos and weekly income is 1,557 pesos (excluding PROSPERA), according to estimates from the Encuesta Nacional de Ingreso y Gasto de los Hogares (ENIGH) 2016. The households in our sample consume 112 pesos worth of food daily (it is common for consumption to exceed expenditures in settings in which households grow or raise some of their own food) and have a weekly income of 1,460 pesos (excluding PROSPERA).

We merged our survey data with administrative records of the transfer dates from the Department of Social Development, which provided the transfers to the beneficiaries.[4] The

[2]For example, suppose that more determined and organized households enrolled sooner and that it took 30 days between becoming enrolled and receiving the first transfer. In communities in which PROSPERA rolled out in the middle of the month, more organized households would receive transfers in the middle of the month, and less organized households at the end and start of a month. In communities with a late-month roll-out, more organized households would receive transfers at the end of month, and so on.

[3]We define localities as peri-urban if they have a BANSEFI branch within 10 kilometers from their centroid. BANSEFI is a government-owned bank where PROSPERA transfers are deposited. Its branches are generally located in predominantly urban areas.

[4]We do not observe banking data, including transfer withdrawals dates or debit card use. Our test strongly relies on the payment date being accurate. While we had access to planned dates, we could not independently verify that the transfers were deposited the intended day.

top-left panel of Appendix Figure A1 shows that the transfer dates are evenly distributed across calendar dates. Since this even distribution occurs both between and within villages, it rules out price and cyclical expense effects (Hastings and Washington 2010; Gelman et al. 2014). The remaining three panels of Figure A1 show the distribution of the previous day's total, perishable, and junk food consumption by the interview date. This distribution is approximately uniform. This is expected, since no holidays or festivities occurred in the relevant time frame.[5]

To study if households smooth consumption around the transfer date, we randomly assigned beneficiaries to be interviewed before or after the transfer date. Specifically, we randomly assigned 25% of the sample to be interviewed in four date groups: 8 to 5 days before the closest transfer payment (group 1), 4 to 1 days before the closest transfer payment (group 2), 0 to 3 days after the closest transfer payment (group 3), and 4 to 7 days after the closest transfer payment (group 4). All respondents were interviewed within the scheduled date range. However, the surveyors front-loaded interviews on the first day of each group (days -8, -4, 0, and 4). A few interviews *did not* occur on the first day, for reasons ranging from respondents being unavailable to enumerators running out of time. In those cases, the enumerators surveyed participants on subsequent days. 86.5% of households were surveyed on the first day of each group, and the remaining 13.5% of households were surveyed 1-3 days after the first day. Respondents' and households' predetermined covariates do not jointly vary for surveys that occurred on day one of each date group and surveys that occurred on days 2 to 4. Appendix A.1 and Appendix Table A1 provide more details.

Our main outcome of interest is *total food consumption* the day before the interview. We measured household food consumption on the previous day by asking the respondents to recall the total quantity and monetary value of food consumed inside the household

[5]Trends or cyclicality in food consumption would not affect our identification strategy. The small spikes in junk food consumption take place on Sundays (e.g., February 1st was a Monday, so people interviewed that day would report their Sunday's food consumption.)

and the total value of the food consumed outside the household on the previous day.[6] We also report consumption of perishables (fruits and vegetables and food of animal origin) and junk food consumption (junk food and snacks and non-alcoholic beverages), which may be more sensitive to income fluctuations.[7] Households interviewed 0-7 days after the transfer belong to the "after" group, while households interviewed 1-8 days before the transfer date belong to the "before" group.

Since we measure food consumption the day before the interview, people interviewed the day of the transfer report their consumption *before* the transfer, despite being part of the "after" group. This feature of our data may attenuate any differences between the "before" and "after" group. To address this issue and other potential sources of mismeasurement of our primary outcome, Section 4 discusses alternative ways to sort households into "before" and "after" groups and shows that our results are robust to these alternative classifications.

We also consider the following families of outcomes: household finances and employment; cognition; healthy habits; mental health; and preferences.

Household finances and employment. Household net savings are monetary savings plus credit minus debt. We measure asset fire sales as the number of assets sold in the previous week. We consider labor supply and income separately for adults and children: labor income in the last 3 days; the fraction of adults/children who were paid in the last 3 days; the fraction of adults/children who worked in the last 3 days; and hours worked in the last 3 days.[8]

[6] 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. We sum the monetary value of each food group and add the value of food eaten out.

[7] About one quarter of households did not consume junk food the previous day and about 4% did not consume any fruits or vegetables.

[8] Since households surveyed 1 to 3 days after the transfer date provide information that encompass days both before and after the transfer date, we also omit these households from the analysis as a robustness check. When doing that, we drop 4.5% of the observations. The results are qualitatively unchanged.

Cognition, healthy habits, and mental health. We create three indices: cognitive function, healthy habits, and mental health. The cognition index uses digit recall (backward and forward) and Raven's matrices. The mental health index uses depression, stress, and locus of control instruments. The time frame for these questions is the survey date. For the healthy habits index, we are interested in habits that are not costly or difficult to implement, but that can have large future health benefits. We consider hours of sleep, hand washing, tooth brushing, and physical activity. The time frame for these questions is yesterday (the day before the interview).

Preferences. We also create a risk tolerance index, a patience indicator, and an indicator assessing whether the beneficiary exhibits time-consistent preferences. Appendix A.2 and Appendix Table A2 provide further details about each outcome.

3 Identification and Estimation

Our empirical analysis consists of comparing consumption for households interviewed right before and right after their PROSPERA transfer date, following the approach pioneered by Stephens (2003). For example, consider all households whose transfer date falls on day t. Since the timing of the interview is random, households interviewed before and after day t do not systematically differ from each other. Therefore, if we find that consumption is *lower* for households interviewed before the transfer date, we conclude that this is inconsistent with the permanent income hypothesis.

To study how the timing of the transfer affects food consumption, Y_i, we estimate the parameters of the following equation:

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

The indicator B_i equals 0 for groups 3 and 4 – households interviewed up to 7 days after the transfer date and 1 for groups 1 and 2 – households interviewed 0 to 7 days before

the transfer date.[9] The latter group has *not* received a transfer for almost two months. The predetermined variables X_i include age and education of the PROSPERA beneficiary, couple-headedness, 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 fixed effects; 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. If the PIH holds, α_1 is zero, whereas it is negative when consumption tracks transitory income. This parameter is identified under the assumption that there are no systematic differences in the outcome determinants for households surveyed before and after their transfer date. As both the day of the interview and the transfer date are exogenous, this assumption seems realistic. This is consistent with the evidence in Appendix Table A3, which shows that the recipient and household predetermined characteristics are balanced by both the four interview date groups and the two "before" and "after" groups.

When investigating possible mechanisms and additional outcomes for which there may be a transfer date effect, we estimate versions of equation 1 with different outcomes. The identification assumptions are unchanged.

We estimate the parameters of all equations by OLS, with standard errors robust to heteroskedasticity. In addition, when we consider outcomes that belong to the same "family," we either compute sharpened q-values (Anderson 2008) or create an index for the family of outcomes (as we did for cognition, healthy habits, and mental health).

[9] We pool the two "before" and the two "after" groups to increase the precision of our estimates.

4 Consumption Smoothing around the Transfer Date

Receiving the PROSPERA transfer has a large transitory effect on income. Considering all recorded sources of income, weekly income is 1,466 *Mexican pesos* the week before the transfer and 3,069 the week after (p-value of the difference <0.01). That is, transitory income doubles the week of transfer receipt. This large income fluctuation occurs because the transfer is paid every other month, whereas labor earnings, the other most common source of household income, are often paid daily or weekly. When we consider it as a share of total annual income, the transfer is only about 13% of income.

Figure 1 shows estimates of the average differences between the "before" and "after" groups (the estimate of the α coefficient from equation 1), standardized around the control group mean. These are 0.06 standard deviations (SD) (standard error (SE) 0.03), 0.09 SD (SE 0.03), and -0.02 SD (SE 0.03). That is, we can reject the null hypothesis that consumption is systematically different before and after the transfer date, and that consumption is *lower* before the transfer date, despite the large fluctuation in transitory income. The small positive effects on consumption of perishable food shows that, while the value of food consumption is unchanged, there may be small effects on its composition.

To investigate this issue further, Figure 2 shows the cumulative density functions (CDFs) of total, perishable, and junk food consumption for households interviewed before and after the transfer date. Each pair of CDFs appears remarkably similar. In addition, Appendix Table A4 shows the average consumption of households grouped by the four interview group dates, our "experimental" design. It shows that consumption is not lower before the transfer date even with this grouping.

Next, we discuss various robustness checks (see Appendix A.3 for additional details). Appendix Figure A2 shows that the estimates are robust to: dropping households whose reported transfer date is within plus or minus 3 days since the interview date, which minimizes concerns about measurement error in transfer dates; grouping households by

actual date instead of scheduled date; dropping households whose scheduled and actual survey dates do not coincide; using calendar days fixed effects instead of weekday fixed effects; clustering standard errors by locality; and omitting all additional covariates from equation 1. An additional way to measure consumption smoothing is to use the quantity consumed instead of its monetary value. The quantity consumed is easier to recall than the peso value of consumption; however, it cannot be aggregated across food items in the absence of unit values. Appendix Figure A3 shows that the quantities consumed do not vary around the transfer date for any food group.

As our final check, we consider consumption smoothing for temptation goods eaten or purchased the day before the interview: pastries, cookies, carbonated drinks, juices, candy, and chips eaten by the respondent (and not the entire household); household purchases of lottery tickets and tobacco products; and food eaten outside by the household. Appendix Figure A4 shows that individual and household consumption of these items does not change before and after the transfer date.[10]

The literature suggests that time preferences and liquidity constraints may make consumption smoothing difficult and costly (for a review, see Jappelli and Pistaferri 2010). Therefore, we consider three dimensions of heterogeneity: impatience, time-inconsistent preferences, and household dependence on the transfer. The impatience indicator equals one for people whose behavior is consistent with 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. We define high transfer dependence as a transfer dependence ratio (total yearly transfer as a share of total yearly income) higher than 20%. We create a "large transfer" indicator for these households. 28.45% of respondents are impatient, 47.14% are time inconsistent, and 24.28% receive a large transfer.

[10]The figure omits lottery tickets because we observe no purchases the day before the interview, either before or after the transfer date. We do not observe other types of non-durable consumption, such as transportation.

Appendix Table A5 shows the estimated effects for each subgroup by interacting the "Before" indicator in equation 1 by each subgroup dummy. Once we account for multiple hypothesis testing, we do not find any statistically significant effects for these subgroups. Therefore, we fail to reject the PIH also in these subgroups. However, we acknowledge that total and perishable food consumption is lower before the transfer for impatient respondents. Thus, specific subgroups of households may fail to smooth consumption around the transfer date, although the average household in our sample does not.

Next, we compare our findings with the results in Shapiro (2005), which investigates the food stamps nutrition cycle among U.S. recipients.[11] While living in different countries, both sets of households are eligible for a means-tested anti-poverty program. Shapiro (2005) estimates that the value of log food consumption drops by 0.0073 (SE 0.0038) per day over a monthly cycle. We perform the same test by regressing log-food consumption on the number of days after receiving the last transfer. We consider both all households, and households whose transfer is at least 20% of total income (analogous to the population in Shapiro (2005)). In addition, to minimize measurement error, alternatively we drop all households whose transfer date is within three days from the interview. Appendix Table A6 shows that the estimated coefficients for the whole sample are 0.0005 (SE 0.0003), and 0.0006 (SE 0.0004). These estimates are 12 to 15 times smaller than the effects for SNAP recipients.[12] Moreover, we can reject the hypothesis that our coefficients are identical to Shapiro's at the 95% confidence level. Lastly, the confidence interval shows that the effect of the transfer date is at most very small: effects lower than minus one-hundredth of a percentage point are highly unlikely. Therefore, we conclude that our estimates are both statistically and economically insignificant, as well as different from the findings of Shapiro (2005).

Finally, we compare our findings with the broader payday effect literature. Stephens

[11] Mastrobuoni and Weinberg (2009) also studies the payday effects on food consumption considering US Social Security recipients. This is an older population. Therefore, it is less comparable than our sample of families with young children.

[12] This is the ratio between our two coefficient estimates and 0.0073.

(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, the costs to access and spend food stamps and SSI are likely lower than for the PROSPERA transfers, as we discuss in Section 6.

5 Is smoothing consumption costly?

Smoothing consumption may be costly. While households may manage their income throughout the transfer pay cycle, it is also likely that, in an effort to smooth consumption, they may increase their labor supply, dissave, reduce their physical and human capital investment, sell their assets, or experience mental health and cognitive costs. Moreover, income fluctuations may have cognitive or health costs regardless of consumption smoothing (Mani et al. 2013; Carvalho et al. 2016; Lichand and Mani 2020). Therefore, we investigate whether these outcomes change around the transfer dates.

Figure 3 shows the effect of being about to receive the transfer on household net savings, asset sales, labor income, and employment. To make the effect sizes comparable, we standardized all outcomes. Figure 3 shows that we cannot reject the hypothesis that these outcomes do not vary around the transfer date: after adjusting for the False Discovery Rate, none of these estimates is statistically significant and all of them are small.[13]

A notable exception is the fraction of household adults who worked the previous three days, which increases by 0.07 standard deviations ($p < 0.10$), a 2 percentage point increase over a mean of 65%. While this difference is statistically insignificant once we control for multiple hypothesis testing, this finding suggests that increasing adult labor supply in

[13]The lack of an effect on net savings is consistent with people considering the transfer as disposable income or committed consumption and with mental accounting in savings Thaler (1999); Dupas and Robinson (2013).

times of scarcity may help some households smooth consumption.

Overall, these findings suggest that smoothing consumption around the transfer date is not achieved through dissaving, fire sales, income smoothing, or higher adult or child labor. Therefore, under this metric, smoothing food consumption around the transfer date is not costly for the households in our sample. These findings are consistent with the hypothesis that households do not run out of money by the end of the transfer pay cycle.

Income fluctuations may cause additional cognitive and health costs. Figure 4 considers this hypothesis. The estimates show that 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 future income changes. In our case, and in Carvalho et al. (2016), 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).

Figure 4 also provides suggestive evidence that being about to receive the transfer has small and *positive* health effects: mental health improves and healthy habits increase (results are significant at the 90% confidence level). Unreported regressions show that a reduction in the symptoms of depression drives the positive effects on the mental health index. These results may seem counterintuitive, as they imply that mental health and healthy habits are higher when income is lower. Nevertheless, they are loosely 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). In this case, an incoming transfer may provide the means to face unforeseen circumstances. Therefore, it can improve mental health even if transfer access is not costless.

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" current state of the world and an anticipated "good" future state of the world.

Lastly, Figure 4 also suggests that, while we find no change in time preferences, people have higher risk intolerance before the transfer date. This finding is consistent with the hypothesis that people are less tolerant of risk when their income is lower or when they are in a state of scarcity. The higher risk intolerance may also be a consequence of lower depression: Angelucci and Bennett (2022) find that offering pharmacotherapy to adults with depression improves their depression symptoms and also increases their risk intolerance. Other explanations are also possible.

Overall, we also fail to find heterogeneous effects by impatience, time-inconsistent preferences, and high transfer dependence. As Appendix Table A7 and A8 show, once we control for multiple hypothesis testing, none of the estimated coefficients is statistically significant.[14]

6 Mechanisms

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 date 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 date.

[14]We note a 5% reduction in adult labor supply ($p < 0.05$) for households with impatient transfer beneficiaries in Appendix Table A7. This drop in hours worked may be one reason why these households also experience lower consumption before the transfer date. However, this effect is no longer statistically significant once we adjust for multiple hypothesis testing. We also note a 0.1SD decline in cognition for time-inconsistent respondents before the transfer date.

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 those 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 transfer date.

Lastly, we consider whether households smooth consumption because some of them are paid daily.[15] About 40% of households have at least one member whose primary job pays wages daily. To test this hypothesis, we repeat the analysis omitting households with daily income. Appendix Figure A6 shows that the estimated differences in food consumption and its components before and after the transfer date are almost identical for this restricted sample and for the overall sample.[16]

Next, we investigate the possibility that consumption smoothing around the transfer date occurs because beneficiaries' preferences make accessing the transfer costly for some households. To understand why that may be the case, recall that the PROSPERA transfer is deposited into a savings account with BANSEFI. This type of financial transaction is unusual in this setting, as most financial transactions, including salary payments and commodity purchases, are made in cash. Indeed, while the account is linked to a debit card, and debit card payments are available in the area, PROSPERA beneficiaries use debit cards infrequently to pay for purchases (Bachas et al. 2021).[17] 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 the point of sale on average 1.48 times per month and 58% of users report never using their debit card to make a purchase.[18]

[15]About 88% of employed people are paid at least 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.

[16]We verified that the smaller sample is balanced.

[17]If access to ATMs were also limited, which may be the case in some areas, the statistical power of the PIH test could be reduced, as the funds would not be readily accessible.

[18]Data from 2018 ENIF (Encuesta Nacional de Inclusión Financiera). 55.5% of ENIF respondents with 5-9 years of education report not using the card because they simply prefer using cash. Another 7.4% mention

In addition, while cash withdrawals from BANSEFI ATMs are free, BANSEFI had very few ATMs at the time of the study. ATM withdrawals from banks other than BANSEFI are not free, except for one withdrawal per month (Bachas et al. 2021).[19] Therefore, most people incur charges for withdrawing cash from ATMs more than once per month. All withdrawals from BANSEFI branches are free. However, the median distance from the nearest BANSEFI branch is 6.2 kilometers in our sample. To conclude, there are non-negligible access costs for the many households that do not like to use their debit card.[20]

These access costs may be especially high for people who do not live close to a BANSEFI branch (from which they can make unlimited free withdrawals). About three quarters of the respondents live at least a 30 minute walk from the closest BANSEFI branch and only 16% of them own a car.[21] Distance from the branch, coupled with the limited free withdrawals from ATMs and the reluctance to use the debit cards to make purchases, increases the cost of accessing the transfer for these households.

The effect of distance from a BANSEFI branch on consumption smoothing around the transfer date is ambiguous. On one hand, a higher distance may cause households to make fewer and larger withdrawals, which may deplete the transfer sooner and, consequently, cause consumption to drop before the transfer date and to increase after. On the other hand, not having a branch nearby can prevent impulse purchases, thus helping smooth consumption around the transfer date. This latter effect may be more relevant for time-inconsistent transfer recipients, for whom distance may act as a commitment device, or for people who have not owned a debit card for long, and thus would be less likely to pay for purchases by card. Bachas et al. (2021) show that transfer beneficiaries gain confidence and trust in the banking system gradually after receiving the BANSEFI debit card.

lack of trust, and yet another 7.4% say they did not know they could use the debit card to make purchases.

[19] This is common in Mexico, there is a charge for withdrawals from other banks' ATMs as well.

[20] Nevertheless, the adoption of debit cards has decreased these costs: many beneficiaries whose transfer was deposited in a savings account without debit cards usually withdrew the full transfer shortly after receiving it, and started making 2-3 withdrawals per transfer period since receiving the card (Bachas et al. 2021).

[21] The median distance from the nearest BANSEFI branch was 6.2 kilometers.

To compare consumption smoothing for households living close and far from the closest BANSEFI branch, 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 and without a car as living far from the bank. We then estimate versions of equation 1, replacing the "before" indicators with groups 1, 3, and 4 indicators, adding a "close to bank" indicator, and letting the group coefficients (the analogous of α_1) vary for households close to and far from a bank.

Appendix Figure A5 plots the differences in total food consumption between Groups 1, 3, and 4 and Group 2 separately for households close to and far from the nearest BANSEFI branch, according to the above definition. Next to each pair of coefficient estimates, we report the coefficient of each respective double-difference and its standard error. We consider all households (upper panel), households with time-inconsistent transfer recipients (middle panel), and households with time-inconsistent transfer recipients who are also relatively inexperienced cardholders (lower panel).[22]

While we find no clear pattern when considering all households, the middle and lower panels provide suggestive evidence that consumption drops more before the transfer date and increases more after the transfer date for households with time-inconsistent recipients, especially if they are also relatively new cardholders. Despite being often statistically insignificant, the double-difference coefficient estimates are economically relevant. For example, in the lower panel, the double differences for Groups 3 and 4 show that, compared to Group 2, consumption for time-inconsistent and new cardholder households is 14 and 16 pesos higher if households live close to a bank than if they live far from it. This is a 13% and 15% difference from a 110 *peso* average consumption in Groups 3 and 4. Patterns for perishables and junk food are similar (results available upon requests).

This finding is consistent with the conjecture that distance from a bank branch helps time-inconsistent and inexperienced cardholders smooth consumption around the transfer date.

[22]We call "new card holders" all transfer recipients who owned a debit card associated with their BANSEFI savings account for 28 months or less (the median time in our sample is 26 months).

It is conceptually consistent with findings in Huffman and Barenstein (2004), Mastrobuoni and Weinberg (2009), and Shapiro (2005), and it helps us reconcile the differences between our main results and the ones in these other studies.

7 Conclusion

We study consumption smoothing around the transfer date for a sample of low-income cash transfer recipients in peri-urban Mexico. Their behavior is consistent with the permanent income hypothesis because food consumption does not increase after receiving the transfer, which is an anticipated and transitory income change. This finding also holds for households with time-inconsistent or impatient recipients as well as for households with high transfer dependence.

Furthermore, we find no evidence of large financial, employment, health, or cognitive changes around the transfer date. This result suggests that smoothing consumption around the transfer date may not be costly for the households in our sample and that the lack of uncertainty in transitory income fluctuations may not impair cognition, unlike in Mani et al. (2013) and like Carvalho et al. (2016).

Finally, our findings suggest that the costs of accessing the transfer may help some households smooth consumption around the transfer date. This access cost, which may be welfare-reducing for some households, may benefit financially inexperienced and time-inconsistent recipients.

The literature acknowledges 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 introducing ordeal mechanisms to transfer access, such as access costs, may help some households smooth consumption around the transfer dates.

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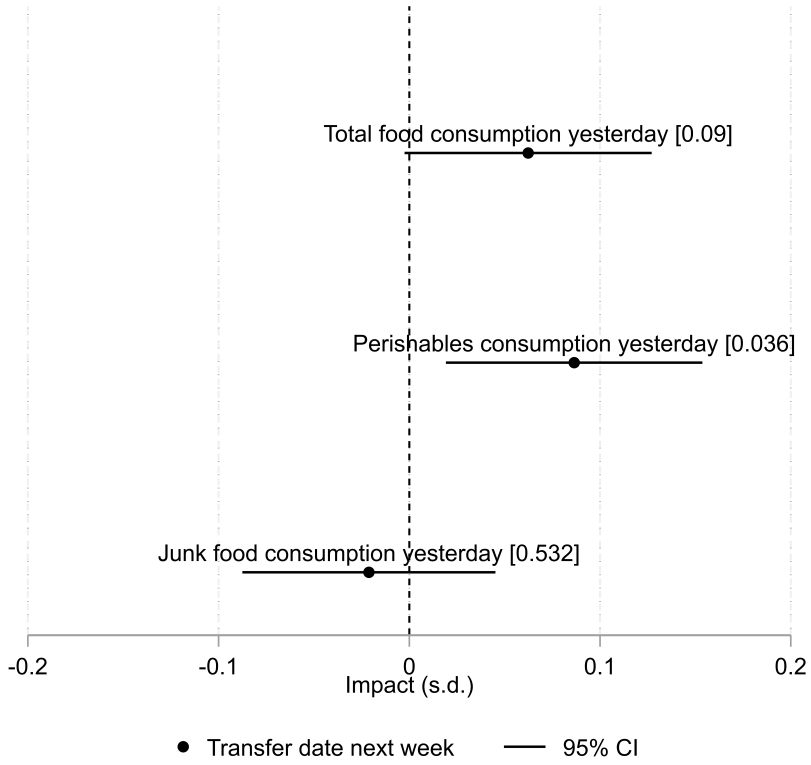
Figures

FIGURE 1: Differences in food consumption outcomes before transfer date



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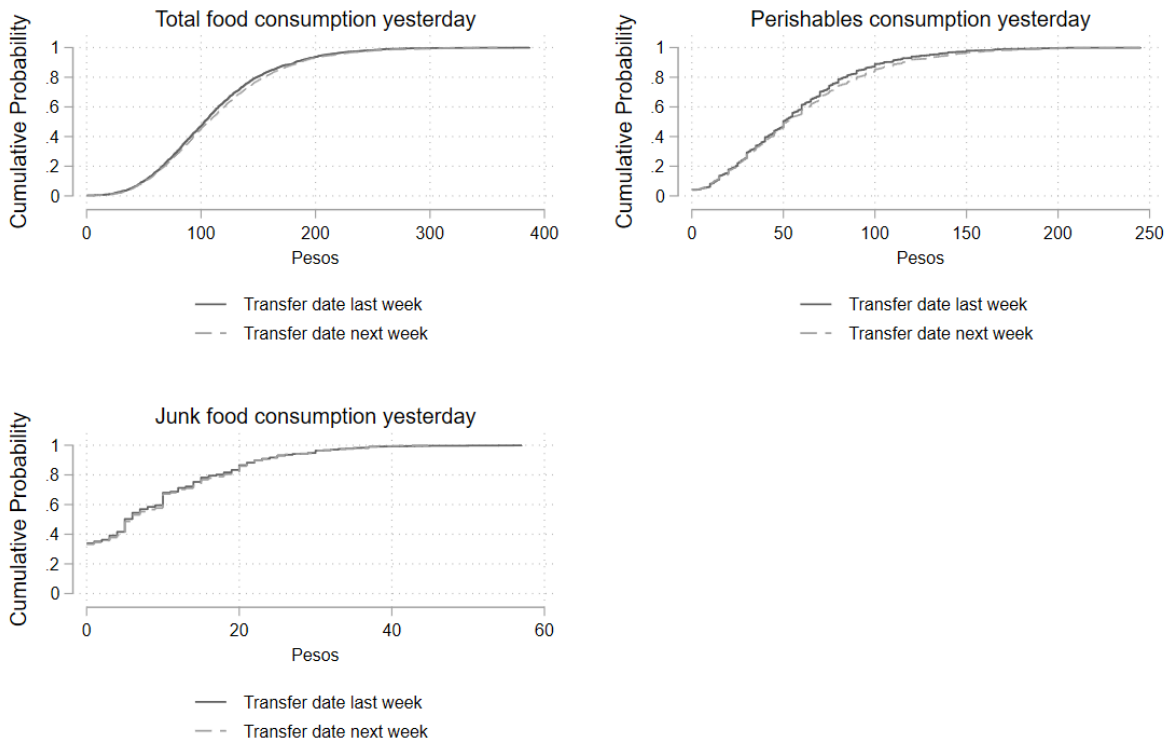
Note: Effect size in standard deviations of the group that just received the transfer. Sharpened q-values in brackets (Anderson 2008). Robust standard errors used to construct confidence intervals.

FIGURE 2: Distribution of food consumption before and after the transfer date

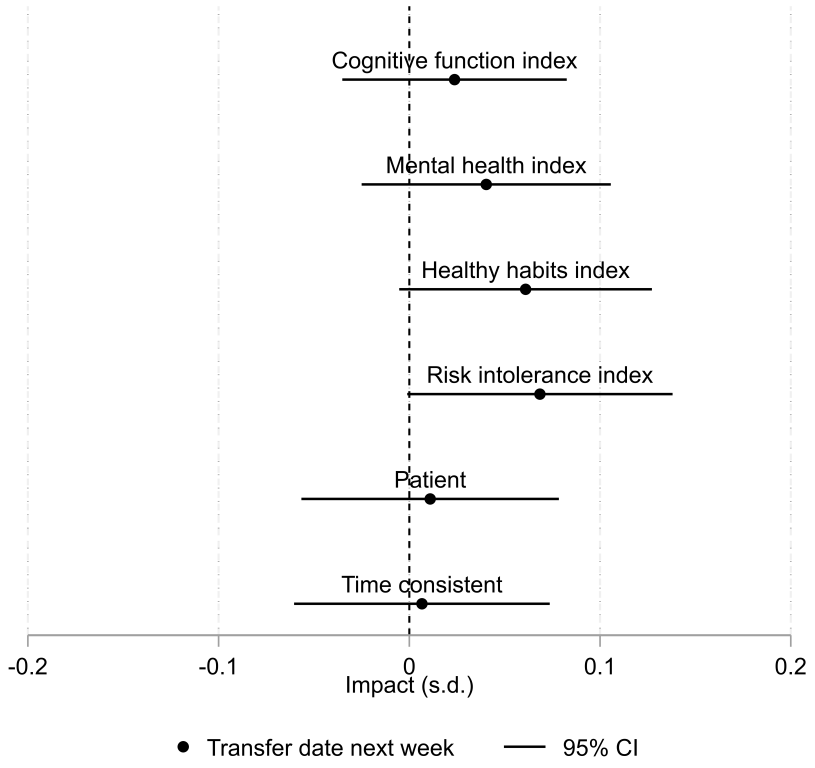


Note: The three panels show the cumulative density functions of total, junk food, and perishable consumption in *pesos* for households interviewed before and after the transfer date.

FIGURE 3: Differences in household finances and labor supply before the transfer date

Note: Effect size in standard deviations of the group that just received the transfer. Sharpened q-values in brackets (Anderson 2008). Robust standard errors used to construct confidence intervals.

FIGURE 4: Differences in cognition, mental health, healthy habits and preferences before the transfer date



Note: Effect size in standard deviations of the group that just received the transfer. We do not compute sharpened q-values because we accounted for multiple inference by creating family-wide indices. Robust standard errors used to construct confidence intervals.

A Appendix

A.1 Validity Checks

This appendix checks the validity of the consumption data. As a validity check, we regress log-food consumption on household size, income, and transfer one by one. We find positive and statistically significant associations in all cases: a one unit increase in household size is associated with an extra 10% consumption (pvalue<0.01), a one thousand peso transfer increase is associated with an extra 7.2% consumption (pvalue<0.01), and a one thousand income increase is associated with an extra 13% consumption (pvalue<0.01).

To check that consumption does not vary by transfer date, Figure A1 shows mean food consumption (overall, and for perishables and junk food separately) by date of PROSPERA transfer. Consumption is generally stable by date. In addition, we tested the hypothesis that payment dates vary systematically across localities. The p-value of the test of joint significance is 0.68. We also checked whether household and transfer size vary by payday (day of the month) and we found that there is no statistically significant association between day of transfer payment and household and transfer size. Results available from the authors.

To check that respondents surveyed on day 1 and days 2 to 4 of the group dates have similar socioeconomic characteristics, we create an indicator for being interviewed on days 2 to 4 and check for balance along household (couple-headedness, household composition, having experienced health/employment shocks in the past year and past fortnight, weekly income, and PROSPERA transfer) and respondent characteristics (age and education). Table A1 shows that, while some of the individual covariates are unbalanced, we cannot reject the test of joint significance (p-value=0.22).

A.2 Further Details about the Outcomes

We consider the following families of outcomes: food consumption; household finances; labor supply; cognition; healthy habits; mental health; and preferences. Below we describe how we computed each outcome variable. Finally, Table A2 lists each outcome variable we consider in this paper (column 1) and provides the specific recall time (column 2).

A.2.1 Food consumption

There are 10 food groups: fruits and vegetables; desserts (for example cookies, pastries, bagged cupcakes, sugar); cereals and grains (e.g, tortillas, bread, pasta soup, rice, boxed cereals); legumes (e.g. beans, chickpeas, lentils); foods of animal origin (e.g. chicken, meat, seafood, eggs, milk, yogurt, cheese); butter, cream, lard, and vegetable oil; alcoholic beverages (e.g. beer, rum, tequila); non-alcoholic drinks (for example soft drinks, syrup or powder to prepare water, coffee or tea); junk food and snacks (for example chips);

and bottled water. For each food group, the survey first asks, "Did this household eat [food name] yesterday?." If the reply is affirmative, then the survey elicits both the quantity consumed (in kilos, liters, pieces, or other, depending on the food groups) and the monetary value of the quantity consumed. The enumerators help participants compute the monetary value for each food group using the quantities consumed.[23]

We use the monetary value of the food groups to compute our three food consumption outcomes: total food consumption yesterday; perishables consumption yesterday; and junk food consumption yesterday.

Our main outcome of interest is total food consumption the day before the interview. 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.[24]

We also calculate perishables food consumption the day before the interview, which is defined as the consumption of fruits and vegetables and foods of animal origin) and junk food consumption yesterday defined as the consumption of junk food and snacks and non-alcoholic beverages.[25]

A.2.2 Household finances

Households' **net savings** are monetary savings, plus credit, minus debt.

Monetary savings are measured using the question "Approximately, as of today, how much money do you have saved in this household? Please do not consider saving on animals, land or property."

Debt is measured using the question "Considering all of this household's loans or credits, how much total money does this household currently owe?

The **Asset fire sales** variable is built by counting the assets sold the previous week. In particular, we consider the question "How many article/s dis you sell or pawn last week?" The list of durable goods (from A to BB) considered includes car, truck, motorcycle, television, DVD player, etc.

A.2.3 Labor supply

We consider labor supply and income separately for adults and children.

Labor income in the last 3 days is built from the individual responses on recent work payments. We ask every household member who work when was he or she last paid. We

[23] We trained our enumerators to guide respondents through the exercise by mentioning commonly-consumed food items within each food group. The enumerators also helped respondents compute the monetary value of all food they ate the previous day. The enumerators then summed up the monetary value of each item in a food group and entered the total in the survey.

[24] PROSPERA beneficiaries are the survey respondents and are generally the household member in charge of buying and preparing food.

[25] About one quarter of households did not consume junk food the previous day and about 4% did not consume any fruits or vegetables.

sum the labor income over those who answer they were paid within the last three days.

The variable **fraction of adults/children who were paid in the last 3 days** is built from the individual responses on recent work payments. We sum adults/children who were paid within the last three days and divide by the number of adults/children in the household.

Similarly, the variable **fraction of adults/children who worked in the last 3 days** is built from individual responses on hours of work supplied. We ask every household member for his or her number of worked hours within the last three days. We sum adults/children with positive hours in the last three days and divide by the number of adults/children in the household.

Finally, **hours worked in the last 3 days** is built from individual responses on hours of work supplied. We ask every household member for his or her number of worked hours within the last three days. We sum over all household members worked hours within the last three days.

A.2.4 Cognition, healthy habits, and mental health

We create three indices for cognitive function, healthy habits, and mental health.

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).[26] (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.[27] To match the one-day recall that we use for food consumption, we changed the recall period in the stress and depression scales from a 2-week or 4-week recall to a one-day recall. This short recall time is non standard. 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

[26] 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."

[27] 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?

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.

A.2.5 Preferences

We created a risk tolerance index, a patience indicator, and an indicator assessing whether the beneficiary exhibits time-consistent preferences. 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.

Regarding 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 1.

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 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.

A.3 Consumption Smoothing Robustness Checks

This Appendix checks the robustness of our findings that households smooth consumption around the transfer date. Classical measurement error in the transfer dates would attenuate the differences between the two groups of households, thus underestimating the true effects on our outcomes. Having administrative records of transfer dates minimizes these concerns. Nevertheless, we repeat the analysis dropping households whose reported transfer date is within plus or minus 3 days since the interview date. Estimate B of Figure A2 shows that the results are qualitatively unchanged.

Grouping households by actual survey date instead of by scheduled survey date may also introduce bias, if consumption is systematically different for households that could not be interviewed on their scheduled date. To address this issue, we alternatively group households by their scheduled date. Estimate C of Figure A2 shows that the results are qualitatively unchanged. One shortcoming of this grouping is that it misclassifies some households, thus underestimating the true transfer date effect.[28] To address this issue, we alternatively drop from the sample all households whose scheduled and actual survey dates do not coincide. Estimate D of Figure A2 shows that the results are qualitatively unchanged.

We also consider alternative specifications. As an alternative to weekday fixed effects, we could have used calendar date fixed effects. We repeat the analysis replacing weekday fixed effects with calendar days fixed effects. Estimate E of Figure A2 shows that the results are qualitatively unchanged.

Lastly, while we computed robust standard errors because the survey date is randomly assigned across households, we could have alternatively clustered standard errors by locality, following a sampling design rationale (Abadie et al. 2017). Estimate F of Figure A2 shows that the results are qualitatively unchanged.

An alternative way to test whether households smooth consumption around the transfer date is to group households by interview group dates, our "experimental" design. We have four groups: respondents surveyed (i) 5 to 8 days before the transfer deposit, (ii) 4 to 1 days before the transfer deposit, (iii) 0 to 4 days after the transfer deposit, and (iv) 4 to 7 days after the transfer deposit. Column 1 of Table A4 shows mean consumption for each of these four groups. Since consumption is measured for the day before the interview, the consumption of households surveyed on day 0 (the day of the transfer receipt) refer to a day *before* the transfer receipt. Therefore, as an alternative way to group household, we assign these households to group 2 – 4 to 1 days before the transfer deposit. Column 1 of Table A4 shows group mean consumption using this alternative definitions of groups 2 and 3. We find that consumption is never lower before the transfer date regardless of how

[28] For example, consider a household scheduled to be interviewed on its transfer date, but which is in fact interviewed two days later. This household describes its food consumption "yesterday," i.e., the day *before* the transfer date. If we go by scheduled interview date, our algorithm assigns it to the "before" group. However, since the survey took place two days after the transfer date, "yesterday's consumption" also took place *after* the transfer date.

we group the households. In fact, consumption for groups 2, 3, and 4 is never statistically different, while group 1 consumption is *higher*.

An additional way to test for consumption smoothing around the transfer date is to use the quantity consumed of food, instead of its monetary value. Quantity consumed may be easier to recall than the peso value of consumption, but it cannot be aggregated across food items in the absence of unit values. Figure A3 shows that quantities consumed do not vary around the transfer date for all food groups.

Appendix Tables and Figures

TABLE A1: Means of predetermined variables by dates of the assigned interview group

	Comparing households interviewed on the 1st day of date group and households interviewed on the subsequent 3 group days	
	Interviewed on the first day	Difference: interviewed on the first day - on the subsequent 3 days
	Mean [SD]	(SE)
	(1)	(2)
Couple headed	0.69	-0.01
	[0.46]	(0.02)
Beneficiary's age	43.7	-0.95*
	[11.87]	(0.55)
Beneficiary's schooling	6.68	0.34*
	[3.62]	(0.18)
Males aged 0-17	1.02	0.03
	[0.97]	(0.05)
Females aged 0-17	0.97	0.01
	[0.99]	(0.05)
Males aged 18-64	1.04	0.06
	[0.79]	(0.04)
Females aged 18-64	1.33	0.01
	[0.72]	(0.04)
Males aged >65	0.08	-0.02**
	[0.28]	(0.01)
Females aged >65	0.11	-0.03*
	[0.32]	(0.01)
Health/employment shock last year	0.46	-0.01
	[0.5]	(0.02)
Health/employment shock last two weeks	0.07	0.00
	[0.26]	(0.01)
Total weekly income (except PROSPERA)	1447.13	100.60*
	[1032.85]	(51.47)
PROSPERA transfer	1609.31	-35.91
	[1000.92]	(47.64)
Test of joint significance (p-value)		0.2279
Sample size:		3,534

Notes: *, **, ***: statistically significance at the 90, 95, and 99 percent level. Columns 1 shows the average outcomes for households interviewed the first day of a date group. Column 2 shows the difference in outcomes between households interviewed the first day of a date group and households interviewed in the subsequent three days of a date group. We regress each outcome on a "Interviewed in the subsequent three days" dummy and show the estimates of the "Interviewed in the subsequent three days in Column 2. Robust standard errors in parentheses. Total weekly income includes all regular labor and non-labor income sources except for the PROSPERA transfer. The PROSPERA transfer is the amount study participants are paid every other month. One US dollar is equivalent to 19.9 Mexican *pesos* in 2021.

TABLE A2: Outcome variables and recall times

Outcome variable (1)	Recall time (2)
Food consumption	
Total food consumption	Day before the interview
Perishables food consumption	Day before the interview
Junk food consumption	Day before the interview
Household finances	
Net savings	Day of the interview
Number of assets sold	Week before the interview
Labor supply	
Labor income last three days, adults	Last three days
Fraction of adults paid last three days	Last three days
Fraction of adults who worked last three days	Last three days
Worked hours last three days, adults	Last three days
Labor income last three days, kids	Last three days
Fraction of kids paid last three days	Last three days
Fraction of kids who worked last three days	Last three days
Worked hours last three days, kids	Last three days
Cognition, healthy habits and mental health	
Cognitive function index	Day before the interview
Mental health index	Day before the interview
Healthy habits index	Day before the interview
Preferences	
Risk tolerance index	Day of the interview
Patient	Day of the interview
Time consistent	Day of the interview

TABLE A3: Means of predetermined variables and balance tests

	Comparing households interviewed 4 to 1 days Before Transfer (Group 2) with households interviewed earlier and later			
		Difference from Group 2:		
	Group 2 4-1 days Before Transfer [SD] (1)	Group 1 8-5 days Before Transfer (SE) (2)	Group 3 0-3 days After Transfer (SE) (3)	Group 4 4-7 days After Transfer (SE) (4)
Couple headed	0.7	-0.01	-0.00	-0.01
	[0.46]	(0.02)	(0.02)	(0.02)
Beneficiary's age	42.92	1.05*	0.73	0.80
	[11.62]	(0.56)	(0.55)	(0.56)
Beneficiary's schooling	6.76	-0.12	0.05	-0.06
	[3.62]	(0.17)	(0.17)	(0.17)
Males aged 0-17	1.1	-0.10**	-0.13***	-0.06
	[1]	(0.05)	(0.05)	(0.05)
Females aged 0-17	0.97	0.02	0.02	-0.01
	[0.95]	(0.05)	(0.05)	(0.05)
Males aged 18-64	1.03	0.02	0.04	0.03
	[0.8]	(0.04)	(0.04)	(0.04)
Females aged 18-64	1.35	-0.05	-0.01	-0.01
	[0.75]	(0.03)	(0.04)	(0.04)
Males aged >65	0.09	-0.01	-0.01	-0.02
	[0.29]	(0.01)	(0.01)	(0.01)
Females aged >65	0.1	-0.00	0.00	-0.00
	[0.31]	(0.01)	(0.01)	(0.01)
Health/employment shock last year	0.48	-0.04	-0.07***	0.00
	[0.5]	(0.02)	(0.02)	(0.02)
Health/employment shock last two weeks	0.07	-0.00	0.01	0.01
	[0.25]	(0.01)	(0.01)	(0.01)
Total weekly income (except PROSPERA)	1475.67	-19.98	-50.26	10.69
	[1024.06]	(49.33)	(46.57)	(51.13)
PROSPERA transfer	1567.85	55.80	72.98	17.43
	[1000.66]	(47.32)	(47.94)	(46.92)
Test of joint significance (p-value)		0.146		
Test of joint significance [before vs. after]		0.784		
Sample size:		3,534		

Notes: *, **, ***: statistically significance at the 90, 95, and 99 percent level. We regress each outcome on Group dummies (Group 2 is the excluded category) using seemingly unrelated regressions. We show the estimates on the Group dummies on Columns 3 to 5. Robust standard errors in parentheses. Total weekly income includes all regular labor and non-labor income sources except for the PROSPERA transfer. The PROSPERA transfer is the amount study participants are paid every other month. One US dollar is equivalent to 19.9 Mexican *pesos* in 2021.

TABLE A4: Mean consumption by interview group

	Assigned dates			Assigned dates (but day 0 assigned to group 2)		
	Total (1)	Perishables (2)	Junk food (3)	Total (4)	Perishables (5)	Junk food (6)
Panel A:	Mean [SD]					
8 to 5 days before transfer (Group 1)	116.15 [53.45]	61.74 [40.44]	9.32 [9.88]	116.15 [53.45]	61.74 [40.44]	9.32 [9.88]
4 to 1 days before transfer (Group 2)	111.84 [55.53]	57.95 [42.34]	8.85 [9.71]	111.27 [52.97]	57.69 [39.78]	8.7 [9.64]
0 to 3 days after transfer (Group 3)	110.2 [49.97]	56.9 [36.93]	8.57 [9.45]	108.52 [51.41]	54.79 [39.13]	8.82 [8.93]
4 to 7 days after transfer (Group 4)	110.18 [55.52]	55.92 [39.27]	9.16 [10.41]	110.18 [55.52]	55.92 [39.27]	9.16 [10.41]
Panel B:	Pairwise tests of equality of consumption, p-values					
Group 1 and 2	0.09	0.04	0.31	0.03	0.01	0.13
Group 1 and 3	0.02	0.01	0.11	0.10	0.04	0.56
Group 1 and 4	0.02	0.00	0.73	0.02	0.00	0.73
Group 2 and 3	0.52	0.58	0.55	0.54	0.38	0.88
Group 2 and 4	0.52	0.28	0.51	0.63	0.29	0.27
Group 3 and 4	0.99	0.60	0.21	0.72	0.74	0.69
Sample size	3534	3534	3534	3534	3534	3534

Notes: Panel A shows the mean and standard deviation of each consumption outcome by interview group. Panel B reports the p-value of a pairwise test of equality of means for each consumption outcome across interview groups. Panel C reports the p-value of a joint significance test from a regression of each consumption outcome on interview group dummies with robust standard errors. Columns 1, 2, and 3 group households according to the original interview schedule. In Columns 4, 5, and 6, households that received the transfer the day of the interviewed are included in Group 2 since their consumption report is from the previous day.

TABLE A5: Differences in food consumption before the transfer date

	Food consumption yesterday		
	Total (1)	Perishables (2)	Junk food (3)
Panel A:	Overall Effects		
Before Transfer	3.29*^	3.29**^^	-0.21
	(1.74)	(1.31)	(0.34)
Panel B:	Effects by subgroup		
Before Transfer	2.81	2.62	-0.03
	(2.69)	(1.99)	(0.53)
Before Transfer	-0.1	-1.05	-0.69
x Impatient	(3.72)	(2.82)	(0.73)
Before Transfer	0.2	2.04	0.12
x Time Inconsistent	(3.43)	(2.6)	(0.66)
Before Transfer	1.09	-0.42	-0.17
x Large Transfer	(3.77)	(2.87)	(0.72)
Mean for households that just received transfer	110.19	56.41	8.86
Sample size	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 from sharpened q values (Anderson 2008). Panel A shows estimates of α_1 from equation 1. Panel B shows estimates from adding subgroup dummy indicators and interacting them by the "Before Transfer" indicator in equation 1. All regressions control for a set of observable characteristics, weekday dummies, and state dummies. Robust standard errors in parentheses. Before Transfer 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 annual transfer/income ratio greater than 0.20.

TABLE A6: Effects of days since transfer on log food consumption

Panel A:	All households
Days since last transfer	0.00055*
	(0.00031)
Test of equality with Shapiro (2005)	$p < 0.05$
N	3522
Panel B:	Drop households surveyed 3 days before/after transfer
Days since last transfer	0.00060
	(0.00042)
Test of equality with Shapiro (2005)	$p < 0.05$
N	2564

Notes: *, **, ***: statistically significant at the 90, 95, and 99 percent level. We estimate a regression of log of food total consumption reported on the number of days since the household received the last PROSPERA transfer. All regressions control for a set of observable characteristics, weekday dummies, and state dummies. Panel A uses all households in the sample. Panel B drops surveys within 3 days from the transfer date. Column 1 shows the results when using households in the whole range of the transfer/income ratio. Column 2 shows the results when restricting the sample to households with a transfer/income ratio greater than 0.20. We test the hypothesis that these coefficients are identical to the ones in table 1, column 4, from Shapiro (2005). That coefficient estimate is -0.0073 (SE 0.0038). We can reject this hypothesis at the 95% confidence level.

Table A7: Differences in finances and employment before the transfer date

			Adults				Kids			
	Net Savings (1)	Number of Assets Sold (2)	Labor Income Last 3 days (3)	Fraction Paid Last 3 days (4)	Fraction Who Worked Last 3 days (5)	Worked Hours Last 3 days (6)	Labor Income Last 3 days (7)	Fraction Paid Last 3 days (8)	Fraction Who Worked Last 3 days (9)	Worked Hours Last 3 days (10)
Panel A:					Overall Effects					
Before Transfer	278.56	0.01	-4.07	0.01	0.02**	0.8	-0.85	0.01	0.01	0.05
	(176.93)	(0.01)	(24.78)	(0.01)	(0.01)	(0.67)	(4.49)	(0.01)	(0.01)	(0.26)
Panel B:					Effects by Subgroup					
Before Transfer	197.22	0.01	-13.24	-0.01	0.02	1.62	1.93	0.01	0	0.14
	(281.34)	(0.01)	(39.28)	(0.02)	(0.01)	(1.04)	(8.01)	(0.01)	(0.01)	(0.42)
Before Transfer	134.28	0.01	-3.46	0.02	0.01	-2.89**	-2.41	0.01	0.01	-0.01
x Impatient	(390.41)	(0.01)	(51.71)	(0.03)	(0.02)	(1.38)	(12.07)	(0.01)	(0.02)	(0.6)
Before Transfer	8.28	0.01	51.03	0.03	0.01	-0.10	-3.17	0.01	0.02	0.04
x Time Inconsistent	(349.74)	(0.01)	(46.81)	(0.02)	(0.02)	(1.28)	(9.37)	(0.01)	(0.02)	(0.53)
Before Transfer	180.95	0.01	-67.5	-0.02	0.01	-0.11	-4.09	-0.01	-0.03*	-0.57
x Large Transfer	(381.82)	(0.01)	(42.41)	(0.03)	(0.03)	(1.29)	(6.30)	(0.01)	(0.02)	(0.44)
Mean for households that just received transfer	-1578.16	0.01	446.49	0.35	0.65	32.52	17.39	0.03	0.06	1.96
Sample size	3,252	3,534	3,423	3.423	3.423	3,423	2,913	2,913	2,913	2,913

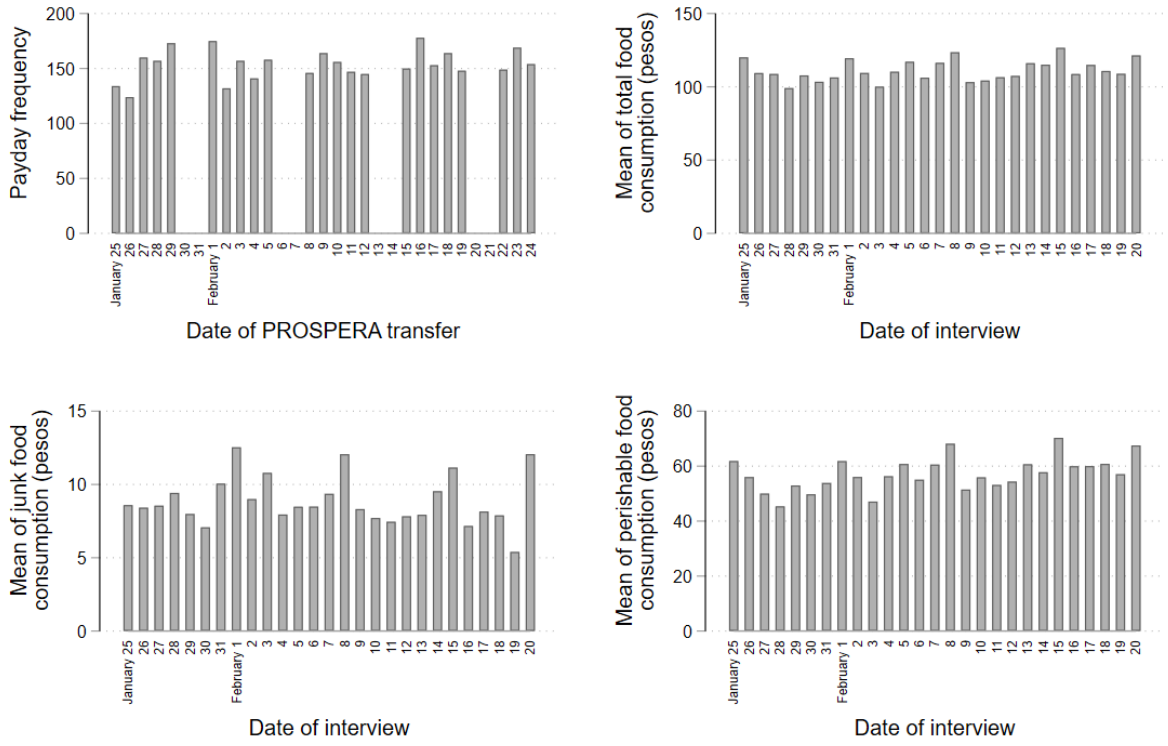
Notes: *, **, ***: statistically significant at the 90, 95, and 99 percent level. The symbols ^, ^^, ^^^ mean statistical significance at the 90, 95, and 99 percent level from sharpened q values (Anderson 2008). These symbols do not appear in the table because none of the estimates has sharpened q values that are less than 0.10. Panel A shows estimates of α_1 from equation 1. Panel B shows estimates from adding subgroup dummy indicators and interacting them by the "Before Transfer" indicator in equation 1. All regressions control for a set of observable characteristics, weekday dummies, and state dummies. Robust standard errors in parentheses. Before Transfer 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 annual transfer/income ratio greater than 0.20. The sample size in columns 7-10 is smaller because some households do not have 5-17 year old children.

TABLE A8: Differences in cognition, mental health, healthy habits, and preferences before the transfer date

	Cognitive function index (1)	Mental health index (2)	Healthy habits index (3)	Risk intolerance index (4)	Patient (5)	Time consistent (6)
Panel A:			Overall Effects			
Before Transfer	0.02	0.04	0.06*	0.07*	0	0
	(0.03)	(0.03)	(0.03)	(0.04)	(0.02)	(0.02)
Panel B:			Effects by subgroup			
Before Transfer	0.08	0.04	0.05	0.02	0	0
	(0.05)	(0.05)	(0.05)	(0.05)	(0.02)	(0.02)
Before Transfer	0.05	0.03	0.06	0.12		
x Impatient	(0.06)	(0.07)	(0.07)	(0.08)		
Before Transfer	-0.1*	-0.07	0.03	0.04		
x Time Inconsistent	(0.06)	(0.07)	(0.07)	(0.07)		
Before Transfer	-0.08	0.09	-0.06	-0.01	0.03	0.01
x Large Transfer	(0.07)	(0.08)	(0.07)	(0.08)	(0.04)	(0.04)
Mean for households that Just Received Transfer	0.00	0.00	0.00	0.00	0.71	0.53
Sample size	3,375	3,372	3,369	3,199	3,373	3,375

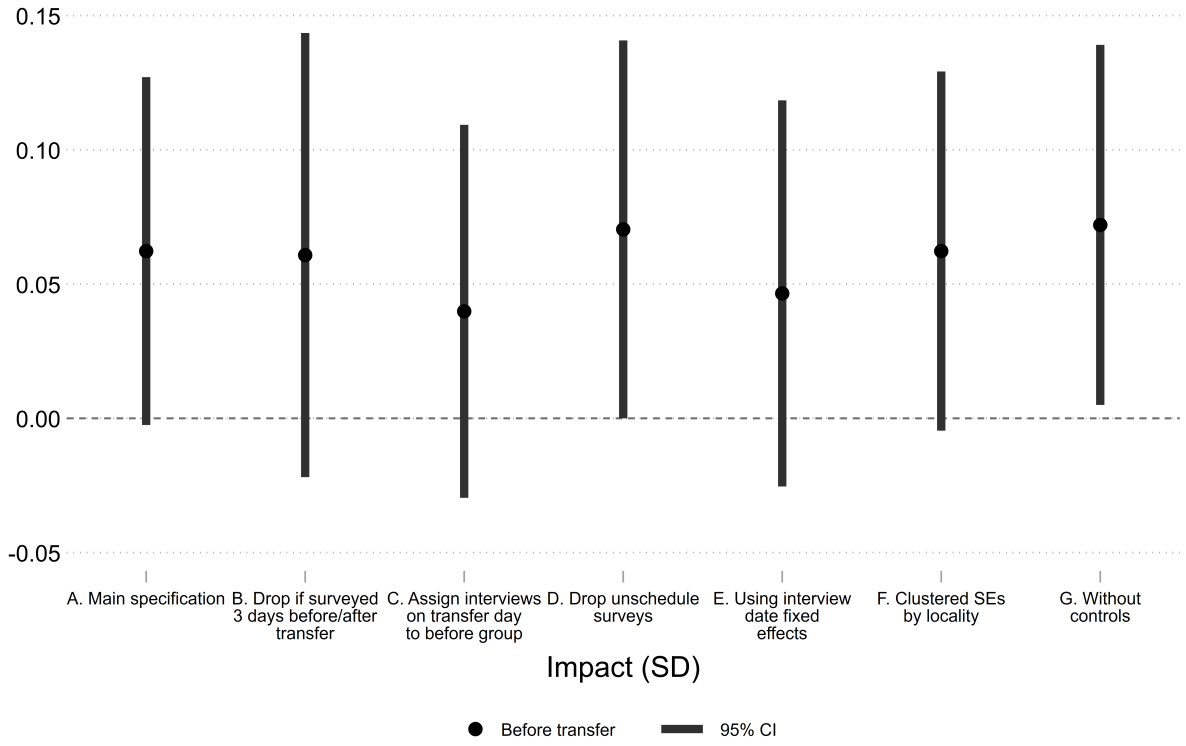
Notes: *, **, ***: statistically significant at the 90, 95, and 99 percent level. We do not compute sharpened q values because we accounted for multiple inference by creating family-wide indices. Panel A shows estimates of α_1 from equation 1. Panel B shows estimates from adding subgroup dummy indicators and interacting them by the "Before Transfer" indicator in equation 1. All regressions control for a set of observable characteristics, weekday dummies, and state dummies. Robust standard errors in parentheses. "Before Transfer" 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 annual transfer/income ratio greater than 0.20.

FIGURE A1: Transfer frequency and consumption means by transfer date



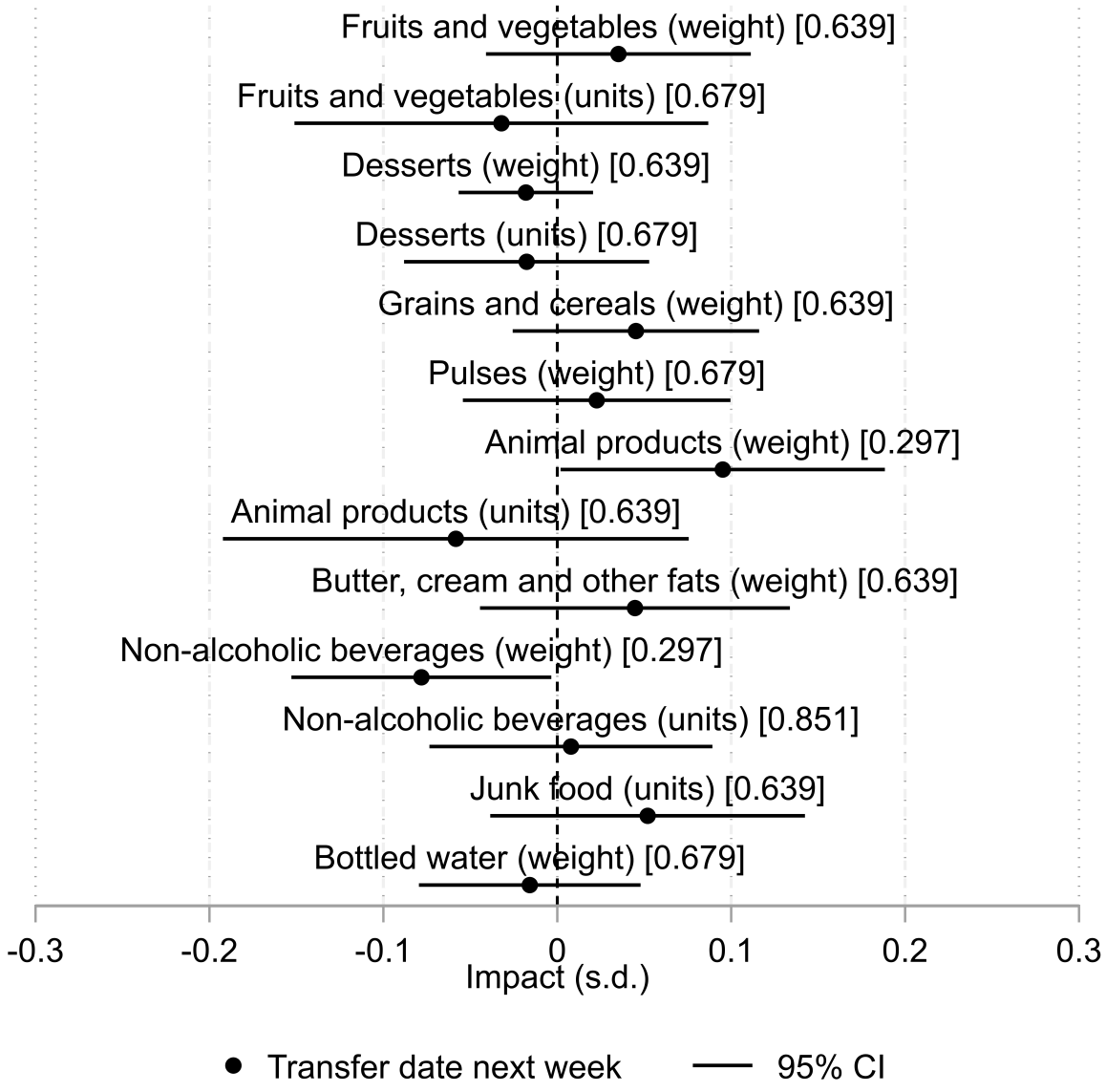
Note: The top left panel shows the distribution of pay dates in our sample (from administrative records). The remaining three panels show the distribution of previous day's total, perishable, and junk food consumption by transfer date.

FIGURE A2: Consumption smoothing – Robustness checks



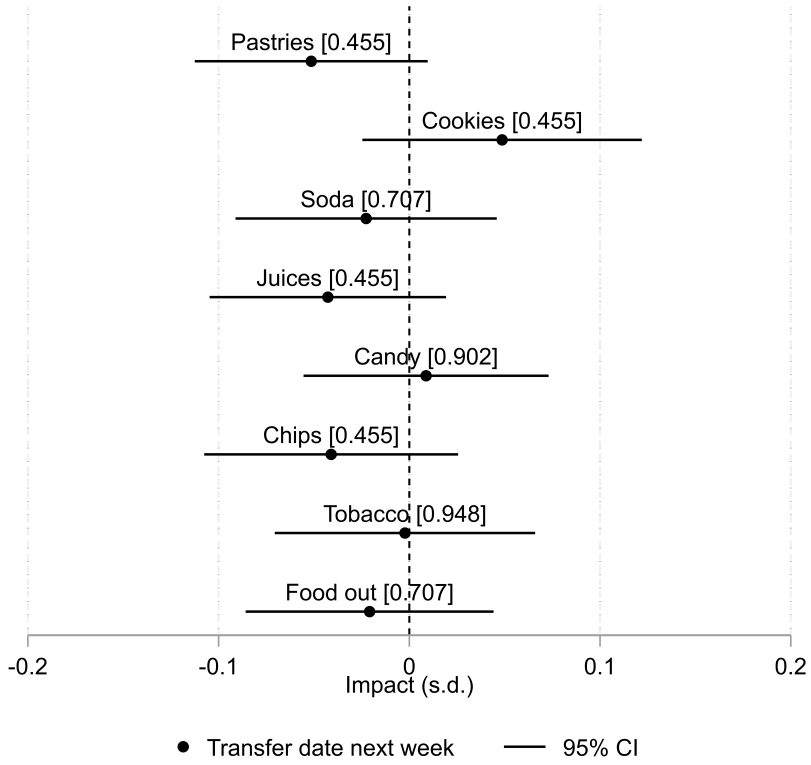
Note: Effect size in standard deviations of the group that just received the transfer. A: estimate from the full sample, with controls. B: drop surveys within 3 days from the transfer date. C: assign households interviewed on the same day they received the transfer to the "Before Transfer" group, as their previous day consumption refers to the day before receiving the transfer. D: drop surveys that did not take place on scheduled date. E: same as specification A, but replace weekday fixed effects with interview date fixed effects. F: same as specification A, but cluster standard errors by locality. G: same as specification A, but drop all controls. Robust standard errors used to construct confidence intervals unless mentioned otherwise.

FIGURE A3: Differences in consumption before the transfer date using quantities consumed



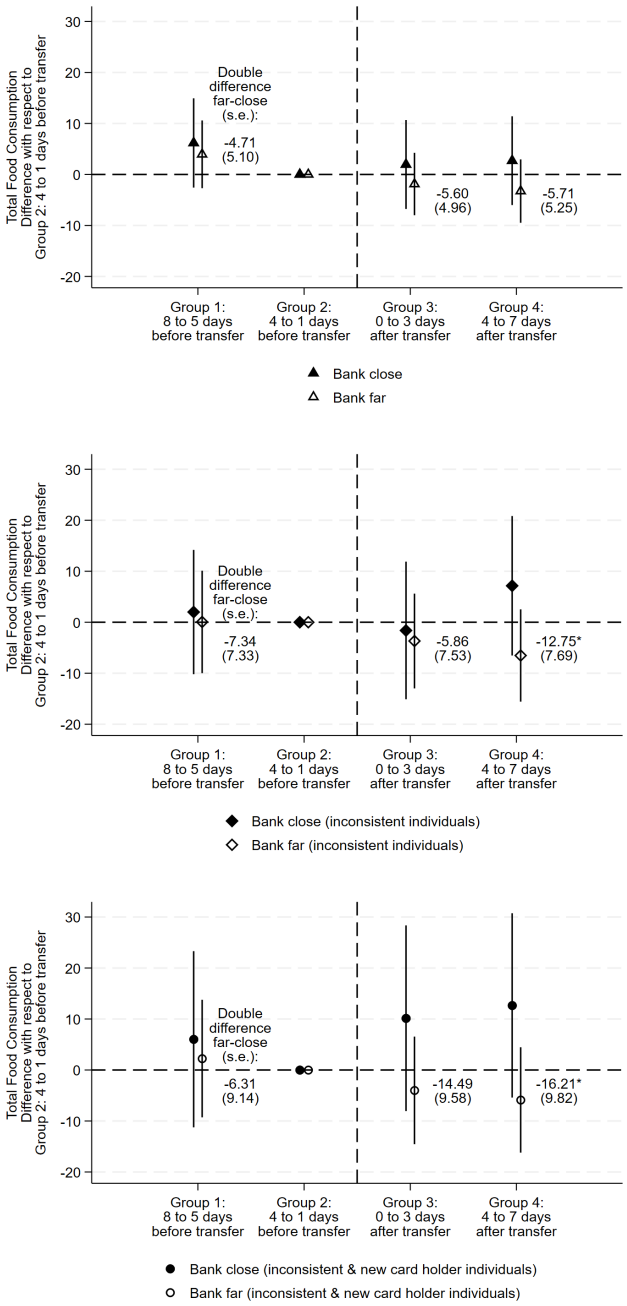
Note: Effect size in standard deviations of the group that just received the transfer. Sharpened q-values in brackets (Anderson 2008). Robust standard errors used to construct confidence intervals.

FIGURE A4: Differences in consumption of temptation goods before the transfer



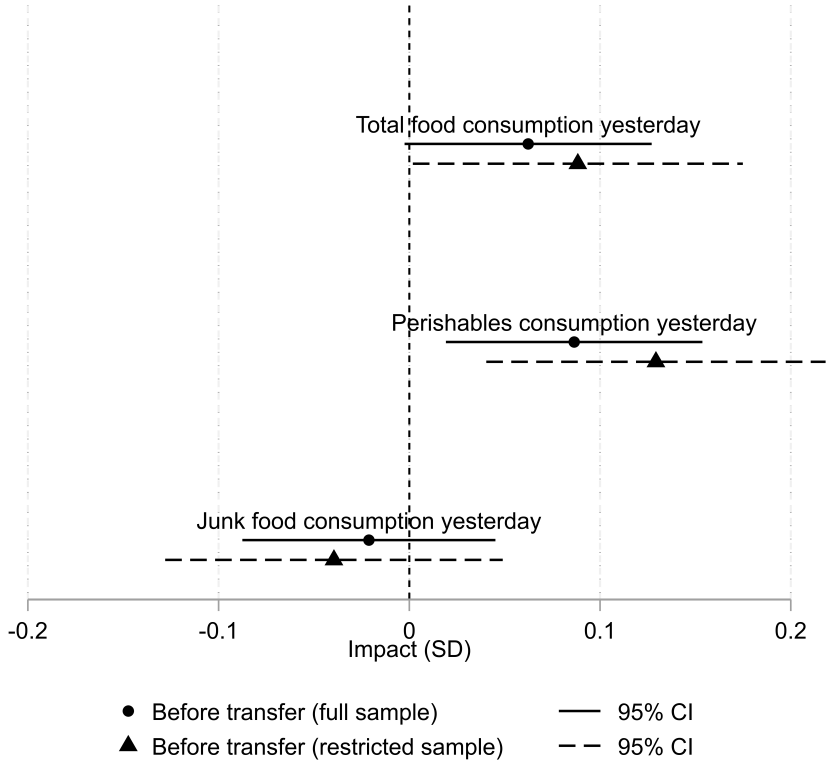
Note: Effect size in standard deviations of the group that just received the transfer. Pastries, cookies, sodas, juices, candy, and chips are in units (such as cans or bags) consumed by the survey respondent the day before the survey. Tobacco and food outside the home are in pesos spent by the household the day before the interview. Sharpened q-values in brackets (Anderson 2008). Robust standard errors used to construct confidence intervals.

FIGURE A5: Total food consumption by interview date and distance from bank



Note: Effect size in standard deviations of the group that just received the transfer. Robust standard errors used to construct confidence intervals. "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. "New card holders" are households that owned a debit card associated with their BANSEFI savings account for 28 months or less (the median time in our sample).

FIGURE A6: Differences in consumption outcomes before the transfer date - full sample and restricted sample (households without daily income earners)



Note: Effect size in standard deviations of the group that just received the transfer. Robust standard errors used to construct confidence intervals.