

Consumption Smoothing and Economic Behavior in the Presence of Expected and Unexpected Shocks

Manuela Angelucci*

Carlos Chiapa†

Silvia Prina‡

Ruben Irvin Rojas Valdes§

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Abstract

We study whether the economic behavior of the poor differs in the presence of expected and unexpected income shocks using data from 3,000 low-income households beneficiaries of PROSPERA, Mexico's conditional cash transfer (CCT) program. We consider as expected income shocks the bimonthly PROSPERA transfers (which are positive and transitory income changes) and as unexpected income shocks, health and employment shocks (which are negative and potentially more permanent income changes). To control for the potential endogeneity of these income changes, we exploit plausibly exogenous variation in their timing. Specifically, we randomly interview beneficiaries who have received a transfer in the last 1 to 7 days and beneficiaries who are about to receive a transfer within a week, thus generating exogenous variation in CCT delivery dates. Similarly, we compare households that suffered a job or health shock in the past 2 weeks with households that experienced this shock in the past year. We find that an expected shock (having just received a CCT) does not change any of the outcomes we consider besides increasing disposable income. Conversely, having experienced an unexpected shock reduces labor supply, food security, mental health, and healthful behavior, but it does not affect parenting, cognition, risk and time preferences, and expectations and aspirations about children's educational attainment.

JEL Codes: J12, J13, I15, I18

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*Department of Economics, University of Michigan, mangeluc@umich.edu.

†Bates White, LLC

‡Department of Economics, Case Western Reserve University, silvia.prina@case.edu

§University of California at Davis, rirojas@ucdavis.edu

1 Introduction

Households face income fluctuations, which affect their economic behavior and wellbeing. The literature has traditionally focused on the impact of income changes on consumption with and without binding financial constraints. We add to this literature in two dimensions. First, we consider additional outcomes related to human capital investment (i.e. current behaviors with long-term repercussions such as healthful habits, parenting, child school attendance). Second, we explore the potential mechanisms, besides financial market imperfections, through which these outcomes may be affected – risk and time preferences, cognition, and mental health.

Our conjecture is that, besides affecting current consumption, income changes may also affect future consumption by changing investment in human capital for the self and one’s offspring. Cognitive biases may limit the *desire* to invest by changing risk and time preferences: present bias, impatience, and risk tolerance may lead to less smooth consumption, with offsetting effects on investment (present bias and impatience hinder investment, while risk tolerance increases it). Impaired cognition may limit the agents’ *proficiency* in making the cognitively complex decisions needed to invest optimally in the presence of variable and unpredictable income. Lastly, worse mental health, as measured by feeling not in control of one’s life, stressed, or depressed, may limit the agents’ *will* to make optimal inter-temporal decisions. These effects should be stronger for unexpected than expected income changes, and should decrease with financial inclusion, which helps households smooth consumption and investment in the presence of income variation.

This paper considers the effects of expected and unexpected income changes for households beneficiary of PROSPERA, Mexico’s conditional cash transfer program. We consider two classes of income fluctuations: the bimonthly PROSPERA transfer, which is an expected, positive, and transitory income fluctuation, and health- and employment-related shocks, which are unexpected, negative, and potentially more permanent income fluctuations.

To measure the effect of an expected income fluctuation, we interviewed 3,534 PROSPERA

beneficiaries either the week before or the week after receiving their bimonthly cash transfer. For this income change, the identification comes from the fact that both the day of the interview and the day of the transfer receipt are exogenous. First, households that are paid on, e.g., the 10th day of the month are not systematically different from households paid on the 11th day of the month. Second, conditional on being paid at a given date, we randomly interviewed some households before and after their pay-date.

To measure the effect of an unexpected income fluctuation, we also collected data on employment and health shocks suffered by household members in the previous 12 months, including the dates in which these shocks were experienced. Since the likelihood of experiencing these shocks may be correlated with unobserved determinants of our outcomes of interest, we rely on the timing of the shock by considering only the 1,617 households that experienced at least one such shock in the previous 12 months. Within this set, we grouped households depending on whether they experienced the latest shock in the previous two weeks or sooner. Our identification assumption is that experiencing this type of shock sooner or later within the previous year is exogenous.

We have six main findings. First, we find that expected and unexpected income fluctuations have similar magnitude (about 10-15% of income), but affect household finances and labor supply differently. For the former case, the lower income is from not having received a cash transfer recently, while for the latter is due to lower labor supply and income.

Second, the effects we find are consistent with the Life-Cycle Permanent Income Hypothesis: unexpected income shocks reduce food security and consumption more than expected income shocks, for which these two outcomes do not vary. However, these effects are small and show that households by and large smooth consumption in both cases. This is consistent with the LCPIH and with recent efforts to provide financial tools to the poor. Specifically, each beneficiary receives a debit card linked to a free savings account. A subset of beneficiaries further receives a line of credit and (health?) insurance (please check what type of insurance PROIIF provides). The lack in drop of food consumption in response to an ex-

pected transitory income shock is in contrast with the existing literature, which finds, for the United States, that food consumption decreases before pay dates for low-income households (e.g., Stephens 2003). Since PROSPERA provides its beneficiaries with a savings account, a debit card, and, for some, a line of credit, this effort at financial inclusion may have helped households better smooth expected income changes and may explain the difference between our findings and the rest of the literature.

Third, the two income shocks have different effects on healthy habits and mental health. While being hit by an unexpected employment or health shock decreases the beneficiary's healthy habits (exercising, sleeping, brushing teeth, and washing hands), being about to receive a conditional cash transfer increases healthy habits.

Fourth, we look at stress, anger, and depression to add to the recent literature that has documented a relationship between income and mental health. We find that people who have not received the PROSPERA transfer in a long time and who, therefore, are less depressed than people who have just been paid. This effect is driven by beneficiaries with below median schooling. Conversely, people who recently experienced a health or employment shock are more depressed than people who experienced similar shocks earlier in the year. This differential response may depend on the nature of the two types of income changes considered - an unexpected, possibly large, shock, which likely increases permanent income, versus a smaller and expected transitory income change.

Fifth, we study the effect of expected and unexpected income changes on cognition to contribute to the nascent literature on poverty and cognitive function. We find that in our setting, income fluctuations do not seem to affect cognition, on average. However, we find that cognition is statistically lower (and lower than zero) when poorer households, defined as households above median transfer-income ratio, have not received the transfer for a long time. This is in line with Carvalho et al. (2016) who find no effect of pay-date on cognition when the income change is relatively small, while Mani et al. (2013) find that farmer's cognition decreases before harvest, when their income is lowest, and the income change is

much larger.

Finally, we also consider the effect of income fluctuations on risk and time preferences to test the hypothesis that preferences are malleable. We find that people who have not received the PROSPERA transfer in a long time are more risk averse, with no differential effects by poverty. Unlike Carvalho et al. (2016), who find that scarce resources affect one's willingness to delay gratification, we do not find any impact of income fluctuations on time preferences.

The remainder of the paper is organized as follows. Section 2 describes the data and setting, Section 3 explains the identification strategy and estimation, and Section 4 presents the results, looking at the effects for the entire sample first and then studying the heterogeneous effects. Finally, section 5 concludes.

2 Data and Balance Tests

PROSPERA is one of the largest and most well-known conditional cash transfer programs worldwide, covering approximately 6.8 million households and 28 million people, or about one fourth of Mexican households (Ministry of Social Development, 2017). It was started in 1997 by the Mexican government in an effort to alleviate current poverty and to break the intergenerational transmission of poverty.¹

The primary objective of the program is to improve the educational, health, and nutritional status of poor families, particularly of children and mothers.² PROSPERA delivers bimonthly cash transfers to its beneficiaries conditional on sending their children to school and having preventive health check-ups. In the 2002-2005, Oportunidades started opening savings account to some of its beneficiaries living in urban areas and depositing its conditional cash transfers (CCTs) directly into these savings accounts. Next, in 2009, the government

¹The first name of the program was PROGRESA (Programa de Educación, Salud y Alimentación). Since then the program has changed name twice: it was called Oportunidades and it is now called PROSPERA. We use its current name.

²For a more detailed description of the program and evaluation data set, see Gertler (2000) and Skoufias (2005). For a review of the program's impacts and results, see Parker and Todd (forthcoming).

started issuing debit cards to beneficiaries who were receiving their CCTs directly deposited into a savings accounts. The cards enable account holders to withdraw cash and check account balances at any bank’s ATM, as well as to make electronic payments.³ This allows beneficiaries to withdraw and spend the cash transfer as soon as it is readily available. Furthermore, all beneficiaries have a calendar listing the dates in which their transfers will be deposited in their savings account.

We surveyed 3,534 PROSPERA beneficiaries who received their CCTs directly into a savings account linked to a debit card. Our sample considered beneficiaries living in 52 localities in six states.⁴ The sample was interviewed between January 25, 2016 and February 25, 2016. We had information about the CCT pay dates for each beneficiary household in our sample. Accordingly, within each locality, we randomly assigned beneficiary households to be interviewed 0-6 days *after* receiving their CCT pay date and 1-7 days *before* their CCT pay date.

The survey collected data pertaining to both the household and the PROSPERA’s beneficiary, who was the survey respondent. It included a series of questions to measure income, labor supply, finances, food security, and consumption, investment in human capital, mental health and cognitive ability, and preferences.

To measure the effect of an expected income change, we interviewed households either the week before or the week after receiving their bimonthly cash transfer, whose date varies across households depending on the day in which they enrolled in the program . For this income change, the identification comes from the fact that both the day of the interview and the day of the transfer receipt are exogenous. First, households that are paid on, e.g., the 10th day of the month are not systematically different from households paid on the 11th day

³For a more detailed description of the savings accounts and debit cards roll out to PROSPERA’s beneficiaries and its impact, see Bachas et al. (2017). Beneficiaries can make two free ATM withdrawals per bimester at any bank’s ATM; additional ATM withdrawals are charged a fee that varies by bank. Bachas et al. (2017) exploit the rolling-out of the introduction of debit cards to study their impact on savings and find that debit cards caused savings to increase for PROSPERA’s beneficiaries.

⁴For budgetary purposes we only surveyed PROSPERA beneficiaries in six central states of Mexico: Distrito Federal (2 localities), Hidalgo (8 localities), Estado de México (31 localities), Michoacán (2 localities), Morelos (4 localities), and Veracruz (5 localities).

of the month. Second, conditional on being paid at a given date, we randomly interviewed some households before and after their pay-date.

To measure the effect of an unexpected income change, we also collected data on employment and health shocks suffered by household members in the previous 12 months, including the dates in which these shocks were experienced.⁵ Since the likelihood of experiencing these shocks may be correlated with unobserved determinants of our outcomes of interest, we rely on the timing of the shock by considering only the 1,617 households that experienced at least one such shock in the previous 12 months. Within this set, we grouped households depending on whether they experienced the latest shock in the previous two weeks or sooner. Our identification assumption is that experiencing this type of shock sooner or later within the previous year is exogenous.

To provide evidence consistent with our identification assumption, we regress dummies for the likelihood of experiencing the expected and unexpected income changes sooner on the marital status, age, and education of the respondent, the age and education of her spouse, if present, numbers of household members grouped by gender and age (0 to 17, 18 to 65, and older than 65), and average monthly PROSPERA transfer per adult equivalent. These are all the plausibly predetermined variables available in our data. We also include dummies for the week day in which the interview was conducted, as we do in all remaining regressions.⁶ We cluster the standard error by locality, as we do for all other specifications in our data analysis.

Table 1 shows the results from the balance tests. Specifically, column 1 shows the mean of the relevant group, while column 2 shows the estimate of OLS regressions of each group dummy on the predetermined covariates. In general, the variables we consider do not differ across groups, consistent with the F tests of their joint significance, which are never statisti-

⁵In particular, we define a household to receive a health shock if any of the household members was sick for a long time or if some of the household members died. We define households to receive an employment shock if they suffered unemployment for a long period of time or if some household member lost its business.

⁶We also ran our main analysis without weekday indicators and check that our conclusions still hold. These results are available upon request.

cally different from zero. The two exceptions are that households that are about to receive the cash transfer have husbands with 0.3 more schooling years and that households that just suffered an unexpected shock have 0.07 more older female members. We control for all characteristics shown in Table 1 in our main analysis.

We repeated the balance tests for the groups that received the PROSPERA transfer a week prior or after the interview date, limiting the sample to households that have experienced employment or health shocks in the previous 12 months. The characteristics are balanced also within this subgroup.⁷

We consider outcomes at the household and at the respondent level. At the household level, we divide our outcomes of interest in two broad categories: Income, labor supply, and finances, and food security and consumption.

Income, labor supply, and finances. We measure total weekly household income as the sum of wage and non-wage income earned by all household members the week prior to the survey date. Regarding labor supply, we consider four measures that focus on the three days prior to the interview date: total household labor income; the fraction of adult household members who received a wage payment; the number of worked hours per individual older than 18; and the fraction of adults that supplied labor. Regarding household finances, we measure the total amount of household saving and credit and the assets owned and purchased by the household the week prior to the survey date. We consider the following categories of assets owned or bought last week: vehicles, media devices, house appliances, house durables, business assets, and animals.

Food security and consumption. To measure food security, we create an index using three questions to assess the availability of food in the household last week.⁸ Regarding food consumption, we consider four measures that focus on the day prior to the survey date:

⁷Results available upon request.

⁸The three questions are: last week, on how many days did any members of this household ask for money or borrow money to eat? Last week, in how many meals there was not enough food to eat and some adults in the households remained hungry? Last week, in how many meals there was not enough food to eat and some children in the households remained hungry?

total food consumption at the household level; perishable food consumption (i.e. meat, fruits and vegetables, eggs, and milk); instantaneous food consumption (i.e. perishable food consumption and food consumed outside the household); and junk food consumption (i.e. snacks and non-alcoholic beverages, such as sodas, tea, and coffee).

In Table A.1 we present the summary statistics of the outcomes at the household level and test for the balance of these measures across the groups of interest. Column 1 shows the mean for the group of households who received a CCT in the past week, column 3 shows the mean for the group of households that experienced an unemployment or health shock in the last year excluding the last two weeks, and columns 2 and 4 report the coefficient on treatment assignment (1=will receive a transfer within a week, 0=has already received a transfer for column 2, and 1=experienced a shock in the past two weeks, 0=did not for column 4) when the variable in the row is regressed on treatment assignment.

Not surprisingly, columns 1 and 2 show that beneficiary households who are about to receive their CCT have 151 pesos less (about 10% less) than beneficiary households who receive the transfer the previous week, who have on average a weekly income of 1,568 pesos. Beneficiary households who will receive a transfer in the next seven days are also more likely to work more, earning 56 pesos more from labor income in the past three days (12% more than the comparison group that earned 456 pesos from labor income in the past three days) and working more hours in the three days.

Considering columns 3 and 4, we see that households hit by an unemployment or health shock in the past two weeks have 189 pesos less (about 12.5% less) than beneficiary households who were hit by a shock in the last 12 months, who have on average a weekly income of 1,507 pesos. It is worth noting that the impact on household weekly income of expected and unexpected shocks is of a similar size. Also, beneficiary households who were hit by a shock recently, have earned less labor income, worked less hours, and had a lower fraction of adult members being paid and working in the past three days.

At the respondent level, we divide our outcomes of interest in three broad categories:

parental investment in own's and children's human capital; mental health and cognitive ability; and risk and time preferences.

Parental investment in own's and children's human capital. We consider the following variables measured the day prior to the survey date: minutes spent working, minutes spent sleeping, number of times the respondent brushed her teeth, and an indicator variable for washing hands properly with soap and water. We also use these four variables to create a healthy habits index. Furthermore, we measure the amount of time the respondent spent with her kids the day before the survey date. And we build a variable measuring the difference between the actual amount of time spent the respondent spent with her children and the desired amount of time she would like to spend them. Moreover, we measure the aspirations and expectations in terms of years of schooling the respondent has for each of her girls and boys. Finally, we measure the fraction of kids who attended school in the past three days.

Mental health and cognitive ability. We create a cognitive index based on three tasks: forward and backward digit recall, and a battery of Raven's matrices. The aggregate cognitive index is created by normalizing the score of each individual test, adding up the sum of scores, and normalizing again the sum. We also create a locus of control measure using a battery of three questions to assess to what degree the respondent see of herself as being in control of her actions. Furthermore, we construct an index of (lack of) stress using four questions that measure how many times in the previous weeks the respondent faced stressful situations. Finally, we create an index of (lack of) depression using five questions to measure whether the respondent felt unhappy and unsatisfied with her life.

Preferences. We consider three variables: a risk index, a patience index, and a measure of preferences non-reversal. 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, Slonim, and Sydnor (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 aversion: subjects that are more risk averse will choose the lotteries with lower 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 built the risk index assigning values from 1 to 5 to the respondent's lottery choice, with 1 being the lowest and 5 being the highest expected value chosen. So, this means 5 is associated with the lottery the highest expected value. Hence, the higher the index, the more risk-loving the respondent is.

Furthermore, we measured 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, Rapoport, and Yagil 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. These incentivized choices in the intertemporal choice task allow us to rank subjects according to their willingness to delay gratification: the more impatient subjects will be less willing to wait to receive a larger reward. We also asked a second set of questions varying the time frame: in 6 versus 7 months. In order to build the patience index, we code the preferences of the respondent to be patient if she was willing to wait for 200 pesos instead of 100, in both sets of questions (i.e. one month from a week, or one month after six months).

Finally, we construct an indicator for individuals exhibiting consistent preferences by asking the respondent to choose first between 100 pesos to be paid at the moment of the survey or 200 pesos after a month, and secondly to choose between the same prizes but with a

time horizon of one month and seven months, respectively; our indicator for “non-reversal of preferences” takes the value of one if they are willing to wait one month for a larger prize in both questions or if they are anxious to take the smaller prize in both questions. Finally, an index of patience is defined as an indicator of whether the respondent is willing to wait for the larger prize in both opportunities.

In Table A.2 we present the summary statistics of the outcomes at the individual level and show that there is balance of these measures across the groups of interest.

3 Identification and Estimation

To study how expected or unexpected income changes affects individual and household behavior, we create two dummies. One dummy, P , takes the value of 1 for households that will receive a CCT payment in the next 0 to 6 days (0 included) and 0 for households that received a payment in the last 1 to 7 days. The second dummy, S , takes the value of 1 for households that experienced employment or health losses in the previous two weeks and 0 for households that experienced these losses between 3 weeks and 12 months before.

We estimate an OLS regression of the household/beneficiary i 's outcome variables, Y_i , on the variables P and S , a dummy for not having experienced employment or health shocks in the previous 12 months, NS , and further controlling for predetermined variables X (age and education of the PROSPERA beneficiary, whether the beneficiary is married or living with a partner, age and education of the spouse, number of male and female senior, adult, and junior household members, and weekday dummies):

$$Y_i = \alpha_0 + \alpha_1 P_i + \alpha_2 S_i + \alpha_3 NS_i + \alpha_4 X_i + D_t + \epsilon_i \quad (1)$$

The coefficients α_1 and α_2 identify the effects of being about to receive the PROSPERA transfer and having recently experienced employment or health shocks. These parameters are identified under the assumptions that the variables P and S are conditionally independent

of the error term ϵ , which is consistent with our balance tests. We estimate the parameters of this equation by OLS, clustering the standard errors at the locality level. Finally, D_t are the day of the week dummy variables (the excluded category is Monday).

We use this econometric specification with multiple outcome variables, some of which belong to the same “family,” in the sense that they proxy for some broader outcome or channel of impact. This creates multiple inference problems, which we deal with in two ways. For outcome variables that are individually interesting but plausibly belong to the same family, we calculate adjusted critical values following the approach introduced by Benjamini and Hochberg (1995). In such cases we report both the unadjusted and the adjusted statistical significance. The unadjusted p-value is most useful for making inferences about the treatment effect on a particular outcome. The adjusted critical levels are most useful for making inferences about the treatment effect on a family of outcomes. For outcomes that are defined as indexes aggregating several variables, we do not correct for multiple hypothesis testing.

4 Results

4.1 Main results

We group outcomes into five categories, looking at (i) household income, labor supply, and finances, (ii) household food security and consumption, (iii) respondent’s investment in own and children’s human capital, (iv) respondent’s mental health and cognition, and (v) respondent’s risk and time preferences. Figures 1 and 2 summarize the impact on household- and individual-level outcomes of the effects of two types of income changes: (1) being about to receive a PROSPERA payment, an expected positive income change, and (2) having experienced a health or employment shock in the previous two weeks, an unexpected negative income change. To make the outcomes comparable, we standardize each outcome and report the coefficient of the treatment dummy, which provides the standardized estimate of the effect of the shock. In order to ease the comparison between the two sets of estimates, we

report the 83% confidence intervals.⁹

At a glance, these two figures show us four broad findings. First, the expected and unexpected income changes, despite having similar size, as highlighted previously when discussing the household summary statistics in Table A.1, affect household labor supply and finances differently. Second, households by and large smooth consumption in both cases. However, the way they do so depends on the shock considered. Third, the two income shocks have different effects on healthy habits and mental health. Fourth, we do not find a link between scarcity and cognition, consistent with Carvalho et al. (2014) and unlike Mani et al. (2013). We discuss these findings in details as follows.

Table 2 shows the effects of income changes on weekly income, labor supply, and household finances. Column 1 shows the effect of receiving the PROSPERA transfer within a week (i.e., the effect of not receiving the transfer for 7-8 weeks), while column 2 shows the effect of having experienced an employment or health loss in the previous 2 weeks. Column 3 reports the p-value of the test of equality of each pair of coefficients. We follow this format for all subsequent tables. Unless otherwise specified, we use the FWER adjusted significance levels when discussing the statistical significance of the various estimated effects. Each table reports which variables are grouped in the same family of outcomes. We do not correct the significance level of indices.

We depart from the literature by using a short time horizon – the previous three days – to measure whether households respond to the PROSPERA expected income change by smoothing labor income accordingly. People can do so by increasing their labor supply when they have not received a transfer in a long time or by setting a pay date for their earnings that does not overlap with the PROSPERA transfer, if possible. For unexpected shocks, these outcomes measure the effect of the shock, either directly, when the shock is employment or business loss, or indirectly, when the shock is own or household members' illness.

The main result from Table 2 is that the two types of shocks have similar magnitude.

⁹If the 83% confidence intervals of two coefficient estimates do not overlap, the two coefficients differ at the 95% confidence level.

Weekly household income is 155 and 220 pesos lower (s.e. 34 and 44), a 10% and 15% reduction. Also, as the p-value in column 3 shows, the coefficients are not statistically different from each other. However, the causes and effects are different. In the first case, the lower income is from not having received a cash transfer recently, while in the second case one reason is lower labor income, which drops by 115 pesos (s.e. 51). This is noticeable also by the statistically different coefficient for labor supply: small, positive and statistically insignificant in the week before the cash transfers, but large, negative, and statistically significant in the case of unexpected shocks. For the latter, in the previous 3 days, adults worked 1.4 fewer hours (s.e. 0.47) and 8% fewer adults worked (s.e. 0.02).

The two shocks have also statistically different effects on household debt, which decreases by 165 pesos before the CCT transfer (s.e. 250), while it increases by 1,273 pesos after the unexpected shocks (s.e. 768). Conversely, savings do not change statistically in either case.

Table 3 shows the effect of the various income changes on food security and non-durable and durable consumption. The main results is that the effects are consistent with the permanent income hypothesis: consumption decreases more with unexpected (and possibly more permanent) income shocks. However, these effects are statistically insignificant and suggest that households are well equipped to smooth these types of income fluctuations. This is consistent with the PIH and with recent efforts to provide financial tools to the poor.

Specifically, we find one main difference: after an unexpected shock, previous week's food security decrease by 0.2 (s.e. 0.1).¹⁰ On the other hand, we find no statistically or economically significant effect of being about to receive a cash transfer on food security and consumption. The lack in drop of food consumption in response to an anticipated transitory income shock is in contrast with the existing literature, which finds that food consumption decreases before pay dates for low-income households in the United States (e.g., Stephens 2003). Carvalho et al. (2016) find that the drop in food consumption is bigger for house-

¹⁰Unexpected income shocks in the previous 2 weeks do not change previous daily food consumption significantly. However, when we look at a broader time interval, we see lower food security. It is possible that previous day's food consumption had recovered from the shock by the time we measure it.

holds with more limited financial access. Since PROSPERA provides its beneficiaries with a savings account, a debit card, and, for some, a line of credit, this effort at financial inclusion may have helped households better smooth expected income changes and may explain the difference between our findings and the rest of the literature. Indeed, Bachas et al. (2017) find that PROSPERA’s beneficiaries with access to a debit card began to increase their savings in the savings account 9-12 months after they received the credit card. Our sample consist of beneficiaries with access to a debit card and a fraction of the sample had access to a line of credit. Hence, they were better included in the financial system and had better tools to smooth consumption.

Lastly, none of the small changes in asset purchased and owned by households who are about to receive the cash transfer are statistically significant after correcting for FWER.

Next, in Table 4, we focus on the impact of the two types of income fluctuations on respondent’s investment in own and children’s human capital, switching from household to individual level data. The main findings are that the two types of income shocks have opposite effects on investment on own human capital. Specifically, being about to receive a cash transfer increases respondent’s healthy habits (exercising, sleeping, brushing teeth, and washing hands with soap and water) by 0.07 (s.e. 0.04), while being hit by employment and health shocks decrease healthy habits by 0.13 (s.e. 0.08). The two effects are statistically different from each other. Conversely, neither shock affects investment in children’s human capital (i.e. actual or desired time spent with children), or maternal aspirations and expectations about her children’s educational attainment, as well as children’s school attendance in the previous three days.

There are several reasons why income changes may affect investment in one’s health. Note that none of the activities we considered are expensive, and most are not time consuming. Therefore, we do not expect changes in credit or time constraints to be the main mechanisms through which income fluctuations affect these healthful behavior. On the other hand, income changes may affect respondents’ sense of control of one’s life, depression, and stress.

Mani et al. (2013) also show that poverty impedes cognitive function, so income changes may also cause cognitive changes, which in turn would affect health behavior. Improvements in any of these dimensions may lead people to take better care of their health.

Consistent with the above conjectures, Table 5 shows differential effects of the two income changes on depression, but no effects on cognition. Specifically, the impending receipt of the cash transfer reduces depression by 0.10 (s.e. 0.03). Conversely, having recently experienced an employment or health shock increases depression by 0.13 (s.e. 0.08). While not both coefficients are statistically significant, the difference between them is. The individual effects on stress, and their difference, are not statistically significant. Nevertheless, we find a similar pattern: being about to receive a cash transfer reduces stress, while being hit by an unemployment or health shock increases it.

The finding that being about to receive a cash transfer has modest positive effect on mental health is contrary to our priors. Nevertheless, it is consistent with findings that the anticipation of positive events improves mental health (Berk, Tan, and Berk 2008).

Regarding cognition, we find no statistically significant effect of both types of income changes on our cognition tests (Raven’s matrices, forward and backward digit recall), both individually and aggregated. This is consistent with Carvalho et al. (2016)’s finding that low income US households’ cognition does not change before or after their pay date. We complement their evidence by considering both anticipated and unanticipated income changes. In neither case do we find any effect. We speculate that Mani et al. (2013) may find lower cognition because their income shock is almost two times as large as the one we study.¹¹

An additional channel through which income changes may health behaviors is through changes in preferences. Lower risk aversion and present bias and a higher regard for the future may induce people to take better care their health.

Table 6 shows the impact of the two types of income fluctuations on preferences. We consider three variables: a risk index, a measure of preferences non-reversal, and a patience

¹¹In Mani et al. (2013) the shock in the hard condition scenario corresponded to about 24% of household income. In our case, the CCT represents about 14% of household income.

index. We find that both income changes reduce risk taking by similar magnitudes, 0.06 (s.e. 0.03 and 0.06). Therefore, these similar changes in risk preferences cannot explain the dissimilar health behavior. Time preferences do not change either statistically or economically.

To conclude this section, our key finding is that the impending receipt of a cash transfer, an expected, transitory income change, and the recent experience of employment or health losses, an unexpected and possibly more permanent income change, have different effects despite the fact that both shocks cause an income reduction of a similar magnitude. Some of these differences, such as the better smoothing of the expected shock, are consistent with the LCPIH in the absence of financial constraints. However, the differences in adherence to healthful behaviors and their likely causes are not considered by the LCPIH. Rather, they can be explained through a link between anticipated positive changes, improved mental health, and better self-care.

4.2 Heterogeneous effects by poverty status

Next, we study how the effects of each type of shock vary by poverty status. We consider two different proxies for poverty: respondents' education (above/below the median level of education for the entire sample and dependence on PROSPERA, expressed as the proportion of household income that comes from the cash transfer (above/below the median ratio of the cash transfer over total household income)). We consider both variables to be proxies for poverty, although they capture different features of poverty. While education can be thought of as an important determinant of permanent income, dependence on the transfer is related to both poverty and household demographic, as eligible household must score below a minimum wealth threshold but, once they qualify for the program, households with more children within certain ages and school grades are entitled to larger transfers. Therefore, two households with equally educated respondents will receive different transfers depending on how many children attending late primary school to high school are in it. Consistent with

this explanation, the two proxies are uncorrelated with each other. Households with a ratio of the CCT household income above the median (i.e. the poorest households) have an average income equivalent to 44% of the income of households below the median.¹² Households with the beneficiary's education below the median have an average income equivalent to 92% of the income of households with their beneficiary's education below the median.¹³

The graphs below show the effects of the two sets of income changes for households above and below (i) respondents' median education and (ii) the median ratio of the cash transfer over total household income.

The top panel of Figure 3 shows that households with a greater dependence from the cash transfer, which, therefore, have a bigger income drop from not having received it for 7 to 8 weeks, respond to this bigger expected shock by supplying more labor, and, specifically, by having more adults engaged in income-generating activities. There are no other significant differences on household's savings, debt, and consumption. But, when we look at individual-level outcomes, we find differential effects on the beneficiaries' cognition. The bottom panel of Figure 3 shows that not having received the cash transfer for long has a statistically different effect on cognition, which decreases more in households that are more dependent on PROSPERA. Adherence to healthful habits, locus of control, stress, depression, risk and time preferences, and parenting outcomes do not change differently for these two sets of households.

Figure 4 shows the heterogeneous effects caused by an unexpected employment and health shock, for households above and below the median PROSPERA/household income ratio. We find no statistically significant heterogeneous effects at the household or respondent level.

Next, we group households by respondents' median education. As the top panel of Figure 5 shows, the income changes and the related responses in terms of labor supply, savings, debt, and consumption, are of similar magnitudes among the two groups of households (i.e. above

¹²The average income of the sample above the median ration of the CCT and household income (i.e. the poorest) is 876.92 pesos, while is 1963.11 pesos for those below the median (i.e. less poor).

¹³The average income of the sample for households whose beneficiary's education is below the median is 1341.17 pesos, while is 1450.54 pesos for those below the median.

and below the respondent's median education). The only exception is the stock of animals, that decreases more for households with below-median education respondents, hinting that smoothing income may be costlier for this subgroup.

In the bottom panel of Figure 5, we consider individual-level outcomes and find that there are heterogeneous effects on healthful habits. The imminent receipt of the transfer increases healthful habits for less educated respondents but not for more educated respondents, and this difference is statistically significant. While the effects on cognition, locus of control, and stress are not different for the two subgroup, we find differential effects also on depression, which increases significantly more for less educated respondents (while it does not change for more educated ones). Risk and time preferences and parenting outcome do not change for either group.

In Figure 6 we analyze the effects of unexpected health and employment shocks. Again, we do not find any statistically significant difference across groups with distinct levels of schooling.

We can draw two main conclusions by looking at these findings in their entirety. First, income drops may impair cognition only when they are sufficiently large. We find that cognition is statistically lower for households whose weekly income is 0.25 SD lower, compared to households whose weekly income is only 0.1 SD lower.

Second, we find a positive correlation between the effects of anticipating an income increase on depression and adherence to healthful habits, both of which increase, that suggests a possible link between depression and investment in one's health. Since cognition, preferences, and locus of control are unaffected by the income change, we can rule out that the better self-care comes from changes in these domains. Rather, a possible explanation for this correlation is that anticipating the receipt of the transfer temporarily reduces depression, which, in turns, increases people's will to take better care of themselves.

5 Conclusion

Income fluctuations have important effects on households' economic behavior and wellbeing. Not only they affect current consumption, but they may also impact future consumption because of lower investment in human capital for the self and one's children (i.e. lower healthful habits, worse parenting, lower child school attendance, lower parental aspirations about their children's education). Furthermore, cognitive biases may limit the desire to invest by changing risk and time preferences. Impaired cognition may limit individuals' ability to make cognitively complex decisions needed to invest optimally in the presence of variable and unpredictable income. Lastly, worse mental health (e.g., lower locus of control, stress, or depression) may limit one's will to make optimal inter-temporal decisions.

This paper studies the effects of expected shocks (i.e. conditional cash transfers, which are positive and transitory income changes) and unexpected shocks (i.e. health and employment shocks, which are negative and potentially more permanent income changes) for a sample Mexican households who are PROSPERA recipients. To control for the potential endogeneity of income changes, we exploit plausibly exogenous variation in their timing by randomly interviewing beneficiaries who had received a transfer in the last 1 to 7 days and beneficiaries who were about to receive a transfer within a week, generating exogenous variation in CCT delivery dates. Similarly, we compare households that suffered a job or health shock in the past 2 weeks with households that experienced this shock in the past year.

As conjectured we find a stronger impact for unexpected than expected income changes. In particular, we find that expected income shocks have no effect on household behavior, while unexpected income shocks decrease labor supply, food security, mental health, and healthful behavior, but do not affect cognition, risk and time preferences, expectations and aspiration about children's educational attainment.

Figures and Tables

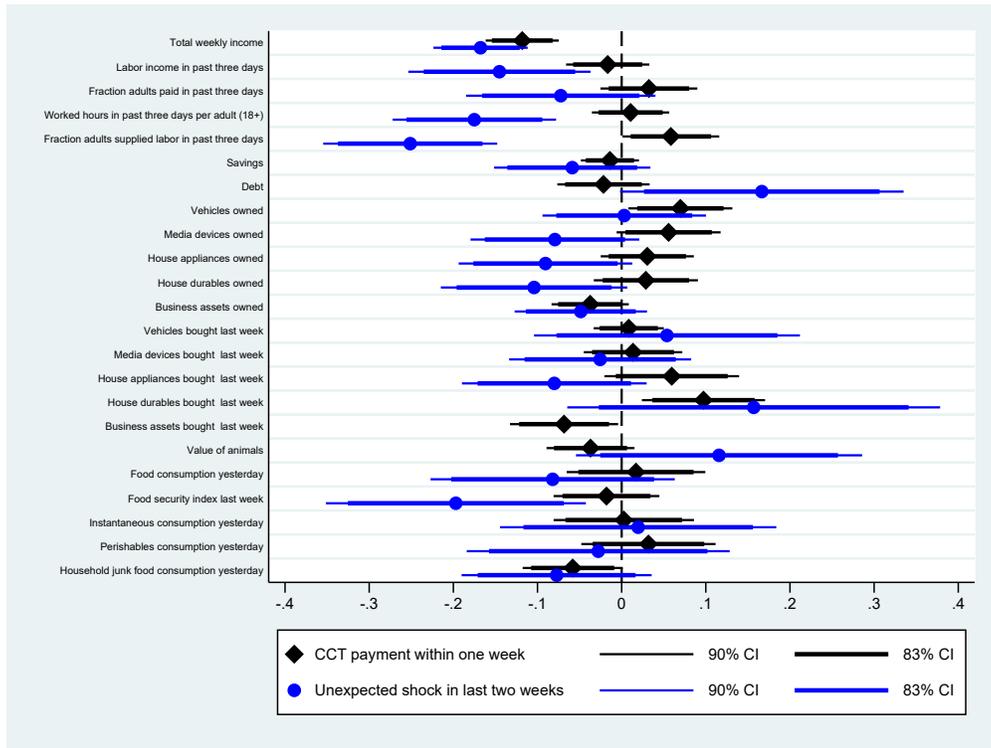


Figure 1: Effect of expected (CCT payment) and unexpected (health and unemployment shocks) income fluctuations on household-level outcomes

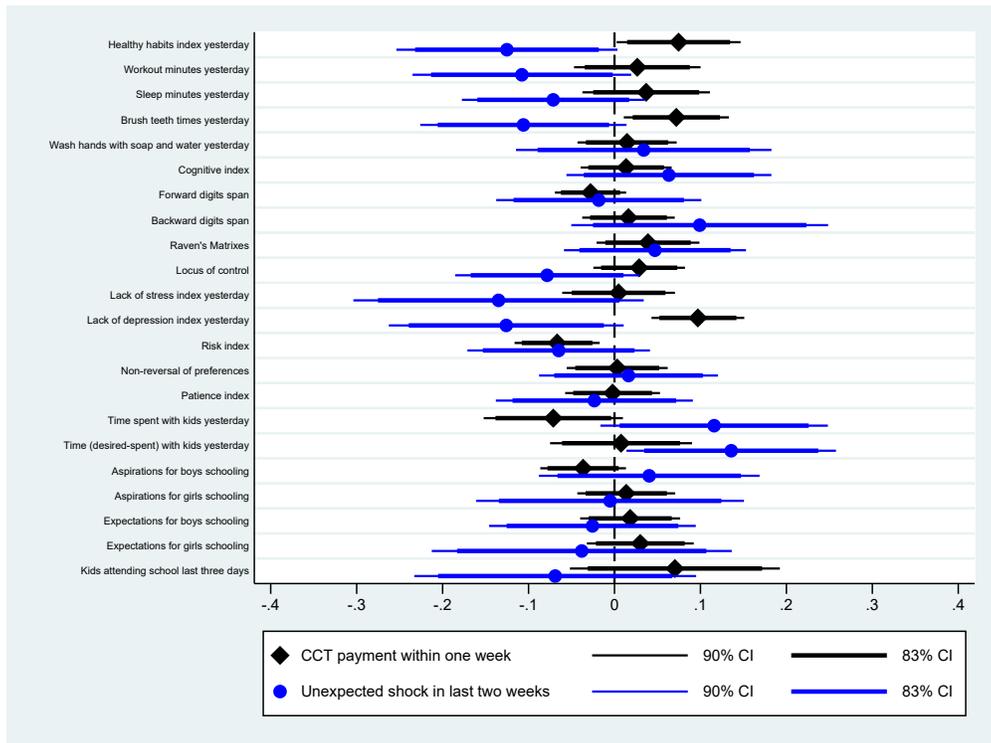
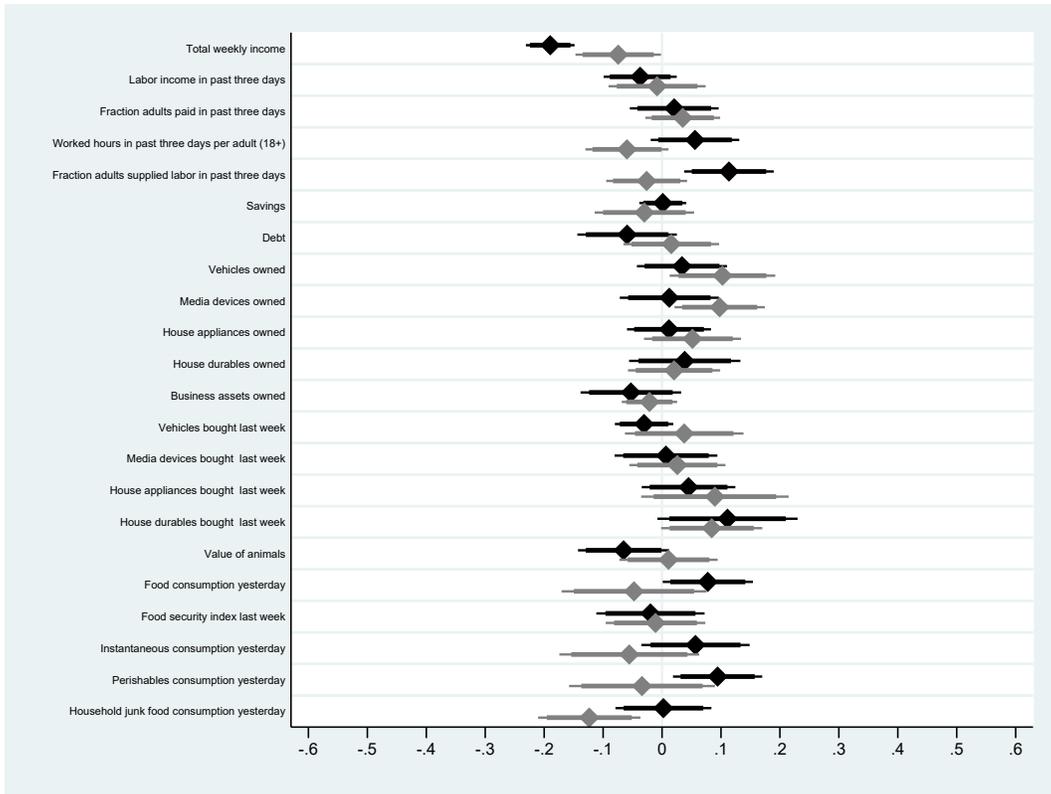
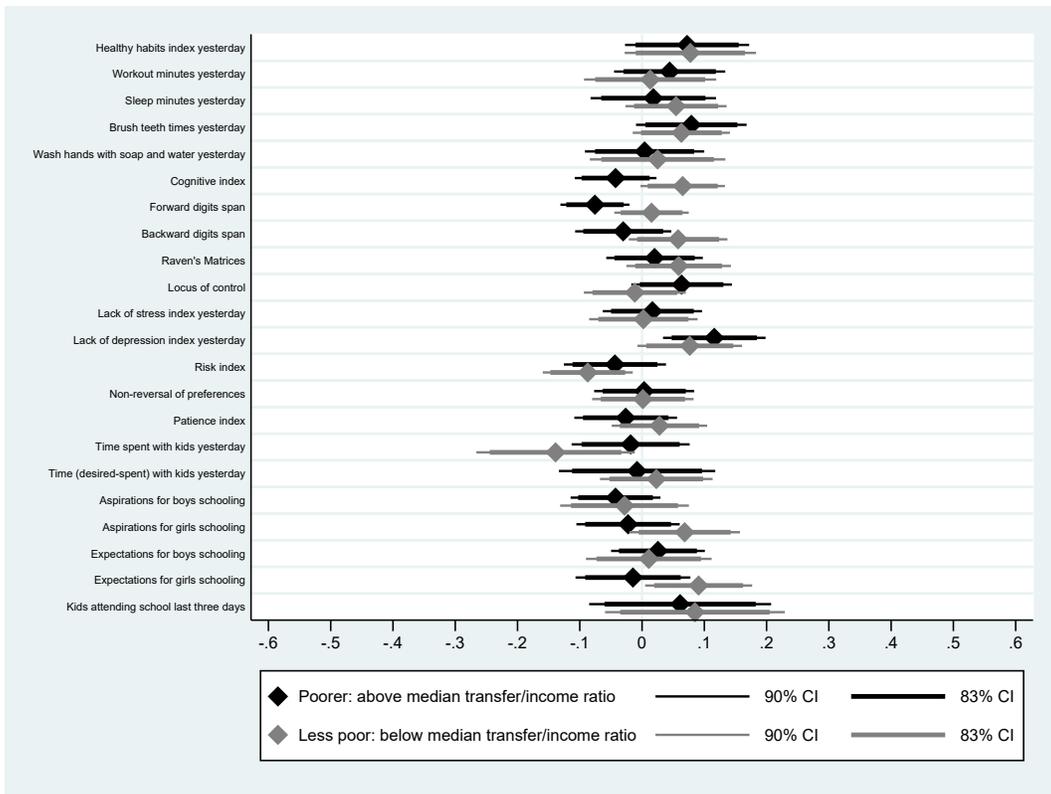


Figure 2: Effect of expected (CCT payment) and unexpected (health and unemployment shocks) income fluctuations on individual-level outcomes

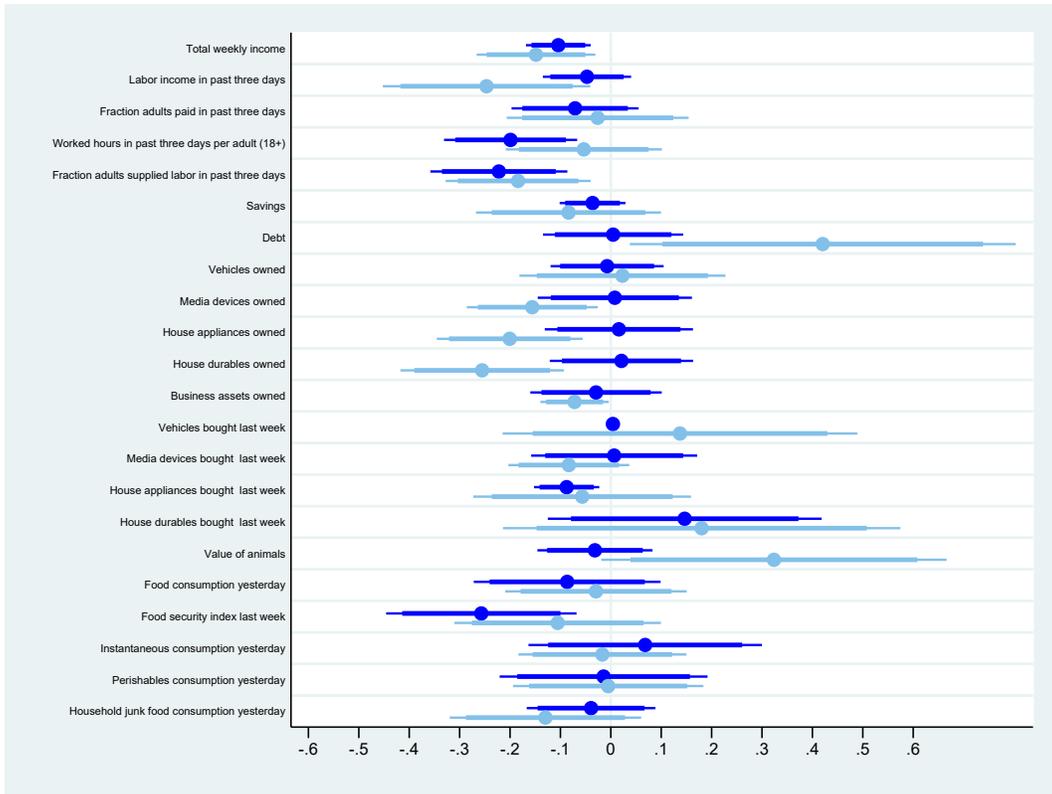


(a) Household outcomes

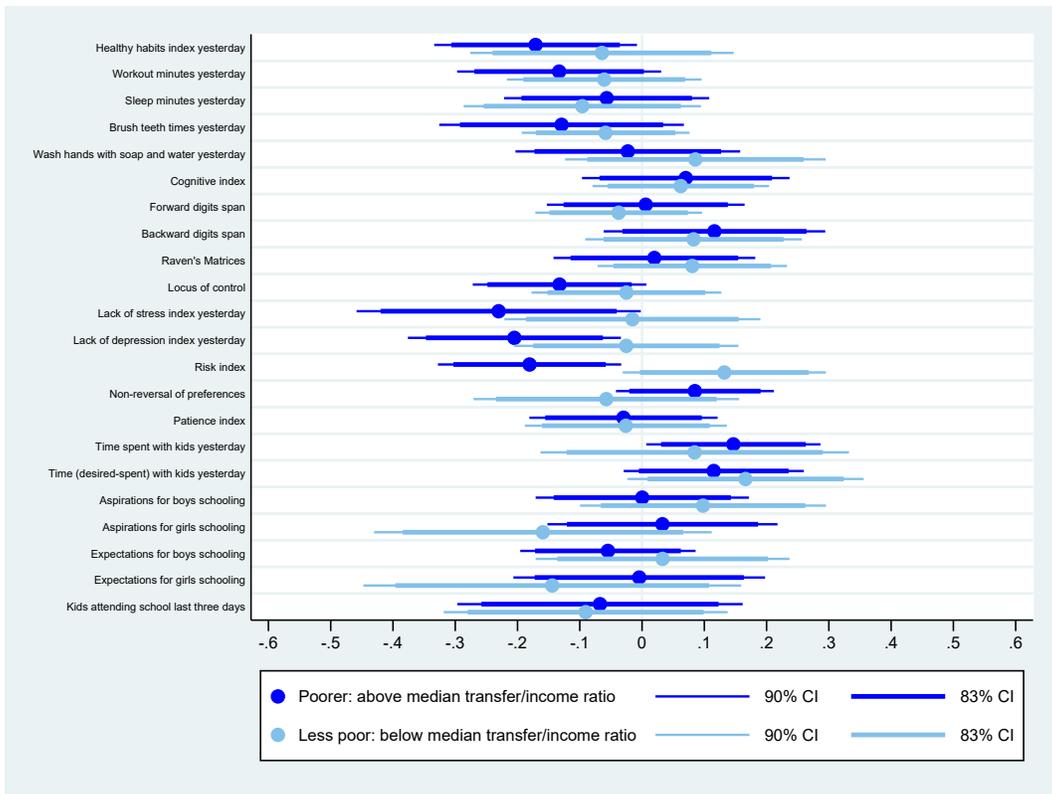


(b) Respondent's outcomes

Figure 3: CCT pay date within one week by transfer/household income ratio category.

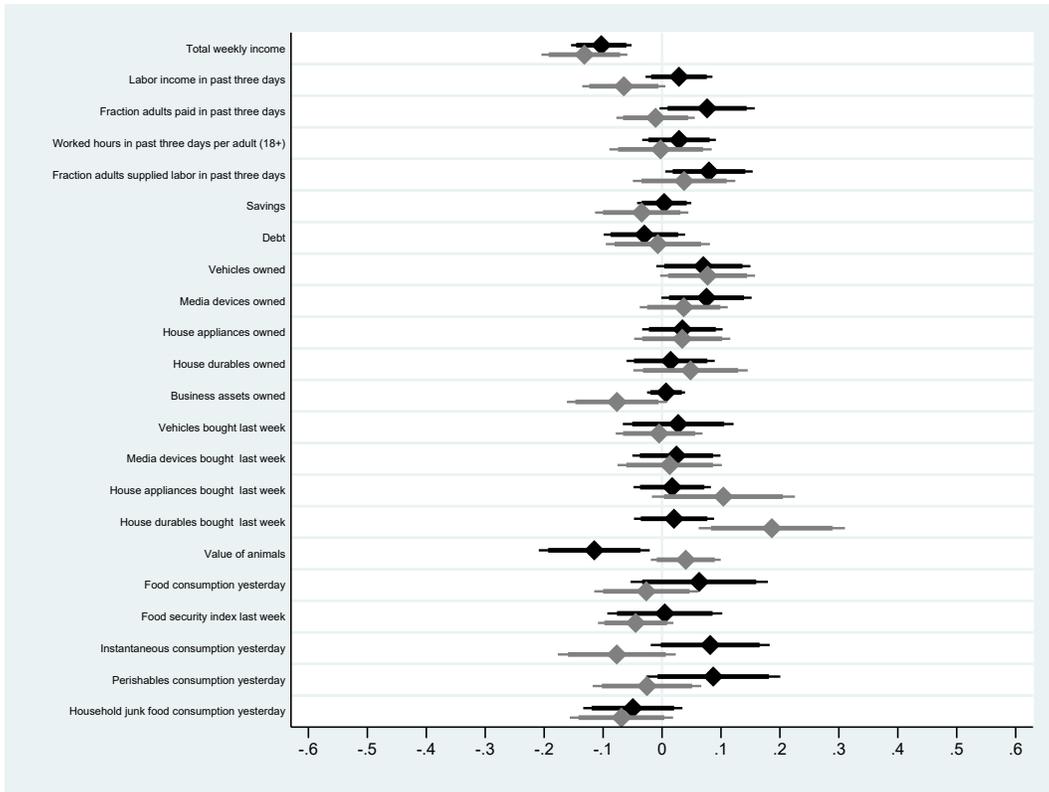


(a) Household outcomes

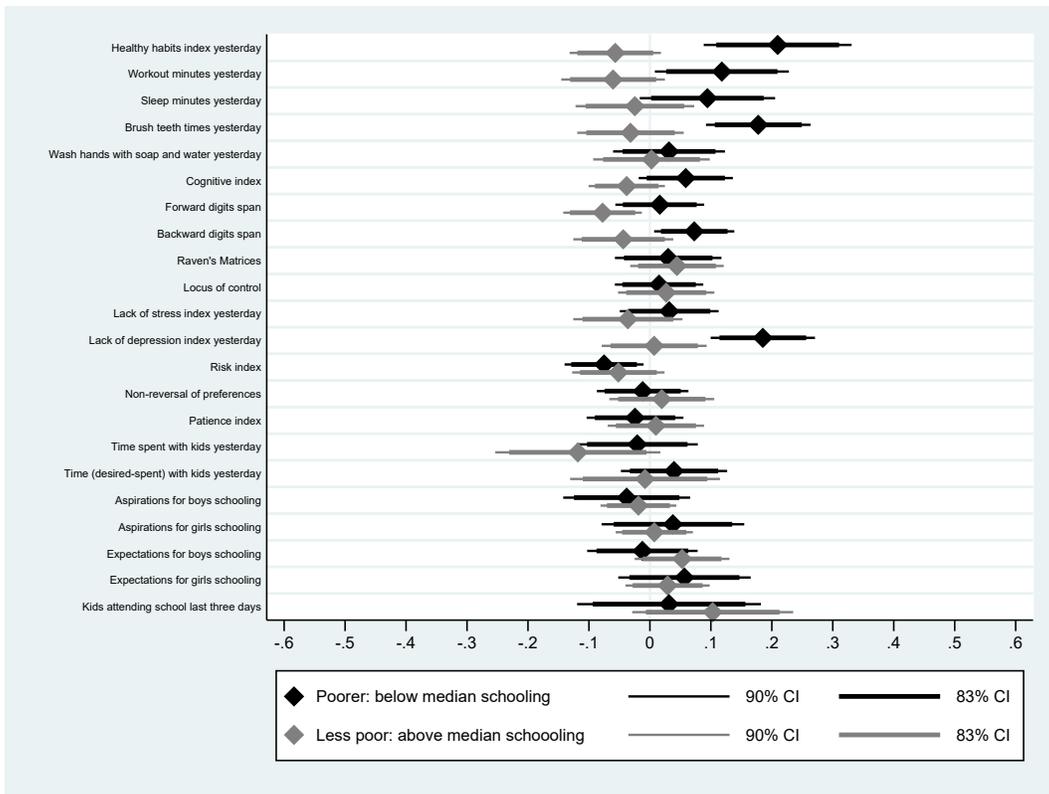


(b) Respondent's outcomes

Figure 4: Unexpected shock in last two weeks by transfer/household income ratio category.

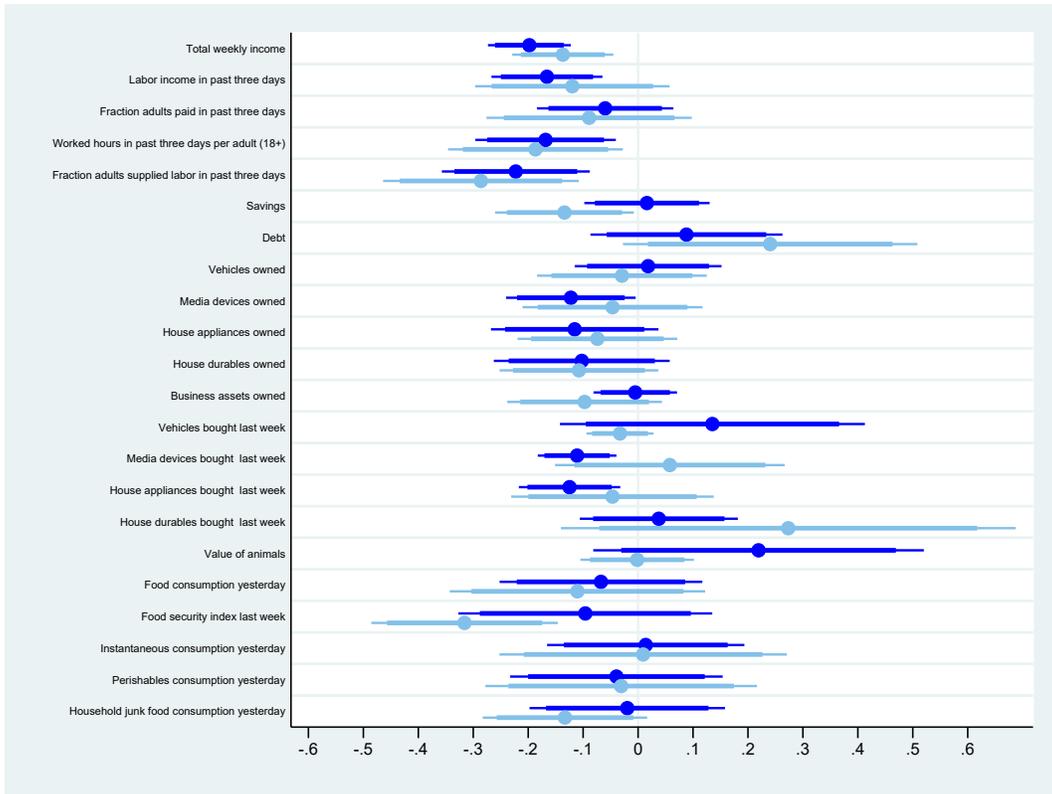


(a) Household outcomes

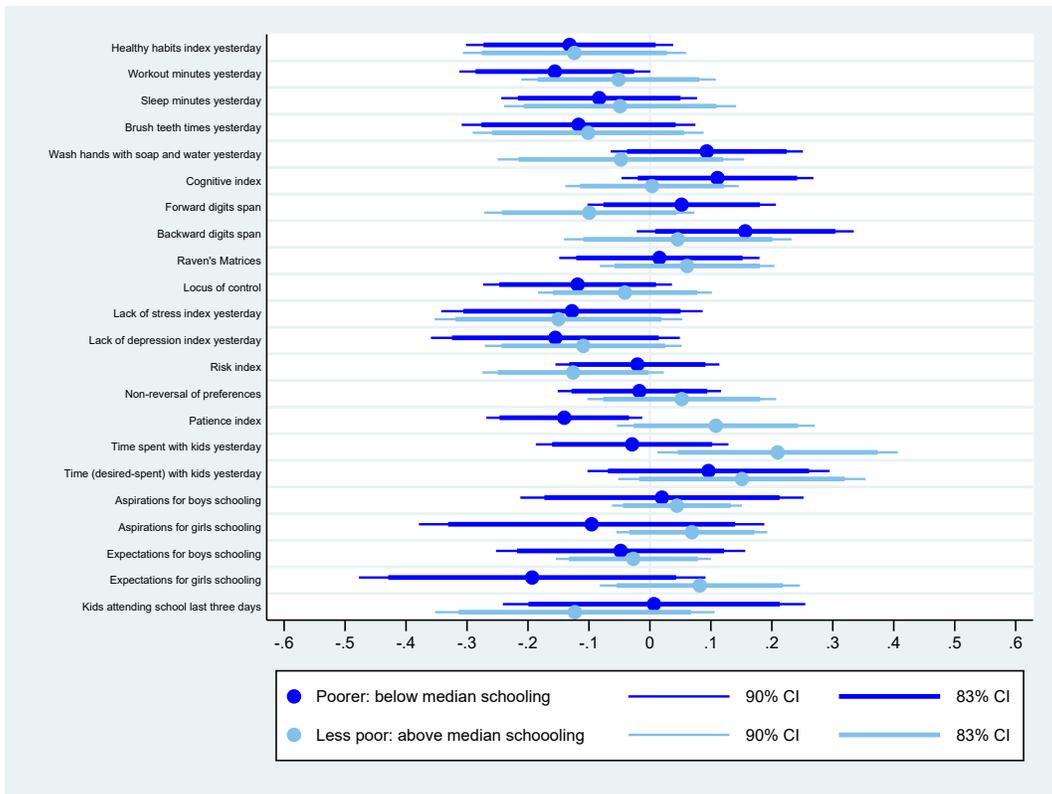


(b) Respondent's outcomes

Figure 5: CCT Pay Date within One Week by respondent's schooling category.



(a) Household outcomes



(b) Respondent's outcomes

Figure 6: Unexpected Shock in Last Two Weeks by respondent's schooling category.

Table 1: Means of predetermined variables and balance across groups

Dependent variable:	Will receive PROSPERA transfer in 1 week		Experienced employment/health shock in past 2 weeks		
	Sample Mean (1)	Coefficient estimate (2)	Sample Mean (3)	Coefficient estimate (4)	
Married/living with partner	0.69	0.00	0.71	-0.04	(0.03)
Age	43.69	-0.24	43.83	0.49	(0.80)
Husband's age	45.27	-0.37	45.74	-0.27	(0.99)
Schooling	6.75	-0.06	6.7	-0.24	(0.26)
Husband's schooling	6.72	0.30*	6.65	-0.24	(0.39)
Males aged 0-17	1.00	0.04	0.99	0.00	(0.06)
Females aged 0-17	0.97	0.00	0.95	0.12	(0.07)
Males aged 18-64	1.06	-0.02	1.10	-0.02	(0.05)
Females aged 18-64	1.35	-0.01	1.35	0.06	(0.05)
Males aged >65	0.07	0.00	0.08	0.02	(0.02)
Females aged >65	0.10	-0.00	0.09	0.07**	(0.03)
PROSPERA amount	218.27	-4.22	214.91	-9.23	(14.10)
H0: observables are jointly significant (p-value)		0.44		0.15	
Observations		3,534		1,617	

Notes: *, **, *** mean statistical significance at the 90, 95, and 99 percent level. Robust standard errors clustered at the locality level reported in parentheses. The means are calculated using the entire sample when testing the balance of groups that just received and that are about to receive the PROSPERA transfer. When testing for balance across groups that received a health or employment shock in the past two weeks, we calculate the means using a sub-sample that includes households that received at least one negative shock in the past year. Individuals are categorized as children when they are younger than 18, as adults when they are 18 to 65 years of age, and as elders when they are older than 65. The PROSPERA transfer amount is calculated as the monthly transfer per adult equivalent.

Table 2: Effect of income changes on income, labor supply, and finances. Variables measured at the household level

	Effects of CCT Pay Date within One Week		Effect of Unexpected Shock in Last Two Weeks		p-value of difference (3)
	Coefficient	Control group mean (1)	Coefficient	Control group mean (2)	
Total weekly income	-155.04***^^^ (34.05)	1,567.71	-219.99***^^^ (43.91)	1,504.81	0.22
Labor income in past three days	-13.19 (23.44)	456.11	-115.38**^^ (51.35)	495.02	0.06
Fraction adults paid in past three days	0.01 (0.01)	0.34	-0.03 (0.02)	0.36	0.13
Worked hours in past three days per adult (18+)	0.09 (0.22)	12.87	-1.43***^^ (0.47)	13.10	0.00
Fraction adults supplied labor in past three days	0.02* (0.01)	0.63	-0.08***^^^ (0.02)	0.64	0.00
Savings	-110.77 (163.31)	1,253.55	-465.7 (439.53)	1,216.62	0.38
Debt	-164.96 (250.18)	2,442.06	1,272.65 (768.44)	2,250.85	0.06

Notes: *, **, *** mean statistical significance 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 FWER from Benjamini and Hochberg (1995). Robust standard errors clustered at the locality level reported in parentheses.

Table 3: Effect of income changes on food security and consumption. Variables measured at the household level.

	Effects of CCT Pay Date within One Week		Effect of Unexpected Shock in Last Two Weeks		p-value of difference (3)
	Coefficient	Control group mean (1)	Coefficient	Control group mean (2)	
Food consumption yesterday	1.01 (2.92)	112.59	-4.86 (5.14)	114.24	0.32
Food security index last week	-0.02 (0.04)	0.00	-0.2**^^ (0.09)	0.02	0.09
Instantaneous consumption yesterday	0.14 (2.66)	64.37	1.05 (5.23)	65.15	0.88
Perishables consumption yesterday	1.40 (2.10)	57.27	-1.22 (4.1)	59.02	0.57
Household junk food consumption yesterday	-0.64 (0.39)	9.41	-0.85 (0.74)	9.27	0.76
Vehicles owned	0.03* (0.02)	0.16	0.00 (0.03)	0.17	0.3
Media devices owned	0.10 (0.07)	3.68	-0.14 (0.11)	3.73	0.04
House appliances owned	0.03 (0.04)	2.32	-0.1 (0.07)	2.34	0.08
House durables owned	0.03 (0.04)	2.24	-0.11 (0.07)	2.25	0.09
Business assets owned	-0.04 (0.03)	0.17	-0.05 (0.05)	0.15	0.79
Vehicles bought last week	0.00 (0.00)	0.00	0.00 (0.00)	0.00	0.70
Media devices bought last week	0.00 (0.01)	0.02	0.00 (0.01)	0.02	0.64
House appliances bought last week	0.01 (0.01)	0.01	-0.01 (0.01)	0.02	0.15
House durables bought last week	0.01** (0.00)	0.00	0.02 (0.01)	0.01	0.63
Business assets bought last week	0.00* (0.00)	0.00	0.00 (0.00)	0.00	0.09
Value of animals	-92.29 (77.77)	725.66	288.61 (253.1)	655.88	0.19

Notes: *, **, *** mean statistical significance 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 FWER from Benjamini and Hochberg (1995). Robust standard errors clustered at the locality level reported in parentheses.

Table 4: Effect of income shocks on parental investment in own and children's human capital.
Variables measured at the respondent level.

	Effects of CCT Pay Date within One Week		Effect of Unexpected Shock in Last Two Weeks		p-value of difference (3)
	Coefficient	Control group mean (1)	Coefficient	Control group mean (2)	
Healthy habits index yesterday	0.07 [^] (0.04)	-0.04	-0.13 (0.08)	0.01	0.03
Workout minutes yesterday	1.93 (3.20)	38.24	-7.84 (5.53)	38.37	0.17
Sleep minutes yesterday	3.57 (4.29)	460.8	-6.91 (6.15)	462.9	0.18
Brush teeth times yesterday	0.06 [*] (0.03)	1.89	-0.09 (0.06)	1.94	0.03
Wash hands with soap and water yesterday	0.01 (0.01)	0.80	0.01 (0.04)	0.80	0.83
Time spent with kids yesterday	-0.32 (0.22)	4.31	0.52 (0.35)	4.10	0.04
Time (desired-spent) with kids yesterday	0.04 (0.25)	3.59	0.68 [*] (0.36)	3.57	0.12
Aspirations for boys schooling	-0.18 (0.14)	13.84	0.19 (0.37)	13.73	0.34
Aspirations for girls schooling	0.06 (0.16)	13.82	-0.02 (0.43)	14.03	0.84
Expectations for boys schooling	0.09 (0.16)	12.39	-0.12 (0.34)	12.42	0.59
Expectations for girls schooling	0.14 (0.17)	12.71	-0.17 (0.47)	12.98	0.5
Kids attending school last three days	0.02 (0.02)	0.99	-0.02 (0.03)	1.00	0.32

Notes: *, **, *** mean statistical significance 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 FWER from Benjamini and Hochberg (1995). Robust standard errors clustered at the locality level reported in parentheses.

Table 5: Effect of income shocks on mental health and cognition

	Effects of CCT Pay Date within One Week		Effect of Unexpected Shock in Last Two Weeks		p-value of difference (3)
	Coefficient	Control group mean (1)	Coefficient	Control group mean (2)	
Cognitive index	0.01 (0.03)	0.00	0.06 (0.07)	0.00	0.48
Forward digits span	-0.03 (0.03)	2.16	-0.02 (0.07)	2.14	0.89
Backward digits span	0.01 (0.03)	0.93	0.08 (0.07)	0.93	0.33
Raven's Matrixes	0.04 (0.03)	1.24	0.04 (0.06)	1.25	0.92
Locus of control	0.03 (0.03)	-0.01	-0.08 (0.06)	0.01	0.11
Lack of stress index yesterday	0.00 (0.04)	-0.01	-0.13 (0.10)	0.02	0.18
Lack of depression index yesterday	0.1***^^^ (0.03)	-0.07	-0.13 (0.08)	0.02	0.01

Notes: *, **, *** mean statistical significance 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 FWER from Benjamini and Hochberg (1995). Robust standard errors clustered at the locality level reported in parentheses.

Table 6: Effect of income shocks on preferences

	Effects of CCT Pay Date within One Week		Effect of Unexpected Shock in Last Two Weeks		p-value of difference
	Coefficient	Control group mean	Coefficient	Control group mean	
Risk index	-0.07** [^] (0.03)	0.03	-0.06 (0.06)	0	0.98
Non-reversal of preferences	0 (0.02)	0.53	0.01 (0.03)	0.53	0.85
Patience index	0 (0.03)	0	-0.02 (0.07)	0	0.76

Notes: *, **, *** mean statistical significance 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 FWER from Benjamini and Hochberg (1995). Robust standard errors clustered at the locality level reported in parentheses.

Appendix A. Supplementary Tables

Table A.1: Means of household outcomes and balance across groups

Dependent variable:	Will receive PROSPERA transfer in 1 week			Experienced employment/health shock in past 2 weeks		
	Sample Mean (1)	Coefficient estimate (2)		Sample Mean (3)	Coefficient estimate (4)	
Total weekly income	1,567.71	-151.14***	(41.32)	1,507.15	-189.03**	(56.29)
Labor income in past three days	456.11	55.92*	(25.06)	478.49	-134.66*	(50.69)
Fraction adults paid in past three days	0.34	0.04**	(0.01)	0.36	-0.05*	(0.02)
Worked hours in past three days per adult (18+)	12.87	0.16	(0.23)	12.74	-1.73**	(0.51)
Fraction adults supplied labor in past three days	0.63	0.02	(0.01)	0.64	-0.09***	(0.02)
Savings	1,253.55	-152.57	(217.44)	1,054.49	-391.52	(407.30)
Debt	2,442.06	-123.80	(234.02)	2,850.78	1,205.49	(775.40)
Vehicles owned	0.16	0.03*	(0.02)	0.17	-0.00	(0.03)
Media devices owned	3.68	0.09	(0.06)	3.73	-0.14	(0.12)
House appliances owned	2.32	0.03	(0.04)	2.41	-0.11	(0.07)
House durables owned	2.24	0.01	(0.04)	2.30	-0.10	(0.07)
Business assets owned	0.17	-0.05	(0.04)	0.18	-0.05	(0.05)
Vehicles bought last week	0.00	0.00	(0.00)	0.00	0.00	(0.00)
Media devices bought last week	0.02	0.00	(0.00)	0.01	-0.00	(0.01)
House appliances bought last week	0.01	0.01	(0.01)	0.02	-0.01	(0.01)
House durables bought last week	0.00	0.01*	(0.00)	0.01	0.02	(0.01)
Business assets bought last week	0.00	-0.00	(0.00)	-	-	-
Value of animals	725.66	-94.71	(74.00)	609.29	356.91	(289.57)
Food consumption yesterday	112.55	3.20	(1.85)	117.65	-4.57	(5.26)
Food security index last week	-0.01	0.02	(0.03)	-0.10	-0.21*	(0.09)
Instantaneous consumption yesterday	64.25	2.07	(1.78)	66.39	0.79	(5.17)
Perishables consumption yesterday	57.55	2.84*	(1.36)	59.88	-1.47	(4.05)
Household junk food consumption yesterday	9.13	0.25	(0.36)	9.86	-0.91	(0.75)

Notes: *, **, *** mean statistical significance at the 90, 95, and 99 percent level. Robust standard errors clustered at the locality level reported in parentheses. The means are calculated using the entire sample when testing the balance of groups that just received and that are about to receive the PROSPERA transfer. When testing for balance across groups that received a health or employment shock in the past two weeks, we calculate the means using a sub-sample that includes households that received at least one negative shock in the past year.

Table A.2: Means of respondent's outcomes and balance across groups

Dependent variable:	Will receive PROSPERA transfer in 1 week			Experienced employment/health shock in past 2 weeks		
	Sample Mean (1)	Coefficient estimate (2)		Sample Mean (3)	Coefficient estimate (4)	
Healthy habits index yesterday	-0.04	0.07*	(0.03)	0.00	-0.15	(0.08)
Workout minutes yesterday	36.77	2.54	(1.77)	41.93	-8.22	(5.55)
Sleep minutes yesterday	462.28	-0.21	(3.29)	460.48	-7.71	(6.30)
Brush teeth times yesterday	1.91	0.04	(0.03)	1.93	-0.11	(0.06)
Wash hands with soap and water yesterday	0.79	0.03*	(0.01)	0.79	0.01	(0.04)
Cognitive index	0.00	0.01	(0.03)	0.00	0.03	(0.08)
Forward digits span	2.16	-0.03	(0.03)	2.16	-0.04	(0.08)
Backward digits span	0.93	0.02	(0.03)	0.91	0.06	(0.07)
Raven's Matrixes	1.24	0.03	(0.04)	1.27	0.03	(0.06)
Locus of control	-0.01	0.02	(0.03)	-0.02	-0.09	(0.06)
Lack of stress index yesterday	0.00	-0.00	(0.04)	-0.07	-0.15	(0.10)
Lack of depression index yesterday	-0.02	0.04	(0.03)	-0.12	-0.15	(0.08)
Risk index	0.03	-0.06	(0.03)	0.06	-0.06	(0.07)
Non-reversal of preferences	0.53	0.00	(0.02)	0.52	0.01	(0.03)
Patience index	0.00	0.00	(0.03)	0.01	-0.02	(0.07)
Time spent with kids yesterday	4.04	0.15	(0.24)	4.08	0.28	(0.38)
Time (desired-spent) with kids yesterday	3.59	0.08	(0.17)	3.65	0.65	(0.41)
Aspirations for boys schooling	13.84	-0.23	(0.19)	13.43	0.18	(0.46)
Aspirations for girls schooling	13.82	0.31	(0.20)	13.96	-0.70	(0.62)
Expectations for boys schooling	12.39	0.00	(0.19)	12.02	-0.13	(0.43)
Expectations for girls schooling	12.71	0.38	(0.20)	12.85	-0.84	(0.64)
Kids attending school last three days	0.99	0.02	(0.02)	1.00	-0.03	(0.03)

Notes: *, **, *** mean statistical significance at the 90, 95, and 99 percent level. Robust standard errors clustered at the locality level reported in parentheses. The means are calculated using the entire sample when testing the balance of groups that just received and that are about to receive the PROSPERA transfer. When testing for balance across groups that received a health or employment shock in the past two weeks, we calculate the means using a sub-sample that includes households that received at least one negative shock in the past year.

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