NBER WORKING PAPER SERIES

THE IMPACT OF UNCONDITIONAL CASH TRANSFERS ON CONSUMPTION AND HOUSEHOLD BALANCE SHEETS: EXPERIMENTAL EVIDENCE FROM TWO US STATES

Alexander W. Bartik
Elizabeth Rhodes
David E. Broockman
Patrick K. Krause
Sarah Miller
Eva Vivalt

Working Paper 32784
http://www.nber.org/papers/w32784

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
August 2024

Many people contributed to the success of this project. The program we study and the associated research were supported by generous private funding sources, and we thank the non-profit organizations that implemented the program. We thank Jill Adona, Isaac Ahuvia, Oscar Alonso, Francisco Brady, Jack Bunge, Jake Cosgrove, Leo Dai, Kevin Didi, Rashad Dixon, Marc-Andrea Fiorina, Joshua Lin, Sabrina Liu, Anthony McCanny, Janna Mangasep, Oliver Scott Pankratz, Alok Ranjan, Mark Rick, Ethan Sansom, Sophia Scaglioni, and Angela Wang-Lin for outstanding research assistance. Tess Cotter, Karina Dotson, Aristia Kinis, Sam Manning, Alex Nawar, and Elizabeth Proehl were invaluable contributors through their work at OpenResearch. The management and staff of the Inclusive Economy Lab at the University of Chicago, including Carmelo Barbaro, Janelle Blackwood, Katie Buitrago, Melinda Croes, Crystal Godina, Kelly Hallberg, Kirsten Jacobson, Timi Koyejo, Misuzu Schexnider, Stephen Stapleton, and many others have provided important support throughout all stages of the project. We received valuable feedback on the study from the OpenResearch Advisory Board and seminar participants at the University of California-Berkeley and the University of Illinois at Urbana-Champaign. This study was approved by the Advarra Institutional Review Board (IRB) and is pre-registered at the American Economic Association RCT registry with a registration ID of AEARCTR-0006750. This research was supported in part by a J-PAL grant titled "The Impact of Unconditional Cash Transfers on Consumption: Evidence from the United States." The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

The Impact of Unconditional Cash Transfers on Consumption and Household Balance Sheets: Experimental Evidence from Two US States
Alexander W. Bartik, Elizabeth Rhodes, David E. Broockman, Patrick K. Krause, Sarah Miller, and Eva Vivalt
NBER Working Paper No. 32784
August 2024
JEL No. D14,E21,H53,G50

ABSTRACT

We provide new evidence on the causal effect of unearned income on consumption, balance sheets, and financial outcomes by exploiting an experiment that randomly assigned 1000 individuals to receive $1000 per month and 2000 individuals to receive $50 per month for three years. The transfer increased measured household expenditures by at least $300 per month. The spending impact is positive in most categories, and is largest for housing, food, and car expenses. The treatment increases housing unit and neighborhood mobility. We find noisily estimated modest positive effects on asset values, driven by financial assets, but these gains are offset by higher debt, resulting in a near-zero effect on net worth. The transfer increased self-reported financial health and credit scores but did not affect credit limits, delinquencies, utilization, bankruptcies, or foreclosures. Adjusting for underreporting, we estimate marginal propensities to consume non-durables between 0.44 and 0.55, durables and semi-durables between 0.21 and 0.26, and marginal propensities to de-lever of near zero. These results suggest that large temporary transfers increase short-term consumption and improve financial health but may not cause persistent improvements in the financial position of young, low-income households.

Alexander W. Bartik
University of Illinois at Urbana-Champaign
1407 W. Gregory Road
214 David Kinley Hall
Urbana, IL 61821
abartik@illinois.edu

Elizabeth Rhodes
OpenResearch
elizabeth@openresearchlab.org

David E. Broockman
University of California, Berkeley
210 Social Sciences Building #1950
Berkeley, CA 94720
dbroockman@berkeley.edu

Patrick K. Krause
OpenResearch
patrick@openresearchlab.org

Sarah Miller
Ross School of Business
University of Michigan 701
Tappan Street
Ann Arbor, MI 48109
and NBER
mille@umich.edu

Eva Vivalt
Department of Economics
University of Toronto
150 St. George Street
Toronto, ON M5S 3G7
and University of Oxford
eva.vivalt@utoronto.ca

A randomized controlled trials registry entry is available at
https://www.socialscienceregistry.org/trials/6750

1 Introduction

How do households respond to a large and unexpected, but time-limited, change in unearned income? Learning about the effects of such shocks to unearned income on spending, household balance sheets, and financial outcomes is important for understanding household decision making and for predicting household responses to tax and transfer policies. For example, understanding how households spend cash transfers is important for understanding what households value most on the margin and how the effects of cash transfers may differ from in-kind transfers. Whether households save or consume additional unearned income plays a central role in understanding the macroeconomic effects of government spending and taxes as well as the implications of the payment frequency of transfer programs. Answering such questions has been difficult due to the paucity of large, plausibly exogenous variation in unearned income that can be linked to longitudinal data on expenditures, household balance sheets, and financial outcomes.

We provide new evidence on the causal effect of unearned income on spending, financial outcomes, and household balance sheets by studying a large unconditional cash transfer experiment. We partnered with two non-profit organizations in Illinois and Texas to conduct the Open Research Unconditional income Study (ORUS), a large-scale randomized controlled trial (RCT) which randomly assigned 1000 treatment group individuals to receive $1000 a month for three years and 2000 control group individuals to receive $50 a month for the same period of time. Eligibility was limited to individuals 21 to 40 years old whose household income was less than 300 percent of the federal poverty level. Participants began receiving transfers in November 2020 and payments ended in October 2023.

We combine this experimental variation in unearned income with two rich data sources on expenditures, household balances sheets, and financial outcomes. First, we collect extensive surveys from participants, including monthly online surveys on a rotating set of topics and extended midline and endline surveys conducted by trained enumerators. These surveys provide regular data points on expenditures, financial outcomes, and well-being. We achieve very high response rates (exceeding 95% in most years and 93% in all years) in both the treatment and control groups, and the difference in response rates is modest, giving us high confidence in our survey data. Many of these survey outcomes, such as rent payments, informal loans, and self-perceived financial health, are difficult or impossible to measure in administrative data. We compare average monthly expenditures in our survey to the Consumer Expenditure Survey (CEX) administered by the Bureau of Labor Statistics (BLS) and find that measured expenditures in the control group from our survey almost exactly match average expenditures for demographically similar individuals in the CEX. Second, we link participant data to Experian credit histories which provide comprehensive records on debt and repayment that do not rely on participant recall. This study is rare in its ability to combine detailed self-reported expenditure data, financial perceptions and outcomes with credit report data at the individual level with experimental variation in unearned income. This provides us a multi-faceted window into households' consumption and financial choices over time.

Starting with the effect of the transfer on expenditures, we find that treatment increased measured spending by at least $310 per month in response to the transfer.[1] Spending rises in essentially every detailed expenditure category, reflecting the flexibility of an unconditional cash transfer. The largest spending treatment effects in dollar terms are concentrated in the most important spending categories at baseline: food, rent, and transportation expenditures. Combined, these three categories make up over half of the total estimated $310 effect on spending. In percent terms, the spending category with the highest treatment effect is spending on gifts or loans to family or charity, which rise 25 percent (but only $22 because the base of $84 per month is low). Approximately two-thirds of the overall treatment effect on spending is concentrated in non-durable spending categories. Treatment effects rise slightly over the course of the program, rising from $290 per month in the first year of the program to $330 by the third year. The transfers enable recipients to move to different housing units and neighborhoods, which both rise over 4 percentage points for the treatment group relative to the control group, or roughly 10 percent of the control mean.

Our detailed survey questions on financial outcomes, coupled with credit record data, provide a unique window into financial behavior and health. In addition to the increase in expenditures, we find that the treatment reduced expenditure volatility, with the standard deviation of log monthly regular expenditures declining by 0.14 in the treatment group relative to the control group. This suggests that households are more easily able to maintain a consistent level of spending. Credit access is broadly unchanged by the treatment—both total and available credit limits do not move and credit scores rise only slightly by 6 points. Similarly, credit delinquencies, default (bankruptcies and foreclosures), and credit utilization are unchanged. Self-reported financial health and resilience temporarily improve in the treatment group relative to the control group, driven by a combination of higher savings as a share of total income (excluding the transfer), improved scores on a self-assessed financial health index, and

[1] We believe this understates the effect on expenditures, similar to how expenditures are often under-reported in the CEX. When computing MPCs, we make adjustments to account for this under-reporting.

higher financial resilience and ability to weather shocks. However, the effect on the index of self-reported financial health decays over the course of the study; by year three there is no significant difference between the treatment and control group. It is possible that part of this decay is explained in part by recipients' knowledge that the transfer was coming to an end, leaving recipients feeling less secure about their financial futures—particularly if they did not accumulate significant savings as a buffer.

Treatment households raise their savings modestly in response to the transfer, increasing total financial assets by between $1000 and $2300 over the course of the study depending on the measure. This is driven primarily by higher bank account balances. We estimate small negative (and insignificant) treatment effect on homeownership, small positive (and insignificant) effects on car ownership (of about 2 percentage points), and no effect on the number of cars owned per household. Reflecting these small or null effects on car ownership and homeownership, the total value of real assets is unchanged by the treatment. If we take out the value of real estate (given that it is highly variable, homeownership rates are low in our sample, and homeownership is not substantially changed by the treatment), we can reject rises or declines in real asset values of more than $2500. Rather than paying down debt, participants in the treatment group actually increase their debt modestly, by about $1800 if mortgage balances are included and by about $500 if mortgage balances are excluded, though these estimates are imprecisely estimated and we cannot reject a null of no change in total debt balances. This treatment effect on debt is driven in large part by auto loan debt which aligns with the small changes we see in car ownership; we find a marginally significant change in auto loans of $800, though the estimate is not statistically significant after FDR adjustment. This rise in debt may also reflect greater ability to afford down payments or debt service payments, greater access to credit—we find a modest increase in participant credit scores—or changes in risk tolerance driven by the transfer. These estimates are imprecise, but our standard errors are small enough that we can reject total debt reductions of more than $2000 and we can reject much smaller changes for most important particular categories of debt. For example, we can reject treatment effects on credit card balances larger than -$140, effects on credit union and bank loans larger than -$200, and any decline in auto debt.

Combined, these treatment effects on assets and debt indicate that the transfer decreased household net worth by about $1000.[2] The net worth estimates are noisy, but we can rule out rises in net

[2] It is important to note that our sample consists of low-income households, many of whom had little in the way of savings or assets at baseline—median net worth was essentially zero, median savings was less than $1000, and only 61 percent of participants had at least $100 in savings.

worth of more than $5700 (including real estate and mortgages) or $3000 (excluding real estate and mortgages). Moreover, the combination of quite small estimates for effects on car and homeownership, the modest estimated effects on bank account balances, and the modest estimated rise in monthly minimum debt payments give us further confidence that there is unlikely to be a substantial rise in net worth. The estimated effects on net worth are non-monotonic over time, with net worth actually rising during the middle of the transfer period but declining by almost -$3000 by the third year of the study.

Combined with estimates reported in a companion paper (Vivalt et al. (2024)) that show that the treatment reduced earned income, we can develop a picture of how participants allocated the $950 net transfer. Roughly $310 was spent on higher expenditures, $210 on increased leisure (i.e., reduced earned income), and roughly $30 was spent on higher savings, but this was offset by the accumulation of about $60 per month of higher debt. Taking these estimates at face value, they imply that about 33 cents for every dollar of transfer was spent on increased expenditures, 22 cents was spent on higher leisure, and -3 cents was spent on lower net worth, leaving 47 cents of the transfer unexplained. To translate these numbers into marginal propensities to consume (MPC) and de-lever (MPD), we need to then make assumptions about how the unmeasured portion of the transfer was allocated.[3] Although there is substantial uncertainty, given that we think consumption is more likely to be mismeasured than income, the bulk of the remaining unexplained transfer is likely to be unmeasured consumption. If we assume that all of the unexplained transfer is unmeasured spending, then our estimates imply an MPC of 0.81, with an MPC non-durables of 0.55 and durables or semi-durables of 0.26 (made up of a 0.14 treatment effect on durables and a 0.12 effect on human capital expenditures, which we classify as semi-durable). Alternatively, if we assume that we are mismeasuring the change in net worth and assume that the actual change in net worth is positive $5,000 and that the remainder of the unobserved spending was allocated to consumption, then we find an MPC of 0.65, with an MPC to consume non-durables of 0.44 and durables or semi-durables of 0.21 (made up by a 0.11 effect on durable goods spending and a 0.10 effect on human capital spending).

We make three primary contributions to the literature. First, we characterize how households

[3]Following some other papers in this literature, such as Fagereng et al. (2021), we refer to the share of the transfer spent within the years of the transfer as the marginal propensity to consume, although marginal might be a misnomer in this case given the large size of the transfer. Similarly, we define marginal propensity to de-lever as the share of the transfer that was spent on reducing total debt during the period of the transfer. Alternatively, one could define the MPC as the share of the transfer that was not saved that was allocated to consumption. We present alternative estimates of the MPC that use this alternative definition in Appendix Table A16. Given the low share of the transfer that we estimate was saved, this alternative definition yields quite similar estimated MPCs as our main estimates.

use an unconditional cash transfer. There is a long literature studying the consumption responses to in-kind transfers or conditional transfers, such as SNAP or TANF (see e.g. Fraker (1990), Hoynes and Schanzenbach (2009), Hoynes and Schanzenbach (2016), Hastings and Shapiro (2018), Bronchetti et al. (2019), Hastings et al. (2021), and Han et al. (2021)). However, there has been a growing interest in studying the consumption and household finance response to unconditional cash transfers because the flexibility they offer may result in different effects from in-kind or conditional transfers. Given this flexibility, rich data on a wide variety of outcomes is required to understand how households use such transfers and how the transfers affect their lives. Several recent papers have investigated the effects of unconditional cash transfers in low-income countries (Haushofer and Shapiro (2013), Banerjee et al. (2023)) and middle-income countries (Salehi-Isfahani and Mostafavi-Dehzooei (2017), but there was limited evidence on the effect of such transfers in high-income countries (Forget (2011), Evans et al. (2012)) until recently.

A literature studying the growing number of cash transfer pilots that have been conducted in the past 5 years in the US has started to fill in this gap. The Chelsea Eats Study measures the impacts of a program that randomly assigned households to receive up to $400 a month for 9 months through a cash card between November 2020 and August 2021; they find that financial well-being, weekly food expenditures and consumption of fresh vegetables, fish, and meat rise (Liebman et al., 2022). Baby's First Years (Noble et al., 2021) studies a program that recruited mothers with newborn children between May 2018 and July 2019 and randomly assigned them to receive transfers of $333 or $20 a month via a pre-paid debit card for the first several years of their child's life. The transfers increased child-related expenditures and food consumed away from home but had little impact on other types of expenditures (Gennetian et al. (2022), Gennetian et al. (2024)). Jaroszewicz et al. (2023) study a program that randomly assigned households to receive a one time payment of $500, $2000, or nothing between July 2020 and May 2021. They linked some participants to bank account data and surveyed participants withdrawing program transfers or survey incentives about their intended use of the withdrawal. In the bank transaction data, they find that spending rises in the treatment groups, mostly through transfers such as ATM withdrawals, and in the withdrawal use survey they find that the treatment groups were more likely to intend to use the funds for general bills or housing costs than the control group. Our study complements these studies by examining a larger transfer and taking advantage of more detailed expenditure data and information on household balance sheets.

Second, we provide new evidence on the relationship between income and consumption using experimental variation in a large, unexpected cash transfer with a known end date. A rich existing literature studies small unexpected shocks to income from tax refunds, stimulus checks, small experiments with detailed expenditure data, or "reported responses" to hypothetical income shocks (see e.g. Hsieh (2003), Johnson et al. (2006), Parker et al. (2013), Misra and Surico (2014), Jones and Marinescu (2018), Fuster et al. (2021), Boehm et al. (2023), and Colarieti et al. (2024)). However, although informative, there are limitations to what these studies can teach us because the small size of the transfer means that many transfer uses that would involve shifting consumption commitments (such as moving to a new residence) are unlikely (Chetty and Szeidl (2016)). Furthermore, the welfare consequences of the choice of how to use a small transfer are minimal for many households, so there are limitations about what existing studies can tell us about household decision-making (Kueng (2018)).

A smaller literature estimates the effects of large changes in income on household consumption or financial behavior and outcomes by studying lottery winners. The average lottery winnings studied in these papers are often of the same order of magnitude as or higher than the total value of the transfers in our study, providing a useful comparison. Several aspects of our study distinguish it from this literature. First, households who self select into playing the lottery may differ in their preferences or other characteristics from those who do not play the lottery. This potentially makes it difficult to extrapolate from estimates on a sample of lottery winners to the broader population. Second, these studies typically either do not directly measure consumption at all and instead impute consumption using measures of asset returns (Golosov et al. (2023)) or wealth (Fagereng et al. (2021)), have a much more limited set of direct measures of consumption than we observe (Imbens et al. (2001)), or focus on other aspects of financial behavior like stock market participation (Briggs et al. (2021)). Our paper is unique relative to this literature in combining detailed consumption surveys and information on household balance sheets, including administrative data on debt from credit records.[4,5]

Fagereng et al. (2021) and Golosov et al. (2023) generate estimates of the overall consumption response to lottery prize winnings and so warrant more detailed comparison to our results. Fagereng et al. (2021) study lottery winners in Norway, taking advantage of the detailed administrative data available through the Norwegian government on household balance sheets, which include debt, deposit accounts, and stockholdings (for some years). They estimate a high marginal propensity to

[4]Imbens et al. (2001) measures the effect of lottery winning on the value of cars, real estate, and savings, and doesn't include any measures of total or non-durables consumption.

[5]Another set of papers studies the effects of lottery winnings on outcomes other than consumption and financial outcomes, studying the effect of lotteries on employment in Sweden (Cesarini et al. (2017)) and Holland (Picchio et al. (2017)), on college attendance in the US (Bulman et al. (2021)), or on well-being (Lindqvist et al. (2020)).

consume out of lottery winnings, with roughly 58% of the winnings spent in in the first year and around 85% spent by the third year. Golosov et al. (2023) study lottery winners in the US and impute consumption responses using asset returns, estimating marginal propensities to consume out of unearned income of roughly 0.6. Our preferred estimates of the marginal propensity to consume of 0.65 to 0.81 are arguably higher than Golosov et al. (2023) and of similar size to Fagereng et al. (2021).[6]

Third, our rich data also allow us to explore new dimensions of household responses to income shocks. We combine our experiment with survey data on expenditures and consumption, financial behavior, and financial health with credit bureau credit histories, providing a more complete picture of household behavior in response. This allows us to contribute to literatures on the relationship between income and well-being (Lindqvist et al. (2018), Stevenson and Wolfers (2013), Deaton (2008), Clark et al. (2008)) and expenditures and consumption (Aguiar and Hurst (2005)) and provide a more complete picture of the response of household behavior to income changes across a wide variety of measures.

Our results show that large, sustained, but time-limited cash transfers cause substantial increases in household expenditures, driven primarily by non-durable core expenses such as food and rent payments. Over the three year period of the transfer, participants spend the majority of the transfer on a combination of this large increase in expenditure and a moderately sized reduction in earned income (Vivalt et al. (2024)). Household financial savings rise only modestly and financial assets are unchanged, while household debt rises slightly. Credit access, utilization, delinquencies, bankruptcies, and foreclosures are similarly unchanged. Possibly reflecting this lack of improvement in long-term financial standing, financial health and resilience rise sharply in year one of the study but the effect decays to zero by the third year of the transfer. Overall, these results suggest that large, but temporary, transfers increase expenditures, consumption, and financial resilience in the short term but do not substantially improve low-income households' medium-term financial position, suggesting that the long-term effects on consumption or financial outcomes may be small. Time-limited cash transfers may be a tool to increase spending on basic needs, reduce consumption volatility, improve financial health, and strengthen households' ability to weather financial shocks in the short term, but policymakers interested in increasing wealth or improving long-run financial outcomes may want to consider either more permanent transfer programs or other policy options. More broadly, these results suggest that, at least in this sample of low-income US households, marginal propensities to consume

[6] Differences in payment sizes, payment frequency, samples, and data make this comparison difficult.

are quite high and that households have little propensity to smooth consumption over the long run following large increases in unearned income.

This paper proceeds as follows. In the next section, we describe the intervention, the participant recruitment, and the experimental design. Section 3 introduces the details of our empirical approach. We then provide an overview of the data sources we use in Section 4. Section 5 presents our main results, while Section 6 discusses what these results imply about households' propensity to consume and de-lever. Section 7 concludes.

2 Open Research Unconditional Income Study

In the OpenResearch Unconditional income Study (ORUS), OpenResearch collaborated with two non-profit non-profit partners (NPs) in Illinois and Texas to study the effects of a three year unconditional cash transfer program. As part of the study, the NPs recruited a sample of 3,000 individuals and randomly assigned 1,000 to receive an unconditional transfer of $1,000 a month and 2,000 to receive an unconditional transfer of $50 a month.[7] The analysis was pre-registered. Small changes to the original pre-analysis plan are detailed in Appendix Section B, and all versions of the complete pre-analysis plan are available through the AEA RCT Registry.[8] In this section we provide an overview of the study and describe recruitment and eligibility criteria.

2.1 Eligibility and Recruitment

Individuals ages 21 to 40 living in 18 counties in Illinois and Texas whose household income did not exceed 300% of the federal poverty threshold at baseline were eligible to participate in the study. The 18 counties included 9 counties in Illinois centered broadly around the Chicago area and 9 counties in Texas centered around the Dallas region. Counties were chosen to include a mix of counties containing large cities (Chicago, Dallas, and Fort Worth), counties containing medium sized cities (Waco, TX and Rockford, IL), suburban counties, and rural counties.

The NPs recruited participants through three methods. First, starting in August 2019, the NPs sent mailers to individuals in study counties inviting them to participate in a pilot program that would

[7] Given the diverse set of outcomes that large cash transfers could affect and the extensive and unique data that were collected, we pre-registered analyses for a variety of topics. In companion papers, Miller et al. (2024) and Vivalt et al. (2024), we present results on the effects of the transfers on health and employment outcomes respectively. In some cases, these results shed light on the consumption and household balance sheets findings described in this paper, so they are discussed in subsequent sections when appropriate. Some figures and tables describing the study and data as a whole, including Figure 1, Table 1, Appendix Figures A1, and A5, and Appendix Tables A1 are included in all three papers. Figure 2 is also included in Vivalt et al. (2024). Results on other pre-registered topics will be released in subsequent papers.

[8] See AEARCTR-0006750.

provide "$50 or more" per month in unconditional cash assistance. As in Broockman et al. (2017), mailed individuals were provided with a link and unique sign-in code to fill out an online screener with basic questions to determine respondents' eligibility for the program; if eligible, they were also asked to consent to link their information to administrative data. Consent to share administrative data was optional and did not affect the chances of being selected to participate in the program. Second, the study was advertised on Facebook, targeting individuals in study counties. Third, advertisements were included in the FreshEBT mobile application, which is used by many SNAP recipients to manage their benefits. Participants recruited through Facebook or FreshEBT, like those recruited via mailers, were provided with a personalized code to access the eligibility screener. In all three recruitment methods, potential participants were told that, if selected for the study, they would receive $50 or more in transfers for 3 years and have the opportunity to participate in optional research activities. Individuals received between $0 and $20 in the form of online gift cards to complete the screener, sent by email immediately after completion of the screener.[9]

Individuals whose answers on the screener indicated that they were eligible for the study were then randomly assigned into an “active" study group whose contact information was given to the Survey Research Organization (SRO) at the University of Michigan. Trained enumerators would then contact the individual to attempt to schedule a time to enroll them in the program. After participants were enrolled in the program they were invited to participate in the research, and a baseline survey was administered to consenting participants. Random assignment into the “active" study group was stratified on income relative to the federal poverty threshold and county type (large urban, medium urban, suburban, rural). We over-sampled individuals living in households making less than 200% of the federal poverty level. Overall 6,133 individuals were assigned to the “active" study group, of whom 3,000 completed baseline surveys. Baseline surveys were conducted in-person between September 2019 and March 2020 and were conducted by phone between March 2020 and September 2020. Individuals were told during the recruitment, screening, and baseline survey process that participants would receive "$50 or more" each month, but they were not informed that some individuals would receive $1000 per month during the program. All participants began receiving a $50 monthly transfer upon enrollment into the study.

We investigate the representativeness of the sample in Table 1, which compares the enrolled ORUS participant characteristics to the eligible population. Panel A reports average values for the active

[9]We used Tango card online gifts cards. These online gifts cards are very flexible and can be used at a wide variety of online and brick-and-mortar retailers including Amazon.com, Walmart, and many others.

group stratification variables, while Panel B reports average values for basic demographic variables. Columns (1)-(3) report values for the eligible population from the American Community Survey (ACS) using different weights and samples, Columns (4) and (5) report average values for eligible screener respondents, and Column (6) reports the values for the enrolled sample. Overall, the study sample closely matches the characteristics of the eligible population in the ACS, particularly once we reweight to reflect the oversampling of the lowest income households and the sample distribution across county types. The only characteristic that differs notably is the share who are renters, which is over-represented (79% versus 66%) in our sample relative to the average for the eligible population in study counties reweighted to match the FPL-type and county type distributions. Broadly, we think of the ORUS sample as being representative of the young, low-income population of the US.[10]

2.2 Randomization

In October 2020, 1,000 of the 3,000 individuals who completed the baseline survey were randomly assigned to receive $1,000 a month for three years (the "treatment" group) and 2,000 were assigned to continue receiving $50 a month for three years (the "control" group). To minimize differences in average characteristics and spillovers between the treatment and control groups we conducted a blocked and clustered randomization with re-randomization. We first assigned individuals to clusters. The vast majority of the sample was assigned to a cluster containing just themselves. However, we discovered during the pre-randomization period that some enrolled participants know one another (both through survey questions about whether they knew another participant in the study and based on the address where they lived). We placed such individuals into the same cluster so that they would receive the same treatment assignment and avoid any spillover between the treatment and control groups.

We then created strata based on combinations of state (TX and IL), income relative to the federal poverty level (0-100%, 100-200%, or 200% and up), and race and ethnicity (Non-Hispanic White, Black, and Hispanic). Within the strata, we constructed blocks of three clusters to minimize within block differences on a few dozen pre-treatment characteristics using the Mahalanobis distance measure. After the first round of blocking, some clusters were left unassigned because the number of clusters within a strata did not evenly divide into three. These observations were then assigned to blocks ignoring the strata, but again trying to place clusters into blocks with other clusters with similar characteristics.

[10] Note that the share renters in Column (6) differs from the share in Column (5) more than one would expect given that we randomized households into the active study group in Column (6) because we use different rental variables. The screener data which is used in Column (4) and (5) only asked if the respondent owned their home or rented, while the baseline survey data which we can use in Column (6) asked whether anyone in their household owned the house they lived in, which matches the homeownership definition used in the ACS data that we are comparing these data to in Columns (1)-(3).

Once all clusters had been assigned to a block, we randomly chose two clusters within each block to be assigned to the control group and one cluster to be assigned to the treatment group.

After a candidate block randomization following this procedure was completed, we conducted a series of covariate balance tests for several dozen pre-treatment covariates. For each baseline covariate, we chose a p value floor below which we would reject the candidate randomization. We also rejected any candidate randomizations if the F statistic for the test that all the coefficients were equal to zero was over 0.25.

One concern with this re-randomization approach is that outliers may lead to implicit restrictions on the randomization that result in some individuals not having equal probabilities of assignment to treatment and control. We address this concern by drawing 1000 randomizations that satisfy the criteria described above and testing for whether we could reject the hypothesis that the probabilities of treatment assignment for all individuals were drawn from a Bernoulli distribution with equal assignment probabilities. The results from this test are reported in Appendix Figure A5. We failed to reject the hypothesis that all observations were assigned to the treatment with probability one-third.

Table 2 reports the sample means for selected key baseline characteristics for both treatment and control and reports the p value of the estimated difference between these two groups. As expected given the randomization, the treatment and control groups are near identical in terms of their baseline characteristics.[11]

Table 2 and Appendix Table A3, which shows the median values of continuous variables by treatment and control at baseline, also provide a more detailed picture of the characteristics of the ORUS study sample at baseline. Average annual household income was slightly under $30,000, roughly 58 percent of the sample was employed, and 57 percent had children. Households spend about $3000 a month on average, which is actually somewhat larger than self-reported monthly household income, possibly reflecting debt households are incurring or in-kind benefits such as SNAP. Consistent with this high spending relative to income, household net worth is low at baseline—around $6000 including real estate assets and debt, and around -$5000 when real estate assets are excluded. The average

[11] One potential exception is net worth, where the treatment group has an average net worth that is several thousand dollars lower than the control group at baseline, driven primarily by differences in estimated asset values (particularly real estate assets). Although this difference is only significant at the 10% level, this could potentially raise concerns about analysis of this outcome. In Appendix Table A3, we report a balance table with the difference in medians between treatment and control and find that the median net worth differs by less than $5 between treatment and control. This suggests that the difference between treatment and control in baseline net worth is driven by a few outliers in the control group who own valuable real estate assets. Our main specifications include a rich set of LASSO selected controls, which often include the baseline value. In addition, we present robustness checks for our net worth results using median regression and difference-in-differences estimates and find results very similar to our main results. Combined, these patterns make us confident that the modest imbalance in Table 2 does not drive our results.

numbers are driven by a few participants with particularly high assets or debt, and median net worth is almost exactly zero and median net worth excluding real estate assets and debt is around -$500. The median household only has about $800 of financial assets. Consistent with these relatively low levels of net worth and savings, a large share of households in the sample face financial challenges: less than 40 percent of households could pay an unexpected $400 expense, household savings totaled only 5 percent of income, and almost 9 percent recently stayed in a shelter, car, or other non-permanent housing.

2.3 Intervention Details

Transfers to both the $1000 and $50 per month groups were made via direct deposit into the participant's bank account on the third Wednesday of every month.[12] Participants who did not have bank accounts were assisted in setting up a bank account. A very small number of participants were not able to open accounts and instead received the transfer through what was essentially a pre-paid debit card with no fees. As described above, all participants began receiving a $50 a month transfer upon enrollment into the study. The treatment group began receiving their $1000 a month transfers in November 2020 and transfers for both treatment and control groups continued through the end of the three year period in October 2023. Participants were clearly informed at all stages that the transfers would last for exactly three years. The cash payments were unconditional: recipients continued to receive transfers regardless of their participation in research activities, labor supply choices, consumption choices, residential choices, or other decisions. The NPs employed program staff who were available by phone, text message and email to answer questions and concerns from participants and help address any problems such as monthly payments not being received and updates to direct deposit accounts.

Because the study transfers were unconditional and provided by non-profit organizations, the transfer is a considered a gift and does not affect income tax liabilities. However, the transfers provided in this study were sufficiently large that, in some circumstances, they could potentially influence an individual's eligibility for government benefits. Following efforts led by our partners, the Illinois State Legislature passed SB 1735 in 2019, which exempts income received as part of research studies not funded by the state government's general revenue from affecting eligibility for benefits programs, to the extent permitted by federal law. In practice, this meant that in Illinois eligibility for Medicaid,

[12]During the study period the third Wednesday of the month never fell on a holiday, so there was never any delay in receiving the transfers.

SNAP, and TANF were not affected by the transfers received by the treatment or the control group. Supplemental Security Income (SSI) benefits could potentially be affected by the study transfers given federal law, so the non-profit organizations excluded individuals who indicated they were on SSI from the program because losing SSI benefits due to the transfers could have made them worse off in the long run. Individuals on SSDI (Social Security Disability Insurance) were also ineligible because they are already receiving a sustained cash transfer.

The extent to which housing assistance would be affected by the transfers depends on the local housing authority. Federal guidelines generally count the transfers as income for the process of determining eligibility for housing assistance, but certain housing authorities, including the Chicago Housing Authority (CHA), are granted discretion while participating in demonstration projects. CHA agreed not to consider the transfers as income for determining eligibility for public housing and housing vouchers in Chicago, but housing assistance in other locations could be affected. As a consequence, the implementing partners excluded individuals whose housing assistance would be affected so they would not be made worse off by participating.

In Texas, no such state law was passed so there was more potential for the transfers to affect benefit eligibility. Though Medicaid eligibility was not affected by study transfers, eligibility for some other benefits, such as SNAP and TANF, was potentially affected by receipt of the transfers for participants living in Texas. Appendix Table A1 more fully describes the extent to which eligibility for different government benefits could have been affected by the study transfers in both states.

During the course of the study, participants who agreed to take part in the research were asked to complete monthly online surveys and web application activities and interviewer-enumerated surveys at midline and endline. Participants received additional incentives for completing these surveys above and beyond the cash payments. Data collection activities are described in more detail in Section 4.

All program enrollees, including both treatment and control groups, were informed at treatment assignment that the transfers would end after three years, and reminders were sent in October 2022 and every few months thereafter until the transfers ended in October 2023. As the end of the transfer period approached, the NPs provided participants with updated resource lists with services or providers in their area and with national services and hotlines if they had moved to different counties or states. The NPs also had staff available by email, phone, and text message who were able to answer questions and refer participants to additional resources or services that might be able to assist them as the transfers ended. The full timeline of the study is shown in Figure 1.

3 Estimation and Inference

Cash transfers can affect a wide array of participant outcomes. This large set of plausibly affected outcomes, combined with the extensive surveys we conduct and rich administrative data we link to, results in a large set of potential outcomes for which we estimate treatment effects. This large number of estimates could result in inaccurate inference due to multiple hypothesis testing. Additionally, in many cases we measure a number of different variables related to a similar concept; aggregating such variables may make it easier to interpret the results and improve statistical power. We developed and pre-specified an estimation procedure that addresses these two issues by pooling outcomes across time, constructing indices for key outcomes, and computing q values to control the false discovery rate.

3.1 Estimation

We start by dividing outcomes into nested groups of topics, families, components, and items. Items are particular outcomes measured in the data. Following Anderson (2008), to reduce the number of primary hypotheses we test, we then aggregate these items into component indices. We then aggregate these component indices into family indices of a set of related components. A topic is a broad set of related types of outcomes, such as employment, financial outcomes, or health.

For our main specifications, we pool items across time and surveys. Specifically, we take the weighted average for a given outcome of the monthly surveys and the midline and endline surveys, placing 30% weight on the monthly surveys and 70% weight on the midline and endline surveys (when a given variable is observed in both). When computing the midline and endline average, we put 70% weight on the endline and 30% weight on the midline. When computing the monthly survey average, we put 50% weight on the third year of the study, 30% on the second year of the study and 20% on the first year of the study.[13] A given item is only missing for a given survey year if that item is missing from all surveys in that survey year. If an item is completely missing within a given survey year for a particular participant but we observe that item in other survey years for that participant, we use the mean of non-missing survey years for that participant in the pooled estimates. In additional specifications we also separately estimate treatment effects for each time period.

We estimate OLS models where we regress item j for study participant i, $Y_{i,j}$, on an indicator

[13]We define years of the study relative to the start of the transfer. Study year one is November 2020 through October 2021, study year two is November 2021 through October 2022 and study year three is November 20222 through October 2023. Surveys conducted before randomization occurred in October 2020 are referred to as baseline surveys.

for whether participant i was assigned to \$1,000 a month treatment, D_i. In our main specifications, we also use the rich set of pre-treatment covariates observed in the baseline survey, denoted by X_i^0, to improve the precision of our estimator. Specifically, as in Bloniarz et al. (2016), for each item $Y_{i,j}$ we estimate the relationship between $Y_{i,j}$ and X_i^0 using the Least Absolute Shrinkage and Selection Operation (LASSO) and select the subset of X_i^0, $\tilde{X}_{i,j}^0$, with non-zero estimated coefficients. Note that we perform this exercise separately for each item j, so the selected subset of covariates is index by j, $\tilde{X}_{i,j}^0$. The resulting model we estimate is:[14]

$$Y_{i,j} = \tilde{X}_{i,j}^0 \beta + \delta D_i + \epsilon_{i,j} \tag{1}$$

We then use seemingly unrelated regression (SUR) to aggregate these item level estimates into the component they fall within, weighting each item equally. These family level estimates are then estimated using SUR as the equally weighted average of the constituent components. We pre-specify which items within each family are included in the component level index and which components are included in the family level index.

In cases where the items within a component and components within a family do not have naturally comparable units, we standardize the item level estimates to be in control group standard deviation units before aggregating to the component level and do the same before aggregating from the component level to the family level. We do not do this for cases where the items within a component have a naturally interpretable unit. For example, all items in the consumption family are in dollars per month, so we do not standardize before aggregating to the component or family level so that the components and families can be interpreted as the aggregate value of the given measure.

3.2 Inference

Given the substantial number of outcomes that we measure, our inference would be inaccurate if it did not adjust for multiple hypothesis testing. We use Benjamini and Hochberg (1995)'s false discovery rate adjustment to compute q values within a topic using a tiered approach following Guess et al.

[14] We created a wait list during the randomization process in anticipation of imperfect compliance. In practice, compliance was almost perfect. One participant assigned to treatment initially declined the treatment and was replaced with someone from the wait list, but that participant later changed their mind and was enrolled in the treatment group. Another participant was removed from the treatment group by an NP for violating the participation agreement, but that individual continued to participate in research activities. Ultimately, 1000 of 1001 participants assigned to treatment eventually took up the treatment. Given that there is this small amount of imperfect compliance with treatment assignment, these are technically "intent-to-treat" (ITT) estimates of the effect of the cash transfer. However, given that only one person offered treatment ultimately did not take up treatment for the full study, the ITT estimates are effectively identical to Treatment on the Treated (TOT) estimates in this case, with the first stage higher than 0.999.

(2023). Family level estimates are adjusted for k_0 hypotheses, where k_0 is the total number of families within the topic. Component level estimates within family f are adjusted for $k_0 + k_{1,f}$ hypotheses, where $K_{1,f}$ is the number of unique components within family f, while item level estimates within a family are adjusted for $k_0 + k_{1,f} + k_{2,f}$, where $k_{2,f}$ is the number of unique items in family f. We also estimate results on secondary hypotheses that do not enter an index or are not of main interest, including heterogeneous treatment effects. We compute q values for these outcomes to account for $k_0 + k_{1,f} + k_{2,f} + k_{3,f}$ hypotheses, where $k_{3,f}$ is the number of secondary outcomes within family f. This paper includes outcomes from the financial health and mobility topics. Any supplementary hypotheses not described in the PAP are adjusted for $k_0 + k_{1,f} + k_{2,f} + k_{3,f} + k_{4,f}$ hypotheses, where $k_{4,f}$ is the number of supplementary hypotheses. Inference for robustness checks is not adjusted to control the false discovery rate. In our tables, we report standard errors, unadjusted p values, and the q values computed using this procedure. Appendix Table A2 describes which estimates were put into which FDR tier in detail.

As described above, there are a small number of enrolled participants who we found out might know one another during the pre-randomization period. We randomized these participants as clusters to have the same treatment status. We incorporate this into our inference by clustering our standard errors at the cluster level.

4 Data

To measure how households respond to this exogenous increase in unearned income, we need data on expenditures, household balance sheets, and financial behavior. We measure these outcomes through a combination of online surveys, phone and in-person surveys, and administrative data. We also collected detailed time and nutrition diaries and biomarkers which are discussed in Vivalt et al. (2024) and Miller et al. (2024). Below we discuss each type of data in more detail.

4.1 Enumerated Survey Data

As described above, all participants completed a baseline survey prior to randomization. This baseline survey was conducted in person (before March 2020) or over the phone (after March 2020) by trained SRO enumerators. Enumerators also conducted a midline survey (between April 3 and August 2, 2022) and an endline survey (between March 30 and August 15, 2023). The midline and endline surveys were administered over the phone for most participants, but a small number were administered in person for participants who preferred this option. These surveys covered a similar range of topics to the baseline survey. To prevent the length of the baseline, midline, and endline enumerated surveys from being too long, we asked additional questions through web-based Qualtrics surveys at baseline, midline, and endline; we refer to these as the "mobile baseline," "mobile midline," and "mobile endline" surveys. There were three such surveys at baseline and midline and four at endline. They were conducted between April 12 and October 14, 2022 for the midline and April 11 and October 11, 2023 for the endline.

The incentive to complete the enumerated baseline was $50. Towards the end of the recruitment period, there was also a $50 bonus if you kept your appointment for completing the survey. For the midline and endline enumerated surveys, the base incentive for completing the survey was $50 and participants were randomized to receive an incentive of $0, $25, or $50 for keeping their first appointment. Participants were also paid $15-30 per mobile baseline, midline, or endline survey.

Combined, these surveying efforts yielded high response rates for both the treatment and control groups with low differential attrition. We achieved 97% response rates at midline and 96% response rates at endline; control group response rates ranged from 96 to 95% and the treatment group response rate was 98% for both surveys.[15]

4.2 Online Surveys

In addition to the baseline, midline, and endline surveys, participants were invited to take monthly online surveys conducted through Qualtrics which could be completed on a mobile device or computer. The monthly online surveys covered a more limited set of topics and included a smaller set of questions than the baseline, midline, and endline surveys. These surveys were designed to provide higher frequency measures of key outcomes, to elicit additional measures of some outcomes to increase precision, to keep participants engaged with the research study, and to collect updated contact information to help maintain high response rates. Topics included in the monthly online surveys varied from month to month to cover a range of topics. For example, questions about expenditures were covered quarterly in the monthly surveys. Participants were paid $10 to complete each monthly survey.

Extensive efforts were made to get survey non-responders to complete the surveys. These included emails, phone calls, text messages, and postcards sent to the participant. If several outreach

[15] Baseline survey completion rates were 100% for both treatment and the control group as baseline survey completion was a requirement for randomization into the study. Participation in the research was not a requirement for participation in the program and receipt of the cash transfer, however, but there was only one person who chose to take part in the program but did not consent to the research.

attempts with these methods had been made without success, OpenResearch staff reached out to alternative contacts outside of the person's household (which the participants provided at baseline) to obtain new contact information. For the enumerated midline and endline surveys, SRO enumerators would visit the last known address of the participant as a last resort, as long as this address was within the geographic area covered by the enumerators.[16] More extensive detail on the survey approach is provided in an online guide to the ORUS study here, including the full text of all survey instruments.

Response rates were lower for the monthly online surveys and activities than the enumerated surveys, but still quite high at 85.3 percent overall, with average monthly survey response rates ranging from 82.2 percent for the control group to 88.5% for the treatment group. These response rates to the online surveys are even higher if you consider whether respondents completed any monthly Qualtrics surveys in a particular year. Figure 3 reports response rates for monthly Qualtrics surveys defined by whether the respondent ever completed a monthly Qualtrics survey and, for comparison, response rates to the enumerated midline and endline surveys. We see that about 98% of both the treatment and control group responded to at least one monthly Qualtrics survey in the first year of the study and 94% of participants responded to at least one monthly Qualtrics survey in the third year of the study.

4.3 Administrative Data

We link individuals to their Experian credit records to provide a more complete picture of their household liabilities. Participants were asked to consent to link their information to administrative records, including credit records, during the online screener, again during the baseline survey, and again after the program began. The vast majority consented at baseline or during the screener. Overall, this resulted in 86.9 percent of participants in the control group and 88.8 percent of participants in the treatment group consenting to link their study data to administrative records. We match individuals to their Experian credit reports using name, address, date of birth and, when available, Social Security Number. Of those who agreed to link their data to administrative records, 95.2 percent of the control group participants and 96.7 percent of the treatment were successfully linked. This means that ultimately 82.7 percent of control group participants and 86 percent of treatment group participants were linked to their Experian records.

[16] The enumerators were based in the 18 study counties in IL and TX from which participants were recruited and had a geographic radius around these areas.

5 Results

We present four sets of results on the effects of the treatment on household outcomes. First, we present results on how the treatment affected expenditures. Second, we present results on how the treatment affected financial health and resilience. Third, we present results on how the treatment affected household balance sheets. Fourth, we present results on how the treatment affected credit and financial behavior.

5.1 Expenditures and Consumption

In this section we present results on the effects of transfer on expenditures and other key consumption choices. We start by discussing the detailed expenditure survey we conducted, then present estimates of the effects of the transfer on expenditures, and finally show evidence on how the transfers have affected location decisions.

5.1.1 Expenditure Data

We measure expenditures through expenditure questions administered in both the online surveys and the enumerated surveys. These surveys are similar in structure to the Interview Survey portion of the Consumer Expenditure Survey (CEX), but our versions are much abbreviated. We convert all spending numbers into implied monthly spending and then combine regular and unexpected spending into an aggregate monthly spending measure. There were slight differences in the expenditure questions between the online and the enumerated surveys and we combine some spending categories and rescale others to make measures from the two surveys equivalent. We provide more detail on the construction of the expenditure data in Appendix Section A.

How well does our survey capture participant spending patterns? In Table 3 we compare total monthly spending by year from our data with total spending from the CEX in the same years. Column (1) reports the average total monthly spending in the control group in the ORUS study and Columns (2) - (4) report CEX total average consumer spending for consumer units containing someone aged 21-40 in 2020 using different weights to approximate the characteristics of the ORUS study sample. Column (2) just uses the CEX weights, showing average spending for the US population as a whole with a person in the ORUS age range. Column (3) then reweights the CEX sample to approximately match the distribution of total household income, highest level of educational attainment, and race of the ORUS sample. Column (4) then applies both CEX population weights and the ORUS income, education, and race weights. Different rows report these values for the years 2021 through 2023 and the average over this time period.

The ORUS survey monthly expenditure totals match the CEX totals remarkably well. As expected, column (2) showing the average spending in the CEX without reweighting to match the ORUS income distribution is substantially higher than the ORUS spending levels, reflecting the much higher income in the non-reweighted CEX sample. Columns (3) and (4) reweight to match the ORUS sample in different ways, with column (3) using the CEX weights and column (4) using income and education weights. In both of these cases, the reweighted CEX monthly spending is extremely close to the monthly spending we measure in the ORUS sample. The ORUS sample average over the first two years of the transfer is about 2 percent lower than the CEX benchmark reweighted to to match the ORUS income, education, and race distribution only in column (3) and about 4 percent higher than the CEX benchmark reweighted using all CEX and ORUS weights (income, education, and race) in column (4). Moreover, looking at the spending comparisons by year, we see that the ORUS monthly expenditure surveys closely match the rise in household spending between 2021 and 2022, suggesting that the ORUS expenditure surveys capture changes in spending as well, increasing our confidence that we are able to capture the treatment effects of the transfer.[17]

Although the ORUS survey matches the CEX, it is well established that CEX expenditure measures don't capture all consumption, particularly irregular expenditures (see e.g. Bee et al. (2015) and Passero et al. (2015)). For example, CEX expenditures in 2022 only represented roughly 73 percent of total consumer expenditures when restricting to comparable categories from the Personal Consumer Expenditures (PCE) data produced by the Bureau of Economic Analysis (BEA). The PCE, among a variety of sources, includes data on sales from retailers, and is less likely to be subject to underreporting than consumer surveys like the CEX. Consequently, it is likely that the ORUS survey, like the CEX survey, understates spending—particularly on irregularly purchased non-durable goods and durable goods. In our discussion of the results in section 6 we present some versions of our estimates where we scale up the results to reflect this underreporting of spending on surveys like the ORUS survey and the CEX.

[17]The full 2023 CEX data have not been released as of the release of this paper. For this reason 2023 data are not included.

5.1.2 Expenditure Results

Table 4 reports estimates of Equation 1 of the difference in outcomes between the treatment group and the control group for total expenditures and detailed expenditure categories. We aggregate the consumption surveys to the study year level and estimate these treatment effects for outcomes pooled over the three years of the study using the weights described in section above.[18] We winsorize expenditure items at the 99th percentile. Column (1) reports the control group mean and standard deviation in parentheses for this pooled outcome. Column (2) then reports the treatment effect estimates. Standard errors are shown in parentheses below and the FDR-adjusted q values are shown in brackets below the standard error. Statistical significance relative to a null hypothesis of no effect using traditional inference is shown using asterisks, while statistical significance after FDR adjustment is indicated using daggers. Family outcomes are shown in bold, components within families are underlined, and items within components are shown indented.[19] Secondary,tertiary, and post-PAP outcomes are shown in italics although, except for a few cases, these outcomes are reported in appendix tables. This same layout is repeated in all of our main results in Tables 4 through 13 and in additional results reported in Appendix Tables A4 through A15.[20]

Expenditure categories are ordered top to bottom by the control mean, with the largest spending categories at the top of the table and the smallest spending categories at the bottom of the table. We also classify these detailed expenditure categories into broader expenditure categories of non-durable spending, durable spending, housing spending, and human capital expenditures. For reference, we include an additional column describing the broad spending category of each detailed category.

Starting with the top of the figure, we see that overall total spending (the family outcome for expenditures) rises by $310 per month over the course of the study—roughly 7 percent or 0.17 standard deviations higher than the control mean. This estimate is precise; we can reject treatment effects of smaller than $184 or greater than $435 per month and the q value is 0.001.

Our rich survey data allow us not only to estimate treatment effects on total spending, but also to investigate the treatment effects on individual detailed spending categories that make up the total

[18] As mentioned above, study year 1 is November 2020 through October 2021, study year 2 is November 2021 through October 2022 and study year 3 is November 20222 through October 2023.

[19] No components are reported for families like expenditures with only one primary component that enters the family because the component and the family point estimates will be identical

[20] Note that given the evidence that expenditure surveys like the ORUS survey and the CEX understate expenditures, when we present results on the marginal propensity to consume in Section 6 we report both estimates based on the raw estimates reported here and rescaled estimates reflecting underreporting by category.

expenditures treatment effect. This more detailed picture of the spending effects of the transfer helps us to understand how the allocation of cash transfers might differ from in-kind transfers. It also allows us to classify how much of the change in spending is allocated to non-durable goods and services versus durable goods and services, which helps us understand how much of the change in expenditures is going towards current versus future consumption.

We report these detailed expenditures in the rows below the total expenditures family outcome. Four patterns stand out in the these results. First, we estimate the largest treatment effects in dollar terms for the three categories with the highest spending in the control group. Food and non-alcoholic beverage consumption spending rises $67 per month (6.9 percent of the control mean), rent expenditures increase by $52 a month (8.6 percent of the control mean), and car payment and insurance expenditures rise $30 a month (8.2 percent of the control mean). Second, estimated treatment effects are positive in almost every category, with the one exception of mortgage payments which fall $7 per month. This negative effect on mortgage spending is very imprecisely estimated, reflecting the low share of the sample which owns a home. Moreover, the positive estimated effects are typically fairly precisely estimated and we can reject zero effect, with the exception of education expenditures, which are a small share of spending at baseline. Third, the largest treatment effect in percent terms is loans and gifts to family and charity, which rises almost 26 percent. Fourth, spending on “vice" goods, such as alcohol, tobacco, drugs, and gambling do not rise notably more than other spending categories in either dollars or as a percent of the control mean. In total, spending on these vice goods only rises by about $13 per month, or a little over 1% of the total transfer.

Appendix Table A5 reports treatment effects aggregating these detailed spending categories into the broader categories of non-durable goods and services spending (excluding housing), durable spending, housing spending, human capital expenditures (such as education spending and child-care spending), and other. The largest treatment effects are on non-durable goods excluding housing, which rise by $143 per month, accounting for almost half of the total increase in spending. We find similar increases of $46, $48, $41, and $43 for housing spending, human capital spending, durable goods and services spending, and other spending, respectively.

5.1.3 Housing Unit and Neighborhood Mobility

These changes in spending patterns are leading to significant changes in some important household consumption choices, such as mobility. During every monthly survey, people were asked whether or not they had moved since the last address they provided and, if so, what their new address was. These questions allow us to construct a panel of place of residence for recipients throughout the study. We geocode these locations to construct measures of changes in neighborhood, which we define as changing census tracts. Table 5 reports the estimated treatment effects on household mobility across housing units and neighborhoods. The family level outcomes for both measures is an index of two components: the first component is an indicator for making the given type of move since the baseline period, while the second component is a measure of interest and search activities related to the type of move in question. Starting with the first panel of Table 5, we see that the housing unit mobility index rises by 0.11 standard deviations. The standard errors are small enough to rule out small increases in the housing unit mobility index, and the estimate is significant at the 0.1 percent level even after FDR adjustment. This change in the index reflects both higher moving rates and higher housing unit search. Moving housing units was 4 percentage points higher in the treatment group, or roughly 10 percent higher than the control mean. This difference is significant at the 1 percent level after FDR adjustment. The second component reports housing unit search interest and behavior. This component rose 0.12 standard deviations; this change is precisely estimated enough to allow us to rule out changes in the housing search index of smaller than 0.06 standard deviations and is statistically significant at the 0.1 percent level after FDR adjustment. Whether or not someone was interested in moving housing units rose 5 percentage points, looking to move housing units rose 4 percentage points, and whether or not someone reported any active housing search behaviors rose 4 percentage points, while the number of active search actions rose by 0.3 actions. These changes are all statistically significant with FDR adjustment.

The second panel of Table 5 then reports the estimated effect of the transfer on neighborhood mobility. We define neighborhood mobility as changing census tracts (using the 2010 US Census definitions). We see that, like housing unit mobility, there is a substantial increase in neighborhood mobility. The overall neighborhood index rises a precisely estimated 0.12 standard deviations. Like for moving housing units, this is driven both by a rise in moving neighborhoods since baseline and the neighborhood search and interest index. Moving neighborhoods relative to baseline rises somewhat more than moving housing units—4.4 percentage points compared to 4.1 percentage points. This difference is significant at the 0.5 percent level even after FDR adjustment. Turning to the second component of the neighborhood mobility index, the neighborhood search index, we see that all elements of the index rose 20 to 25 percent relative to their control means, with the share of households interesting in moving neighborhoods rising 6 percentage points, the share of households engaging in any active neighborhood search behaviors rising 5 percentage points, and the number of active search behaviors rising 0.1 actions. Combined, these increases in neighborhood move interest and search resulted in a neighborhood search index that is 0.14 standard deviations higher than the control mean.

Appendix Table A15 presents results for a number of secondary outcomes that provide further detail on the effect on different types of moves made by different respondents. In particular we see that the share of people moving at least 2 miles from their baseline address rose about 5 percentage points, while the share that changed jurisdiction or cities rose about 3 percentage points. These findings suggest that the transfer is not just inducing people to move across neighborhood boundaries, but is also causing people to move substantial distances from their baseline neighborhood.

In addition to allowing individuals to spend more on goods and services, cash transfers may also permit individuals to work less and spend more on leisure. In a companion paper, Vivalt et al. (2024), we estimate how the transfer affected labor market outcomes, including both individual and household income. Vivalt et al. (2024)'s preferred estimates show that the transfer reduced individual annual income by around \$1500 and household annual income by around \$2500 a year, which translate into reductions in monthly income of \$125 and \$210 respectively.[21] Consistent with our finding that spending on essentially all categories of goods and services rises, this finding shows that households choose to consume more leisure as well.

5.1.4 Expenditure Volatility

The greater stability of the monthly ORUS transfer may allow households to avoid fluctuations in spending from month to month due to income or expenditure shocks. Table 6 reports estimates of the effect of the treatment on the standard deviation of log regular monthly expenditures. We see that the standard deviation of log monthly expenditures declines by 14 log points, or about 20 percent of the control mean. In Appendix Table A14, we see that that this decline in the standard deviation of monthly expenditures is largely driven by a reduction in the standard deviation of non-durable goods and services. These are precisely the expenses that would be easiest to adjust when negative shocks

[21] The estimated effects on self-reported total individual income and household income are somewhat larger, \$2500 and \$4100 respectively. The estimated effect on individual income calculated by adding up sub-categories of individual income is -\$1500. Vivalt et al. (2024) argue that this calculated individual income is likely more accurate and adjust treatment effect on self-reported household income by the ratio of the treatment effect estimate for calculated individual income to self-reported individual income, i.e. $-4100 \times \frac{1500}{2500} \approx 2500$.

occur.[22]

5.2 Financial Health and Resilience

Changes in spending choices and consumption volatility may change financial health and resilience. Table 7 presents estimates of the effects of the treatment on an index of financial resilience and its component indices, which measure exposure to financial hardship, savings relative to income, self-reported financial health, and financial resilience. Appendix Tables A11 report the individual items for the financial health component, while Appendix Table A12 reports the individual items for the financial resilience component. The family level index for financial health rises 0.05 standard deviations. The estimates are precise enough to rule out effects smaller than 0.01 standard deviations and are significant at the 2 percent level with FDR adjustment. The improvements in financial health are driven by an increase in the share of participants with at least $100 in savings, decrease in reported reliance on financial help from friends and family, and improvements in savings relative to income, self-reported financial well-being, and the financial resilience index, which all rise from 0.03 to 0.11 standard deviations. Conversely, the index of financial hardship falls slightly (although insignificantly), indicating increased hardship. This increased hardship is driven by a greater incidence of exposure to financial shocks but is partially offset by an imprecisely estimated decline in running out of money between paychecks or before the end of the month.

We use the Consumer Financial Protection Bureau (CFPB) Financial Well-being Scale to measure participants' perceptions of their present and future financial health. While the overall financial health score improves significantly in the first two years of the study and fades in year three, the largest effects are observed on individual items related to respondents' current financial circumstances. The treatment group report increases in the extent to which they can handle a major unexpected expense, give a gift for a wedding, birthday, other occasion without straining finances, and enjoy life because of the way they manage money, as well as increases in the frequency of having money left over at the end of the month. For other items focused on respondents' longer-term financial circumstances, such as level of confidence in retirement savings, concern that savings will not last, and extent to which they are "just getting by financially," however, there is no effect. The short-term influx of cash provided by the transfers reduces consumption volatility, improves financial well-being, and reduces vulnerability in the short run, but it does not appear to reduce long-term financial anxieties—at least

[22] The consumption volatility measure uses the volatility of the core expense questions and does not add expenses from the unexpected expenses module. The measure also does not rescale enumerated expense measures to match the Qualtrics online survey expense measures as we do for the main expenditure measures and described in Appendix Section A.

for the younger, low-income households in this study.

5.3 Household Balance Sheets

Treatment households can also use the transfer to change their borrowing and saving behavior, potentially affecting the household stock of real assets, financial assets, and debt. In this section we present results on how the transfer has affected household balance sheets. Table 8 presents our estimates of the treatment effect on the asset side of the balance sheet, while Table 10 presents estimates of the effect of the treatment on total debt. To provide additional information on changes in assets and debt, we also present evidence on the treatment effect on homeownership in Table 9, vehicle ownership in Appendix Table A7, and on minimum required debt payments in Table 11. Finally, Table 12 reports how these changes in the household balance sheet have affected overall net worth. For all three of these categories, we report assets and debts that are individually or jointly held by the respondent as our preferred estimates. To provide a more complete picture, in Appendix Table A8 we report effects on assets that are only individually held and assets that are owned by someone in the household, even if they are not owned jointly by the respondent.

5.3.1 Asset Results

We collect data on assets through survey questions asked during the midline and endine, while we receive information on debt, minimum debt payments, and delinquencies both through survey questions and credit history data from Experian. We ask participants about individual and household savings, retirement, and investing accounts and the value of real estate holdings and business assets. We also asked for the make, model, and year of any cars at least partially owned by household members and estimate the value of the car using a database of vehicle valuations. Recipients are also asked about the value of any cryptocurrency holdings.

Starting with the first panel of Table 8, which reports estimates of how the cash transfer has affected the value real assets, such as cars, real estate, and business assets, we see that the estimated effect on total real assets is near 0—about -$200, and imprecisely estimated. The estimated effect on individual categories of assets are all less than $350 in absolute magnitude. For the value of vehicles owned, the standard errors are small enough to reject positive effects larger than $800 or negative effects larger than -$400, while for business assets we can reject positive effects larger than $2100 or negative effects larger than -$1700. For other assets we can reject positive effects larger than $1000 or negative effects larger than -$1200.

The estimated effects on total real assets discussed above are imprecise; we cannot rule out increases or declines in assets of less than $6000. Given the low share of households at baseline that own real estate, the high variance of real estate valuations, and the null estimated effect on homeownership which we describe below, there is unlikely to be a large treatment effect on real estate valuations and their inclusion likely adds noise to the estimates. To investigate this, we also present results on the effect on total real assets excluding real estate assets. We find a similarly small effect, -$11, but the estimate is now much more precise. When excluding real estate we can rule out rises or falls in total real assets of larger than $2500.

Respondents may be better able to report assets they individually hold rather than those they jointly hold with their partner or others in the household. To investigate this concern, in Table A8 we also report the value of real assets and financial assets individually held by the study participant. The pattern we see here is broadly similar, with a very imprecisely estimated decline in total real asset values (although the magnitude is larger for the individually held assets), but a small and moderately precisely estimated decline in asset values when real estate assets are excluded. In the case of individually held assets, we can rule out increases in asset valuations of larger than $1500 or decreases larger than $2500.

These small estimated effects on the value of vehicles and real estate are confirmed by the estimates in Appendix Table A7 and Table 9, where we estimate the effect of the transfer on vehicle ownership and homeownership. Starting with the vehicle ownership results, we estimate a positive roughly 2 percentage point estimate for whether the participant individually or jointly owns a vehicle and whether the household owns or leases a vehicle. The estimated effect on the household owning or leasing a vehicle is significant without FDR p value adjustment but insignificant with it. In both cases, we can reject increases in vehicle ownership of larger than 4 percentage points. These small estimated effects on vehicle ownership are broadly consistent with the estimated effects on the value of owned vehicles reported above. For example, if we assume the two percentage point estimate of the effect on vehicle ownership is correct and the value of the average vehicle purchased was a $10,000 used vehicle, then that would exactly imply the $200 effect on average asset valuations that we find. Even if the average individual bought a much more expensive $30,000 vehicle, the implied change in the average value of vehicle assets would be only $600, which is within our confidence interval.

Turning to Table 9, which reports estimated effects on homeownership, we see that, if anything, homeownership declines in the treatment group relative to the control group, although this change is insignificant and we can't rule out small rises of up to around 4 percentage points. Below the main homeownership indicator we report several secondary outcomes that use alternative measures of homeownership or the change in homeownership. In the first alternative measure, we use a more expansive definition of homeownership that includes participants who indicated that they lived in a household where someone within the household owned the home but that they were not on the mortgage and did not share ownership of the home themselves. This measure generates a tighter zero, allowing us to rule out rises in homeownership of more than 2.4 percentage points. We also estimate small negative effects of the cash transfer on an indicator for becoming a homeowner, conditional on being a renter at baseline, using both our base and more expansive definitions of homeownership and are able to reject increases in homeownership among those who were renters at baseline of more than 0.7 percentage points. The final two rows explore whether there have been changes in shared homeownership and the number of people homeownership is shared with. Again, we estimate quite small estimates that, if anything, imply a slightly negative relationship between receipt of the transfer and homeownership. Combined, this evidence on vehicle and home ownership confirms our finding that the treatment has no or modest effects on real asset values.

The second panel of Table 8 reports estimates of the effect of the cash transfer on financial assets, including bank accounts, retirement accounts, investments, and cryptocurrency holdings. As with real assets, all of these report results for the effect of the transfer on individually and jointly held accounts, with the exception of cryptocurrency holdings for which we just asked about individual holdings. Overall total financial assets rise by about $1000 for the treatment group relative to the control group, although the standard error is large enough that we can't reject financial assets falling by about $1000 or rising by about $3000. These estimated effects are driven by a roughly $800 rise in savings in bank accounts, which is marginally significant without FDR adjustment but insignificant after FDR adjustment. However, it is non-trivial in magnitude, representing an increase of roughly 12 percent of the control mean. The total value of savings in retirement and pension accounts falls by about $400, possibly reflecting the decline in employment during the transfer period documented in Vivalt et al. (2024), but this effect is imprecisely estimated. Savings in other types of investment accounts increases by almost $500, but the standard errors are large enough that we can't reject the null hypothesis that there was no change. We find small changes in the value of cryptocurrency holdings and we can reject increases of more than $300 in the value of cryptocurrency holdings.

As with real assets, one concern is that individuals have better information about their own asset holdings or the transfer affects individual assets more than joint assets. This is particularly salient given that the transfer is provided to the individual, not to the household. We investigate this possibility in the second panel of Appendix Table A8, where we report estimates for financial assets that are only individually held. We generally find large effects. Total savings held individually rise by about $2200, although this change is not significant after performing the FDR adjustment. Half of this increase is made up by an over $1000 increase in the value of individual savings in bank accounts, which is significant at the 10% level after FDR adjustment and is economically large, representing an almost 25% increase relative to the control mean. The estimated total change in other investments also changes more for these solely individually held accounts, rising about $750, although this change is not significant after FDR adjustment. These findings suggest either better measurement of individual savings or, potentially, that participants preferred to increase their individually held savings rather than their jointly held savings.

5.3.2 Debt Results

A large rise in unearned income could not only affect the assets side of the household balance sheet, it could also affect the liabilities side if households decide to pay down or increase debt in response to the cash transfer. Households may pay down debt in order to improve their net worth, save on monthly payments, and improve their credit, or they may take out more debt if the transfer makes them feel more able to afford the payments or down payment for a loan or if changes in other aspects of their financial behavior result in greater access to credit.

For individuals who did not consent to link to administrative data, the endline and midline surveys include questions about the total amount of outstanding debt in different categories (mortgage, student loan debt, auto loan debt, credit card balances, and more) and about minimum monthly payments for outstanding loans. For those who did consent, we link cash transfer recipients and their households to Experian data. We use the Experian data to construct measures of the individual's total outstanding debt and minimum monthly debt payments for the 87 percent of participants who consented to share administrative data. We aggregate these quarterly data to the study year level. For debt balances and monthly payments, we supplement these data with self-reported values of these same variables for participants who did not consent to share administrative data. Because the household balance sheet questions were only asked in the enumerated surveys, our main specifications use both Experian and survey data only from baseline, study year two, and study year three.[23] We winsorize asset and debt items at the 99th percentile.[24]

Table 10 reports the estimated effect of the cash transfer on debt. In our preferred estimates, we combine credit bureau data from Experian on debt with survey question answers on debt for individuals who did not consent to link to their credit records. The overall estimated effect on debt is positive, with debt rising by almost $1800. However, the standard error is large enough that we can't reject falls in debt of as much as $2000. This rise in debt is driven in roughly equal parts by rises in auto loan debt and mortgage debt. The change in auto loans of $800 is fairly precisely estimated and moderate negative effects on auto debt are ruled out, although the estimate is not statistically significant after FDR adjustment. The estimated effect on mortgage debt is imprecisely estimated, and we can't rule out up to $4000 increases in mortgage debt or $3000 declines. The transfer reduces student loan debt by $200, although the standard error of $910 is large enough that we can't reject moderate rises or falls in student loan debt. Estimated effects on other types of debt are small, and the standard errors are small enough that we can reject large increases or decreases. For example, we can reject increases in credit card balances of more than $400 or decreases of more than $300.

As in the case of real assets, housing related debt substantially increases the imprecision of the estimated effect on overall debt. Table A9 presents alternative measures of debt, including the estimated effect on total debt excluding mortgage debt. Once mortgage debt is excluded, we see that the total change in debt is smaller (roughly $500) and more precisely estimated, allowing us to rule out rises in debt of more than $2500 or reductions in debt of more than $2000. Another concern with these debt data are that self reports of debt balances may not be very accurate if people infrequently check their balances. To investigate this concern, we also present estimates in this table on total debt and the individual debt types using only the Experian data. Note that we do not include informal debt in this set of debt variables because we only measure informal debt in the surveys. The picture of the change in debt using just the Experian data is qualitatively very similar, although the quantitative magnitudes are slightly higher. As before, we can reject debt reductions of more than about $2500. Once we exclude mortgage debt from the Experian only measure, we can reject reductions in debt of more than $1500.

The estimates of the effect of the treatment on total debt reported above suggest that households

[23] As mentioned above, study year 1 is November 2020 through October 2021, study year 2 is November 2021 through October 2022 and study year 3 is November 20222 through October 2023.

[24] We separately winsorize the survey and Experian data before combining them.

did not use the transfer to pay down debt (on net). However, the estimates are noisy and could potentially mask modest debt reductions. Moreover, even if households did not reduce debt on net, they could have consolidated or restructured debt to reduce their required monthly payments. Table 11 reports the estimated effects of the cash transfer on minimum required debt payments, providing further evidence of the effects of the cash transfer on debt obligations. Our preferred measure uses Experian data as the primary outcome and relies on survey data for households with missing Experian data and those who did not consent to link their credit histories. Starting with the first row, which shows the component level outcome, we see that total minimum monthly payments on all loans rises about \$50, or 11% of the control mean. This is driven mostly by rises in monthly payments on vehicle loans and leases, which rise \$17 per month, and monthly payments on bank loans (including revolving debt such as credit cards), which rise \$21 per month. The effects on total monthly payments and payments on vehicle loans are highly significant without FDR adjustment but insignificant with FDR adjustment. We find student loan payments unchanged and can rule out more than modest rises in student debt obligations. Mortgage debt obligations rise by \$7 per month, but this estimate is imprecise.

These estimates are quite consistent with the less precisely estimated rises in total debt presented in Table 10. For example, total auto debt rose by about 17% of the control mean and monthly minimum payments on auto loans rose by 16.5% of the control mean while both total student loan debt and student loan debt minimum payments are unchanged. The notable exception is bank loans, for which total debt only rises 6% (combining the total revolving credit and bank and credit union loans), while minimum payments on such loans rise roughly 20%. This inconsistency may reflect differences in the minimum payment requirements of the marginal loans, greater imprecision in estimates of total loan balances, or some other factor. Regardless, in combination with the evidence on total debt, Table 10 suggests that households did not use the cash transfer payments to reduce debt obligations and instead modestly increased such obligations, both in terms of total debt and required monthly payments.

5.3.3 Net Worth Results

Table 12 presents estimates of how these changes in real assets, financial assets, and debt have translated into changes in total net worth. We compute total net worth as the sum of total real and financial assets minus total debt. The bolded first row presents estimates of the effect of the cash transfer on our main measure of net worth, which includes real estate assets and mortgage debt. We estimate that net worth declined about $1000 over the course of the study. This decline is roughly 4.5 percent relative to the control mean of $39,000. The estimated effect on net worth is fairly imprecise, but the standard errors are small enough that we can rule out increases in net worth above $5,600. Below this main estimate, we present an alternative estimate excluding real estate assets and mortgage debt. This estimate qualitatively similar and quantitatively somewhat larger, implying a roughly $2000 decline in net worth.

One potential concern with these estimates is that, as discussed above, there is some imbalance in net worth at baseline between the treatment and control group driven primarily by a small number of observations with particularly high asset values in the control group. Our strategy uses LASSO to choose a rich set of controls and the selected controls often include the baseline values of the dependent variable, which ameliorates this concern significantly. Nevertheless, we explore the possibility that this baseline imbalance may have affected our results in Appendix Table A10, which reports estimates of the treatment effects of the cash transfer using two alternative approaches: median regression and a differences-in-differences strategy. Column (1) repeats our estimates from our main specification, Column (2) reports results using median regression, while Column (3) estimates difference-in-differences results where we estimate the treatment effect by regressing outcomes on a treatment indicator, an indicator for the data being from after the treatment, and the interaction of these two variables. The different rows report results for different measures of net worth. The first row presents results for our main estimates of net worth where we aggregate the item level estimates to components (financial assets, real assets, and debt) and then aggregate these component level estimates to generate an estimate of the effect on net worth. Unfortunately, median regression did not converge for some of the individual items that make up the components, so we are unable to report the median regression outcomes for this approach to computing net worth. To compensate for this, in the next row we report estimates for a variable where we construct a net worth variable directly and estimate treatment effects on that variable instead of aggregating treatment effect estimates on sub-components. For this variable, we're able to compute all three robustness checks.[25] Finally, the third row reports estimates using a measure of net worth that excludes real estate assets and mortgage debt. All three measures use debt measures that combine the Experian and survey data.

Starting with the median regression estimates, we see that estimates using both the net worth

[25]Estimates for this variable differ slightly from estimates aggregating the item and component level treatment effect estimates because of slight difference in how missing values are treated.

individual variable and net worth excluding real estate assets and debt are -$1,000 and -$800, quite close to our estimates using our main specifications of -$1,900 and -$2,100. Moreover, the median regression estimates are more precise and we can rule out improvements in median net worth in the treatment group of more than $1,400 and $800. Turning to the different-in-differences estimates, we again find estimates quite close to those using our main specification. The family level treatment effect estimate is -$700, very close to the main family level estimate of -$1,000. The estimate using the individual net worth variable is -$1,000. Finally, the difference-in-differences estimates using the net worth variable excluding real estate is $600. The standard error is small enough that we can rule out positive effects of more than $4,800 and we cannot reject our main treatment effect estimates. Combined, this evidence confirms our main finding that, at least over the course of the transfer period, treatment households did not substantially increase their net worth.

The estimated effects on expenditures and household balance sheets above may miss part of the treatment effect if irregular expenditures such as down payments are not reported or households have difficulty remembering current financial asset balances.[26] We investigate this possibility by surveying participants after the transfer ended about financial outcomes over the full three year period of the transfer. These estimates are reported in Appendix Table A4. The first row reports the estimated treatment effect on the largest amount of savings held at any point during the program; we find that the treatment increased the maximum savings by around $2400 during the course of the study. This is somewhat larger than our estimate for the total effect on individual and jointly held financial assets of positive $1000 and our estimate for the total effect on solely individually held financial assets of $2100. However, this estimate is still qualitatively consistent with our broader findings that households saved a relatively small portion of the total transfer. This higher maximum savings also suggests that participants may have accumulated savings during the course of the program that they then spent before the end of the program to cover unexpected expenses or other financial shocks. This is consistent with the (noisy) time series estimates for net worth, where the estimated effect on net worth is positive in year 2 of the study before becoming negative in year 3.

The next five rows then report the total amount of money during the program that was used as a down payment to purchase a new home, spent on court costs, fines, and fees, taken from the participant without permission, spent on home or apartment renovation costs, or given to others. All

[26] There is an unexpected expenses module that is incorporated into the expenditure figures, but some irregular expenses may not be included by participants' in their responses to these questions. For example, if a household makes a down payment on a house or gives a onetime gift to a family member, these expenses may be expected despite the fact that they are irregular and consequently may not be included by participants when they report unexpected expenditures.

of these estimates are small in magnitude and insignificant with the exception of the amount of money given to others; treatment participants spend an estimated $770 more than the control group over the course of the program. This estimate is extremely close to the estimated monthly expenditures on net gifts to charity, family, and friends, which totals to $792 when integrated over the full program. For the other categories, the estimates are not only near zero, the standard errors are small enough to rule out large effects. We can rule out treatment effects on down payments of over $1500, on the amount paid for court costs and fines of over $100, the amount taken without permission of over $260, and the amount spent on renovation costs of over $200. Combined, these results suggest that the treatment did not have a substantial effect on these types of irregular, potentially large expenses listed in this table, with the exception of money given to others. These findings confirm the picture portrayed by our main expenditure and household balance sheet surveys.

5.4 Credit Access and Financial Behavior

The transfer to the treatment group may also change credit repayment and other financial behavior because the increased unearned income may allow participants to more consistently make debt payments, take on more financial obligations in response to the transfer, and plan for the future more. These outcomes may not be apparent in the household balance sheet because some behaviors, like on-time payment, may have little influence on the overall household balance sheet. Small changes in average savings and debt may also mask larger changes in other moments of these variables, such as the share of people with some amount of liquid savings. We use a combination of survey data and credit bureau records from Experian to understand how the cash transfer influenced the financial behavior of participants. We use the Experian data to construct measures of the individual's credit delinquencies, credit scores, repayment history and other financial outcomes for the 87 percent of participants who consented to share administrative data. We also ask survey questions on financial health and resilience such as self perceptions of financial health, the largest unexpected payment the recipients could make, and recent bill payments history.

We start with Appendix Table A13, which reports the estimated effect of the transfer on financial behavior, including bankruptcies and foreclosures, credit utilization, and credit delinquencies. We find no change in bankruptcies or foreclosures, although given the infrequency of this outcome our standard errors are large enough that we can't rule out moderate sized positive or negative effects. Similarly, credit utilization in the past three months is also unchanged and our standard errors are small enough that we can rule out more than a 3 percentage point change in credit utilization in either direction. The treatment does not appear to change measures from Experian on credit delinquencies: estimated effects on having any delinquencies in the past 6 months, total balance past due in the past 6 months, number of derogatory trades in past 6 months, and current worst present status are all small. The standard errors are small enough to rule out more than moderate improvements or deteriorations in credit delinquencies.

These changes to borrowing, spending, income, and financial behavior (as well as the cash transfer directly itself) could influence participants' ability to access credit. Table 13 uses credit bureau data from Experian to estimate how the cash transfer influenced an index of credit access. The family level index is composed of two primary elements: available credit limit in the past three months and total credit limit in the past three months. These variables both rise, with the overall credit limit rising almost seven percent, but both estimates are insignificant. Combined, this leads to a small 0.02 standard deviation rise in the overall index of credit access. We also report several other secondary items to provide more information on the change in the respondents' credit access. Credit scores rise by about 6 points, or 1 percent of the control mean. This change, although modest, is statistically significant with FDR adjustment. There is no change in whether or not the respondent has a credit score or in proxies for the credit approval rate, revolving inquiries per trade in the last three months, and total inquiries per trade in the past three months. Combined, these findings suggest that the transfer allowed participants to obtain some gains in credit access on average, but these gains were relatively small.

5.5 Treatment Effect Estimates Over Time

The pooled estimates presented above may mask temporal patterns in treatment effects that shed light on the mechanisms driving our findings. Figures 4 through 7 present estimates of how treatment effects for family level outcomes and selected key elements of these family outcomes evolved over the course of the transfer.

Figure 4 shows how total spending and the broader spending categories evolved over time. The figure includes the baseline period and the black dots show how the control mean has evolved since the baseline period, while the red dots show how the control mean plus the estimated treatment effect for the given time period.[27] 95% confidence intervals around the estimated treatment effect are shown

[27] For study years 1, 2, and 3 we present estimates of the treatment effect using Equation 1. For the baseline period, we present estimates of Equation 1 where we remove the the baseline value of the outcome from the LASSO selected controls

in red. Starting with the first panel showing total spending, we see a fairly stable treatment effect on total spending over time, starting around $270 in year 1 and rising slightly to over $330 by year 3. Spending in other categories is mostly stable as well, with the largest exception being housing spending which rises from a treatment effect of only around $11 per month in year 1 to over $58 per month by year 3. This rising treatment effect on housing spending matches a rising treatment effect on mobility which will be discussed below and may reflect the time it takes for individuals to identify and move to new housing units. Durable spending also exhibits some changes over time, with spending rising from just over $23 per month in year 1 to over $61 per month in year 2, before dropping back to around $34 a month in year 3. This pattern could reflect individuals saving up to make durable goods purchases in the second year of the study and then, once those purchases are made, durable goods treatment effects may decline.

Figure 5 shows how net worth and its three components, real assets, financial assets, and debt, evolved over the course of the study for the treatment group and the control group. Financial savings rise almost $2000 by survey year 2, but then the treatment effect declines to only around $500 by the endline survey. Similarly, total real assets actually rise somewhat by year two but then decline to a negative point estimate by year three. Debt follows a different pattern and steadily rises over the course of the study, rising from $700 higher at midline to $1600 higher at endline. Combined, these patterns result in net worth treatment effects that slightly rises by $1000 by year 2 but then fall to roughly -$2800 by year three of the transfers. This pattern is consistent with households somewhat building up their assets at the start of the transfers but then spending down those assets by the end of the transfers.

Figure 6 shows how housing unit and neighborhood mobility evolved over the course of the study for the treatment group and the control group. Because migration relative to the baseline survey is not defined at baseline, we do not include estimates of the treatment control difference at baseline in these figures. The first column reports estimates of the effect of the treatment on moving neighborhoods within the past year, while the second column reports estimates of the effect of the treatment on housing unit mobility. For both outcomes a few patterns stand out. First, the effect of the treatment on absolute mobility is highest in the first two years of the study and then declines substantially in year three (although the effect remains positive for both outcomes). Second, the effects on moving housing units or neighborhoods relative to baseline rises over the cycle. This suggest that the transfers relax the

(if the baseline value was selected). We do this because because otherwise the controls would perfectly predict the outcome in the baseline period.

financial constraints preventing some households from changing housing units or neighborhoods at baseline and that these households gradually move to new units and neighborhoods over the course of the study.

Figure 7 reports the estimated treatment effects for the expenditure volatility and financial health families over time. These figures only show the estimated treatment effect and not the control mean over time. The black dot in these figures shows the estimated treatment effect and the red line shows the 95% confidence interval. Expenditure volatility declines throughout the course of the transfer, and the decline is actually higher in the final year. Financial health improves at the start of the study but the effect decays to zero by the end of the study. This pattern suggests that the improvement in financial outcomes is unlikely to be permanent. This is consistent with the results regarding net worth, which imply that households have not substantially improved their long-run financial position.

5.6 Heterogeneous Treatment Effect Estimates

Appendix Figures A2 through A4 report heterogeneous treatment effect estimates by gender, college degree holding, and whether household income at baseline was above or below the median for the family and aggregate sub-category level outcomes for expenditures, household balance sheets, and financial health and behavior measures.[28] Given the smaller sample sizes, these estimates are less precise than our main estimates and were pre-registered as exploratory. However, a few patterns stand out. First, for almost all outcomes the sign of the point estimates are the same across subgroups. The main exceptions are net worth and real assets, where the original estimates were near zero already. Second, there are signs of heterogeneity, particularly for households with below median income versus above median income. Households with incomes below the median at the time of enrollment appear to experience a larger increase in financial assets, a greater decrease in consumption volatility, and more improvements in financial health compared to the overall average and to households with incomes above the median. Conversely, households below the median report worse access to credit and larger increases in debt, though the estimates are not significant and the estimate on net worth is positive due to the increase in financial assets. These analyses are exploratory—the estimates are imprecise and in most cases we can't reject a null of no treatment effect heterogeneity. Thus, they should be interpreted with caution, but the point estimates are consistent with the effects being somewhat larger for lower-income households within the sample.

[28] Note that in Figure A4 the sign of expenditure volatility is reversed from in Table 6, so a higher value is less volatility.

6 Discussion

In this section, we combine these results to understand how households allocated the cash transfer across different consumption, asset, and debt categories. Table 14 reports our estimates of how treatment households allocated the \$34,200 (\$950 $\times$ 36) net transfer that they received over the course of the study to different categories.[29] The rows present the share of the transfer per month allocated to nine different categories during the three years of the transfer: durables consumption, non-durables consumption excluding housing, housing non-durables consumption, human capital consumption, leisure, financial savings, real assets, debt, and unexplained.[30,31] Estimates of the effects of the transfer on leisure come from Vivalt et al. (2024). The first column presents results based on our main point estimates, while columns (2) through (4) present results with alternative assumptions of how to allocate the unexplained portion of the transfer. Following the approach of some related papers such as Fagereng et al. (2021), we refer to the share of the transfer allocated to these different categories over the course of the program as marginal propensities to consume (MPC) or de-lever (MPD), respectively. Note that an alternative approach to defining MPCs is as the MPC out of the share of the transfer that is allocated to consumption or leisure rather than savings (see e.g., Golosov et al. (2023)). We report results for this alternative definition of MPC in Appendix Table A16. Given that we estimate that the transfer had little effect on net worth, the MPCs implied by our alternative approach are quite similar to our main estimates. [32][33]

Starting with column (1), we see that roughly 33 cents of each dollar of the transfer was spent on higher expenditures, of which 23 cents went to non-durables (either housing or non-housing), 4 cents

[29]We multiply estimates of the monthly effect on different spending categories by 36 and annual earnings estimates by 3 to make this calculation. Recall that since the control group received \$50/month, the difference in the transfer between the two groups was \$950/month.

[30]Note that in order to have all spending allocated to one of these categories, we use a slightly different categorization approach than is used in Appendix Table A5. The most notable difference is that there is an "Other category" in the aggregate groups in Appendix Table A5. This "Other" category in Appendix Table A5 includes expenditures on gifts to family or friends and charity, an other category that includes unclassified expenditures, and debt payments other than mortgages or car payments. For these calculations, we classify gifts to family and friends as non-durable expenditures, unclassified expenditures as non-durable expenditures, and non-mortgage and car debt payments as human capital expenditures because the majority of these expenditures are likely student debt payments. We think the other debt category is mostly student debt payments because the control group mean for other debt payments is \$108 per month and the control group mean for required student debt payments is \$68 per month.

[31]We exclude mortgage payments from non-durable housing expenditures because the mortgage payments (and other data on real estate assets) is noisy and because our small estimated effects on homeownership make it unlikely that mortgage payments were substantially affected by the transfer. Given the small estimated effect on mortgage payments of -\$7, this causes almost no change in the estimates.

[32]This approach to compute MPCs out of the share of the transfer allocated to consumption and leisure rather than savings is what is followed in Vivalt et al. (2024) for computing Marginal Propensities to Earn (MPEs).

[33]These calculations ignore changes in tax rates that the earnings response to the transfer may generate.

to durable goods, and 6 cents to human capital related expenditures. Households spent a further 22 cents of each dollar on higher leisure (i.e., reduced work hours and labor force participation). About 3 cents of each dollar of transfer was allocated to higher savings, but this is outweighed by the 1 cent of lower real assets and 5 cents of higher debt for each one dollar of transfer. Combined, these six categories explain 53 cents of each dollar of transfer, while 47 cents are unexplained. Consequently, the numbers in this column provide lower bounds on the marginal propensities to consume and de-lever but do not necessarily reflect the full values.

There is considerable evidence that consumption surveys such as the one we conduct understate total consumption (see e.g. Passero et al. (2015) and Bee et al. (2015) from the Christopher D. et al. (2015) conference volume on consumption measurement). In Column (2), we present results where we rescale consumption to reflect the evidence that household consumption surveys tend to under report total expenditures relative to sources that rely on data from retailers and administrative data on purchases. For example, in 2022 the Bureau of Labor Statistics (BLS) estimated that overall expenditures measured in the CEX were only 73% of total measured expenditures in the Personal Consumption Expenditures data.[34] Given the documented close match of the measured expenditures in our surveys to demographically similar consumer units in the CEX in Table 3, these estimates of underreporting in the CEX are a reasonable benchmark for our survey data as well. The reporting differences between the CEX and the PCE vary by type of expenditure, with measured expenditures on regular, salient payments like rent matching very closely, while more infrequent or less regular purchases are reported less comprehensively in the CEX. We use estimates of the underreporting of expenditures from the BEA to rescale our consumption estimates to reflect the general underreporting of expenditures in surveys. We use separate rescaling factors for durable goods, human capital expenses, non-durables (excluding housing), and housing non-durables.[35] This increases the portion of the transfer allocated to higher expenditures to 44 cents, leaving 37 cents still unexplained.

Columns (3) and (4) then make different assumptions of how to allocate the remaining 37 cents in unexplained transfer. In Column (3), we assume that the true change in net worth is equal to the upper bound of the our confidence intervals, which is roughly $5000. For simplicity, we assume the additional net worth relative to our main estimates comes from financial savings. The remaining un-

[34]See https://www.bls.gov/cex/cecomparison/data_comparisons_database.xlsx

[35]Specifically, we use the ratio of PCE/CEX spending on all non-durable goods and services (1.35) to rescale non-durables, the ratio of PCE/CEX spending for durable goods (1.81) to rescale durable goods, the ratio of PCE/CEX spending on non-durable services (1.1) to rescale human capital expenses, and we do not rescale non-durable housing because survey reports of rent payments tend to be quite accurate.

explained transfer we assume was equally distributed among expenditure categories in proportion to their respective treatment effects. The expenditures by category implied by this approach are quite similar to what you would arrive at by taking the upper bounds of the confidence intervals of our estimates and rescaling them by the ratio of CEX to PCE expenditures used above. In Column (4), we present results assuming that the unexplained portion of the transfer entirely reflects missing expenditures and apportion the spending in proportion to the spending categories' share of total spending.

The results in Column (3) suggest that the MPC to consume non-durables was 0.44, the marginal propensity to consume durables was 0.11, and the marginal propensity to consume human capital-related expenses was 0.10, while the MPD was -0.05. In Column (4), we estimate an MPC to consume non-durables of 0.55, durables of 0.14 and human capital expenses of 0.12. Classifying human capital expenses as a semi-durable good, we compute overall MPCs to consume durables of 0.21 to 0.26 and non-durables of 0.44 to 0.55, and an overall MPC of 0.65 to 0.81. Using both sets of assumptions, the vast majority of the transfer was used to increase expenditures or reduce leisure, rather than to increase net worth.

We can also compare our estimates of the effect of unearned income on food consumption and compare these to estimates of the effect of SNAP on food consumption to help understand the extent to which the effects of unconditional cash transfers differ from the effects of in-kind transfers. Our estimates imply a marginal propensity to consume food (MPCF) of 0.07 to 0.18, depending on the extent to which we adjust for unobserved spending.[36] This range is generally below the estimated range of the effect of SNAP on food spending, which ranges from around 0.16 (Hoynes and Schanzenbach (2009)) to 0.60 (Hastings and Shapiro (2018)). Given that the lower end of estimated MPCF out of SNAP typically uses survey data that may not be fully adjusted for underreporting and the higher end of the range uses retail scanner data, our estimates are consistent with the general finding in the literature that MPCF out of cash benefits is lower than the MPCF out of SNAP see e.g. (Fraker (1990), Hoynes and Schanzenbach (2009), or Hoynes and Schanzenbach (2016)).[37]

[36] We compute these bounds using our unadjusted estimate of the effect of the transfer on monthly food spending ($67) divided by the size of the monthly transfer ($950) and by scaling up the unmeasured food spending by the amount that we scale up overall non-durables spending in Column (4) of Table 14 (2.5).

[37] Approximately 41 percent of study participants were receiving SNAP at the time of enrollment. Consequently, for these study participants we are estimating MPCF conditional on also receiving SNAP.

7 Conclusion

The effects of a number of policies, including cash transfers and other social insurance programs and government stimulus and spending programs, depend on how households make consumption and saving decisions in response to unexpected and expected changes in unearned income. Do households save or consume unexpected increases in unearned income? How are such increases in unearned income consumed?

We find that low-income households receiving large monthly cash transfers—over 40 percent of the average household income of the sample at baseline—spend a large share of the transfer on a combination of non-durable goods and services, durable goods and services, and leisure time during the three years of the study. Although liquid savings do rise during the program, real assets are mostly unchanged (driven by null effects on homeownership and very small positive effects on car ownership and the value of vehicles owned). This suggests that low-income, young households have marginal propensities to consume non-durables of out of unearned income of 0.44 to 0.55, marginal propensities to consume durables and semi-durables of 0.21 to 0.26, and marginal propensities to delever out of unearned income of near zero. The lack of improvement in net worth described above, combined with the small effects on credit access and null effects on credit delinquencies, bankruptcies, and foreclosures, suggests that the transfer did not improve participants' long-run financial position. Consistent with this, self reported financial health rises at the start of the transfer but this effect decays to zero by year three of the transfer. These findings suggest that, at least for the young, low-income households in our sample, large, temporary transfers may not generate persistent improvement in financial outcomes.

Treated recipients concentrate their increased spending on the largest spending categories at baseline: food, rent, and transportation. The category that rises most relative to the baseline level of spending is net gifts to family and friends. Spending increases on categories such as entertainment, alcohol and tobacco, and other less essential spending items, but less so than the core expenses described above. This concentration of spending on core expense categories suggests that policymaker concerns that large portions of cash transfers will be allocated to vice goods or other non-core expenses may be unfounded. At the same time, the unconditional transfer we study was not allocated in the same proportions as particular in-kind transfers. For example, we estimate effects on food consumption of roughly two to three times less than corresponding estimates of the propensity to consume food out of SNAP.

Households appear to make some new consumption commitments, such as moving to new housing units and neighborhoods and potentially taking on new debt obligations. Given the estimated near zero effect on household net worth, an important open question is the extent to which households are able to maintain these consumption commitments or if they cause financial distress or need to be unwound over time. More broadly, future research should investigate the drivers of the high marginal propensities to consume and near zero marginal propensities to de-lever that we find among young, low-income Americans.

References

Aguiar, M. and E. Hurst (2005). Consumption versus expenditure. *Journal of Political Economy 113*(5), 919–948.

Anderson, M. L. (2008). Multiple inference and gender differences in the effects of early intervention: A reevaluation of the abecedarian, perry preschool, and early training projects. *Journal of the American statistical Association 103*(484), 1481–1495.

Banerjee, A., M. Faye, A. Krueger, P. Niehaus, and T. Suri (2023). Universal basic income: Short-term results from a long-term experiment in kenya. Technical report, UC San Diego.

Bee, A., B. D. Meyer, and J. X. Sullivan (2015). The validity of consumption data: Are the consumer expenditure interview and diary surveys informative?

Benjamini, Y. and Y. Hochberg (1995). Controlling the false discovery rate: a practical and powerful approach to multiple testing. *Journal of the Royal statistical society: series B (Methodological) 57*(1), 289–300.

Bloniarz, A., H. Liu, C.-H. Zhang, J. S. Sekhon, and B. Yu (2016). Lasso adjustments of treatment effect estimates in randomized experiments. *Proceedings of the National Academy of Sciences 113*(27), 7383–7390.

Boehm, J., E. Fize, and X. Jaravel (2023). Five facts about mpcs: Evidence from a randomized experiment. Working paper.

Briggs, J., D. Cesarini, E. Lindqvist, and R. Östling (2021). Windfall gains and stock market participation. *Journal of Financial Economics 139*(1), 57–83.

Bronchetti, E. T., G. Christensen, and H. W. Hoynes (2019). Local food prices, snap purchasing power, and child health. *Journal of Health Economics 68*, 102231.

Broockman, D. E., J. L. Kalla, and J. S. Sekhon (2017). The design of field experiments with survey outcomes: A framework for selecting more efficient, robust, and ethical designs. *Political Analysis 25*(4), 435–464.

Bulman, G., R. Fairlie, S. Goodman, and A. Isen (2021, April). Parental resources and college attendance: Evidence from lottery wins. *American Economic Review 111*(4), 1201–40.

Cesarini, D., E. Lindqvist, M. J. Notowidigdo, and R. Ostling (2017, 12). The Effect of Wealth on Individual and Household Labor Supply: Evidence from Swedish Lotteries. *American Economic Review 107*(12), 3917 – 3946.

Chetty, R. and A. Szeidl (2016). Consumption commitments and habit formation. *Econometrica 84*(2), 855–890.

Christopher D., C., C. Thomas F., and S. John (2015). *Improving the Measurement of Consumer Expenditures.* Number volume 74 in NBER Studies in Income and Wealth. University of Chicago Press.

Clark, A. E., P. Frijters, and M. A. Shields (2008, February). Relative Income, Happiness, and Utility: An Explanation for the Easterlin Paradox and Other Puzzles. *Journal of Economic Literature 46*(1), 95–144.

Colarieti, R., P. Mei, and S. Stantcheva (2024, March). The how and why of household reactions to income shocks. Working Paper 32191, National Bureau of Economic Research.

Deaton, A. (2008). Income, health, and well-being around the world: Evidence from the gallup world poll. *Journal of Economic Perspectives 22*(2), 53–72.

Evans, W., B. Wolfe, and N. Adler (2012). The ses and health gradient: A brief review of the literature. in The Biological Consequences of Socioeconomic Inequalities, eds. B. Wolfe, W. Evans, and T. E. Seeman (New York, Russell Sage Foundation: 2012).

Fagereng, A., M. B. Holm, and G. J. Natvik (2021). Mpc heterogeneity and household balance sheets. *American Economic Journal: Macroeconomics 13*(4), 1–54.

Forget, E. L. (2011). The town with no poverty: the health effects of a Canadian guaranteed annual income field experiment. *Canadian Public Policy 37*(3), 283–305.

Fraker, T. M. (1990). The effects of food stamps on food consumption: A review of the literature. Technical report, Mathematica Policy Research, Inc.

Fuster, A., G. Kaplan, and B. Zafar (2021). What would you do with $500? spending responses to gains, losses, news, and loans. *The Review of Economic Studies 88*(4), 1760–1795.

Gennetian, L., G. J. Duncan, N. A. Fox, K. Magnuson, S. Halpern-Meekin, , K. G. Noble, and

H. Yoshikawa (2022). Unconditional cash and family investments: Evidence from a large scale cash transfer study in the us. Working Paper.

Gennetian, L., M. Maury, L. Stilwell, H. Shah, K. Magnuson, K. G. Noble, G. J. Duncan, N. A. Fox, S. Halpern-Meekin, and H. Yoshikawa (2024). The impact of monthly unconditional cash on food security, spending, and consumption: Three year follow-up findings from the baby's first years study. Working Paper.

Golosov, M., M. Graber, M. Mogstad, and D. Novgorodsky (2023, 10). How Americans Respond to Idiosyncratic and Exogenous Changes in Household Wealth and Unearned Income*. *The Quarterly Journal of Economics 139*(2), 1321–1395.

Guess, A. M., N. Malhotra, J. Pan, P. Barberá, H. Allcott, T. Brown, A. Crespo-Tenorio, D. Dimmery, D. Freelon, M. Gentzkow, S. González-Bailón, E. Kennedy, Y. M. Kim, D. Lazer, D. Moehler, B. Nyhan, C. V. Rivera, J. Settle, D. R. Thomas, E. Thorson, R. Tromble, A. Wilkins, M. Wojcieszak, B. Xiong, C. K. de Jonge, A. Franco, W. Mason, N. J. Stroud, and J. A. Tucker (2023). Reshares on social media amplify political news but do not detectably affect beliefs or opinions. *Science 381*(6656), 404–408.

Han, J., B. D. Meyer, and J. X. Sullivan (2021). The consumption, income, and well-being of single mother–headed families 25 years after welfare reform. *National Tax Journal 74*(3), 791–824.

Hastings, J., R. Kessler, and J. M. Shapiro (2021, August). The effect of snap on the composition of purchased foods: Evidence and implications. *American Economic Journal: Economic Policy 13*(3), 277–315.

Hastings, J. and J. M. Shapiro (2018, December). How are snap benefits spent? evidence from a retail panel. *American Economic Review 108*(12), 3493–3540.

Haushofer, J. and J. Shapiro (2013). Household response to income changes: Evidence from an unconditional cash transfer program in kenya. *Massachusetts Institute of Technology*.

Hoynes, H. W. and D. W. Schanzenbach (2009). Consumption responses to in-kind transfers: Evidence from the introduction of the food stamp program. *American Economic Journal: Applied Economics 1*(4), 109–139.

Hoynes, H. W. and D. W. Schanzenbach (2016). U.s. food and nutrition programs. In *Economics of Means-Tested Transfer Programs in the United States*, Volume 1, pp. 219–302. Chicago: University of Chicago Press.

Hsieh, C.-T. (2003). Do consumers react to anticipated income changes? evidence from the alaska permanent fund. *American Economic Review 93*(1), 397–405.

Imbens, G. W., D. B. Rubin, and B. I. Sacerdote (2001). Estimating the effect of unearned income on labor earnings, savings, and consumption: Evidence from a survey of lottery players. *American Economic Review 91*(4), 778–794.

Jaroszewicz, A., J. Jachimowicz, O. Hauser, and J. Jamison (2023). Cash can make its absence felt: Randomizing unconditional cash transfer amounts in the us. Working paper.

Johnson, D. S., J. A. Parker, and N. S. Souleles (2006). Household expenditure and the income tax rebates of 2001. *American Economic Review 96*(5), 1589–1610.

Jones, D. and I. Marinescu (2018). The Labor Market Impacts of Universal and Permanent Cash Transfers: Evidence from the Alaska Permanent Fund. NBER Working Paper 24312.

Kueng, L. (2018). Excess sensitivity of high-income consumers. *The Quarterly Journal of Economics 133*(4), 1693–1751.

Liebman, J., K. Carlson, E. Novick, and P. Portocarrero (2022). The chelsea eats program: Experimental impacts. Working paper.

Lindqvist, E., R. Östling, and D. Cesarini (2018). Long-Run Effects of Lottery Wealth on Psychological Well-being. Technical report, National Bureau of Economic Research.

Lindqvist, E., R. Ostling, and D. Cesarini (2020, 02). Long-Run Effects of Lottery Wealth on Psychological Well-Being. *The Review of Economic Studies 87*(6), 2703–2726.

Miller, S., E. Rhodes, A. Bartik, D. Broockman, P. Krause, and E. Vivalt (2024). Does income affect health? evidence from a randomized controlled trial of a guaranteed income. Working Paper.

Misra, K. and P. Surico (2014). Consumption, income changes, and heterogeneity: Evidence from two fiscal stimulus programs. *American Economic Journal: Macroeconomics 6*(4), 84–106.

Noble, K., K. Magnuson, L. Gennetian, G. J. Duncan, H. Yoshikawa, N. A. Fox, and S. Halpern-Meekin (2021). Baby's first years: Design of a randomized controlled trial of poverty reduction in the united states. *Pediatrics 148*(4).

Parker, J. A., N. S. Souleles, D. S. Johnson, and R. McClelland (2013). Consumer spending and the economic stimulus payments of 2008. *American Economic Review 103*(6), 2530–2553.

Passero, W., T. I. Garner, and C. McCully (2015). Understanding the relationship: Ce survey and pce.

Picchio, M., S. Suetens, and J. C. van Ours (2017, 07). Labour Supply Effects of Winning a Lottery. *The Economic Journal 128*(611), 1700–1729.

Salehi-Isfahani, D. and M. H. Mostafavi-Dehzooei (2017). Cash transfers and labor supply: Evidence from a large-scale program in iran. Working Paper.

Stevenson, B. and J. Wolfers (2013, May). Subjective Well-Being and Income: Is There Any Evidence of Satiation? *American Economic Review 103*(3), 598–604.

Vivalt, E., E. Rhodes, A. Bartik, D. Broockman, and S. Miller (2024). The employment effects of a guaranteed income: Experimental evidence from two u.s. states. Working Paper.

Table 1: Study Sample Characteristics Compared to Eligible Population

	Eligible Population Comparison (ACS)			Study Sample		
	Full US Population		Study Counties	Eligible Screener Respondents		Enrolled Active Survey Group
	Unweighted	Reweighted to Match Enrolled Sample FPL and County Type Distribution	Reweighted to Match Enrolled Sample FPL County Type Distribution	Unweighted	Reweighted to Match Enrolled Sample FPL County Type Distribution	Unweighted
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Key active group stratification variables						
Income <100% of FPL	0.25	0.34	0.34	0.30	0.34	0.33
Income 100-200% of FPL	0.36	0.41	0.41	0.33	0.41	0.40
Income 200% + of FPL	0.38	0.24	0.24	0.37	0.24	0.24
Rural County	0.26	0.13	0.13	0.13	0.13	0.13
Suburban County	0.32	0.18	0.18	0.22	0.18	0.18
Medium-Sized Urban County	0.16	0.16	0.16	0.15	0.16	0.16
Large Urban County	0.24	0.53	0.53	0.51	0.53	0.53
Panel B. Demographic Characteristics						
Any Children	0.59	0.59	0.62	0.57	0.59	0.57
HH Size	3.36	3.25	3.34	3.14	3.20	2.98
Age <30	0.52	0.54	0.53	0.54	0.54	0.54
White (non-hispanic)	0.59	0.46	0.41	0.48	0.46	0.47
Black (non-hispanic)	0.17	0.25	0.29	0.25	0.26	0.30
Hispanic	0.17	0.22	0.25	0.22	0.22	0.22
Female	0.57	0.59	0.61	0.68	0.69	0.67
HH Income	36,199	30,521	31,204	32,327	29,245	29,942
College Degree or more	0.17	0.16	0.16	0.28	0.25	0.20
Renter	0.56	0.68	0.66	0.82	0.84	0.79
N	919395	904792	35086	14573	14573	3000

This table compares the study sample to estimates of the characteristics of the study in the US as a whole. Eligible individuals are those ages 21-40 with household incomes of less than 300% of the federal poverty line. Columns (1) - (3) report estimates of the characteristics of eligible households using the American Community Survey (ACS) 2013-2017 pooled sample. Column (1) presents the unweighted means for eligible individuals, Column (2) reweights the ACS sample to match both the income group distribution and the county-type distribution in the enrolled active survey group sample, and Column (3) presents estimates of characteristics of eligible individuals in study counties, reweighted to match the enrolled sample FPL group and county type distribution. Columns (4)-(6) report characteristics of the study sample. Columns (4) and (5) report characteristics of eligible respondents to the mailer and online advertisement recruitment methods. Column (4) is unweighted, while Column (5) is reweighted to match the enrolled sample FPL and county type distribution. Column (6) reports the unweighted mean of the ultimate enrolled active survey group (i.e. the 3000 individuals assigned to the active group who completed the baseline survey and participated in the program). In some cases variables may not add to one due to missing values.

Table 2: Baseline characteristics by treatment arm

	Treatment	Control	p-value
Demographic			
Age	30.169	30.035	0.542
Male	0.328	0.319	0.627
Female	0.669	0.678	0.628
Non-binary/other	0.003	0.003	0.999
Non-Hispanic Black	0.295	0.305	0.554
Non-Hispanic Asian	0.036	0.038	0.790
Non-Hispanic White	0.473	0.463	0.597
Non-Hispanic Native American	0.020	0.025	0.428
Hispanic	0.220	0.214	0.694
Household Size	2.943	2.996	0.435
Any children	0.568	0.571	0.897
Number of children	1.435	1.398	0.558
Economic			
Employed	0.578	0.586	0.675
Personal income ($1000s)	21.355	21.217	0.861
Household income ($1000s)	29.991	29.917	0.922
Under FPL	0.323	0.336	0.475
Total government benefits ($1000s)	5.378	5.290	0.735
Number of government benefit programs	1.982	2.018	0.571
HS Degree/GED or higher	0.953	0.939	0.100
Expenditures			
Total monthly expenditures ($1000s)	3.015	2.968	0.425
Housing monthly expenditures ($1000s)	0.687	0.660	0.207
Non-durable goods/services monthly expenditures ($1000s)	1.486	1.473	0.656
Durable goods monthly expenditures ($1000s)	0.304	0.321	0.150
Human capital expenditures ($1000s)	0.411	0.391	0.423
Monthly net gifts or loans to family and charity ($1000s)	0.069	0.064	0.346
Monthly Debt Payments			
Monthly minimum auto loan payments	87.048	88.557	0.817
Monthly minimum credit card and bank loan payments	81.035	71.303	0.297
Monthly minimum student loan payments	44.520	46.468	0.634
Monthly minimum mortgage payments	81.768	73.891	0.466
Monthly minimum total debt payments	285.461	275.491	0.555
Monthly minimum total debt payments, excl. mortgage	217.958	218.554	0.960
Any cars in household	0.752	0.754	0.881
Household Balance Sheet			
Total cars in household	1.191	1.218	0.501
Total debt ($1000s)	29.448	28.840	0.745
Total debt excluding mortgage ($1000s)	21.310	20.469	0.516
Total HH real assets ($1000s)	25.554	28.791	0.214
Total HH financial savings ($1000s)	7.250	8.143	0.366
Net worth ($1000s)	3.943	8.798	0.078
Net worth excluding real estate assets & debt ($1000s)	-6.500	-3.759	0.117
Financial Circumstances			
HH savings as a share of income	0.051	0.057	0.498
Could pay unexpected $400 expense	0.388	0.386	0.910
Has at least $100 in savings	0.610	0.611	0.973
Financial well-being index (1-40)	17.040	16.904	0.657
Homeowner	0.127	0.137	0.467
Stayed in shelter, car, or other non-permanent housing	0.086	0.084	0.811
Any eviction/foreclosure in previous 12 months	0.008	0.006	0.471

Notes: This table reports the baseline characteristics of the study sample separately by those assigned to the treatment group who receive transfers of $1000 a month and those assigned to the control group who receive transfers of $50 a month.

Table 3: Comparison of Monthly ORUS Survey Expenditures to CEX Expenditures

	ORUS Survey	CEX Consumer Units Ages 20-40 in 2020		
		CEX Weights	Income + Education + Race Weights	CEX + Income + Education + Race Weights
	(1)	(2)	(3)	(4)
2021	3,660	5,294	3,814	3,493
2022	4,069	5,661	4,151	3,916
Average	3,865	5,478	3,983	3,705

Notes: This table compares the average measured total monthly household expenditures in the ORUS expenditure survey within the control group to average monthly consumer unit spending in the CEX using different sets of weights to match the ORUS sample characteristics. Different rows report the spending measures for different years.

Table 4: Impact of Unconditional Cash Transfer on Monthly Expenditures

	Category	Control Mean (1)	Effect (2)	N (3)
Total Expenditures		3979 (1610)	**$310^{***\dagger\dagger\dagger}$ (64) [0.001]**	**2988**
Food and beverage consumption	Non-durables	967 (547)	$67^{***\dagger\dagger\dagger}$ (20) [0.005]	2980
Rent expenditure	Housing	602 (569)	$52^{**\dagger\dagger}$ (23) [0.031]	2974
Car payment & insurance expenditures	Durables	366 (292)	$30^{**\dagger\dagger}$ (12) [0.025]	2987
Utilities, phone, cable, internet	Non-durables	327 (192)	9 (7) [0.119]	2979
Health expenditures	Human Capital	233 (256)	$20^{*\dagger\dagger}$ (10) [0.045]	2987
Mortgage, home-insurance, and property tax expenditure	Housing	204 (471)	-7 (18) [0.264]	2970
Childcare and expenditures on children	Human Capital	189 (217)	$22^{**\dagger\dagger}$ (9) [0.030]	2988
Non-durable transportation expenditures	Non-durables	189 (134)	$20^{***\dagger\dagger\dagger}$ (5) [0.002]	2980
Clothing, apparel, and personal care expenditures	Non-durables	176 (137)	$15^{***\dagger\dagger\dagger}$ (5) [0.008]	2980
Alcohol, tobacco, marijuana, and gambling	Non-durables	138 (175)	$13^{*\dagger}$ (7) [0.071]	2980
Household expenditures	Durables	119 (128)	$11^{**\dagger\dagger}$ (5) [0.031]	2987
Debt payments (other than car/mortgage)	Human Capital	108 (200)	10 (8) [0.119]	2933
Gifts or loans to family and charity	Non-durables	84 (143)	$22^{***\dagger\dagger\dagger}$ (6) [0.002]	2979
Vacations and trips	Non-durables	80 (105)	$7^{*\dagger}$ (4) [0.062]	2987
Other expenses	Non-durables	71 (129)	$11^{**\dagger\dagger}$ (5) [0.031]	2987
Education expenditure	Human Capital	66 (169)	5 (7) [0.186]	2979
Pet expenditures	Non-durables	52 (75)	4 (3) [0.106]	2987
Recreation and entertainment	Non-durables	51 (43)	$5^{***\dagger\dagger\dagger}$ (2) [0.010]	2977

Notes: This table reports estimates of Equation 1 of the difference in outcomes between the treatment group and the control group for total expenditures and detailed expenditure categories. We estimate these treatment effects for outcomes pooled over the three years of the study using the weights described in Section 3.1 above. Column (1) reports the control group mean and standard deviation in parentheses for this pooled outcome. Column (2) then reports the treatment effect estimates. Standard errors are shown in parentheses below and the FDR-adjusted q-values are shown in brackets below the standard error. Statistical significance relative to a null hypothesis of no-effect using traditional inference is shown using asterisks, while statistical significant after FDR adjustment is indicated using daggers. Family outcomes are shown in bold, components within families are underlined, and items within components are shown indented.

Table 5: Impact of Unconditional Cash Transfer on Housing Unit and Neighborhood Mobility

	Control Mean (1)	Effect (2)	N (3)
Housing unit mobility index		**0.105***†††** **(0.024)** **[0.001]**	**3000**
<u>Moved housing unit relative to baseline</u>	0.433 (0.455)	0.041***††† (0.016) [0.010]	2993
<u>Unit Search Index</u>		0.120***††† (0.032) [0.001]	2848
Any active housing-search behaviors	0.252 (0.371)	0.039***††† (0.014) [0.004]	2848
Number of active search actions	1.286 (2.400)	0.301***††† (0.092) [0.002]	2848
Interest in moving housing units	0.365 (0.415)	0.054***††† (0.015) [0.001]	2848
Looking to move housing units	0.254 (0.372)	0.039***†† (0.014) [0.034]	2848
Neighborhood Mobility Index		**0.117***†††** **(0.024)** **[0.001]**	**3000**
<u>Moved neighborhood since baseline</u>	0.390 (0.448)	0.044***††† (0.016) [0.005]	2993
<u>Neighborhood Search Index</u>		0.136***††† (0.033) [0.001]	2848
Any active neighborhood-search behaviors	0.221 (0.356)	0.049***††† (0.013) [0.001]	2848
Number of active neighborhood-search actions	0.410 (0.816)	0.104***††† (0.031) [0.001]	2848
Interest in moving neighborhoods	0.320 (0.402)	0.057***††† (0.014) [0.001]	2848
Looking for a new neighborhood	0.223 (0.357)	0.051***††† (0.014) [0.003]	2848

Notes: This table reports estimates of Equation 1 of the difference in outcomes between the treatment group and the control group on outcomes listed in the rows. We estimate these treatment effects for outcomes pooled over the three years of the study using the weights described in Section 3.1 above. Column (1) reports the control group mean and standard deviation in parentheses for this pooled outcome. Column (2) then reports the treatment effect estimates. Standard errors are shown in parentheses below and the FDR-adjusted q-values are shown in brackets below the standard error. Statistical significance relative to a null hypothesis of no-effect using traditional inference is shown using asterisks, while statistical significant after FDR adjustment is indicated using daggers. Family outcomes are shown in bold, components within families are underlined, and items within components are shown indented.

Table 6: Impact of Unconditional Cash Transfer on Monthly Expenditure Volatility

	Control Mean (1)	Effect (2)	N (3)
Standard deviation of log monthly expenditures	0.71 (0.75)	**-0.14*****††† ** **(0.02)** **[0.001]**	**2834**

Notes: This table reports estimates of Equation 1 of the difference in outcomes between the treatment group and the control group on outcomes listed in the rows. We estimate these treatment effects for outcomes pooled over the three years of the study using the weights described in Section 3.1 above. Column (1) reports the control group mean and standard deviation in parentheses for this pooled outcome. Column (2) then reports the treatment effect estimates. Standard errors are shown in parentheses below and the FDR-adjusted q-values are shown in brackets below the standard error. Statistical significance relative to a null hypothesis of no-effect using traditional inference is shown using asterisks, while statistical significant after FDR adjustment is indicated using daggers. Family outcomes are shown in bold, components within families are underlined, and items within components are shown indented. Secondary items are shown in italics.

Table 7: Impact of Unconditional Cash Transfer on Financial Health

	Control Mean (1)	Effect (2)	N (3)
Financial Health (Family Index)		**0.05***†††** **(0.02)** **[0.004]**	**2989**
Financial Hardship (Component Index)		-0.02 (0.03) [0.133]	2951
Number of recent financial shocks experienced by the participant	0.59 (0.93)	0.07**† (0.03) [0.059]	2868
Sometimes or often runs out of money between paychecks or before end of month	0.56 (0.41)	-0.01 (0.02) [0.326]	2894
Savings as Share of Income (Component Index)		0.11***††† (0.03) [0.002]	2901
Household savings as a share of income (excluding transfer)	0.08 (0.24)	0.03***††† (0.01) [0.003]	2863
Individual savings as a share of income (excluding transfer)	0.09 (0.25)	0.02***†† (0.01) [0.025]	2885
Additive index of self-reported financial health Likert scale outcomes	18.19 (7.43)	0.67***††† (0.19) [0.002]	2898
Financial Resilience (Component Index)		0.03*†† (0.02) [0.042]	2940

Notes: This table reports estimates of Equation 1 of the difference in outcomes between the treatment group and the control group on outcomes listed in the rows. We estimate these treatment effects for outcomes pooled over the three years of the study using the weights described in Section 3.1 above. Column (1) reports the control group mean and standard deviation in parentheses for this pooled outcome. Column (2) then reports the treatment effect estimates. Standard errors are shown in parentheses below and the FDR-adjusted q values are shown in brackets below the standard error. Statistical significance relative to a null hypothesis of no effect using traditional inference is shown using asterisks, while statistical significant after FDR adjustment is indicated using daggers. Family outcomes are shown in bold, components within families are underlined, and items within components are shown indented.

Table 8: Impact of Unconditional Cash Transfer on Assets ($1000s)

	Control Mean (1)	Effect (2)	N (3)
Total Real Assets (individual and jointly held)	62.3 (123)	-0.2 (3.2) [1.000]	2941
Vehicles owned (individual & joint)	7.6 (9.3)	0.2 (0.3) [1.000]	2939
Real estate owned owned (individual & joint)	49.3 (116.6)	-0.3 (3.0) [1.000]	2889
Business assets (individual & joint)	2.2 (25.9)	0.2 (0.9) [1.000]	2936
Other assets owned (individual & joint)	1.6 (12.0)	-0.3 (0.4) [1.000]	2935
Total Financial Assets (individual and jointly held)	17.9 (47.2)	1.0 (1.1) [1.000]	2939
Total savings in bank accounts (individual & jointly held)	6.9 (15.5)	0.8* (0.5) [1.000]	2849
Total savings in retirement/pension accounts (individual & jointly held)	8.6 (29.2)	-0.4 (0.8) [1.000]	2936
Total savings in cryptocurrency (individual)	0.3 (3.7)	0.1 (0.1) [1.000]	2935
Total savings in other investments and accounts (individual & jointly held)	2.0 (15.4)	0.5 (0.4) [1.000]	2937

Notes: This table reports estimates of Equation 1 of the difference in outcomes between the treatment group and the control group on outcomes listed in the rows. We estimate these treatment effects for outcomes pooled over the three years of the study using the weights described in Section 3.1 above. Column (1) reports the control group mean and standard deviation in parentheses for this pooled outcome. Column (2) then reports the treatment effect estimates. Standard errors are shown in parentheses below and the FDR-adjusted q values are shown in brackets below the standard error. Statistical significance relative to a null hypothesis of no effect using traditional inference is shown using asterisks, while statistical significant after FDR adjustment is indicated using daggers. Family outcomes are shown in bold, components within families are underlined, and items within components are shown indented.

Table 9: Impact of Unconditional Cash Transfer on Homeownership

	Control Mean (1)	Effect (2)	N (3)
Does the respondent own or share ownership in their residence	0.209 (0.388)	**-0.009** **(0.027)** **[0.294]**	**2939**
Does the respondent own their residence (more expansive definition)	0.283 (0.436)	-0.001 (0.012) [1.000]	2940
Became homeowner (previously renter)	0.032 (0.131)	-0.004 (0.005) [1.000]	2940
Became homeowner (previously renter) more expansive definition	0.036 (0.137)	-0.003 (0.005) [1.000]	2940
Number of people ownership is shared with	0.091 (0.318)	0.000 (0.010) [1.000]	2932
Homeownership shared	0.154 (0.347)	-0.007 (0.010) [1.000]	2937

Notes: This table reports estimates of Equation 1 of the difference in outcomes between the treatment group and the control group on outcomes listed in the rows. We estimate these treatment effects for outcomes pooled over the three years of the study using the weights described in Section 3.1 above. Column (1) reports the control group mean and standard deviation in parentheses for this pooled outcome. Column (2) then reports the treatment effect estimates. Standard errors are shown in parentheses below and the FDR-adjusted q values are shown in brackets below the standard error. Statistical significance relative to a null hypothesis of no effect using traditional inference is shown using asterisks, while statistical significant after FDR adjustment is indicated using daggers. Family outcomes are shown in bold, components within families are underlined, and items within components are shown indented. Secondary items are shown in italics.

Table 10: Impact of Unconditional Cash Transfer on Debt ($1000s)

	Control Mean (1)	Effect (2)	N (3)
<u>Total Debt (Survey + Experian)</u>	41.1 (66.7)	1.8 (1.9) [1.000]	2981
Credit Card Balance (Survey + Experian)	2.4 (4.3)	0.1 (0.1) [1.000]	2976
Auto Loans (Survey + Experian)	4.7 (8.8)	0.8** (0.3) [0.394]	2915
Mortgages (Survey + Experian)	15.2 (48.7)	0.9 (1.6) [1.000]	2975
Student Loans (Survey + Experian)	17.0 (35.7)	-0.2 (0.9) [1.000]	2895
Unpaid Medical Bills (Survey + Experian)	0.6 (2.4)	-0.0 (0.1) [1.000]	2979
Credit Union and Bank Loans (Survey + Experian)	1.4 (5.1)	0.2 (0.2) [1.000]	2895
Informal loans	0.6 (3.8)	0.1 (0.1) [1.000]	2937

Notes: This table reports estimates of Equation 1 of the difference in outcomes between the treatment group and the control group on outcomes listed in the rows. We estimate these treatment effects for outcomes pooled over the three years of the study using the weights described in Section 3.1 above. Column (1) reports the control group mean and standard deviation in parentheses for this pooled outcome. Column (2) then reports the treatment effect estimates. Standard errors are shown in parentheses below and the FDR-adjusted q values are shown in brackets below the standard error. Statistical significance relative to a null hypothesis of no effect using traditional inference is shown using asterisks, while statistical significant after FDR adjustment is indicated using daggers. Family outcomes are shown in bold, components within families are underlined, and items within components are shown indented.

Table 11: Impact of Unconditional Cash Transfer on Minimum Monthly Debt Payments

	Control Mean (1)	Effect (2)	N (3)
<u>Monthly Payments on All Loans (Survey + Experian)</u>	454 (597)	50** (24) [0.109]	2979
Monthly Payments on Vehicle Loans and Leases (Survey + Experian)	127 (213)	17** (9) [0.120]	2917
Monthly Payments on Bank and Credit Card Debt (Survey + Experian)	124 (334)	21 (14) [0.229]	2895
Monthly Payments on Student Loans (Survey + Experian)	68 (138)	4 (6) [0.490]	2893
Monthly Payments on Mortgages (Survey + Experian)	130 (386)	7 (15) [0.547]	2983
Monthly Payments on All Loans, Excl. Mortgages (Survey + Experian)	345 (414)	37**† (17) [0.090]	2979

Notes: This table reports estimates of Equation 1 of the difference in outcomes between the treatment group and the control group on outcomes listed in the rows. We estimate these treatment effects for outcomes pooled over the three years of the study using the weights described in Section 3.1 above. Column (1) reports the control group mean and standard deviation in parentheses for this pooled outcome. Column (2) then reports the treatment effect estimates. Standard errors are shown in parentheses below and the FDR-adjusted q values are shown in brackets below the standard error. Statistical significance relative to a null hypothesis of no effect using traditional inference is shown using asterisks, while statistical significant after FDR adjustment is indicated using daggers. Family outcomes are shown in bold, components within families are underlined, and items within components are shown indented.

Table 12: Impact of Unconditional Cash Transfer on Net Worth ($1000s)

	Control Mean (1)	Effect (2)	N (3)
Net worth		**-1.0** **(3.4)** **[0.344]**	**2981**
Net worth excluding real estate	5.8 (70.6)	-2.1 (2.6) [1.000]	3000

Notes: This table reports estimates of Equation 1 of the difference in outcomes between the treatment group and the control group on outcomes listed in the rows. We estimate these treatment effects for outcomes pooled over the three years of the study using the weights described in Section 3.1 above. Column (1) reports the control group mean and standard deviation in parentheses for this pooled outcome. Column (2) then reports the treatment effect estimates. Standard errors are shown in parentheses below and the FDR-adjusted q values are shown in brackets below the standard error. Statistical significance relative to a null hypothesis of no effect using traditional inference is shown using asterisks, while statistical significant after FDR adjustment is indicated using daggers. Family outcomes are shown in bold, components within families are underlined, and items within components are shown indented. Secondary items are shown in italics.

Table 13: Impact of Unconditional Cash Transfer on Credit Access

	Control Mean (1)	Effect (2)	N (3)
Credit Access (index)		**0.02** **(0.03)** **[0.288]**	**3000**
Available Credit Limit Past 3 Months (Experian)	7659.63 (14230.52)	100.69 (466.00) [1.000]	2512
Credit Limit Past 3 Months (Experian)	8342.28 (15545.12)	607.19 (485.00) [1.000]	3000
Credit Score (Experian)	630.21 (90.82)	6.28**† (2.52) [0.099]	2494
Has Credit Score (Experian)	0.81 (0.38)	0.01 (0.01) [1.000]	3000
Revolving Inquiries per Trade Past 3 Months (Experian)	0.40 (0.42)	-0.00 (0.01) [1.000]	2512
Total Inquiries per Trade Past 3 Months (Experian)	0.25 (0.32)	-0.01 (0.01) [1.000]	2512

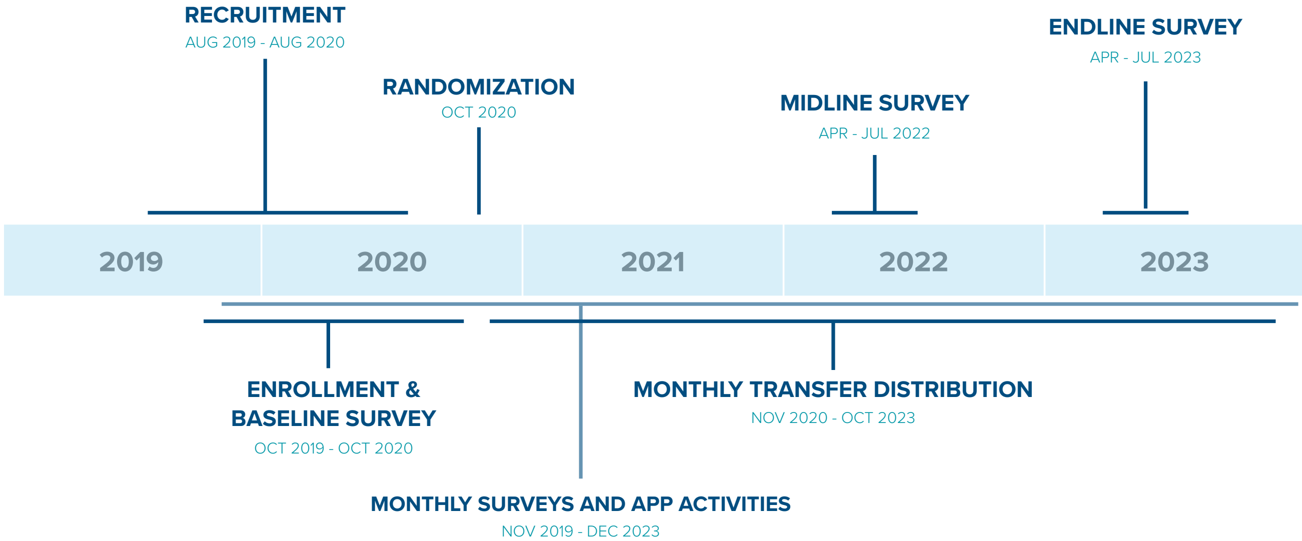
Notes: This table reports estimates of Equation 1 of the difference in outcomes between the treatment group and the control group on outcomes listed in the rows. We estimate these treatment effects for outcomes pooled over the three years of the study using the weights described in Section 3.1 above. Column (1) reports the control group mean and standard deviation in parentheses for this pooled outcome. Column (2) then reports the treatment effect estimates. Standard errors are shown in parentheses below and the FDR-adjusted q values are shown in brackets below the standard error. Statistical significance relative to a null hypothesis of no effect using traditional inference is shown using asterisks, while statistical significant after FDR adjustment is indicated using daggers. Family outcomes are shown in bold, components within families are underlined, and items within components are shown indented. Secondary items are shown in italics.

Table 14: Marginal Propensities to Consume and De-lever

	Main Results	Rescale Expenditures by Ratio of CEX/PCE	(2) + Assume Change in Net Worth $5,000 + Allocate Remainder to Consumption	Allocate All Under-Reporting in (2) to Consumption
Change in net worth	-$1,000	-$1,000	$5,000	-$1,000 Change in Net Worth
	(1)	(2)	(3)	(4)
Consumption				
Durables Goods	0.04	0.08	0.11	0.14
Human Capital	0.06	0.07	0.10	0.12
Non-durables, (excluding housing)	0.18	0.25	0.36	0.45
Housing non-durables	0.05	0.05	0.08	0.10
Leisure	0.22	0.22	0.22	0.22
Houshold Balance Sheet				
Financial Savings	0.03	0.03	0.20	0.03
Physical Assets	-0.01	-0.01	-0.01	-0.01
Debt	-0.05	-0.05	-0.05	-0.05
Unexplained				
Residual	0.47	0.37	-	-

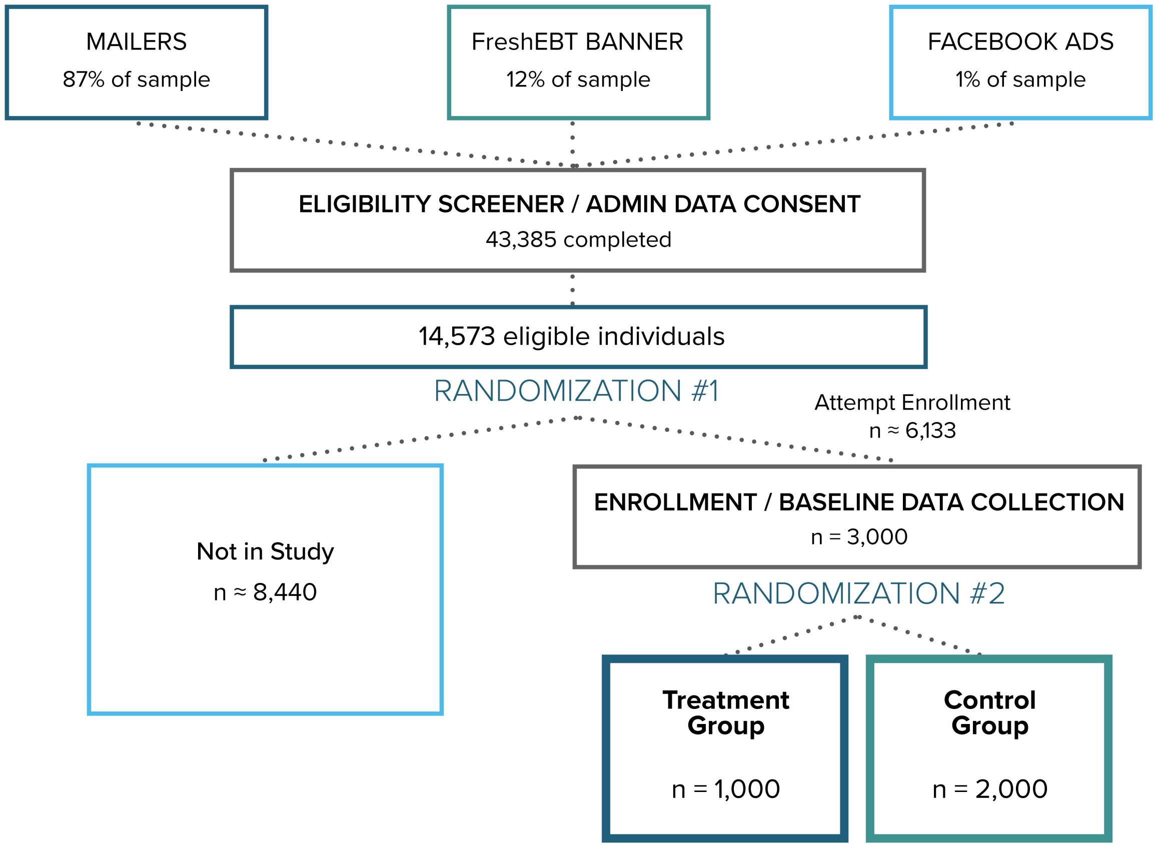
Notes: This table reports estimates of how treatment households allocated the $34,200 net transfer that they received during the course of the study. The rows present the share of the $34,200 transfer allocated nine different categories: durables consumption, non-durables consumption excluding housing, housing non-durables consumption, human capital consumption, leisure, financial savings, physical assets, debt, and unexplained. Estimates of the effects of the transfer on leisure come from Vivalt et al. (2024). The first column presents results based on our main point estimates, while columns (2) through (4) present results with alternative assumptions of how to allocate the unexplained portion of the transfer.

Figure 1: Open Research Unconditional income Study Timeline



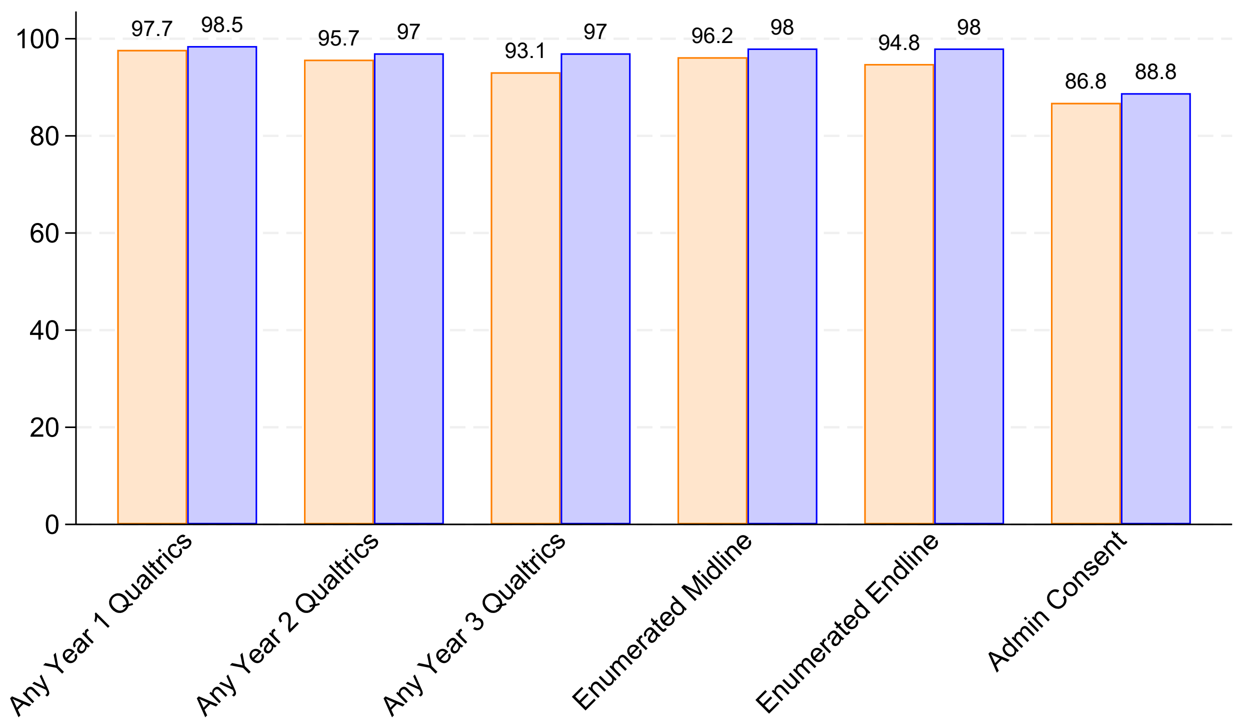
Note: This figure shows the timeline of ORUS participant enrollment, randomization, transfers, and data collection.

Figure 2: Randomization Structure



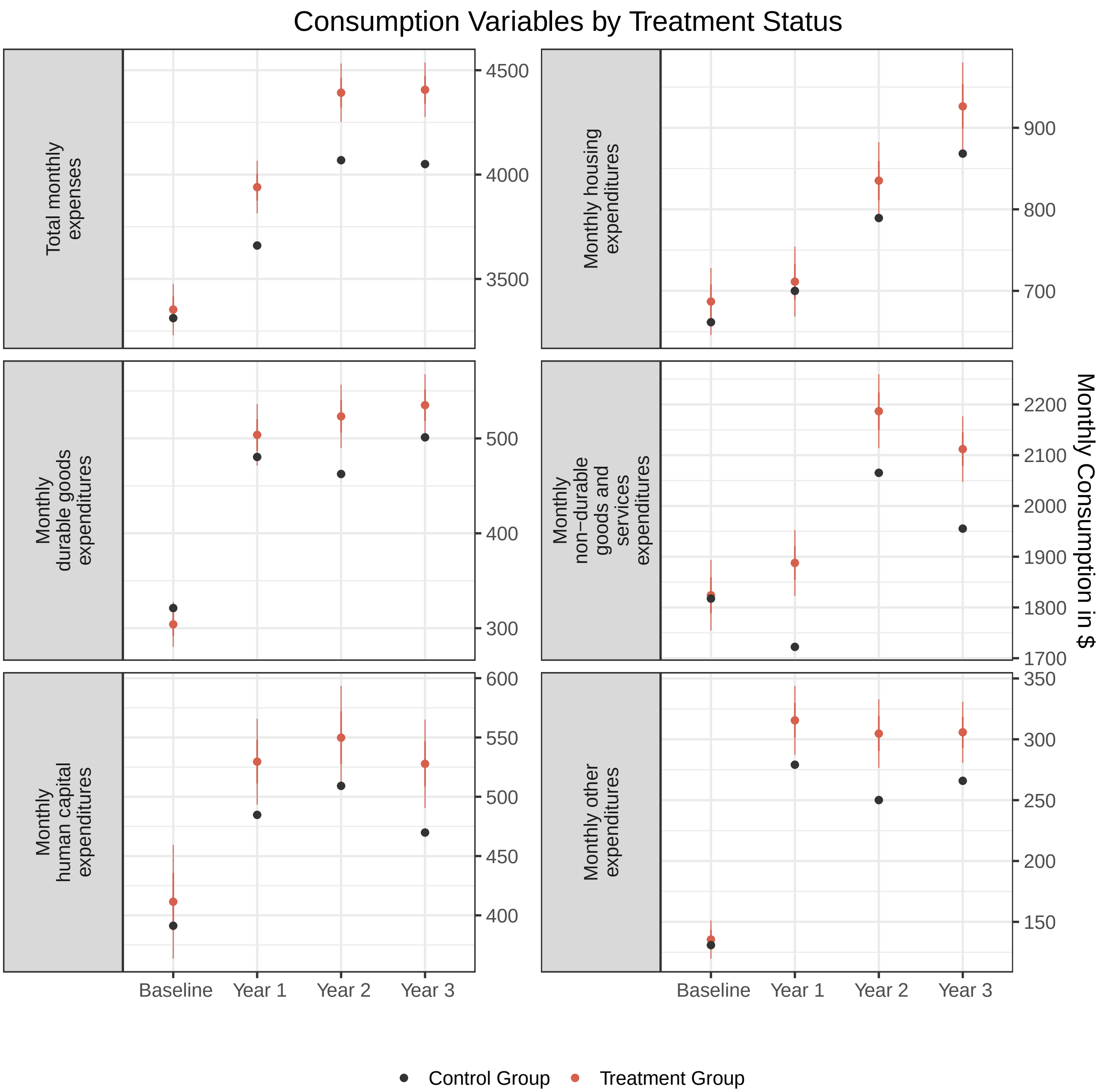
Note: This figure shows the flow of recruitment and randomization in the ORUS study.

Figure 3: Response Rates by Survey



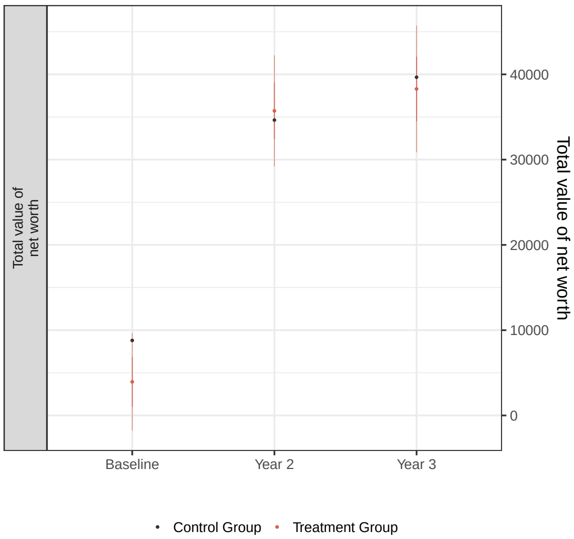
Note: This figure reports response rates by survey separately for the treatment group ($1000 per month) and the control group ($50 per month).

Figure 4: Expenditure Treatment Effects Over Time

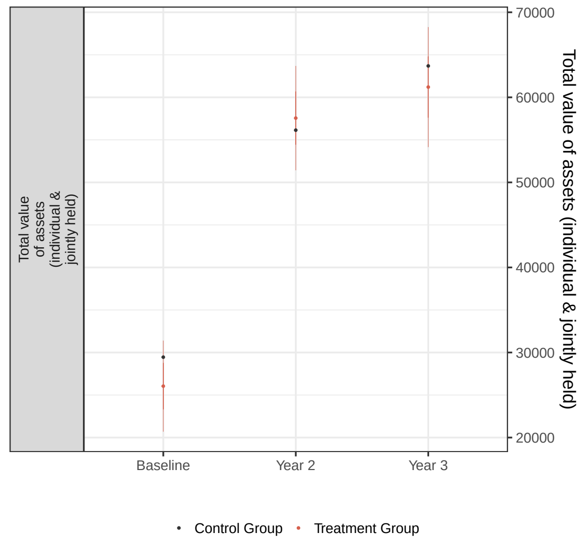


Note: This figure reports estimated treatment effects and control means by time period for overall consumption and major consumption categories without the post-transfer time period). The figure includes the baseline period. The black dots show how the control mean has evolved over time while the red dots show how the control mean plus the estimated treatment effect for the given time period. For study years 1, 2, and 3 we present estimates of the treatment effect using Equation 1. For the baseline period, we present estimates of Equation 1 where we remove the the baseline value of the outcome from the LASSO selected controls (if the baseline value was selected). We do this because because otherwise the controls would perfectly predict the outcome in the baseline period. 95% confidence intervals around the estimated treatment effect are shown in red.

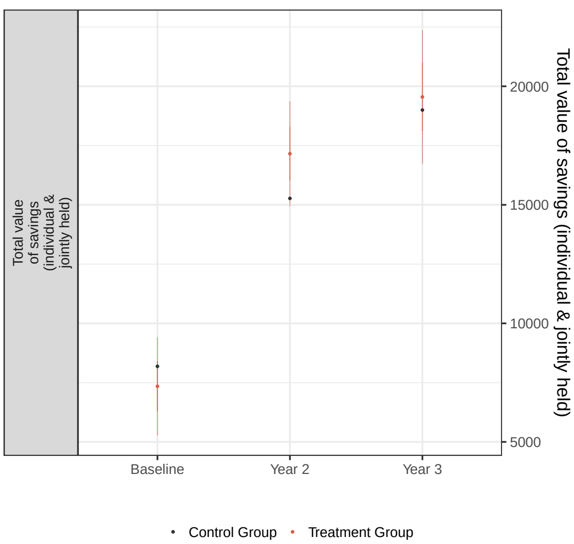
Figure 5: Treatment Effects Over Time for Household Balance Sheet Components



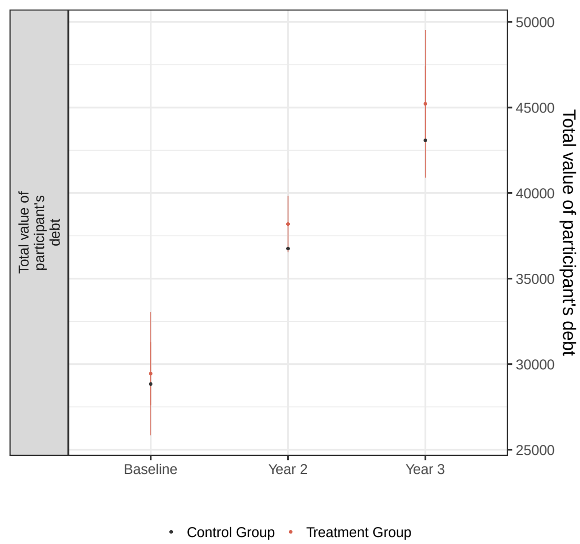
(a) Net Worth



(b) Real Assets



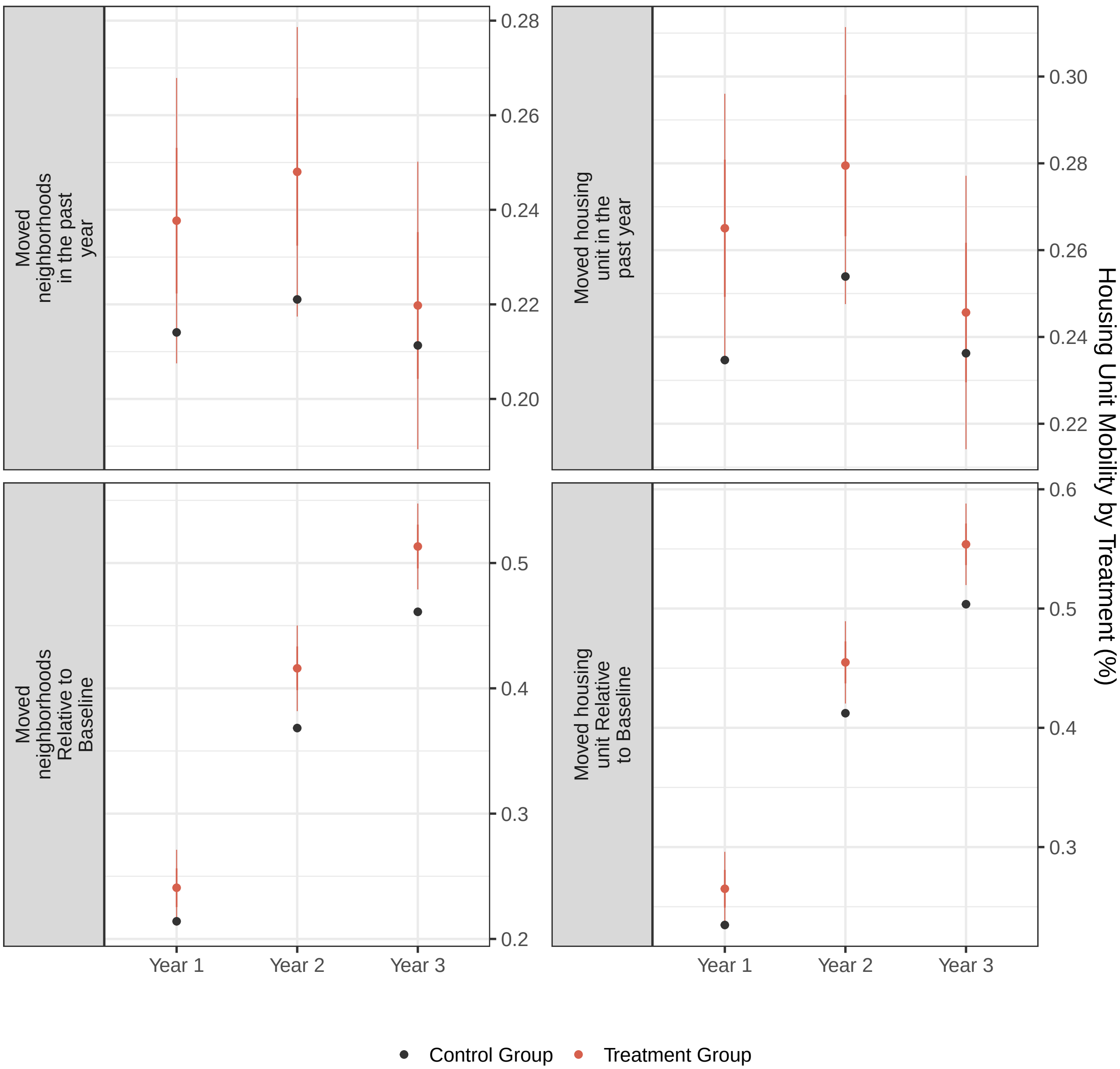
(c) Financial Assets



(d) Debt

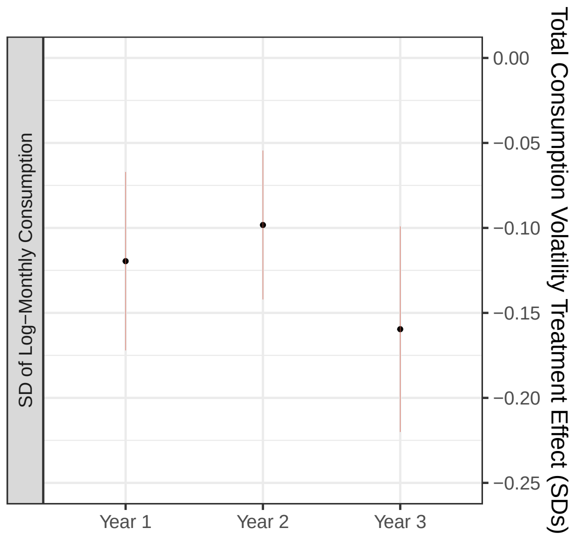
Note: This figure reports estimated treatment effects and the control mean by time period for net worth and the components of net worth. The figure includes the baseline period. The black dots show how the control mean has evolved over time while the red dots show how the control mean plus the estimated treatment effect for the given time period. For study years 1, 2, and 3 we present estimates of the treatment effect using Equation 1. For the baseline period, we present estimates of Equation 1 where we remove the the baseline value of the outcome from the LASSO selected controls (if the baseline value was selected). We do this because because otherwise the controls would perfectly predict the outcome in the baseline period. 95% confidence intervals around the estimated treatment effect are shown in red.

Figure 6: Housing Mobility Treatment Effects Over Time

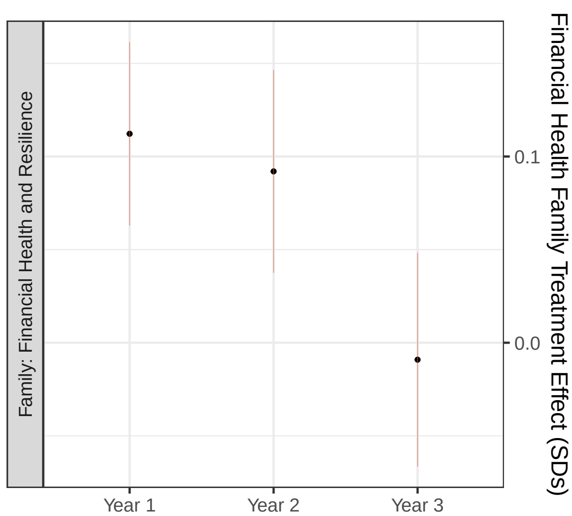


Note: This figure reports estimated treatment effects and the control mean by time period for moving housing units and moving neighborhoods. The black dots show how the control mean has evolved over time while the red dots show how the control mean plus the estimated treatment effect for the given time period. The treatment effect estimates come from estimating Equation 1. 95% confidence intervals around the estimated treatment effect are shown in red.

Figure 7: Treatment Effects Over Time for Financial Outcomes Families



(a) Expenditure Volatility



(b) Financial Health

Note: This figure reports estimated treatment effects by time period for different financial outcomes families. The black dots show the estimated treatment effect and the red line shows the 95% confidence interval.

The Impact of Unconditional Cash Transfers on Consumption and Household Balance Sheets: Experimental Evidence from Two US States

Appendix

Alexander Bartik Elizabeth Rhodes David Broockman Patrick Krause Sarah Miller
Eva Vivalt

In this appendix, we provide additional details about the study design, present results for additional outcomes, and present robustness checks of our main results.

A Expenditure Data Further Details

In this section, we describe some of the details of the construction of the expenditure data in more detail.

A.1 Housing Costs Panel

We construct a monthly panel of individual housing costs. Specifically, as described in Section 4 above, each month participants were asked a set of “trigger modules" that asked questions about changes in contact information and current address, , and there were changes an additional set of questions were asked. In the case of the move module, these questions included new address, whether they rented, owned, or were "just staying" at the residence, what was the reason for their move, what was the monthly rent + utilities payment (if renting), and, if the home had been purchased, what the purchase price had been and how large of a down payment they had made. Given that these trigger modules were asked every month and survey response rates were very high, this allows us to construct a close to monthly panel of individual addresses over time. In order to do this we assumed that a participant lived at a given address until they either reported a new address, or did not respond to any research activities in a given year. To apply the cost of housing to all months where a participant lived at a given residence, we prioritized the housing cost data gathered in the enumerated surveys when available, and utilized data from the onlinetrigger modules when not. To do this if an residence was first provided in a onlinesurvey or other means, we backfilled the cost information for that address when it was available from subsequent SRC surveys. For example, if a participant moved from residence A (where they lived at the time of the baseline survey) to residence B in study month 14, then answered an enumerated midline survey in month 20, and continued to live at that residence through monthly 24; we applied the housing cost reported at baseline for all months lived at address A (1-13) and housing cost reported at midline for all months lived at address B (14-24). There are a small number of cases where an individual moves to a new address, but then never completes an enumerated survey for that address, either because they move to a different address before they complete their next enumerated survey or because they never complete an additional enumerated survey. In these cases we used the information provided in the Qualtics to estimate either the monthly rent (excluding utitlities) or mortgage payment to align with the other expenditure data. For mortgages, we assume participant's take out 30-year fixed rate mortgagse and estimate their monthly mortgage payment using the reported total purchase prices, down payment amount, and the average 30-year fixed rate mortgages rate that month from Federal Reserve Bank of St. Louis data to estimate the monthly mortgage payment .[38] We then scale up this mortgage payment by 1.3 to reflect the fact that the monthly mortgage payment question in the enumerated survey asks about mortgage payments as well as property taxes and home insurance. For rent, if the participant provided total cost of rent and utilities we leveraged responses from other participants to back out the utilities piece. To accomplish this we subtracted the median utility payment amount by quantile of total rent + utilities (split by state to account for higher cost of energy in Texas compared to Illinois).

A.2 Making Online and Enumerated Survey Expenditure Measures Comparable

Our expenditure data comes from both enumerated and quarterly online surveys. While there is significant overlap between the two survey modules, there are notable differences. First, the quarterly online survey was designed to be shorter and to focus on expenditures that could have high variability. Therefore this module asked more limited set of expenditure questions, most notably not including questions on housing costs and asking some questions in more aggregated categories, relative to the enumerated expenditure surveys. Second, the enumerated surveys allow the respondent to choose their own lookback period for each expenditure category (weekly, monthly, or yearly) whereas the quarterly online survey have a pre-determined lookback period for each expenditure category (weekly for food expenditures, monthly for childcare, etc...). Third, while the enumerated surveys asked about usual expenses, the online surveys asked for spending in the specific lookback period (last week or last month).

We made the expenditure data more comparable in these different surveys using the following

[38]Data is available here: https://fred.stlouisfed.org/series/MORTGAGE30US

procedure. First, for questions not asked in the online survey we applied the responses from the enumerated survey in that study year, or in the case of housing costs the value from that month in the housing panel. Second, for questions asked in weekly terms in the online surveys(spending on food, alchohol, tobacco/marijuana, and public transportation) we rescaled to the enumerated survey responses, with a scaler specific to survey question and lookback period. This was designed to account both for differences in lookback period as well as "usual" vs. specific, assuming that responses to the more specific and shorter lookback period questions were a more accurate representation of spending levels. To generate the scaler we compared monthly spending responses from the online and enumerated surveys by question, participant and study year; then using the mean ratio of spending between two survey methods to rescale.

A.3 Unexpected Expenses

Some unexpected expenses, such as car or home repairs, will not be included in responses regarding usual expenses (midline and endline) and will also likely be missed in questions about recent expenses (online surveys). To capture these expenses, approximately every quarter in the online surveys we also asked questions about any major unexpected expenses over the previous quarter. In order to incorporate this information in our measures of monthly spending we did the following. We assigned these different unexpected expenses to one of the following detailed expenditure categories (car, household, health, children, pets, vacation, or other), and if participant indicated that the total unexpected expenses were split across more than 1 category, the total amount was evenly split among the categories. We then generated the average monthly unexpected expense spending for each category in a study year and added that to all non-missing survey responses for a given category within the survey year.

B Deviations from Pre-Analysis Plan

We made a small number of deviations from the pre-analysis plan, based on feedback we received on the study, to correct ambiguities or errors in the pre-analysis polan, or because study or data collection procedures changed during the course of the study or because we decided upon further reflection that an alternative procedure was superior. These changes are listed below:

- These changes were made and posted to the Pre-Analysis Plan after transfers began, but before midline data had been received:

- Following Guess et al. (2023), we indicated that we would construct q-values that control the False Discovery Rate (FDR) rather than constructing p-values that controlled the Family Wise Error Rate (FWER) as we had originally specified.
- In the original pre-analysis plan, we said that when pooling results across time periods for a particular item that we would impute the item using the treatment-group specific mean for each missing time period and then pool across all time periods, including the imputed time periods. An observation would be considered non-missing for an outcome if there was data for at least one time period. We modified the pre-analysis plan to instead construct pooled outcomes for an item using the average of the non-missing time period for that respondent. As in the original approach, an observation is considered non-missing for an outcome if there was data for at least one time period. Results are extremely similar using both approaches.

- These changes to the posted PAP were made after the transfers began and after midline data was received but before endline data was received:
  - We clarified that the primary measure of housing unit and neighborhood mobility would be relative to the baseline address. This was always the intention of the pre-analysis plan but the language was ambiguous in the original version.
- These changes were made after the end of the transfers:
  - In the assets results reported in Table 8, Figure 5, and Appendix Figure A3 we used the total individually and jointly held financial savings and assets rather than solely individually held savings and assets We report results for individually held savings and asset in Appendix Table A8. We did this to make sure that the financial and real asset outcomes more closely correspond to the concept covered in the expenditure data (will covers all household spending) and the debt data (the experian credit histories include all debt obligations that the individual is listed on, regardless of whether or not it is jointly or individually held. Making the concept closer to individually and jointly held than individually held). The results using only individually held financial and real assets are quite similar to those using individually and jointly held assets.
  - We followed a slightly different procedure for cleaning the expenditure data to make the

enumerated and qualtrics survey estimates more comparable, fully incorporate information on housing costs generated by our monthly trigger module, and make the measures more comparable with the post-transfer surveys which are ongoing but for which we will presents results in the future. The changes we made to the expenditure data cleaning procedure are as follows:

* Rent and mortgage costs were not reported in the qualtrics surveys, but only in the SRC surveys. However, we have additional information about housing and location decisions that we instead use to construct a much more detailed panel of information on housing costs over time. This procedure is described in Appendix Section A. We use this panel dataset to provide a monthly rent or mortgage payment measure in the expenditure data for each quarterly qualtrics surveys, even though they the quarterly qualtrics surveys do not ask about rent.
* We use a slightly different procedure to rescale the expenditures measures to make the qualtrics and enumerated survey expenditure measures comparable. The qualtrics questions ask a more limited set of expenditure questions, most notably not including questions on housing costs and asking some questions in more aggregated categories. In the Pre-Analysis Plan, we specified that we would construct an alternative measure that scaled up the SRC numbers by choosing a scaling factor that minimized the mean squared deviation between the SRC and qualtrics expenditure numbers for different categories. However, as described above, our trigger modules allowed us to construct a more temporally granular data on housing expenditures that that we could plug into the quarterly qualtrics expenditure measures. Additionally, the reporting in more aggregated categories did not seem to generate significantly different estimates of spending for quarterly and enumerated surveys. Instead, differences in lookback period frequencies between qualtrics and the enumerated surveys for more frequently purchased products seemed to generate larger differences in measured expenditures. We use the procedure described in Appendix Section A to adjust the expenditure measures for differences in the lookback period between the enumerated surveys and the quarterly qualtrics surveys. The estimates are very similar to those using the originally specified rescaling procedure.
* We combined the Clothing and Apparel primary expenditure category with the Per-

sonal Care primary expenditures category because these categories are combined on the post-transfer surveys and we want to make the primary expenditure categories comparable.

Figure A1: ORUS Study Counties

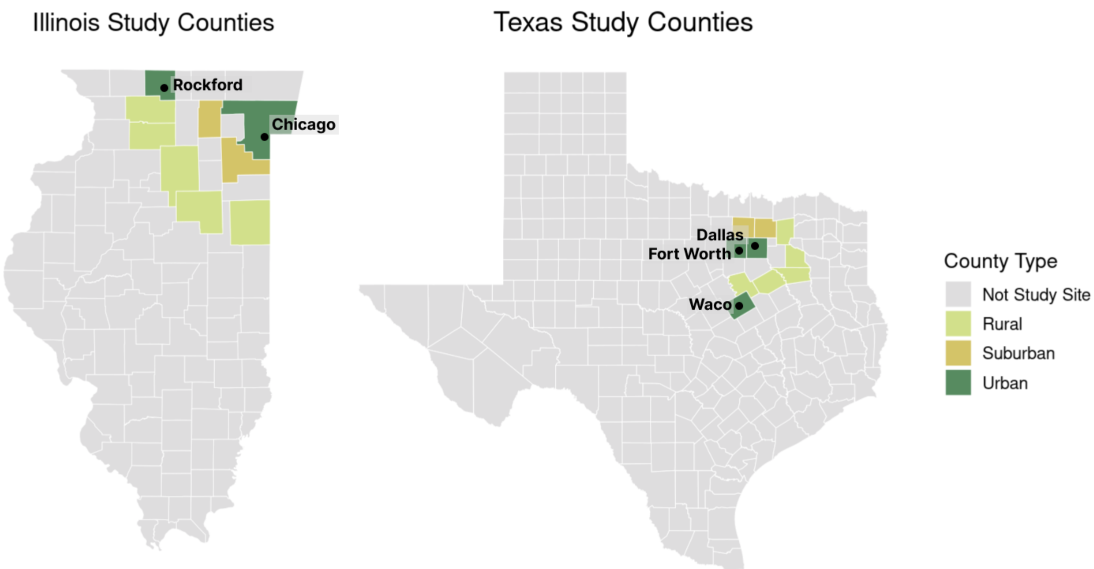
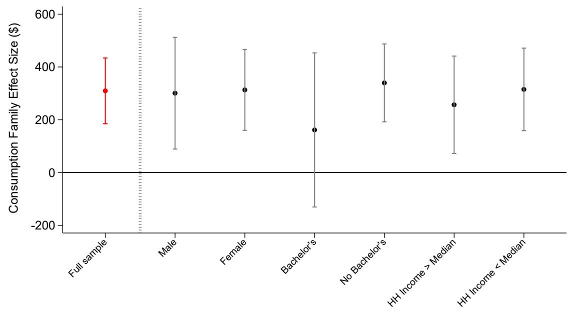
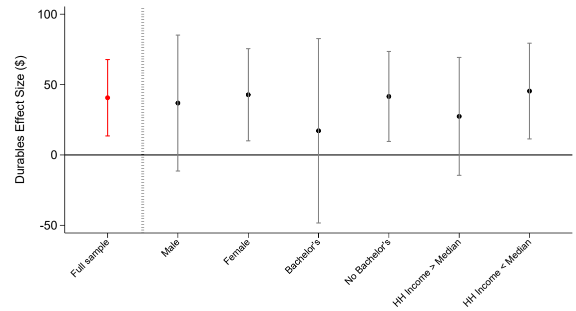


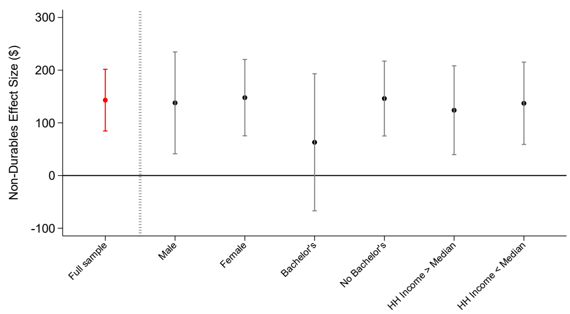
Figure A2: Heterogeneous Treatment Effects for Total Expenditures and Major Categories



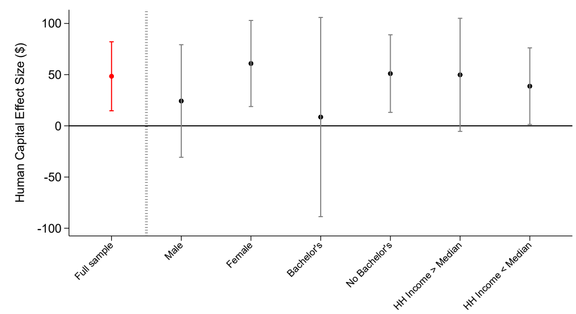
(a) Total



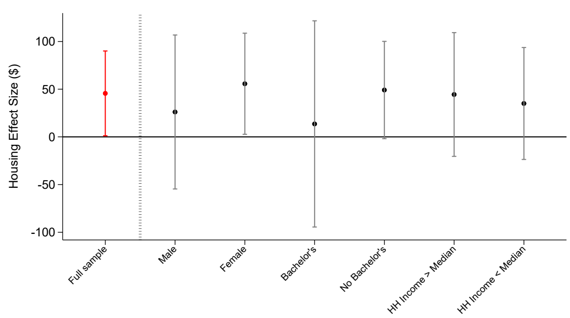
(b) Durable Goods and Services



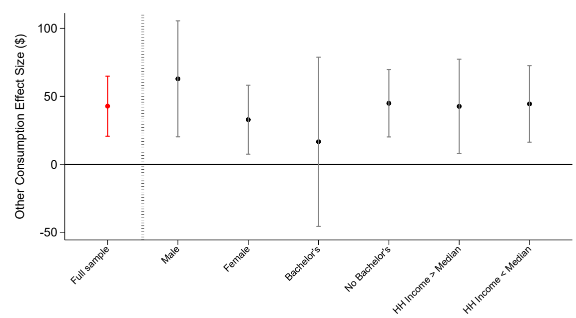
(c) Non-Durable Goods and Services



(d) Human Capital Expenditures



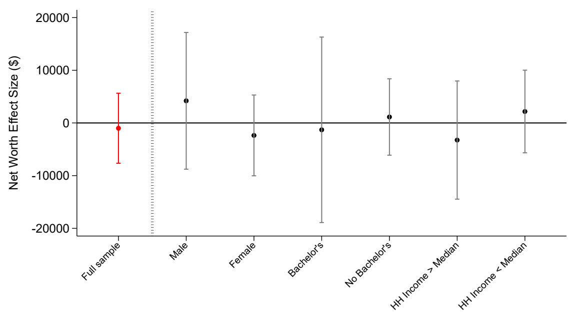
(e) Housing



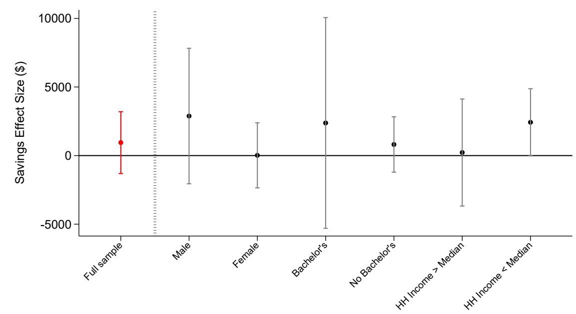
(f) Other

Note: This figure reports estimated heterogeneous treatment effects by subcategory for components of the consumption family.

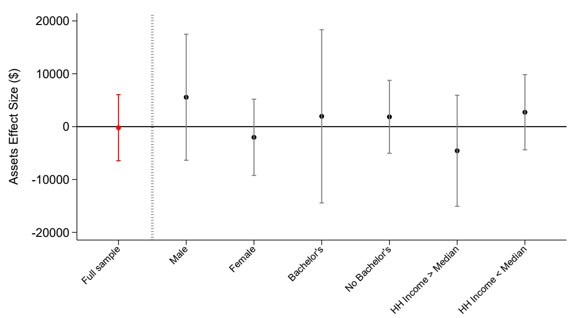
Figure A3: Heterogeneous Treatment Effects for Household Balance Sheet Components



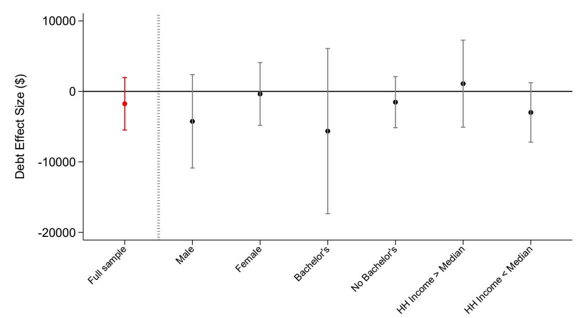
(a) Net Worth



(b) Financial Assets



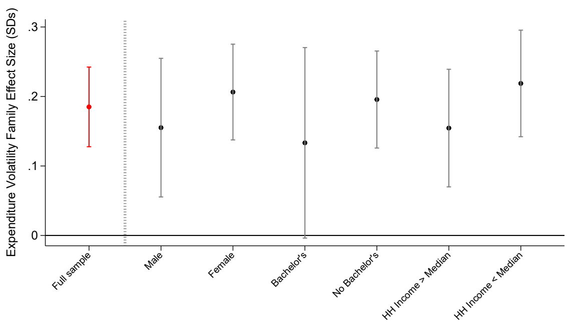
(c) Durable Assets



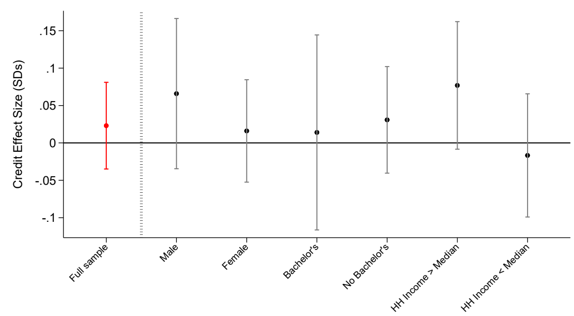
(d) Debt

Note: This figure reports estimated heterogeneous treatment effects by subcategory for components of the savings (household balance sheet) family.

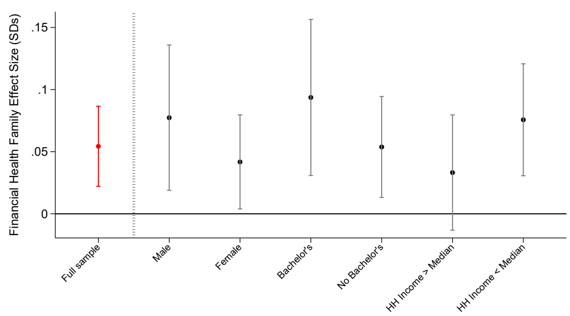
Figure A4: Heterogeneous Treatment Effects for Financial Outcome Related Families



(a) Expenditure Volatility



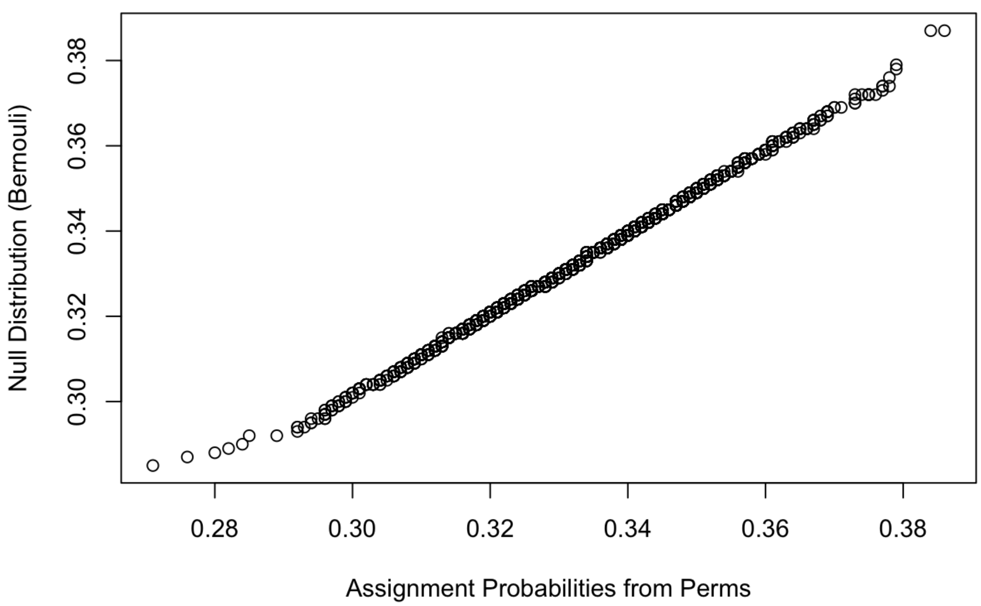
(b) Credit Access



(c) Financial Health

Note: This figure reports estimated heterogeneous treatment effects for the expenditure volatility, credit acces, and financial health families. Note that the sign of expenditure volatility in this figure is reversed from in Table 6, so a higher value is less volatility.

Figure A5: QQ-plot of treatment probability against Bernoulli distribution with one third success probability



Note: This figure compares the distribution of treatment assignments (quantiles plotted on x-axis) to what we would expect from a random one-third probability of assignment (quantiles plotted on y-axis). A Kolmogorov-Smirnov test fails to reject the null hypothesis that these distributions are the same (p=0.5226).

Table A1: Protection of Benefits

Benefit	Illinois	Texas
Medicaid	Eligibility was not affected	Eligibility was not affected
SNAP	Eligibility was not affected	First $300 per quarter did not affect SNAP, but the remaining amount of the transfer was considered unearned income for the purposes of determining eligibility and the amount of the benefit
TANF	Eligibility was not affected	First $300 per quarter did not affect TANF, but the remaining amount of the transfer was considered unearned income for the purposes of determining eligibility and the amount of the benefit
Housing Assistance	Did not affect eligibility for Chicago Housing Authority, eligibility was affected for other localities	Eligibility was affected by the cash transfer.
SSI	Not eligible to participate	Not eligible to participate

Notes: This table shows which major benefits were preserved or not preserved in Illinois and Texas.

Table A2: FDR Tiers

	Pooled Across Midline/Endline and Monthly Surveys	Pooled Across Midline/Endline Surveys Only	Estimates At Each Time Period
Family	K0	K0	K3
Primary Components	K1	K1	K3
Primary Items	K2	K2	K3
Secondary Items	K3	K3	K3
Tertiary Items	K3	K3	K3
Heterogeneous treatment effects	K3	K3	Not calculated
Any post-PAP tests	K4	K4	K4

Table A3: Baseline median characteristics by treatment arm

	Treatment	Control	p-value
Economic			
Household income ($1000s)	28.425	27.521	0.239
Total government benefits ($1000s)	2.888	2.772	0.601
Expenditures			
Total monthly expenditures ($1000s)	2.822	2.798	0.687
Housing monthly expenditures ($1000s)	0.692	0.675	0.493
Non-durable goods/services monthly expenditures ($1000s)	1.364	1.330	0.284
Durable goods monthly expenditures ($1000s)	0.208	0.210	0.849
Human capital expenditures ($1000s)	0.180	0.163	0.224
Monthly net gifts or loans to family and charity ($1000s)	0.013	0.008	0.206
Monthly Debt Payments			
Monthly minimum auto loan payments	0.000	0.000	1.000
Monthly minimum credit card and bank loan payments	0.000	0.000	1.000
Monthly minimum student loan payments	0.000	0.000	1.000
Monthly minimum mortgage payments	0.000	0.000	1.000
Monthly minimum total debt payments	90.000	100.000	0.431
Monthly minimum total debt payments, excl. mortgage	79.000	80.000	0.962
Household Balance Sheet			
Total HH real assets ($1000s)	4.045	4.159	0.808
Total HH financial savings ($1000s)	0.700	0.900	0.129
Total debt ($1000s)	9.812	9.608	0.844
Total debt excluding mortgage ($1000s)	8.054	7.920	0.883
Networth ($1000s)	-0.002	0.000	1.000
Networth excluding real estate assets & debt($1000s)	-0.468	-0.562	0.843

Notes: This table reports the median baseline characteristics of the study sample separately by those assigned to the treatment group who receive transfers of $1000 a month and those assigned to the control group who receive transfers of $50 a month. Median characteristics are reported for the continuous variables in Table 1.

Table A4: Impact of Unconditional Cash Transfer on Post-Transfer Recall Questions

	Control Mean (1)	Effect (2)	N (3)
Largest amount of savings held at any point during program	6768 (14827)	2372***††† (589) [0.002]	2190
Total amount used as a down payment to purchase a home since baseline	1335 (10486)	377 (553) [0.256]	2938
Amount of money participant paid for court costs, & fines/fees	357 (1181)	1 (46) [0.447]	2351
Amount of money taken without permission	461 (3219)	18 (120) [0.430]	2369
Total amount spent on home/apt renovation cost	757 (4866)	-140 (160) [0.200]	2929
Amount of money the participant gave to others	1002 (2839)	766***††† (118) [0.001]	2398
Money disputes or pressures has caused conflict	0.31 (0.46)	-0.04*† (0.02) [0.085]	2374
Conflicts over money negatively affect well being	0.56 (0.95)	-0.04 (0.04) [0.245]	2373
Felt pressured to give money to or pay expenses for others	2.55 (0.65)	0.01 (0.03) [0.467]	2378
Any money was taken without participants permission	0.21 (0.41)	-0.04***†† (0.02) [0.025]	2378

Notes: This table reports estimates of Equation 1 of the difference in outcomes between the treatment group and the control group on outcomes listed in the rows. We estimate these treatment effects for outcomes pooled over the three years of the study using the weights described in Section above. Column (1) reports the control group mean and standard deviation in parentheses for this pooled outcome. Column (2) then reports the treatment effect estimates. Standard errors are shown in parentheses below and the FDR-adjusted q values are shown in brackets below the standard error. Statistical significance relative to a null hypothesis of no effect using traditional inference is shown using asterisks, while statistical significant after FDR adjustment is indicated using daggers. Family outcomes are shown in bold, components within families are underlined, and items within components are shown indented. Secondary items are shown in italics.

Table A5: Impact of Unconditional Cash Transfer on Aggregate Expenditure Categories

	Control Mean (1)	Effect (2)	N (3)
Non-durable goods and services expenditures	1944 (805)	143***†††	2987
		(30)	
		[0.001]	
Housing expenditures	809 (582)	46**†	2964
		(23)	
		[0.050]	
Human capital expenditures	485 (409)	48***††	2988
		(17)	
		[0.011]	
Durable goods expenditures	484 (346)	41***†††	2987
		(14)	
		[0.009]	
Other expenditures	264 (284)	43***†††	2987
		(11)	
		[0.001]	

Notes: This table reports estimates of Equation 1 of the difference in outcomes between the treatment group and the control group on outcomes listed in the rows. We estimate these treatment effects for outcomes pooled over the three years of the study using the weights described in Section above. Column (1) reports the control group mean and standard deviation in parentheses for this pooled outcome. Column (2) then reports the treatment effect estimates. Standard errors are shown in parentheses below and the FDR-adjusted q values are shown in brackets below the standard error. Statistical significance relative to a null hypothesis of no effect using traditional inference is shown using asterisks, while statistical significant after FDR adjustment is indicated using daggers. Family outcomes are shown in bold, components within families are underlined, and items within components are shown indented. Secondary items are shown in italics.

Table A6: Impact of Unconditional Cash Transfer on Charity and Help Given and Received Details

	Control Mean	Effect	N
	(1)	(2)	(3)
Charity expenses	41 (108)	7*†	2974
		(4)	
		[0.084]	
Gifts or loans given to others (excluding charity)	28 (66)	13***†††	2968
		(3)	
		[0.001]	
Gifts or loans received	35 (115)	3	2969
		(5)	
		[0.267]	
Net gifts or loans to others (excluding charity)	-7 (127)	10*†	2970
		(5)	
		[0.070]	

Notes: This table reports estimates of Equation 1 of the difference in outcomes between the treatment group and the control group on outcomes listed in the rows. We estimate these treatment effects for outcomes pooled over the three years of the study using the weights described in Section above. Column (1) reports the control group mean and standard deviation in parentheses for this pooled outcome. Column (2) then reports the treatment effect estimates. Standard errors are shown in parentheses below and the FDR-adjusted q values are shown in brackets below the standard error. Statistical significance relative to a null hypothesis of no effect using traditional inference is shown using asterisks, while statistical significant after FDR adjustment is indicated using daggers. Family outcomes are shown in bold, components within families are underlined, and items within components are shown indented. Secondary items are shown in italics.

Table A7: Impact of Unconditional Cash Transfer on Vehicle Ownership Details

	Control Mean (1)	Effect (2)	N (3)
Value of All Cars Owned by HH	9921.569 (1.1e+04)	217.853 (362.000) [1.000]	2939
Dummy if household owns/leases a car	0.783 (0.372)	0.024* (0.012) [0.374]	2940
Dummy if participant individually or jointly owns ANY vehicles	0.618 (0.439)	0.016 (0.015) [1.000]	2934
Number of vehicles owned individually or jointly by the participant	0.856 (0.792)	0.000 (0.025) [1.000]	2934
Number of vehicles leased by participant's household	0.174 (0.385)	0.003 (0.014) [1.000]	2939
Number of vehicles owned by participant's household	1.107 (0.959)	-0.030 (0.030) [1.000]	2939
Total number of vehicles owned & leased by participant's household	1.280 (0.970)	-0.020 (0.029) [1.000]	2939

Notes: This table reports estimates of Equation 1 of the difference in outcomes between the treatment group and the control group on outcomes listed in the rows. We estimate these treatment effects for outcomes pooled over the three years of the study using the weights described in Section above. Column (1) reports the control group mean and standard deviation in parentheses for this pooled outcome. Column (2) then reports the treatment effect estimates. Standard errors are shown in parentheses below and the FDR-adjusted q values are shown in brackets below the standard error. Statistical significance relative to a null hypothesis of no effect using traditional inference is shown using asterisks, while statistical significant after FDR adjustment is indicated using daggers. Family outcomes are shown in bold, components within families are underlined, and items within components are shown indented. Secondary items are shown in italics.

Table A8: Impact of Unconditional Cash Transfer on Assets Individual Estimates ($1000s)

	Control Mean (1)	Effect (2)	N (3)
Total value participant's assets	43.2 (89.0)	-2.1 (2.4) [1.000]	2941
Value of vehicles owned (participant)	6.3 (7.6)	0.1 (0.3) [1.000]	2934
Value of real estate owned (participant)	34.5 (85.8)	-1.8 (2.2) [1.000]	2888
Value of business assets (participant)	1.6 (18.9)	-0.1 (0.6) [1.000]	2936
Total savings held individually	12.3 (31.3)	2.1** (0.9) [0.151]	2939
Bank accounts not jointly held	4.3 (10.5)	1.0***† (0.4) [0.064]	2849
Other investments and accounts not jointly held	1.2 (10.5)	0.8** (0.3) [0.192]	2937
Retirement/pension accounts not jointly held	6.4 (20.7)	0.2 (0.6) [1.000]	2908

Notes: This table reports estimates of Equation 1 of the difference in outcomes between the treatment group and the control group on outcomes listed in the rows. We estimate these treatment effects for outcomes pooled over the three years of the study using the weights described in Section above. Column (1) reports the control group mean and standard deviation in parentheses for this pooled outcome. Column (2) then reports the treatment effect estimates. Standard errors are shown in parentheses below and the FDR-adjusted q values are shown in brackets below the standard error. Statistical significance relative to a null hypothesis of no effect using traditional inference is shown using asterisks, while statistical significant after FDR adjustment is indicated using daggers. Family outcomes are shown in bold, components within families are underlined, and items within components are shown indented. Secondary items are shown in italics.

Table A9: Impact of Unconditional Cash Transfer on Alternative Debt Measures ($1000s)

	Control Mean (1)	Effect (2)	N (3)
Total Debt Excluding Mortgages (Experian + Survey)	26.0 (39.3)	0.5 (1.0) [1.000]	2981
Total Debt Balances (Experian)	36.2 (61.9)	2.9 (2.7) [1.000]	2512
Credit Card Balance (Experian)	2.6 (4.4)	-0.0 (0.2) [1.000]	2512
Auto Loans (Experian)	4.8 (8.8)	0.8** (0.3) [0.424]	2512
Mortgages (Experian)	13.9 (46.2)	1.5 (1.7) [1.000]	2512
Student Loans (Experian)	17.7 (36.1)	-0.3 (1.0) [1.000]	2512
Credit Union and Bank Loans (Experian)	1.5 (4.9)	0.2 (0.2) [1.000]	2512
Total Debt Excluding Mortgages (Experian)	21.7 (32.7)	1.6 (1.5) [1.000]	2512

Notes: This table reports estimates of Equation 1 of the difference in outcomes between the treatment group and the control group on outcomes listed in the rows. We estimate these treatment effects for outcomes pooled over the three years of the study using the weights described in Section above. Column (1) reports the control group mean and standard deviation in parentheses for this pooled outcome. Column (2) then reports the treatment effect estimates. Standard errors are shown in parentheses below and the FDR-adjusted q values are shown in brackets below the standard error. Statistical significance relative to a null hypothesis of no effect using traditional inference is shown using asterisks, while statistical significant after FDR adjustment is indicated using daggers. Family outcomes are shown in bold, components within families are underlined, and items within components are shown indented. Secondary items are shown in italics.

Table A10: Impact of Unconditional Cash Transfer on Net Worth Robustness Checks

	Main Estimate (1)	**Median Regression** (2)	**DD Regression** (3)
Net Worth Family-Level Estimate (Survey & Experian Data)	-1.0 (3.4)	-	-0.7 (3.2)
Net worth individual item (Survey & Experian Data)	-1.9 (3.3)	-1.0 (1.2)	-1.0 (3.2)
Net worth excluding real estate (Survey & Experian Data)	-2.1 (2.6)	-0.8 (0.8)	0.6 (2.1)

Notes: This table reports alternative approaches to estimating the treatment effect of the effect of unconditional cash transfers on net worth. Column (1) reports estimates of Equation 1 of the difference in outcomes between the treatment group and the control group on outcomes listed in the rows. We estimate these treatment effects for outcomes pooled over the three years of the study using the weights described in Section above. Column (2) then reports median regression estimates of the treatment effect. Column (3) reports differences-in-differences estimates of the treatment effect. The first row presents results for our main estimates of net worth where we aggregate the item level estimates to components (financial assets, real assets, and debt) and then aggregate these component level estimates to generate an estimate of the effect on net worth. Unfortunately, median regression did not converge for some of the individual items that make up the components, so we are unable to report the median regression outcomes for this approach to computing net worth. To compensate for this, in the next row we report estimates for a variable where we construct a net worth variable directly and estimate treatment effects on that variable instead of aggregating treatment effect estimates on sub-components. For this variable, we're able to compute all three robustness checks. Estimates for this variable slightly differ from estimates aggregating the item and component level treatment effect estimates because of slight difference in how missing values are treated. Finally, the third row reports estimates using a measure of net worth that excludes real estate assets and mortgage debt. All three measures use debt measures that combine the Experian and survey data. Standard errors are shown in parentheses next to the point estimate. Statistical significance relative to a null hypothesis of no effect using traditional inference is shown using asterisks. Inference for robustness check is not FDR adjusted.

Table A11: Impact of Unconditional Cash Transfer on Self-Assessed Financial Health Index

	Control Mean (1)	Effect (2)	N (3)
<u>Additive index of self-reported financial health Likert scale outcomes</u>	18.19 (7.43)	0.67***††† (0.19) [0.002]	2898
I am behind with my finances.	2.22 (1.05)	0.08***†† (0.03) [0.020]	2896
My finances control my life.	1.84 (1.06)	0.03 (0.03) [0.212]	2896
I am securing my financial future.	1.71 (0.95)	0.09***††† (0.03) [0.007]	2897
I am just getting by financially.	1.95 (0.88)	-0.01 (0.03) [0.347]	2894
Giving gift for wedding, birthday, other occasion would put strain on finances for month	2.01 (0.99)	0.15***††† (0.03) [0.001]	2896
I could handle a major unexpected expense.	1.39 (0.99)	0.07**†† (0.03) [0.029]	2897
I have money left over at the end of the month.	1.72 (1.04)	0.07**†† (0.03) [0.031]	2896
I can enjoy life because of way I am managing my money.	1.70 (0.84)	0.09***††† (0.02) [0.003]	2897
Because of financial situation, I feel like I will never have things I want in life.	2.11 (0.97)	0.07**†† (0.03) [0.033]	2897
I am concerned that the money I have or will save wont last.	1.55 (1.03)	0.02 (0.03) [0.263]	2895

Notes: This table reports estimates of Equation 1 of the difference in outcomes between the treatment group and the control group on outcomes listed in the rows. We estimate these treatment effects for outcomes pooled over the three years of the study using the weights described in Section above. Column (1) reports the control group mean and standard deviation in parentheses for this pooled outcome. Column (2) then reports the treatment effect estimates. Standard errors are shown in parentheses below and the FDR-adjusted q values are shown in brackets below the standard error. Statistical significance relative to a null hypothesis of no effect using traditional inference is shown using asterisks, while statistical significant after FDR adjustment is indicated using daggers. Family outcomes are shown in bold, components within families are underlined, and items within components are shown indented. Secondary items are shown in italics.

Table A12: Impact of Unconditional Cash Transfer on Financial Resilience Index

	Control Mean (1)	Effect (2)	N (3)
Financial Resilience (Index)		0.03*††	2940
		(0.02)	
		[0.042]	
How long respondent could make ends meet selling assets or borrowing	3.27 (2.07)	-0.02	2920
		(0.06)	
		[0.400]	
The extent to which the respondent relies on financial help from others	4.79 (0.58)	0.04*†	2935
		(0.02)	
		[0.054]	
Whether or not the respondent has at least $100 in savings	0.71 (0.41)	0.03**††	2849
		(0.01)	
		[0.030]	
The largest emergency expense that the respondent could cover	2664.42 (6833.86)	124.21	2914
		(229.00)	
		[0.356]	
How confident participant is about retirement savings	2.48 (0.91)	-0.02	2932
		(0.03)	
		[0.267]	
How long the respondent could make ends meet using savings	2.45 (1.92)	0.03	2923
		(0.06)	
		[0.356]	

Notes: This table reports estimates of Equation 1 of the difference in outcomes between the treatment group and the control group on outcomes listed in the rows. We estimate these treatment effects for outcomes pooled over the three years of the study using the weights described in Section above. Column (1) reports the control group mean and standard deviation in parentheses for this pooled outcome. Column (2) then reports the treatment effect estimates. Standard errors are shown in parentheses below and the FDR-adjusted q values are shown in brackets below the standard error. Statistical significance relative to a null hypothesis of no effect using traditional inference is shown using asterisks, while statistical significant after FDR adjustment is indicated using daggers. Family outcomes are shown in bold, components within families are underlined, and items within components are shown indented. Secondary items are shown in italics.

Table A13: Impact of Unconditional Cash Transfer on Financial Behavior Components: Bankruptcies, Delinquencies, and Credit Utilization

	Control Mean (1)	Effect (2)	N (3)
Bankruptcy and Foreclosure (Index)		0.00 (0.04) [0.479]	2512
Any Bankruptcy Past 24 Months (Experian)	0.01 (0.08)	-0.00 (0.00) [0.408]	2512
Any Current Foreclosures (Experian)	0.00 (0.01)	0.00 (0.00) [0.498]	2512
Any Bankruptcies Past 24 Months or Current Foreclosures (Experian)	0.01 (0.08)	-0.00 (0.00) [0.464]	2512
Credit Utilization Past 3 Months (Experian)	55.79 (37.04)	0.02 (1.55) [0.564]	2512
Delinquencies (Experian Variables)		-0.02 (0.02) [0.275]	2977
Any Delinquencies Past 6 Months (Experian)	0.13 (0.22)	-0.01 (0.01) [0.521]	2512
Balance Past Due Past 6 Months (Experian)	2747.91 (4885.74)	20.32 (209.00) [0.819]	2512
Trades Derogatory (Number) Past 6 Months (Experian)	0.47 (0.73)	-0.01 (0.03) [0.630]	2512
Current Worst Present Status (Experian)	2.94 (2.25)	-0.03 (0.10) [0.671]	2512

Notes: This table reports estimates of Equation 1 of the difference in outcomes between the treatment group and the control group on outcomes listed in the rows. We estimate these treatment effects for outcomes pooled over the three years of the study using the weights described in Section above. Column (1) reports the control group mean and standard deviation in parentheses for this pooled outcome. Column (2) then reports the treatment effect estimates. Standard errors are shown in parentheses below and the FDR-adjusted q values are shown in brackets below the standard error. Statistical significance relative to a null hypothesis of no effect using traditional inference is shown using asterisks, while statistical significant after FDR adjustment is indicated using daggers. Family outcomes are shown in bold, components within families are underlined, and items within components are shown indented. Secondary items are shown in italics.

Table A14: Impact of Unconditional Cash Transfer on Expenditure Volatility Additional Outcomes

	Control Mean (1)	Effect (2)	N (3)
Standard deviation of log monthly expenditures	0.71 (0.75)	**-0.14***†††** **(0.02)** **[0.001]**	**2834**
Standard deviation of log monthly human capital expenses	1.69 (1.00)	-0.05 (0.03) [0.163]	2833
Standard deviation of log monthly non-durable goods and services expenses	0.58 (0.73)	-0.14***††† (0.02) [0.001]	2834
Self-assessed extent of month-to-month variation in household's expenses	1.61 (0.48)	0.02 (0.02) [0.153]	2911

Notes: This table reports estimates of Equation 1 of the difference in outcomes between the treatment group and the control group on outcomes listed in the rows. We estimate these treatment effects for outcomes pooled over the three years of the study using the weights described in Section above. Column (1) reports the control group mean and standard deviation in parentheses for this pooled outcome. Column (2) then reports the treatment effect estimates. Standard errors are shown in parentheses below and the FDR-adjusted q values are shown in brackets below the standard error. Statistical significance relative to a null hypothesis of no effect using traditional inference is shown using asterisks, while statistical significant after FDR adjustment is indicated using daggers. Family outcomes are shown in bold, components within families are underlined, and items within components or families are shown indented. Secondary items are shown in italics.

Table A15: Impact of Unconditional Cash Transfer on Secondary Mobility Outcomes

	Control Mean (1)	Effect (2)	N (3)
Moved housing units in the past year	0.244 (0.298)	0.015 (0.010) [0.228]	2993
Moved neighborhoods in the past year	0.218 (0.284)	0.015 (0.010) [0.174]	2993
Moved 2mi (driving) from previous address	0.194 (0.273)	0.021**† (0.010) [0.075]	2993
Moved 2mi (driving) from BL address	0.322 (0.418)	0.047***†† (0.015) [0.012]	2993
Moved 2mi (as crow flies) from BL address	0.306 (0.412)	0.049***††† (0.015) [0.010]	2993
Moved 2mi (as crow flies) from previous address	0.184 (0.268)	0.021**† (0.009) [0.071]	2993
Moved jurisdiction/city since baseline	0.252 (0.400)	0.032**† (0.014) [0.075]	2993
Moved jurisdiction/city from previous address	0.139 (0.239)	0.009 (0.009) [0.309]	2993
Miles moved within labor market since baseline	1.965 (5.069)	0.055 (0.172) [0.571]	2993
Miles moved within labor market since previous address	0.716 (2.424)	0.084 (0.142) [0.463]	2993
Average importance of family-related reasons for moving	3.314 (1.578)	-0.016 (0.081) [0.641]	1360
Average importance of financial-related reasons for moving	3.406 (1.448)	-0.065 (0.078) [0.417]	1359
Average importance of job-related reasons for moving	2.564 (1.484)	-0.098 (0.078) [0.282]	1359
Average importance of location-related reasons for moving	3.496 (1.418)	0.077 (0.071) [0.325]	1359

Notes: This table reports estimates of Equation 1 of the difference in outcomes between the treatment group and the control group on outcomes listed in the rows. We estimate these treatment effects for outcomes pooled over the three years of the study using the weights described in Section above. Column (1) reports the control group mean and standard deviation in parentheses for this pooled outcome. Column (2) then reports the treatment effect estimates. Standard errors are shown in parentheses below and the FDR-adjusted q values are shown in brackets below the standard error. Statistical significance relative to a null hypothesis of no effect using traditional inference is shown using asterisks, while statistical significant after FDR adjustment is indicated using daggers. All items in this table are secondary and are shown in italics.

Table A16: Marginal Propensities to Consume Out of Unsaved Income

	Main Results	Rescale Expenditures by Ratio of CEX/PCE	(2) + Assume Change in Net Worth $5,000 + Allocate Remainder to Consumption	Allocate All Under-Reporting in (2) to Consumption
Change in net worth	-$1,000	-$1,000	$5,000	-$1,000 Change in Net Worth
	(1)	(2)	(3)	(4)
Consumption				
Durables Goods	0.04	0.08	0.13	0.14
Human Capital	0.06	0.06	0.11	0.12
Non-durables, (excluding housing)	0.18	0.24	0.41	0.44
Housing non-durables	0.05	0.05	0.09	0.10
Leisure	0.21	0.21	0.25	0.21
Unexplained				
Residual	0.49	0.39	-	-

Notes: This table reports estimates of the marginal propensity to consume durables consumption, non-durables excluding housing, housing non-durables, human capital related services, and leisure out of the portion of the transfer that was not saved during the program. Estimates of the effects of the transfer on leisure come from Vivalt et al. (2024). The first column presents results based on our main point estimates, while columns (2) through (4) present results with alternative assumptions of how to allocate the unexplained portion of the transfer.