NBER WORKING PAPER SERIES

HOUSEHOLD RESPONSES TO GUARANTEED INCOME: EXPERIMENTAL EVIDENCE FROM COMPTON, CALIFORNIA

Sidhya Balakrishnan
Sewin Chan
Sara Constantino
Johannes Haushofer
Jonathan Morduch

Working Paper 33209
http://www.nber.org/papers/w33209

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

We thank our academic advisory board for their helpful comments and feedback: Donald Green, Evelyn Forget, Greg Duncan, Irwin Garfinkel, Michael Lewis, Suresh Naidu, Wojciech Kopczuk, Katherine O'Regan, Sharoni Little, Brian Elbel, and Ingrid Ellen. We are also grateful for the support and recommendations of our community advisory board: Sharoni Little, Abigail Lopez-Byrd, Sandra Moss, Maritza Agundez, Lori Gay, Keith Curry, Kathryn Icenhower, Michael Fisher, Efrain Garibay, Greg Pitts, Cynthia Nunn, Amen Mandela Rahh, Yolanda Gomez, Annabella Bastida, Sarah Bomani, Sara Silva, Tommy Johnson, and Jaren Savage. We thank former mayor Aja Brown and Nika Soon-Shiong for leading the development and implementation of the Compton Pledge cash transfer program. We also thank the Robert Wood Johnson Foundation, J-PAL, and private donors for research funding. We thank the attendees of seminar and conference presentations at NYU, the Association for Public Policy Analysis and Management, the Robert Wood Johnson Foundation, the Abdul Latif Jameel Poverty Action Lab (JPAL), the Global Action for Policy Initiative (GAP) at Northeastern University, the Center for Effective Global Action (CEGA), as well as Debra Fine and Sheldon Danziger, for their valuable comments and feedback on the project. This study is registered in the AEA RCT Registry as AEARCTR-0007621. We thank our implementation partner, the USC Center for Economic and Social Research (CESR), for their work on data collection. We are grateful to Ege Aksu, Marcella Cartledge, Roberta Costa, Sara Restrepo, and Yunjie Xie for excellent research support. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Household Responses to Guaranteed Income: Experimental Evidence from Compton, California
Sidhya Balakrishnan, Sewin Chan, Sara Constantino, Johannes Haushofer, and Jonathan Morduch
NBER Working Paper No. 33209
November 2024
JEL No. D12, I38

ABSTRACT

We study the effects of a two-year unconditional cash transfer program for lowincome households in Compton, California between 2021 and 2023. 695 households were randomly assigned to receive transfers averaging about $500 per month over a two year period, with 1,402 households randomly assigned to a control group. To measure the impact of transfer frequency, half of the recipients were paid twice per month and the other half received quarterly transfers. We surveyed 1,074 respondents 18 months after the beginning of transfers. Receiving guaranteed income had no impact on the labor supply of full-time workers, but part-time workers (at baseline) had lower labor market participation by 13 percentage points. Income (excluding the transfer) was reduced by $333 per month on average relative to control households, and expenditures were reduced by $302 per month. At the same time, average non-housing debt balances declined by $2,190 over 18 months relative to the control group, although the drop is not statistically significant. We find a significant improvement in housing security, but no overall effects on indices of psychological and financial well-being. The recipients of twice-monthly transfers were more likely to own a car, had lower credit card debt and greater food security than recipients of quarterly transfers, but otherwise transfer frequency had little impact. Compared to male recipients, female recipients reported a greater increase in financial security, and a smaller reduction in earned income and expenditures.

Sidhya Balakrishnan
Jain Family Institute
568 Broadway
Suite 601
New York, NY 10012
sidhya.balakrishnan@jainfamilyinstitute.org

Sewin Chan
Robert F. Wagner Graduate School
of Public Service
New York University
105 East 17th Street
New York, NY 10003
sewin.chan@nyu.edu

Sara Constantino
Stanford Doerr School of Sustainability
Stanford University
473 Via Ortega, Y2E2
Stanford, California 94305
saraconstantino@stanford.edu

Johannes Haushofer
Department of Economics
National University of Singapore
1 Arts Link AS2 #06-02
Singapore 117570
and Stockholm University
and also NBER
johannes.haushofer@nus.edu.sg

Jonathan Morduch
Robert F. Wagner Graduate School
of Public Service
New York University
105 East 17th Street
New York, NY 10003
jonathan.morduch@nyu.edu

A randomized controlled trials registry entry is available at
https://www.socialscienceregistry.org/trials/7621

1 Introduction

Social and economic challenges are often, in part, money problems. This idea motivates unconditional cash transfer (UCT) policy, which provides cash transfers to low-income households without requiring that conditions are met to receive the transfers or imposing restrictions on how the money is spent (Baird et al., 2014; Guarino, 2021).

The global literature on cash transfers yields three promising results. First, cash transfers do not lead recipients to work less (Banerjee et al., 2017). Second, transfers usually generate broad increases in household consumption, psychological well-being, and income and assets (Arnold et al. 2011, Daidone et al. 2019), sometimes (but not always) with greater effects on consumption and income when targeted to women (Crosta et al., 2024). And third, the form of transfers can matter (Kansikas et al., 2023): Regular, frequent transfers tend to improve food security (Aguila et al., 2017), while larger, lumpier transfers increase net assets and purchases of durable goods (Haushofer and Shapiro 2016, Aguila et al. 2017).

In contrast, recent evaluations of one-time and repeated UCTs on household spending and welfare in the United States find mixed results. While there is some evidence of positive impacts on expenditures (Jaroszewicz et al., 2022; Bartik et al., 2024) and financial health (Bartik et al., 2024), especially for very poor families (Pilkauskas et al., 2022), there are also reports of null or even negative impacts on a number of outcomes, including income and employment (Vivalt et al., 2024), material hardship (Jacob et al., 2022), and mental and physical health (Miller et al., 2024; Pilkauskas et al., 2022; Jaroszewicz et al., 2022).

The mixed results are consistent with the different contexts of the global studies compared to the United States. In higher-income countries like the United States, there has long been concern that recipients of cash benefits will reduce labor supply (e.g., Robins 1985, Moffitt 2002, Saez 2002, Grogger 2003, Kleven 2024). Decreases in labor supply may be driven by a demand for leisure or by responsibilities for childcare or eldercare, which may be difficult to combine with regular wage labor (Edin and Lein, 1997). Similarly, the expectation that cash transfers will fuel increased spending is most plausible in contexts where households have little overhanging debt. In the United States, where low-income households carry relatively high debt burdens, households may choose instead to de-leverage (Fulford 2023, Colarieti et al. 2024). The frequency of cash transfers may also have different effects in the United States. In Kenya, receiving lumpy, less frequent transfers led to greater increases in net assets by allowing households to overcome financing constraints (Haushofer and Shapiro, 2016), but in the United States, where credit card use is common, receiving steadier transfers may build net assets by helping households to keep up with bills and avoid taking on additional debt. Since steady cash flows can be a close substitute for wage employment or salaried work, high-frequency transfers may also lead to larger labor supply reductions than larger, irregular transfers, which could attenuate or reverse the positive impacts on food security seen in global studies (e.g., Aguila et al. 2017 in Mexico).

We designed an experiment to systematically investigate the impacts of unconditional cash transfers delivered at high and low frequencies in the US context. We analyze the effects of a two-year unconditional cash transfer program—the Compton Pledge—in Compton, California, a low-income city in Los Angeles County. The Compton sample shares characteristics with program participants in other US cities where UCTs have been introduced—e.g., Jackson, Mississippi; New York City; Atlanta, Georgia; Gary, Indiana; and San Francisco, Stockton, and Long Beach, California. In the Compton sample, 66% of respondents identify as Hispanic or Latino and 30% as Black or African American. Nearly all (99%) of the treatment group had income below 200% of the federal poverty line, and 57% had income below the poverty line.

When it was launched, the Compton Pledge was the largest city-based UCT in the United States (Vesoulis and Abrams, 2021). In collaboration with the Compton Community Development Corporation and the Fund for Guaranteed Income, 695 low-income households were randomly selected from a sample of 2,097 eligible low-income households to receive UCTs averaging about $500 per month for a period of two years.[1] On average, the transfers were equivalent to an increase of 19% of monthly household income at the start of the intervention.[2] A control group of 1,402 households did not receive transfers. The transfer group was further split into low (quarterly) and high (twice-monthly, i.e. twice a month) transfer frequency treatment arms, with total transfer size held constant across the two conditions. This variation allowed us to test not only the impacts of a regular unconditional cash transfer, but also to understand whether the effects of transfer frequency differ in a context where, in comparison with e.g. Kenya or Mexico, households face fewer constraints in access to credit but also have significant debt burdens.

Treated households received the cash transfers between February 2021 and April 2023. Eighteen months after the start of transfers, and six months before they were due to end, we conducted an endline survey of 1,074 households (700 control and 374 treatment households).[3] Our pre-registered primary outcome variables are labor supply, income, expenditure, net assets, psychological well-being, financial security, and food security.[4]

Averaging across transfer arms, we observe significant treatment effects on two of our seven primary outcomes: a $333 reduction in income (net of the cash transfer) and a $302 reduction

[1]To be eligible, potential recipients had to have baseline annual incomes below 220% of the federal poverty line, be between the ages of 23 and 57, and not receiving Social Security Income or Social Security Disability Insurance. Transfer size was dependent on family size and ranged from $300 per month for households without dependents to $600 per month for households with 2 or more dependents.

[2]The average transfer amount for those who received the transfers was $492. However, 70 people did not take up the transfers although they were eligible. When the 70 are included in the average, the mean transfer size was $442. In the sample that we analyze below, the average transfer size for recipients was $487, but the average was $450 when including 28 non-compliers. The $450 figure is the relevant number for the intent-to-treat analyses below.

[3]The overall survey response rate was 51% and response rates were balanced between control and treatment groups.

[4]Our analysis follows a pre-analysis plan filed before data collection was completed (Balakrishnan et al., 2021).

in expenditure in the treatment group relative to the control group.[5] We find no significant effects of the transfers on labor supply; assets; psychological well-being; financial security; or food insecurity.

We also pre-registered seven secondary outcomes. Among these, we find significant treatment effects on three outcomes: an improvement in housing security; a decrease in direct reports of tobacco expenditures; and an increase in tobacco use, measured using a list experiment designed to overcome social desirability bias. We find no significant impacts on the other secondary outcomes, which include participation in unpaid work; alcohol expenditure and alcohol consumption (measured via direct reports and a list experiment); and an index of intimate partner violence.

When comparing the two transfer frequency arms, we find no statistically significant differences for any of the primary or secondary outcomes, except for a marginally significant improvement in a food insecurity index in the twice-monthly relative to the quarterly group.

Echoing the common finding in low-income countries that UCT programs do not reduce labor supply (Banerjee et al., 2017), we find a non-significant positive treatment effect of 0.03 hours on weekly hours worked. We observe a 5 percentage point reduction at the extensive margin of labor market participation, but the effect is not significant.[6] Qualitatively, this finding is similar to the labor effects in the ORUS study of cash transfers in Texas and Illinois (Vivalt et al., 2024), which finds a significant 2 percentage point reduction in labor supply.

When disaggregating by worker characteristics, we find a strong and significant negative treatment effect among part-time workers: for respondents who worked less than 20 hours at baseline, the treatment effect is -13 percentage points and statistically significant. These labor market participation impacts align with the negative impact on household earnings: excluding transfer income, households in the treatment group experience a significant negative impact on household income of $333 per month, a 10% reduction relative to the control group average. This is again qualitatively similar to the ORUS study (Vivalt et al., 2024).

When the value of cash transfers is included in total household income, treatment households experienced a net increase in monthly income of $92 at the endline survey, relative to a control group mean of $3,341 (this represents a 3% increase, and is not statistically significant). However, the $92 increase is much smaller than the average transfer size, which was $450 per month in the sample we analyze.

The treatment effect on the earnings of the household member who directly receives the

[5]The intervention started after the initial shock of the coronavirus pandemic, and average incomes increased for both the treatment and control groups between the baseline survey in January 2021 and the endline survey in 2022. Negative estimated impacts on income therefore indicate smaller increases among the treatment group relative to the control group, not absolute decreases. The patterns are described in Section 4.2.1.

[6]The implied elasticities for hours of work and labor force participation are $+0.01$ and -0.47, respectively.

transfers is $-\$162$ (net of cash transfers) relative to a \$1,976 control group average (8% lower and significant at the 10% level). Treatment effects on other household member income and benefit income were also negative (-13% and -11%, respectively), although neither is statistically significant.

In tandem with the decrease in income, and in contrast to the global literature, we find that the impact on expenditures is also negative: $-\$302$ per month on average (10% lower than control), attributable in about equal parts to negative impacts on housing and non-housing expenditures. This result suggests a demand for saving or to reduce debt among recipients, consistent with the high level of debt in the sample: non-housing debt among the control group was \$19,142 at endline. In line with this possibility, we observe a large but non-significant impact of $-\$2,190$ on non-housing debt (14% relative to control) and a smaller impact on non-housing asset ownership (\$308, 0.8% relative to control). This results in a non-significant positive impact on net asset holdings of \$2,498, relative to a control group mean of $\$17,229$.[7]

The study took place at a similar time as the Open Research Unconditional Income Study (ORUS) in Illinois and Texas (Vivalt et al., 2024; Bartik et al., 2024; Miller et al., 2024). However, there are some notable differences between the two studies, which may explain differences in the results. The ORUS sites reflect a broader cross-section of the US, resulting in substantial differences in sample composition between the two studies. In particular, the ORUS sample has higher average incomes, and is younger, whiter, more rural, and resides in areas with a lower cost-of-living as compared to the Compton sample. We further compare the samples in Section 3.2. In contrast with our study, the ORUS study finds a sizable increase in household expenditures, accompanied by an increase in debt (Bartik et al., 2024). Their findings in Illinois and Texas suggest that while cash transfers increase short-term consumption, they are unlikely to lead to persistent improvements in financial outcomes. We instead find negative impacts on expenditure but evidence consistent with debt reduction. The difference in expenditure could result from the fact that the transfers in the ORUS study (\$1,000) were larger than in Compton (on average \$450 in the sample we analyze), while the reduction in household income was of a similar magnitude ($-\$210$ vs. $-\$333$). The non-significant increase in debt in Bartik et al. (2024) is puzzling and at odds with the sizable yet (non-significant) debt reduction found in our study.

Despite the suggestive evidence of a reduction in debt, potentially alleviating a significant source of stress, we find no significant overall treatment effects on indices of psychological well-being (+0.05 SD), financial security (+0.03 SD), or food security (+0.01 SD). However,

[7] The increase in income and reduction in expenditures induced by the transfers is roughly consistent with the magnitude of the reduction in debt. This can be seen in a back-of-the-envelope calculation which assumes that the changes in income and expenditures were similar over the 18 months between the beginning of transfers and the survey. The monthly \$92 increase in income and the \$302 reduction in expenditure amount to monthly savings of \$394, or \$7,092 over the 18 months before the survey. The \$2,190 decrease in household debt amounts to 31% of this figure, and the \$2,498 increase in net asset holdings amounts to 35%. Thus, the savings implied by the increase in income and reduction in expenditures can account for the increase in households' net worth.

we do find an improvement in housing security (+0.29 SD), one of our pre-specified secondary outcomes, that is driven by a decrease in the perceived likelihood of eviction.

The ORUS study finds large but temporary improvements in stress and food security among cash transfer recipients in Illinois and Texas, as well as increased medical spending and hospital use. However, similar to our findings, they do not observe any effects of the transfers on measures of physical or mental health after the first year of transfers (Miller et al., 2024).

We use list experiments to elicit responses to potentially sensitive questions about intimate partner violence and consumption of alcohol and tobacco products, additional pre-specified secondary outcomes. We find strong evidence of reductions in intimate partner violence among recipients relative to the control group in the list experiment, but the results do not hold in direct self-reports, which is consistent with under-reporting. In a separate list experiment, we find a moderately sized increase in tobacco consumption among recipients relative to the control group. In contrast, direct reports of spending on tobacco show a reduction in spending in the treated group relative to controls, possibly reflecting a demand effect. We find no significant treatment effects on alcohol consumption and expenditure, and directionally the effects are negative in both the list experiment and a direct question. This is consistent with global evidence that shows no changes or reductions in spending on temptation goods after cash transfers (Evans and Popova, 2017).

In the United States, policies like the earned income tax credit (EITC) deliver large, lumpy transfers, while others, like the supplemental nutrition assistance program (SNAP), deliver a steady flow of transfers through the year. The optimal transfer frequency is an important but under-explored question in cash transfer program design that can affect the ways that transfers are used and perceived. Overall, we find few statistically significant differences between households receiving twice-monthly or quarterly transfers. We find that food security is improved in recipients of twice-monthly transfers relative to recipients of quarterly transfers: food security is +0.11 SD higher in the twice-monthly treatment group and −0.10 SD lower in the quarterly treatment group relative to the control group. Neither of these individual effects is statistically significant, but the difference is significant at the 10% level, in line with Aguila et al. (2017). This result mirrors findings in low-income countries suggesting that high-frequency transfers are more likely to be spent on consumption, while low-frequency transfers are spent on durable goods (Haushofer and Shapiro, 2016; Gertler, 2004). We also find that recipients of twice-monthly transfers have $1,074 less credit card debt (relative to a control mean of $4,449; $p < 0.05$), which is significantly different from the impact on the quarterly treatment arm ($214, SE = $598). This result suggests that steady transfers help households to stay on top of bills and credit installments. Our findings are similar to evidence from the Chicago EITC Periodic Payment Pilot, where periodic payments reduced perceived financial stress (Kramer et al., 2019) and food insecurity (Andrade et al., 2019). Similarly, evidence from the expanded 2021 Child Tax Credit (CTC) indicates that lumpier transfers are used to reduce long-term debt (such as outstanding rent and mortgage payments), while more frequent transfers are used to ensure food security (Parolin and Wimer, 2023).

These results reflect aggregation across different recipients, with potentially heterogeneous needs and constraints. Looking at pre-registered heterogeneous treatment effects, we find larger negative treatment effects on income and expenditure among male compared to female recipients ($p = 0.09$ and $p = 0.03$, respectively).[8] We also find that female recipients report a significant increase on our index of financial security, while male recipients show a significant decrease (difference $p < 0.01$). We find little evidence for heterogeneous impacts by baseline income.

In exploratory analyses, we find notable differences when comparing single mother recipients—a group that comprises 22% of the sample and which experiences a high rate of poverty—to the rest of the sample. The treatment effect on weekly hours of work of is +6.4 hours ($p < 0.10$) among single mothers. These results echo Eissa and Leibman (1996) and Meyer and Rosenbaum (2001), who found positive labor supply effects of 1987 EITC expansions on single mothers relative to single women without children (who were not EITC eligible).[9] Consistent with the positive labor supply effects among single mothers, we estimate an impact of \$831 ($p < 0.01$) on income in this group (including the transfer), which is larger than the size of the cash transfer itself. The increase in weekly hours worked is +15.7 ($p < 0.01$) hours among those receiving quarterly transfers, and their income is \$1002 ($p < 0.01$) higher than the relevant controls.

These findings contribute to the small yet growing evidence on the impact of UCT programs on household welfare in the US. Several studies, launched in the first year of the COVID-19 pandemic, show no effects, or even negative effects, of unconditional cash transfers. Specifically, the ORUS study described above finds a reduction in income and employment (Vivalt et al., 2024) and no significant improvements in physical and mental health (Miller et al., 2024), although recipient households have increased consumption and better financial health (Bartik et al., 2024). Jaroszewicz et al. (2022) find increases in expenditures after large one-time cash transfers to low-income households, but null or even negative impacts on a range well-being indices, including financial, psychological, and health outcomes. Similarly, Pilkauskas et al. (2022) and Jacob et al. (2022) find no effects of a one-time \$1,000 cash transfer given to low-income households in the United States by the NGO GiveDirectly on five measures of economic and psychological well-being, although Pilkauskas et al. (2022) find improvements in material hardship for very poor families. Similar to our findings, Jacob et al. (2022) find a directional but non-significant reduction in debt, while Jaroszewicz et al. (2022) and Bartik et al. (2024) find increases in debt.

In contrast, other studies that involve frequent and regular transfers in the US and Canada have found positive impacts, including reduced health-related medical visits, increased

[8] Like the U.S. Census Bureau, our baseline survey asked respondents to report sex (and not gender). We thus mostly refer to "female" and "male" for transparency, but note that we sometimes use female (male) and woman (man) interchangeably.

[9] While the 1987 EITC expansions substantially increased the maximum benefit amount for single parents, the complex changes in effective marginal tax rates due to phase-in and phase-out of the EITC make direct comparisons with unconditional cash transfers inappropriate. More recently, Kleven (2024) investigated every federal and state EITC reform and found that only the federal 1993 expansion is associated with large employment increases for single mothers.

investment in child health, and improved mental health and educational attainment of children (Akee et al., 2010; Forget, 2011; Bastian and Michelmore, 2018), echoing many of the international findings. An experimental evaluation of the Chelsea Eats program, a nine month UCT that began in 2020, found positive impacts on food expenditure and consumption and financial distress, but did not find any significant effects on physical health, mental health or school attendance. (Liebman et al., 2022) Some of the differences between these results and our own findings may have to do with the timing of the studies. Evaluating a two-year UCT trial in Stockton, CA that lasted from the beginning of 2019 to the beginning of 2021, West and Castro (2023) find that transfers reduced income volatility, improved psychological and physical health, and had no labor supply effects, but only until the onset of the COVID-19 pandemic. After the start of the pandemic, most effects dissipated and were no longer significant. Our study started as their study ended in December 2020, right at the height of the pandemic. As such, our study and the recent results of the ORUS study (Vivalt et al., 2024) provide insights about the impacts of a regularly occurring UCT at a time of economic hardship and in the context of publicly-provided stimulus checks and other relief measures.

The remainder of this paper is structured as follows. Sections 2 and 3 describe the program, study design and econometric approach. Section 4 describes our results. Section 5 concludes.

2 Program description

Private donors established the Fund for Guaranteed Income (F4GI) as a 501(c)(3) public charity in August 2020. F4GI aims to provide recurring cash transfers to populations that are typically excluded from the social safety net. In partnership with former mayor Aja Brown, F4GI established the Compton Community Development Corporation (CCDC), also a 501(c)(3) charity, and in December 2020 launched the Compton Pledge, a two-year unconditional guaranteed income program. After an implementation pilot with 30 recipients in December 2020 (who are not part of the present study), CCDC scaled the program to a total of 800 cash transfer recipients between December 2020 and March 2021. 695 of these recipients are part of the study described here. Households were eligible if they resided in the City of Compton, had at least one household member aged 23 to 57, and had a household income below 220% of the federal poverty threshold. Households receiving Supplemental Security Income (SSI) or Social Security Disability Insurance (SSDI) benefits were excluded from the program as cash transfers could affect their benefits.

The study of the program is structured as a randomized controlled trial. To recruit individuals into the study, we obtained the contact information of potential study participants from the Compton 2018 and 2020 voter lists, the Compton Public Housing list, community organizations[10], and a random digit dialing sample. Between February and March 2021, we invited the individuals on these lists to participate in a well-being study

[10] *The Coalition for Humane Immigrant Rights of Los Angeles* (CHIRLA) and *One Fair Wage*

for the City of Compton using email, text messages, voice calls, and mailers. Participants could register by filling out a short online screening survey which asked for their name, age, sex, whether or not they had a disability, SSI/SSDI status, email address, mailing address, phone number, number of household members, number of minors in the household, and household income. Based on the responses to this screening survey, we determined whether respondents met the eligibility criteria for the study. The screening survey is the baseline survey for the study.

We identified 2,100 households that met the eligibility criteria listed above. These households were then randomly assigned to a treatment group (698 households) and a control group (1,402 households), stratified by participant sex. Three transfer recipients (2 in the twice-monthly and 1 in the quarterly group) were dropped later because we realized that they had also been part of the December 2020 implementation pilot. Our program sample is therefore 2097, with 695 participants in the treatment group, and 1402 participants in the control group. [11] In addition, roughly half of the transfer recipients (345) were randomly assigned to receive twice-monthly transfers, while the other half (350) were assigned to receive transfers once per quarter.

Contact information on the 695 randomly chosen recipients was provided to F4GI and CCDC, who invited them via email, text messages, and phone calls to receive cash transfers through the Compton Pledge. The transfers were described to participants as a guaranteed income with no conditions attached. Recipients were told about the magnitude and timing of the transfers, including their end date. They also received information about the possible impact of the transfers on existing benefits. Recipients had to register their participation in the program on F4GI's website or app. They could choose to receive the transfers through a prepaid debit card, direct deposit, PayPal, or Venmo. Out of the 695 households who were offered transfers, 625 took up the treatment and received payments (308 in the twice-monthly and 317 in the quarterly group). Cash transfers began within 14 days after recipients completed the registration, i.e. between February and March 2021, and continued for a total of 24 months. F4GI achieved a 99.9% payment success rate.[12]

The transfers were designed so that with perfect take-up recipients would receive on average \$6,400 per year for two years. Transfers were designed to vary with the number of children: households with no children received \$3,600 per year, households with one child received \$5,400 per year, and households with two or more children received \$7,200 per year. Table 1

[11] The sample of 695 households is a subset of the 800 which received transfers; the 102 households that make up the difference began receiving transfers in December 2020–January 2021, when the study design was still being finalized, and are therefore excluded. The overall allocation of a third of participants to treatment and two thirds to control differed slightly across enrollment waves, depending on the number of participants that the Compton Pledge wanted to enroll at any given time. If more than one person from a household completed the baseline, the recipient was chosen at random. We use weights in the final analyses to reflect the overall ratio in each enrollment wave; thus, for example, we up-weight treatment households that were recruited in waves in which treatment households accounted for less than a third of newly enrolled households. Details are provided in Table A1.

[12] The implementation report of the Compton Pledge was published in May 2023 and is available at https://f4gi.org/app/uploads/2023/06/2023-Implementing-the-Compton-Pledge.pdf.

shows that in our program sample of 695 recipients, the average yearly transfer was $5,304 ($442 monthly), including 70 non-compliers who did not receive the transfers (the average yearly transfer was $5,892 ($491 monthly) excluding non-compliers).[13]

Quarterly transfer recipients received a large lump-sum towards the beginning of each quarter, while twice-monthly recipients received smaller transfers on the 7th and 21st of each month.[14]

The CCDC and F4GI ensured minimal impact of the cash transfers on existing federal and state benefits. Households receiving SSI and SSDI were excluded from participation as the cash transfers could affect these programs and waivers could not be secured. Transfers did not affect recipients' eligibility for the Earned Income Tax Credit, Child Tax Credit, subsidized health care (including Medicaid and Children's Health Insurance Program), the Special Supplemental Nutrition Program for Women, Infants and Children (WIC), and the Low Income Home Energy Assistance Program (LIHEAP). This was ensured by keeping transfers below the federal government's "gift" maximum of $15,000 a year, and obtaining necessary waivers for the state-specific benefits. Specifically, the Compton Housing Authority agreed to exclude the transfers from the means-test for Housing Choice Vouchers. In addition, California's Department of Social Services approved waivers that protected benefits from CalWORKS (the state's version of the Temporary Assistance for Needy Families program, TANF) and CalFRESH (the state's version of the Supplemental Nutrition Assistance Program, SNAP). If recipients lost benefits despite these waivers, a $500,000 "Hold Harmless Fund" fund was in place to compensate them fully for these losses, and counselors were available to re-enroll them once transfers ended. F4GI has not used this fund as of the date of publication of this paper.

[13]Reasons for non-compliance (not taking up the transfers although eligible) are mainly due to difficulties reaching households by email and phone. Reasons also could include fear that the program was a scam and personal preference. The sample that we analyze in the RCT includes 28 non-compliers.

[14]We aimed to hold the net present value constant across the two treatment arms. Specifically, the total amount transferred to each twice-weekly recipient in each quarter, spread out across the quarter, was the same amount that a comparable quarterly recipient would receive in that quarter as a lump sum. A slight difference in net present value (NPV) can still arise if, as was the case, the quarterly transfers are sent towards the beginning of each quarter, while the twice-monthly transfers are spread out over the quarter. To assess the difference in NPV between the two treatment arms, we calculate their respective NPV using an inflation factor of 1.08 for the two-year period 2021–2023. (U.S. Bureau of Labor Statistics, Consumer Price Index Historical Tables for U.S. City Average). This calculation results in an NPV of the twice-monthly transfers totaling $13,389 over the two year period, and an NPV of the quarterly transfers of $13,470. Thus, the NPVs of the two treatment arms are within 0.6% of each other.

3 Data and methods

3.1 Timeline

The baseline survey (referred to as the screening survey above) was conducted between February and March 2021. Table 2 reports the balance across the treatment and control groups on the baseline variables. The table shows balance at baseline between the control and treatment groups (columns 1 and 2) and between the two treatment arms (columns 3 and 4).[15]

The endline survey was conducted roughly 18 months after the start of transfers, between May and September 2022, and 1,074 households completed the survey. Surveys were conducted online or by phone. Each survey was limited to 60 minutes, and divided into two parts to minimize respondent fatigue. Great care was taken to ensure that participation in the study was independent of receipt of the cash transfers; specifically, participants were told explicitly that their participation in the survey was voluntary, and that their cash transfers were unaffected by their decision about whether to participate in the survey.

The response rate was 51%, with no evidence of differential attrition between treatment and control or between the two treatment arms (see Section 3.6). Table A7 shows that neither demographic characteristics nor household income predict treatment status in the survey sample. Table 1 shows that in this sample of 1,074 households used in the analysis, the average transfer was $450 monthly, including 28 non-compliers who did not receive the transfers although they were eligible ($487 excluding non-compliers).

After a preliminary analysis of our endline survey results, we conducted a short follow-up survey in Summer 2023 asking participants about their spending since March 2021 (the start of the cash transfers), the importance of various factors in determining household spending, and household composition. Similar to the endline survey, this follow-up survey was conducted online or by phone. 942 households completed the survey, resulting in a 45% response rate. Seventy-six percent of our endline survey respondents completed the follow-up survey. We did not find evidence of differential attrition. Because we do not find any effects on most outcomes that inform our main survey findings, we do not focus on these results in this paper. For completeness, the results are presented in Table A14. Attrition tables can be provided upon request.

[15] This table excludes the three treatment group members who we later realized had already received transfers during the pilot and were therefore excluded from further treatment and analysis. Table A6 reports baseline balance before this exclusion; the differences with Table 2 are negligible.

3.2 Sample

Table 2 shows that at baseline, 68% of the control group identified as Hispanic and 26% as Black or African-American, a ratio that reflects the demographics of Compton. No households identified as both Hispanic and African American, and a small group (6%) identified as neither. Most respondents are female (74%), with an average age of 35 and annual income of $26,308. The average number of children per household is 1.82.

The income eligibility threshold for the Compton Pledge was 220% of the federal poverty line (FPL). In the sample that was randomly assigned to receive cash transfers in Compton, 57% had income below the FPL, and 42% had income between 100% and 200% of the FPL. Just 1% were above 200%.

These patterns reflect Compton's history as a city challenged by poverty and unemployment. Compton differs in important ways from the Open Research Unconditional Income Study (ORUS), another large guaranteed income study that took place around the same period in Dallas and north central Texas and in Chicago and the surrounding counties (Vivalt et al., 2024; Bartik et al., 2024; Miller et al., 2024). In the ORUS study, recipients were relatively better-off than in Compton, more rural,[16] whiter[17], more likely to be young, more likely to be single [18] and they lived in areas with a lower cost-of-living.[19]

The ORUS study thus provides estimates for a sample that is more representative of the United States as a whole, while still focusing on low-income individuals. Our study provides estimates of impacts in a city and a population that is similar to many of the US locations where guaranteed income pilots are taking place (see Mayors for Guaranteed Income; https://www.mayorsforagi.org/).

[16]In the ORUS study, the overall eligibility cutoff was 300% of the FPL, and only 33% are below the FPL, while 24% have income greater than two times the FPL (Table 1 of Vivalt et al. 2024.)

[17]Just 1% of the Compton sample identifies as non-Hispanic white versus 54% of the ORUS sample. 13% of the ORUS sample live in a rural county and 18% in a suburban county; 53% of the ORUS sample lives in a big city (versus 100% in Compton/Los Angeles). Data for the ORUS sample is for the "enrolled active survey group," Table 1 of Vivalt et al. 2024.

[18]In the Compton sample, 30% of the respondents are younger than 30 (relative to 54% in the ORUS study, where the sample is restricted to recipients between 21 and 40). In the Compton treatment group, there were on average 1.66 other adults in the household apart from the respondent (relative to 0.68 in the ORUS treatment group), and 74% in Compton had children (relative to 57% in the ORUS sample). The average household size in Compton was 4.4 (treatment mean), relative to 3 in the ORUS enrolled active survey group. Both samples have similar rates of having bachelors degrees (17% in Compton and 20% in ORUS). (Vivalt et al. (2024), Table 1.)

[19]To compare costs of living, we use the local adjustments to the Bureau of the Census supplemental poverty measure thresholds, focusing on poverty lines for renters in two-parent, two-child families in 2022 (Shrider and Creamer, 2023). The threshold in the region that includes Compton (the Los Angeles-Long Beach-Anaheim, CA metro area) was $43,983. For comparison, the SPM thresholds in the ORUS study are: Illinois metro area = $30,680 (70% of the LA threshold); Illinois non-metro area = $28,921 (66% of the LA threshold); Dallas-Fort Worth-Arlington, TX metro area = $36,552 (83% of the LA threshold); Tyler, TX = $32,668 (74% of the LA threshold); Waco, TX = $32,056 (73% of the LA threshold).

3.3 Estimation

The main equation to assess the overall treatment effect of transfers across both frequency treatments arms is:

$$y_i = \beta_0 + \beta_1 T_i + \gamma' X_i^0 + \varepsilon_i \tag{1}$$

The equation to distinguish the treatment effects of the twice-monthly and quarterly transfers is:

$$y_i = \beta_0 + \beta_1 T_i^{\text{High Frequency}} + \beta_2 T_i^{\text{Low Frequency}} + \gamma' X_i^0 + \varepsilon_i \tag{2}$$

In both cases, y_i is an outcome of individual or household i, measured at endline, and T_i is an indicator for having been assigned to treatment. We use intent-to-treat, i.e. households which did not take up treatment are considered treated.[20] In equation 2, we separate the treatment into $T_i^{\text{High Frequency}}$ and $T_i^{\text{Low Frequency}}$ for twice-monthly and quarterly transfers, respectively. The coefficient β_1 in equation 1 captures the average effect of treatment across the high- and low-frequency arms, while β_1 and β_2 in equation 2 distinguishes between the treatment arms to analyze their separate effects relative to control, and relative to each other. X_i^0 is a vector of baseline controls that includes the number of people in the household, number of minors in the household, an indicator for the respondent being Hispanic, an indicator for the respondent being Black or African American, age, sex, and labor supply in hours and household income in January 2021.[21] To account for implementation concerns and the pandemic context, we include as further (non-pre-specified) control variables the amount received from the federal Biden Child Tax Credit program, an indicator for whether the respondent received any reminders and/or bonuses to complete the survey, an indicator for whether the respondent lived at the same address as another respondent, and an indicator for whether the household was re-randomized in February 2021.[22]

The error term is ε_i. Because treatment assignment is at the individual level, standard errors are not clustered.[23] Observations are weighted such that in each enrollment wave, the effective share of treatment observations is the same.

[20] Results using treatment-on-the-treated are very similar and therefore not shown separately; they are available upon request.

[21] We had pre-specified that we would control for baseline values of outcome variables only for the outcome variable of the regression in question, but since we only have baseline values for two outcomes, we include them as controls in all regressions.

[22] The randomization process was mostly implemented as designed. Following the pre-specified analysis plan, we weight the sample in order to have the same target proportion of treatment and control households after weighting. 253, out of 2,097, participants randomized in the first wave in February 5 2021 were erroneously re-randomized in the second wave in February 19 2021. We keep their final assignment as is in the analysis, and we account for the process with an indicator variable and adjust weights accordingly.

[23] There is more than one treatment recipient in 38 households (77 respondents), and an additional 18 households have multiple respondents (37 respondents in total) who differ in treatment status. We therefore also present a robustness check in Table A8 in which standard errors are clustered at the household level. The results are qualitatively and quantitatively very similar to our main results.

3.4 Heterogeneous treatment effects

We preregistered four dimensions of interest for heterogeneous treatment effects. Below, we show results for the two elements of heterogeneity that policymakers are most likely to consider when developing targeting criteria: the sex and income of the transfer recipient. In the appendix, we provide results by race and ethnicity, and by whether households received benefits as part of the American Rescue Plan (Child Tax Credit). In exploratory analyses, we show the impact on single mothers versus other households.

The econometric specification for heterogeneous treatment effects is:

$$y_i = \beta_0 + \beta_1 T_i \times \text{Group } 1_i + \beta_2 T_i \times \text{Group } 2_i + \beta_3 \text{Group } 1_i + \gamma' X_i^0 + \varepsilon_i \quad (3)$$

Here, the labels "Group 1" and "Group 2" indicate dimensions of heterogeneity that partition the sample, such as female vs. male. The group-specific results are averages across the high- and low-frequency treatment groups within each group.[24]

3.5 Multiple comparisons

To adjust for multiple comparisons, we define an index or focal variable for each of our primary and secondary outcomes. We then apply false discovery rate (FDR) correction across these summary variables, separately for primary and secondary outcomes (Anderson, 2008).[25] The correction is applied across outcomes, but not across the frequency treatment arms. We do not adjust for multiple comparisons within outcome families.

3.6 Attrition

To limit attrition, participants received a \$50 payment for completing each survey. In addition, we used various data collection techniques to encourage participation, especially among our control group participants. These included calling participants, intensive follow-ups, and additional bonus payments for timely survey completion.

Table A2 tabulates survey participation across all groups, and indicates the reasons for and points of attrition. Our overall response rate was 51% (1074/2097), and was similar in the treatment and control groups (treatment: $(186 + 188)/(345 + 350) = 54\%$, control:

[24] In the pre-analysis plan we describe a statistically-equivalent estimating equation. We use this form here to help exposition.

[25] We exclude the secondary outcomes based on list randomization from FDR correction for econometric simplicity.

700/1402 = 50%) and in the two treatment arms (186/345 = 54% in the twice-monthly arm and 188/350 = 54% in the quarterly arm).

We use three approaches to formally assess the severity of attrition. First, we test whether attrition is correlated with treatment by regressing attrition status on the treatment indicators. We find no statistically significant differential attrition for either the treatment group as a whole relative to the control group or for the two treatment arms relative to each other and to control (Table A3). Second, we test whether attriters differ from non-attriters by asking whether attrition status can be predicted from baseline outcomes and stratification variables. Table A4 shows that African Americans, women, young respondents, and high-income respondents were more likely to complete the endline than others. However, when we test whether attriters in the control group differ significantly from attriters in the treatment group along these and other demographic variables, we find no significant differences (Table A5). Thus, differential attrition is unlikely to have biased our results.

3.7 Outcome variables

We conducted an endline survey 18 months after the start of the cash transfers. We pre-specified seven primary outcomes:

1. Labor supply in hours: measured as weekly hours the respondent worked in formal, informal, and self-employment jobs.
2. Household income: the sum of labor market earnings of the respondent; dividends, interest and rental income; income from other household members; benefits including unemployment insurance, SSI, CalWorks, and CalFresh/SNAP/WIC; and income from any other sources (excluding transfers from relatives).
3. Per capita expenditure: We capture household expenditure in the preceding 30 days on the following categories: food and drinks at home and outside the home; alcohol; cigarettes and tobacco; apparel; housing (rent, mortgage payments, utilities, internet and phone bills); health care; child and elder care; vehicles; and transportation.
4. Movable assets and savings net of debts: The value of movable assets held by household members includes cash, retirement account balances, the value of businesses, gifts from and loans to relatives, as well as the total value of durable goods across a variety of items commonly owned by households.[26] The value of debt held by household members

[26] We used the Consumer Expenditure Survey to assign values to the following categories: washing machine, clothes dryer, dishwasher, microwave oven, vacuum cleaner, home entertainment system with television and audio, gaming console, gym equipment, air conditioner, valuable jewelry or watches, musical instruments, power tools, computer or tablet, mobile phone, car or truck, motorcycle, bicycles.

includes amounts owed on student loans, credit cards, medical debt, gifts to relatives, loans from relatives, and any other debt (e.g. vehicle loans, legal bills, etc.).[27]

5. Psychological well-being index: This index is constructed using principal component analysis (PCA, PC1) from the following survey items: depression frequency (Likert scale from 1 [none of the time] to 5 [all of the time]), stress frequency (1–5), life satisfaction (1 [dissatisfied]–10 [satisfied]), happiness (1 [very happy]–4 [not at all happy]), and the Kessler 6 questionnaire to measure psychological distress(Kessler et al., 1997).

6. Financial security index: We create a standardized index of six dummy variables to capture financial security:[28] Whether the household could pay for a $400 emergency bill with current resources without going into debt; paid all bills in the past 30 days; put money aside for the future in the past 30 days; paid down debt in the past 30 days; had to ever forgo medical care over the past six months because of the expense; and whether the respondent has health insurance.

7. Food security index: A standardized index of two binary items: whether anyone in the household had to eat less than they felt they should in the past 30 days; and whether the household had to eat a lower quality diet because of cost in the past 30 days.[29]

We also pre-specified seven secondary outcomes: a dummy for participation in unpaid work; alcohol and tobacco expenditure in direct reports (two variables) and a list experiment (two variables); and indices of intimate partner violence (IPV) and housing security. The IPV index combines two variables: an indicator variable for whether the respondent reports being physically abused by their partner in the past six months and an indicator for whether they report being sexually abused by their partner in the last six months. We separately show results from a list experiment for physical abuse. In the pre-analysis plan, the IPV index includes all three variables, but we instead show the self-reported results separately from the result from the list experiment (for details see section 4.5). The housing security index combines three self-reported survey responses: whether the household is able to pay their rent or mortgage (binary), their likelihood of eviction (four-point Likert scale), and the

[27] Our pre-analysis plan defined the asset index variable as containing movable assets only, and our analysis follows this definition. We also collected data on housing assets and debt (the value of the respondents' home if owned and the total dollar amount owed on mortgages and all other housing loans by household members), but these variables are analyzed separately in Table A13. The pre-analysis plan includes a variable for "other real estate in the U.S. or another country," but we received very few responses (2%) to this question, and only 60% of those included dollar values. We omit this data from the analysis. The pre-analysis plan also mentions that we will analyze the *net change* in the value of durable goods, but for the index variable we use the total value of durables.

[28] In the pre-analysis plan, the financial security index is labeled as the "financial precarity" index but the content is identical. All indices except for the psychological well-being index are constructed by averaging the standardized component variables; then winsorizing the average at the 95th percentile; and finally standardizing. Standardization is with respect to the control group mean and standard deviation at endline.

[29] In the pre-analysis plan, the food security index is labeled as the "food insecurity" index but the content is identical.

number of months behind on rent or mortgage payments.[30]

In addition to these primary and secondary outcomes, we also measure a number of exploratory outcomes: labor force participation (dummy), participation in unpaid work (dummy),[31] whether the respondent has been looking for work (dummy), and whether the respondent would like to be working more than they were (dummy). Respondent satisfaction with their employment situation is measured on a five-point Likert scale. We measured time spent on unpaid child or eldercare (daily hours over the past 7 days), time spent asleep (daily hours over the past 7 days), and whether any household members had caught COVID-19 or had died from COVID-19 since March 2020 (dummy). We also ask respondents to self-assess their physical health on a Likert scale (1-5). In addition, we measure political engagement based on two questions: 1) Trust in government officials (Likert scale 1-4) and 2) Whether they voted in the 2021 mayoral election (dummy).

We address missing information in outcome variables as follows. For income variables, we impute the mean by treatment status; for labor supply in hours we impute by treatment status and by gender. For expenditure and asset variables, we impute zero. For binary variables, we impute the "positive" outcome. For example, for the question "During the last 30 days, did you or anyone in your household ever eat less than you felt you should because there wasn't enough money for food?", we impute zero when missing. If all components of an index variable are missing, we do not impute a value and leave it as missing.

4 Results

4.1 Overview of results

We begin with a brief overview of the impacts of the transfers on our primary and secondary outcomes. Among the seven primary outcomes—labor supply in hours; household income; per capita expenditure; assets net of debts; and indices of psychological well-being, financial security, and food insecurity)—we observe significant negative treatment effects on income (−\$333) and expenditure (−\$302). Both the expenditure effect and the income effect survive FDR correction. Among the seven secondary outcomes—a dummy for participation in unpaid work; alcohol and tobacco expenditure and consumption in direct reports and a list experiment; and indices of intimate partner violence and housing security—we observe

[30] The housing security index slightly deviates from the pre-analysis plan: first, to keep the survey short, we replaced the two variables measuring housing security in this index with a single question on the likelihood of eviction (four-point Likert scale). Second, we replaced the variable measuring "number of months behind on rent" with "number of months behind on rent or mortgage payments". Both changes were made before the survey was fielded.

[31] The dummy is one if the respondent does not participate in paid work in order to have time to take care of their house or family, or if they spend a positive amount of time on unpaid child or eldercare, regardless of their employment status; otherwise zero.

a significant decrease in tobacco expenditure in direct reports, but an increase in tobacco use in the list experiment; a significant improvement in the housing security index; and no significant effects on the remaining variables.

We were well-powered to detect moderate effects on our primary and secondary outcomes: among the non-significant primary outcomes, we would have rejected the null hypothesis of no treatment effect for changes in labor supply of more than 2.7 hours (7% of the control group mean); of more than 0.14 SD in the indices of psychological well-being, financial security, and food insecurity; and of more than $4,426 (26%) in the value of assets. Among the non-significant secondary outcomes, we would have rejected the null hypothesis for participation in unpaid work of more than 8 percentage points; of more than $4.11 (24%) of spending on alcohol in direct elicitation; and of changes larger than 0.14 SD in the intimate partner violence and housing security indices. The list experiment is necessarily somewhat noisier and allows us to reject changes in the extensive margin of alcohol consumption of more than 16 percentage points.

We find no statistically significant differences in the primary or secondary outcomes when comparing the twice-monthly and quarterly treatment arms, with the sole exception of the food insecurity index, which shows marginally better outcomes for the twice-monthly than the quarterly treatment group. Among the non-significant primary outcomes, we would have rejected the null hypothesis of no differences between twice-monthly and quarterly transfers for effects larger than the following: labor market participation, 10 percentage points (13% of the control group mean); weekly labor supply, 4.4 hours (17%); income, $371 (11%); expenditure, $283 (10%); assets, $4,466 (16%); debt, $4,757 (25%); net assets (assets minus debt), $6,819 (40%); and the indices of psychological well-being and financial security of 0.24 and 0.22 SD, respectively. Among the secondary outcomes, the corresponding thresholds are: participation in unpaid work, 10 percentage points (15% of the control group mean); IPV index, 0.25 SD; housing security index, 0.22 SD; alcohol expenditure in the last 30 days, $6.06 (35%); cigarettes/tobacco expenditure in the last 30 days, $2.20 (44%); and indicators for alcohol consumption and smoking from the list experiment, 25 and 27 percentage points (425 and 457%), respectively.

4.2 Labor supply and income

4.2.1 Simple differences

Table 3 presents means of labor supply and income in January 2021, just before the cash transfers started, and at the endline survey roughly 18 months later. The first row shows that labor supply increased over the study period for both the treatment and control groups, as businesses re-opened with the availability of COVID-19 vaccines in early 2021. Specifically, in January 2021, 58 percent of the respondents in the control group were working, increasing to 74 percent by mid-2022. In line with rising labor force participation, row 4 shows that total household income (in the 30 days before the survey) grew by $775 for the control group, from $2,566 to $3,341 over the same period (income here excludes stimulus checks received from the government in January 2021). Average earnings for control group respondents grew by $535 from $1,441 to $1,976. Public benefits are a smaller share of income and increased as well.

The impacts of the Compton Pledge are measured relative to these changes. Like the control group respondents, respondents in the treatment group increased their labor supply and saw rising average income, but the relative increases were smaller. Respondents in the treatment group increased labor force participation by 6.3 percentage points, which is 8.8 percentage points less than the control group increase. Treatment group household income in the past 30 days increased by $458, which is $317 less than the control group increase. The earnings of respondents in the treatment group increased by $301, which is $234 less than the control group increase. If the average monthly transfer size ($450, Table 1) is added to treatment group household income, the net gain relative to the control group is +$133—which is substantial, but less than a third of the size of the average transfer.

4.2.2 Average treatment effects

In the following we describe the treatment effects estimated with our main specifications. Columns (1) and (2) of Table 4 report the means and standard deviations for the control and treatment groups, respectively. Column (3) is the intent-to-treat effect, averaging across treatment arms, estimated according to equation 1. Columns (4) and (5) are the intent-to-treat effects for each treatment arm following equation 2, and column (6) is the *p*-value on the difference between the coefficients in columns (4) and (5), testing the statistical difference between receiving cash transfers every other week (twice-monthly) versus quarterly.

The first row shows that the overall treatment effect on participating in the labor force is -0.05 ($SE = 0.03$), a 5 percentage point reduction that implies a participation elasticity of -0.47.[32] While not statistically significant, the negative coefficient is relatively large compared to the control mean of 73%, contrasting with the global evidence, which finds limited effects on labor force participation (e.g., Banerjee et al. 2017). The negative treatment effect aligns with the economic intuition that time allocated to leisure and caring for children and others can be seen as normal goods (and, unlike the impact of a wage increase, cash transfers have no substitution effect that offsets the income effect).

We expected that the impact on labor supply would be more negative for the group receiving cash transfers steadily every other week, since receiving a twice-monthly flow of cash is closer to being an income substitute. The quarterly transfers, in contrast, are lumpy and harder to use to cover weekly expenses. In columns (4) and (5) of row 1, however, we find no

[32]Our estimation of labor force participation does not impute any values, even when the respondent's reported earnings is positive. We also estimated this outcome with imputation for those recipients and find qualitatively similar results, on average, and for all predefined subgroups.

significant difference in the overall treatment effects for respondents in the two treatment arms ($p = 0.63$), and the coefficient on quarterly recipients is larger (-0.06 versus -0.03), although not statistically different from zero.

Part-time workers generally have greater flexibility to adjust how they participate in the labor market, and the second and third rows divide the sample into workers who in January 2021 were working 20 hours per week or more ("full-time", row 2) and those working less than 20 hours ("part-time", row 3). Here, results show heterogeneity that is obscured in the average labor supply impact shown in row 1. The labor supply response is very small for "full-time" workers (-0.02, SE$= 0.03$), with small, noisy estimates in the two treatment arms. But "part-time" workers respond sharply: their treatment effect on labor supply is -0.13 ($p < 0.05$) relative to a control mean of 0.54, implying a participation elasticity of -1.33. Consistent with expectations, the negative treatment effect is qualitatively larger for recipients of the steady, twice-monthly transfers (-0.16, $p < 0.05$) than for those of quarterly transfers (-0.09, $p > 0.1$), although the difference by treatment arms is not statistically significant ($p = 0.40$).[33]

We pre-registered weekly labor supply in hours as a primary outcome; the results are shown in rows 4 and 5. Overall, the treatment effect on hours of work is insignificant and small, implying an elasticity of $+0.01$. Conditional on participating in the labor market at endline, the treatment group works 1.97 additional hours per week ($SE = 1.38$) relative to the control group, but again, this effect is not significant. Given a greater tendency for part-time workers to stay out of the labor market (as seen in row 3), the positive impact shown in row 5 is consistent with a shift in the composition of workers toward full-time workers (rather than simply being a direct impact on the intensive margin of hours). We generally find that more negative impacts on labor market participation align with larger, positive impacts on hours conditional on working, consistent with compositional shifts.

The negative impacts on labor market participation translate into negative impacts on household income. While the average monthly cash transfer amount for the treatment group is \$487, row 7 of Table 4 shows that the net impact on total monthly household income over the past 30 days including the cash transfer was just \$92 and not significantly different from zero ($SE = 126$). Households thus, on average, adjusted earnings in response to the cash transfers. Excluding the cash transfers, total monthly household income is \$333 ($p < 0.01$) lower in the treatment than in the control group. Again, given the broad increases in labor supply and income during the period, this represents a smaller relative increase in income. Consistent with expectations, the negative treatment effect is qualitatively larger for the twice-monthly treatment arm ($-\$375$, $p < 0.05$) than the quarterly treatment arm ($-\$287$, $p < 0.05$), although the difference between the two is not significant ($p = 0.64$).

The remaining rows break down the impacts on total household income. Comparing the

[33]In contrast, Jones and Marinescu (2022) find that receiving a yearly cash dividend from the Alaska Permanent Fund increased part-time work, which they connect to improvements in the local economy brought by the broad influx of money.

estimates in these rows shows that 49% of the $-\$333$ treatment effect on income (excluding the transfers) is due to a negative impact on the earnings of the recipients themselves ($-\$162$, $p < 0.01$), and another 39% reflects a reduction in the income of other household members ($-\$130$, $SE = 88$).

A non-significant reduction in benefit income accounts for most of the remaining effect. The estimated combined impact on income from public benefits, including unemployment, SSI, TANF, food and rental assistance, is $-\$39$ (not statistically significant). This negative effect suggests that the treatment group could be reducing efforts to obtain public benefits while receiving cash transfers from the Compton Pledge. The negative impact is twice as large for the twice-monthly group ($-\$51$) relative to the quarterly group ($-\$26$), which is consistent again with twice-monthly flows being a closer income substitute, though the effects are not significant.

The last rows of Table 4 show a negative treatment effect for quarterly transfers on investment income (rental income, dividends or interest), and positive treatment effects on other income, but these are minor sources of income.

4.3 Spending

While we estimate a \$92 increase in total household income in the past 30 days when the transfer is included, the treatment effect on total expenditures is negative: the top row of Table 5 shows a $-\$302$ impact on total household expenditure over the last 30 days ($p < 0.01$). Consistent with the twice-monthly treatment arm facilitating steady spending, the impact on the group receiving steadier transfers is smaller ($-\$229$, $p < 0.05$) than the impact on the group receiving lumpier transfers ($-\$383$, p ¡ 0.01), but the estimates by treatment arm are not significantly different from each other ($p = 0.30$).[34]

Table 5 shows impacts on expenditure broken down by categories. The overall negative impact of $-\$302$ reported in Table 4 reflects a $-\$160$ impact on non-housing spending (panel A, $p < 0.01$), and a $-\$142$ impact on housing spending (panel B, $p < 0.05$). These negative impacts are distributed broadly across the spending categories.

Despite the overall negative impacts on expenditure, the effect on food and beverages consumed at home is relatively small and not significant. The group receiving twice-monthly cash transfers maintains spending levels (the estimated impact is $-\$6$ relative to a control group mean of \$454), while there is a larger negative impact on the group receiving transfers quarterly ($-\$52$, $p < 0.05$). This aligns again with the hypothesis that the twice-monthly transfers are a closer income substitute. The results for clothing, footwear, and other apparel show a similar pattern.

[34] Similar to labor force participation and income, spending could have increased over the study period, but at a slower rate for the treatment group than the control group 4.2.1. However, we did not measure baseline spending and so cannot be sure.

The negative treatment effects on elderly and child care spending, as well as transport expenses (and, qualitatively, vehicle expenses), are consistent with less time spent working outside the home by transfer recipient households. This could reflect a switch to remote work by some household members that reduces their need to commute or pay for elder or child care, though that interpretation is inconsistent with their lower spending on internet and phone bills, and utilities, relative to the control group. It could also reflect less paid work by some household members, which would be consistent with the negative treatment effects on income and labor supply described above.

4.4 Assets and debt

The negative impacts on spending suggest that households are using the cash transfers to save or to reduce debt. In line with this hypothesis, row 1 of Table 6 shows a large positive treatment effect of \$2,498 on net assets, although this increase is not statistically significant ($SE = 2,258$). The effect is driven mainly by households in the bimonthly treatment arm, who experience an increase in net asset holdings of \$5,079 (not significant), whereas households in the quarterly treatment arm report a $-\$337$ reduction, although again neither the coefficients nor their difference is statistically significant.

Separating assets and debts, the impact on non-housing assets is a non-significant increase of \$308 on average ($SE = 1817$). This effect reflects a significant negative impact on cash, bank balances, and non-retirement investments ($-\$339$, $p < 0.05$), particularly for the twice-monthly group ($-\$469$, $p < 0.05$). The twice-monthly treatment also has a negative impact on retirement accounts ($-\$705$, $p < 0.05$), while the treatment effect is positive (though not significant) for the quarterly transfer group (difference $p = 0.05$). The twice-monthly recipients also report an increase in the value of durables ($+\$4,273$, $p < 0.10$), driven almost entirely by an increase in the value of cars and trucks owned ($+\$4,284$, $p < 0.10$). The quarterly recipients do not show the same effect; in fact, the value of their durable goods declines by $\$3,049$ (not significant relative to control, but significantly different from the twice-monthly group ($p = 0.01$)), again mostly driven by a decrease in the value of cars and trucks ($-\$3,087$, not significant relative to control, but significantly different from the twice-monthly group ($p = 0.01$)). Thus, it appears that the bimonthly recipients shift assets from cash and retirement accounts into car ownership.

Panel B shows that the negative treatment effect on overall debt is large—negative \$2,190 or around ten percent—but not statistically significant ($SE = 1,595$). Here, the largest contributor is a negative impact on "other debt" of $-\$1,103$ (also not statistically significant). This category includes payday loans, auto loans, and other informal sources of debt. Student loans and credit card debt are the two other major components of debt, and both have negative although insignificant treatment effects. We observe some differences across the two treatment arms: the twice-monthly transfer group shows a reduction in credit card debt of $-\$1,074$ ($p < 0.05$), with no decrease for the quarterly arm (difference $p = 0.06$). This is consistent with frequent cash transfer payments providing an advantage with respect to paying monthly credit card bills, or being able to meet regular expenses without needing to borrow with credit cards in the first place. Interestingly, the increased car ownership in the twice-monthly recipient group does not appear to be driven by an increased ability in this group to repay auto loans with their more frequent payments; this would predict an increase in "other debt" in this group, but if anything we observe the opposite (−\$877, not significant).

In contrast, the quarterly transfer group experiences a large qualitative reduction in student loan debt of −\$1226, although not statistically significant, while we observe no decrease in the twice-monthly arm. Although insignificant, this result is consistent with the larger infrequent cash transfers providing an opportunity to repay substantial lumpy loan balances.

We consider housing separately from other assets and debts because home values and mortgage balances tend to be very large relative to other components of household balance sheets and could potentially mask other effects. When we estimate models with home equity, home value, or mortgages as the outcome variable, we consistently find insignificant coefficients.[35] Models with home ownership as a binary outcome variable show a lower home ownership rate among treatment households (-0.06, $p < 0.05$). However, it is possible that this reflects baseline imbalance in home ownership rates; indeed, only 76 households (7 percent of the sample) report moving between the baseline and endline survey, which is consistent with the small magnitude of the transfers relative to local home values, and the short time horizon of the study.

4.5 Broader well-being and temptation goods

The top panel of Table 7 shows the impact of cash transfers on five indices of well-being. The first three rows report on summary indices measuring psychological well-being, financial security, and food security (pre-registered as primary outcomes). The average treatment effects on each of the three indices are relatively small and not statistically significant. We find a statistically significant ($p = 0.09$) difference between the treatment arms for the food security index, with an improvement in food security for the twice-monthly transfer group and a negative impact for the quarterly group, although these individual treatment effects are not significantly different from the control group. The difference between the treatment arms aligns with the hypothesis that more frequent resource flows help maintain steady consumption, while lump sums facilitate other financial strategies, like debt reduction.

Row 4 shows a large increase in perceived housing security (a pre-registered secondary outcome) among treated households relative to controls (0.29 SD, $p < 0.01$). The effect

[35] In models using the sample of home owners with home equity as the outcome variable, we find a positive but insignificant coefficient on treatment. Breaking this down further into home value and mortgage debt reveals a positive effect for home value and a negative effect for mortgages, both insignificant. See Table A13 in the appendix. These results are similar when we use home value estimates from Zillow in place of self-reported home values.

is similar in magnitude in both treatment arms. It is driven by a decrease in the perceived likelihood of eviction, among both homeowners (-0.37 SD, $p < 0.01$) and renters (-0.42 SD, $p < 0.01$) (Table A9).

The fifth row shows little impact on an index of intimate partner violence when questions about experience with IPV are asked directly. Anticipating that respondents may be reluctant to report their experiences, we also conducted a list experiment.[36] The bottom panel of Table 7 shows that, when IPV is measured using list randomization, receiving the cash transfers led to a reduction in the probability of experiencing IPV by 20 percentage points ($p < 0.01$). One explanation for this inconsistency is that respondents find it difficult to make direct reports of IPV; indeed, the average control group prevalence of IPV is 7% in direct reports, and 10% in list randomization.

Similar issues of elicitation arise with expenditure on temptation goods (alcohol, cigarettes and tobacco). Again we conducted a list experiment to get around these demand effects.[37]

For alcohol expenditure and consumption, the point estimates are negative in both the direct reports and the list experiment, but they are not statistically significant across the whole sample. We find a 0.18 SD reduction of binge drinking in the twice-monthly group elicited with list randomization, significant at the 10% level.

Although we also find a $1.95 reduction in spending on cigarettes and tobacco in direct elicitation across the whole sample ($p < 0.05$), we find no impact on the extensive margin in direct elicitation, i.e. the share of people who spend any money on cigarettes or tobacco (result not shown). Moreover, in the list experiment, we observe a large, marginally significant increase of 15 percentage points ($p < 0.1$) on the extensive margin—a departure from findings of negative impacts of cash transfers on temptation goods in low-income countries described by Evans and Popova (2017).

In sum, the cash transfers have a strong positive impact on the index of housing security, but no clear impact on the indices of psychological well-being, financial security, or food security. For food security, the evidence by treatment arms is consistent with the expectation that income received as steady flows is most helpful (Aguila et al., 2017). The list experiments show strong evidence of relative reductions in IPV, weak evidence of reduced alcohol consumption, and moderately strong evidence of relative increases in tobacco consumption.

[36] In the list experiment, half of the treatment and control groups were presented with a list of three common activities such as calling or texting friends or taking a vacation, and asked how many of these activities they experienced in the past 6 months. The other half was additionally asked if they experienced physical violence by their partner. The difference in the means between the "short list" and "long list" groups is an estimate of the proportion of respondents experiencing physical violence.

[37] In the list randomization questionnaire for temptation goods, half of the treatment and control groups were presented with a "short" list of three common activities. The other half was additionally presented with an extra item—consuming more than five alcoholic drinks on one occasion (binge drinking), or consuming any tobacco in the past 30 days.

4.6 Heterogeneous treatment effects

The impacts estimated above pool treatment effects across all participants. In Table 8, we show how treatment effects vary with the sex of the person receiving the cash transfers. Previous studies have shown different impacts for female versus male recipients of cash transfers (De Mel et al., 2012), and some programs in the United States, such as Magnolia Mothers of Jackson, Mississippi, only target women. In the Compton sample, 78% of respondents identify as female (835 households).[38]

In columns (1) and (2) of Table 8, we reproduce the results for the pooled sample described in Section 4.2. Following equation 3, column (4) reports the impact on female recipients, and column (5) on male recipients. Column (6) reports the p-value on the difference between these coefficients. The first row provides estimates of labor force participation. We find a qualitatively larger reduction in labor force participation for females (-6 percentage points) than for males ($+1$ percentage point), but neither the individual coefficients nor the difference is statistically significant.[39]

The second row shows weekly hours worked. There is no statistical difference between males and females, with a small negative treatment effect for females, and a small positive treatment effect for males. The third row gives treatment effects on weekly labor supply in hours conditional on working. Column (4) shows that working female recipients increase their hours by 2.73 on average ($p < 0.1$). In contrast, the treatment effect for men is small (-0.1 hours) and not statistically significant, consistent with the lack of impact on male labor force participation.

In the fourth and fifth rows, we report impacts on total income in the past 30 days, with and without the cash transfer, respectively. With the transfer, female recipients experience an income increase of \$212 relative to the control group (not significant), while male recipients experience a decrease of \$308 (not significant; difference in coefficients: $p = 0.09$), suggesting less income displacement by the transfer among women than men. Indeed, without the cash transfer, men experience a reduction in income of \$675, statistically significant at the 5% level. There is still income displacement even among women, however, who report a \$231 reduction in income without the transfer, significant at the 10% level. Data on household expenditures over the past 30 days follow the pattern of income. Row 6 shows that the negative treatment effect on expenditure found in Section 4.2 (reproduced in column (2) as $-\$302$, $p < 0.01$) is an average of very different treatment effects by sex. The average treatment effect for male recipients in column (5) is $-\$683$ ($p < 0.01$). For female recipients, in contrast, the average treatment effect is $-\$189$, less than 30 percent of the magnitude of

[38] We control for family structure in the regressions by including a dummy for female respondents, and variables for the number of household members and the number of minors. Female recipients are more likely to have minor children (80%) relative to male recipients (61%).

[39] Eissa and Hoynes (2004) find the same direction of effects among married couples to the 1993 EITC expansion: a modest overall negative labor force participation response that consisted of a small positive response among married men and a larger-in-magnitude negative response among married women.

the treatment effect on males.

As above, one explanation for these reductions in expenditure is that households are reducing debt.

Rows 7, 8 and 9 in Table 8 turn to non-housing assets and debt. Male recipients experience sizable, but insignificant negative treatment effect on assets ($-\$1,361$), counterbalanced by an even larger although also non-significant reduction in debt ($-\$4,569$), which together result in an overall increase in net assets of $3,207 (not significant). Women report a smaller and non-significant increase in assets ($808), and a non-significant reduction in debt ($-\$1,478$), resulting in a net increase in assets that is about a third smaller compared to that of male recipients (+$2,286, not significant). None of the differences between male and female recipients are significant.[40]

Perhaps as a result of their smaller reductions in income and expenditure, female recipients report greater financial security relative to control ($0.14SD$, $p < 0.10$), while male recipients report lower values relative to control (-0.31 SD, $p < 0.05$); the difference is significant at the 1% level. Both male and female recipients show large and similarly sized increases in the housing security index ($0.27SD$ and $0.34SD$, respectively, not significantly different). Impacts on the indices of psychological well-being and food security are mostly positive but non-significant.

Our list randomization results suggest that intimate partner violence is significantly reduced in households where the man received the transfer, with a 36 percentage point reduction in the likelihood of any episodes ($p < 0.05$), and to a lesser extent in households where the woman received the transfer (16 percentage points, not significant; difference not significant). This result is also reflected in direct reports of intimate partner violence (difference $p < 0.10$).

Overall, the pattern that emerges from the heterogeneity analysis by sex is that some female recipients responded to the transfers by working additional hours and thereby improving their income, while others may have prioritized time with family and therefore dropped out of the labor force (reflected in the non-significant but qualitatively large reduction in labor force participation among female recipients). A prediction that arises from this pattern of results is that the former effect might be more pronounced among women who *have* to work — notably, single mothers — while the latter effect might dominate among women in other types of households, with more latitude to adjust.

In exploratory analyses that were not pre-specified, we therefore analyze impacts on households headed by single mothers, who comprise 22% of the sample (see Table 9).[41] Indeed, we find that this group does not reduce labor force participation at all; instead, they strongly increase their working hours (by 9.57 hours per week, $p < 0.01$, a 30% increase),

[40]While the larger negative treatment effect on expenditures experienced by male recipients might be expected to lead to a larger positive impact on net assets, this is not mechanical because the first is a flow measure and the second is a stock.

[41]As shown in Table A15, 29% of females are single parents, compared with 8% of males.

leading to a large and significant increase in monthly income of \$831 including the cash transfer. This effect is larger than the cash transfer itself, implying that for this group, the cash transfer crowded in additional income, rather than displacing it.[42] (Appendix Table A10 shows that impacts are substantially larger for the quarterly group compared with the twice-monthly group.) Single-mother recipients also show limited reductions in expenditure ($-\$43$, not significant), in contrast to the rest of the sample and no increase in net assets ($-\$2,390$, not significant). Surprisingly, the impact on the food security of households with single mothers is negative, although, consistent with the global literature, that impact is smaller in absolute value for respondents receiving twice-monthly transfers (Appendix Table A10).

In Table 10, we explore heterogeneity by income, where income is measured at the original screening survey (before the baseline). We follow the pre-analysis plan and split the sample in two at the median income of \$3,700 per month, with 534 households in each group[43]. It is *a priori* unclear which group is most likely to experience larger impacts: Poorer individuals may benefit more because the transfers are larger relative to their income, and may be more likely to relax binding credit constraints (Parker et al., 2013). At the same time, economic interventions have repeatedly produced larger effects among wealthier individuals (Haushofer and Shapiro, 2016; De Mel et al., 2012), which would predict the opposite.

Overall, the effects are similar across the two groups, perhaps because the above-median group nonetheless still earns less than 220% of the poverty line according to the eligibility criteria. For instance, the negative impacts on income (excluding transfers) seen in Table 3 are present in both groups: In the better-off part of the sample, the treatment effect on total income in the last 30 days (excluding transfers) is $-\$398$ ($p < 0.05$), and in the below-median group $-\$469$ ($p < 0.01$); the estimates are not statistically different. Expenditure shows the same pattern, again with a qualitatively larger reduction for the below-median group.

The positive impact on housing security is large and statistically significant for both groups. The reduction in the list randomization IPV measure is similarly large and negative for both groups, and marginally statistically significant ($p < 0.10$) for both. The picture is thus different from the heterogeneity by sex analyses, where qualitatively different results emerge.

Following the pre-analysis plan, we also analyzed heterogeneity by race, and by whether or not the households received the Child Tax Credit (CTC) provided during the pandemic. Table A11 shows that the program had a large negative impact on the (non-housing) debt of Non-Black participants (66% of the sample identifies as non-Black Hispanic and 4% as non-Black non-Hsipanic), with a $-\$3,790$ impact (SE = \$1,866; $p = 0.08$ on difference with Black sub-sample). Similarly, the impact on our financial security index is significantly more positive for the non-Black sub-sample ($p = 0.04$). The non-Black sub-sample had a

[42] These positive effects are consistent with research that finds substantial increases in labor supply for eligible single mothers as a result of EITC expansions in the 1980s and 1990s (Eissa and Leibman, 1996; Meyer and Rosenbaum, 2001).

[43] 5 households are omitted as they did not responded to questions regarding income and hence their baseline income can not be imputed.

significant 0.36 SD improvement in housing security ($SE = 0.06$), although the difference with the Black sub-sample is not significant ($p = 0.21$).

Table A12 shows the interaction of treatment with an indicator for receiving funds from the Biden administration's expanded Child Tax Credit, an important form of pandemic-period relief that overlapped with the timing of the Compton Pledge and applied to 48% of the sample with children. (All the specifications in the study also include an indicator for receiving the CTC within the control variables.) Table A12 is restricted to households with children. The main results are similar to the key findings above. Households that did not receive the CTC experienced a significant reduction in non-housing debt ($-\$6,196$; $p = 0.16$ on the difference between subsamples) and increases in net assets ($\$8,539$; $p = 0.07$ on the difference between subsamples) compared to CTC recipients.

5 Conclusion

Unconditional cash transfers have been widely studied in low-income countries, but their potential to improve recipient welfare in high-income countries is less well understood. This study contributes to an emerging literature on the impact of such programs in the United States (Jaroszewicz et al., 2022; Jacob et al., 2022; Pilkauskas et al., 2022; West and Castro, 2023; Vivalt et al., 2024; Miller et al., 2024; Bartik et al., 2024; Liebman et al., 2022; Sauval et al., 2024; Troller-Renfree et al., 2022; Gennetian et al., 2024).

We find an increase in average household income among recipient relative to control households, but this increase masks a displacement of earned income through the cash transfers, suggesting substitution of earned with unearned income. At the same time, households also reduced their consumption expenditures, suggesting that they used the transfers for non-consumption goals. Consistent with this, we observe a significant decrease in credit card debt in the group receiving transfers twice per month. The twice-monthly transfers may be especially well-suited to this goal because the timing of the transfers can be lined up with credit card billing cycles. Debt reduction may also underlie the strong increase in perceived housing security among transfer recipients.

Households receiving transfers twice monthly also see improved food security relative to those receiving transfers once quarterly (although neither effect is different from zero). This result echoes evidence from low-income countries which finds that more frequent transfers are more likely to be spent on everyday expenses such as food (Haushofer and Shapiro, 2016). Overall, however, we observe few differential impacts as a function of transfer frequency. The overall lack of large differences between the treatment arms may reflect that households were focused on recovering from the negative shock of the pandemic. In economically quieter times, one might find that the quarterly transfers would be more likely to facilitate the purchase of durables and large assets (Haushofer and Shapiro, 2016).

We find substantial heterogeneity in the impact of the transfers: Male recipients experience a stronger reduction in income and expenditure than female recipients, and they report lower financial security. In exploratory analyses, we find that single mothers experience an overall increase in income which is larger than the size of the cash transfers. They also see no notable reduction in spending and no reductions in debt.

The heterogeneous effects suggest that one advantage of cash transfers is that households may pursue different strategies to recover from the shock of the pandemic, with some focusing on debt reduction, and others on the recovery of pre-pandemic consumption levels or spending time with family (e.g. female labor market participation declined by 6 percentage points, and we observe a reduction in expenditures on elderly care, childcare, and transportation). More broadly, the indication of decreases in household debt (although not statistically significant) suggests that a focus on consumption as the bottom-line measure for the welfare impact of cash transfers may be overly narrow.

The results come with some important caveats. First, the response rate of just over 50% is low, even though it is not differential across treatment and control groups. Second, our data capture a snapshot 18 months after the start of transfers; impacts likely differed both before and after. Third, the transfers began at the height of the COVID-19 pandemic. At this time, unemployment had risen sharply, and incomes had declined. Incomes increased again after the initial shock of the pandemic, and our endline captures this economic recovery. Thus, as pointed out above, negative effects on expenditure, for example, can be seen as a slower recovery, rather than a decrease in absolute terms. Fourth, households received government grants during the study period (most importantly due to the American Rescue Plan Act of 2021), and the impact of the cash transfers in Compton took place in the context of this (additional) stimulus to both the treatment and the control group.[44] Fifth, the Compton transfers were time-limited and not large enough to allow households to survive entirely on the transfers; larger and longer-lasting transfers may produce different impacts. Indeed, the smaller transfer size (as a share of baseline household income) may account for the qualitatively different results in several recent US studies compared to the typical cash transfer RCT in Africa and South Asia, which finds positive impacts on income and expenditure.[45]

From a policy perspective, the results provide a nuanced view on the possible impact of guaranteed income programs in the United States. The negative impacts on earned income and on expenditure should be seen in the broader context of other impacts, especially the suggestive (but not significant) decrease in debt. The policy debate about unconditional transfers should consider all of the impacts together, and how they vary across households.

[44] The main regression specifications control for receipt of major public benefits.

[45] These results, and their contrast with our findings, suggests that there may be non-linear (e.g. threshold) effects of transfer magnitude. However, higher baseline household income itself is another possible explanation for these differences.

References

Aguila, E., A. Kapteyn, and F. Perez-Arce (2017). Consumption smoothing and frequency of benefit payments of cash transfer programs. *American Economic Review 107*(5), 430–35.

Akee, R. K. Q., W. E. Copeland, G. Keeler, A. Angold, and E. J. Costello (2010). Incomes and children's outcomes: A quasi-experiment using transfer payments from casino profits. *Journal Applied Economics 2*(1), 86–115.

Anderson, M. L. (2008). Inference and sex 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), 1481–95.

Andrade, F. C. D., K. Z. Kramer, A. Greenlee, A. N. Williams, and R. Mendenhalld (2019). Impact of the chicago earned income tax periodic payment intervention on food security. *Preventive Medicine Reports*.

Arnold, C., T. Conway, and M. Greenslade (2011). DFID cash transfers literature review. Technical report, UK Department for International Development, London.

Baird, S., F. Ferreira, B. Özler, and M. Woolcock (2014). Conditional, unconditional and everything in between: A systematic review of the effects of cash transfer programmes on schooling outcomes. *Journal of Development Effectiveness 6*(1), 1–43.

Balakrishnan, S., S. Constantino, J. Haushofer, J. Morduch, and S. Chan (2021). Pre-analysis plan: Effects of an unconditional cash transfer on the economic and psychological well-being of low-income households in Compton, ca.

Banerjee, A. V., R. Hanna, G. Kreindler, and B. A. Olken (2017). Debunking the stereotype of the lazy welfare recipient: Evidence from cash transfer programs worldwide. *World Bank Research Observer 32*(2), 155–184.

Bartik, A. W., E. Rhodes, D. E. Broockman, P. K. Krause, S. Miller, and E. Vivalt (2024). The impact of unconditional cash transfers on consumption and household balance sheets: Experimental evidence from two US states. Working paper 32784, National Bureau of Economic Research.

Bastian, J. and K. Michelmore (2018). The long-term impact of the earned income tax credit on children's education and employment outcomes. *Journal of Labor Economics 36*(4), 1127–1163.

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

Crosta, T., D. Karlan, F. Ong, J. Rüschenpöhler, and C. R. Udry (2024). Unconditional cash transfers: A Bayesian meta-analysis of randomized evaluations in low and middle income countries. Working paper 32779, National Bureau of Economic Research.

Daidone, S., B. Davis, S. Handa, and P. Winters (2019). The household and individual-level productive impacts of cash transfer programs in Sub-Saharan Africa. *American Journal of Agricultural Economics 101*(5), 1401–1431.

De Mel, S., D. McKenzie, and C. Woodruff (2012). One-time transfers of cash or capital have long-lasting effects on microenterprises in Sri Lanka. *Science 335*(6071), 962–966.

Edin, K. and L. Lein (1997). *Making Ends Meet: How Single Mothers Survive Welfare and Low-Wage Work*. New York: Russell Sage Foundation.

Eissa, N. and H. W. Hoynes (2004). Taxes and the labor market participation of married couples: the earned income tax credit. *Journal of Public Economics 88*(9-10), 1931–1958.

Eissa, N. and J. B. Leibman (1996). Labor supply response to the earned income tax credit. *Quarterly Journal of Economics 111*(2), 605–637.

Evans, D. K. and A. Popova (2017). Cash transfers and temptation goods. *Economic Development and Cultural Change 65*(2), 189–221.

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.

Fulford, S. (2023). *The Pandemic Paradox: How the COVID Crisis Made Americans More Financially Secure*. Princeton University Press.

Gennetian, L. A., G. J. Duncan, N. A. Fox, S. Halpern-Meekin, K. Magnuson, K. G. Noble, and H. Yoshikawa (2024). Effects of a monthly unconditional cash transfer starting at birth on family investments among us families with low income. *Nature Human Behaviour*, 1–16.

Gertler, P. (2004). Do conditional cash transfers improve child health? evidence from PROGRESA's control randomized experiment. *American Economic Review 94*(2), 336–341.

Grogger, J. (2003). The effects of time limits, the eitc, and other policy changes on welfare use, work, and income among female-headed families. *Review of Economics and Statistics 85*(2), 394–408.

Guarino, P. (2021). Chicago poised to create one of the nation's largest 'guaranteed basic income' programs. *Washington Post*.

Haushofer, J. and J. Shapiro (2016). Short-term impact of unconditional cash transfers to the poor: Experimental evidence from Kenya. *The Quarterly Journal of Economics 131*(4), 1973–2042.

Jacob, B., N. Pilkauskas, E. Rhodes, K. Richard, and H. L. Shaefer (2022). The COVID-19 cash transfer study II: The hardship and mental health impacts of an unconditional cash transfer to low-income individuals. *National Tax Journal 75*(3), 597–625.

Jaroszewicz, A., J. M. Jachimowicz, O. P. Hauser, and J. Jamison (2022). How effective is (more) money? randomizing unconditional cash transfer amounts in the US. *SSRN Working Paper*.

Jones, D. and I. E. Marinescu (2022). The labor market impacts of universal and permanent cash transfers: Evidence from the Alaska permanent fund. *American Economic Journal: Economic Policy 14*(2), 315–40.

Kansikas, C., A. Mani, and P. Niehaus (2023, February). Customized cash transfers: Financial lives and cash-flow preferences in rural Kenya. *NBER working paper 30930*.

Kessler, R., G. Andrews, L. Colpe, E. Hiripi, D. Mroczek, S.-L. Normand, E. Walters, and A. Zaslavsky (1997). Kessler psychological distress scale. *Archives of General Psychiatry*.

Kleven, H. (2024). The eitc and the extensive margin: A reappraisal. *Journal of Public Economics 236*.

Kramer, K. Z., F. C. D. Andrade, A. J. Greenlee, R. Mendenhall, D. Bellisle, and R. L. Blanks (2019). Periodic earned income tax credit (eitc) payment, financial stress and wellbeing: A longitudinal study. *Journal of Family and Economic Issues*.

Liebman, J., K. Carlson, E. Novick, and P. Portocarrero (2022). The chelsea eats program: Experimental impacts. *Rappaport Institute for Greater Boston Working Paper*.

Meyer, B. D. and D. T. Rosenbaum (2001). Welfare, the earned income tax credit, and the labor supply of single mothers. *Quarterly Journal of Economics 116*(3), 1063 – 1114.

Miller, S., E. Rhodes, A. W. Bartik, D. E. Broockman, P. K. Krause, and E. Vivalt (2024). Does income affect health? evidence from a randomized controlled trial of a guaranteed income. Working paper 32711, National Bureau of Economic Research.

Moffitt, R. A. (2002). Welfare programs and labor supply. In A. J. Auerbach and M. Feldstein (Eds.), *Handbook of Public Economics*, Volume 4, Chapter 34, pp. 2393–2430. Elsevier.

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.

Parolin, Zachary, E. A. S. C. M. C. and C. Wimer (2023). The effects of the monthly and lump-sum child tax credit payments on food and housing hardship. *AEA Papers and Proceedings*.

Pilkauskas, N. V., B. A. Jacob, E. Rhodes, K. Richard, and H. L. Shaefer (2022). The covid cash transfer study: The impacts of a one-time unconditional cash transfer on the well-being of families receiving SNAP in twelve states. *Journal of Policy Analysis and Management 42*(3), 771–795.

Robins, P. K. (1985). A comparison of the labor supply findings from the four negative income tax experiments. *The Journal of Human Resources 20*(4), 567–582.

Saez, E. (2002). Optimal income transfer programs: Intensive versus extensive labor supply responses. *Quarterly Journal of Economics 117*(3), 1039–73.

Sauval, M., G. J. Duncan, L. A. Gennetian, K. A. Magnuson, N. A. Fox, K. G. Noble, and H. Yoshikawa (2024). Unconditional cash transfers and maternal employment: Evidence from the baby's first years study. *Journal of Public Economics 236*, 105159.

Shrider, E. A. and J. Creamer (2023). Poverty in the united states: 2022. Report p60-280, US Bureau of the Census, Washington, DC.

Troller-Renfree, S. V., M. A. Costanzo, G. J. Duncan, K. Magnuson, L. A. Gennetian, H. Yoshikawa, S. Halpern-Meekin, N. A. Fox, and K. G. Noble (2022). The impact of a poverty reduction intervention on infant brain activity. *Proceedings of the National Academy of Sciences 119*(5), e2115649119.

Vesoulis, A. and A. Abrams (2021). Inside the nation's largest guaranteed income experiment. *Time September 6*.

Vivalt, E., E. Rhodes, A. W. Bartik, D. E. Broockman, and S. Miller (2024). The employment effects of a guaranteed income: Experimental evidence from two US states. Working paper 32719, National Bureau of Economic Research.

West, S. and A. Castro (2023). Impact of guaranteed income on health, finances, and agency: Findings from the Stockton randomized controlled trial. *Journal of Urban Health 100*(2), 227–244.

Tables

Table 1: Description of Transfers

	Average monthly transfer ($) overall	N	Average monthly transfer ($) bimonthly	N	Average monthly transfer ($) quarterly	N
Panel A: Nominal values						
Program sample						
No children	300	162	300	73	300	89
1 child	450	128	450	56	450	72
2+ children	600	335	600	179	600	156
Non-compliers	0	70	0	37	0	33
Overall	442	695	448	345	436	350
Analysis sample						
No children	300	92	300	40	300	52
1 child	450	77	450	34	450	43
2+ children	600	177	600	98	600	79
Non-compliers	0	28	0	14	0	14
Overall	450	374	463	186	438	188
Panel B: Inflation-adjusted values (February-March 2021 Base Period)						
Program sample						
No children	268	162	268	73	268	89
1 child	402	128	402	56	402	72
2+ children	536	335	536	179	536	156
Non-compliers	0	70	0	37	0	33
Overall	395	695	400	345	390	350
Analysis sample						
No children	268	92	268	40	268	52
1 child	402	77	402	34	402	43
2+ children	536	177	536	98	536	79
Non-compliers	0	28	0	14	0	14
Overall	402	374	414	186	391	188

Notes: In Panel A, 625 out of 698 study participants who were randomly selected to receive transfers took up the treatment. 3 participants were dropped from our study sample due to duplicate payments from the implementation pilot.
In Panel B, 346 out of 374 households who were randomly selected to receive transfers and answered the endline survey took up the treatment. The transfer amounts in the table above were adjusted for inflation using the Consumer Price Index for all U.S. Urban Consumers. The price level in February - March 2021 (period of program enrollment) was used as the base price level, and the price level in May - September 2022 (the survey period) was used as the post price level.

Table 2: Balance Check—Overall Sample

	(1) Control mean (std. dev.)	(2) Treatment dummy (overall)	(3) Treatment dummy (bimonthly)	(4) Treatment dummy (quarterly)
Respondent is Hispanic	0.68	0.01	0.02	0.00
	(0.47)	(0.02)	(0.03)	(0.03)
Respondent is African-American	0.26	−0.02	−0.03	−0.01
	(0.44)	(0.02)	(0.03)	(0.03)
Respondent is female	0.74	0.01	0.01	0.01
	(0.44)	(0.02)	(0.03)	(0.03)
Number of minors in the household	1.82	0.04	−0.02	0.10
	(1.50)	(0.09)	(0.11)	(0.14)
Number of people in the household	4.45	−0.05	−0.08	−0.02
	(1.90)	(0.10)	(0.13)	(0.12)
Age of the respondent	34.93	0.58	0.50	0.67
	(9.17)	(0.50)	(0.65)	(0.66)
Annual household income, $	26,307.60	−1,584.88	−2,053.91	−1,118.13
	(41,491.07)	(1,450.02)	(1,756.96)	(1,764.07)
N	1,402	695	345	350

Notes: Column (1) shows the mean of each row variable for the control group, and its standard deviation in parentheses. The estimates in Column (2) are from Equation 1, whereas Columns (3) and (4) are from Equation 2 where the dependent variable is the baseline characteristics presented in each row. Columns (2)–(4) show standard errors in parentheses. All regressions are weighted using sampling weights.

Over the course of the study, we were informed that three people who were randomized into treatment during the pilot were included in our sample of 2,100. The treatment sample in this table omits the 3 people who were randomized into treatment during pilot study.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Summary Statistics—January 2021 vs. Survey Values 2022

	(1) Control			(2) Treatment			(3)	
	N	Mean/(std. error)		N	Mean/(std. error)		p-value: control vs. treatment	
		January 2021	Survey Month		January 2021	Survey Month	January 2021	Survey Month
Participated in labor market	700	0.584	0.735	374	0.610	0.673	0.410	0.035**
		(0.019)	(0.017)		(0.025)	(0.024)		
Weekly labor supply in hours	700	19.942	26.414	374	19.646	25.572	0.802	0.521
		(0.695)	(0.743)		(0.924)	(1.103)		
Weekly labor supply in hours, if > 0	402/505	34.151	35.961	223/260	32.198	38.003	0.022**	0.057*
		(0.481)	(0.600)		(0.729)	(0.882)		
Total income in the last 30 days, $	699	2,566	3,341	370	2,390	2,848	0.180	0.000***
		(79)	(83)		(97)	(100)		
Respondent earnings before taxes	699	1,441	1,976	370	1,398	1,699	0.664	0.005***
		(58)	(58)		(76)	(78)		
Household income excluding respondent	699	825	994	370	704	828	0.152	0.072*
		(52)	(56)		(61)	(66)		
Rental income, dividends or interest	699	0	21	370	0	15	N/A	0.233
		(0)	(3)		(0)	(3)		
Income from SSI or OASDI	699	56	76	370	55	60	0.972	0.306
		(8)	(9.396)		(10)	(11)		
Income from CalWORKS	699	81	101	370	62	82	0.164	0.246
		(8)	(10)		(10)	(12)		
Income from CalFresh/SNAP or WIC	699	151	174	370	144	162	0.608	0.466
		(9)	(9)		(12)	(12)		
Other income	699	11	0	370	27	1	0.000***	0.000***
		(2)	(0)		(4)	(0)		
F-test of joint significance (F-stat)							2.211**	5.034***
F-test, number of observations							625	765

Notes: The statistics for weekly labor supply in hours are conditional on labor market participation in the endline; the two numbers reported for N are sample sizes for January 2021 / the survey month.
The total income in the last 30 days omits the stimulus checks received in January 2021.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Treatment Effects on Labor Supply and Income

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Control mean (std. dev.)	Treatment mean (std. dev.)	Treatment effect: overall	Treatment effect: bimonthly	Treatment effect: quarterly	p-value: bimonthly vs. quarterly	N
Participated in labor market	0.73	0.67	-0.05	-0.03	-0.06	0.63	1,074
	(0.44)	(0.47)	(0.03)	(0.04)	(0.04)		
Participated in labor market, if $\geq$ 20 hours in Jan 2021	0.90	0.88	-0.02	0.02	-0.06	0.18	576
	(0.30)	(0.32)	(0.03)	(0.04)	(0.05)		
Participated in labor market, if $<$ 20 hours in Jan 2021	0.54	0.42	-0.13**	-0.16**	-0.09	0.40	498
	(0.50)	(0.49)	(0.06)	(0.07)	(0.08)		
Weekly labor supply in hours[a]	26.41	25.57	0.03	-0.21	0.29	0.85	1,074
	(19.65)	(21.32)	(1.48)	(1.76)	(2.17)		
			[0.99]	[0.98]	[1.00]		
Weekly labor supply in hours, if $>$ 0	35.96	38.00	1.97	0.83	3.25*	0.30	765
	(13.49)	(14.22)	(1.38)	(1.74)	(1.85)		
Participated in unpaid work[b]	0.64	0.64	0.03	0.02	0.03	0.87	1,074
	(0.48)	(0.48)	(0.04)	(0.05)	(0.04)		
			[0.69]	[0.73]	[0.87]		
Total income last 30 days	3,341	3,270	92	70	117	0.81	1,069
including cash transfer, $	(2,201)	(1,962)	(126)	(155)	(164)		
Total income last 30 days	3,341	2,848	-333***	-375**	-287*	0.64	1,069
without cash transfer, $[a]	(2,201)	(1,937)	(123)	(148)	(162)		
			[0.04]**	[0.13]	[0.39]		
Respondent earnings before taxes	1,976	1,699	-162*	-184	-137	0.75	1,069
	(1,545)	(1,495)	(95)	(126)	(114)		
Household income excluding survey respondent	994	828	-130	-147	-112	0.80	1,069
	(1,482)	(1278)	(88)	(101)	(124)		
All benefit income:	350	305	-39	-51	-26	0.64	1,069
	(535)	(465)	(34)	(40)	(46)		
SSI or OASDI	76	69	-11	-10	-12	0.91	1,069
	(248)	(215)	(16)	(18)	(21)		
CalWORKS	101	82	-17	-32	-0	0.23	1,069
	(257)	(226)	(17)	(20)	(24)		
CalFresh/SNAP or WIC benefits	174	162	-11	-8	-13	0.85	1,069
	(245)	(233)	(17)	(22)	(22)		
Rental income, dividends or interest	21	15	-3	5	-12***	0.05**	1,069
	(74)	(66)	(6)	(9)	(5)		
Other income	0	1	1***	2***	0	0.00***	1,069
	(2)	(3)	(0)	(0)	(0)		

Notes: The estimates in Column (3) are from Equation 1, whereas Columns (4) and (5) are from Equation 2. All regressions control for baseline household income, baseline labor supply, Biden Child Tax Credit amount, number of people and number of children in the household, and respondent characteristics: Hispanic indicator, Black or African American indicator, age, and sex. We also control for if the respondent received any reminders and/or bonuses to complete the survey, as well as if they live in the same household with another respondent. Finally, we control for if the respondent was re-randomized in February 2021. All outcome variables are winsorized at the 5^{th} and 95^{th} percentiles. All regressions are weighted using sampling weights.
Labor market participation is equal to one if hours of work is positive, and zero otherwise (regardless of reported earnings).
Standard errors in parentheses. FDR-corrected p-values for index variables are shown in brackets.
a. Pre-registered primary outcome.
b. Pre-registered secondary outcome.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Treatment Effects on Spending

	(1)	(2)	(3)	(4)	(5)	(6)
	Control mean (std. dev)	Treatment effect: overall	Treatment effect: bimonthly	Treatment effect: quarterly	p-value: bimonthly vs. quarterly	N
Total expenditure last 30 days, \$[a]	2,945	-302^{***}	-229^{**}	-383^{***}	0.30	1,062
	(1,378)	(93)	(116)	(123)		
		[0.01]***	[0.28]	[0.01]**		
Panel A:						
Non-housing expenditure, \$	1,542	-160^{***}	-149^{**}	-172^{**}	0.79	1,062
	(829)	(55)	(68)	(72)		
Food and beverages consumed at home	454	−28	−6	-52^{**}	0.12	1,062
	(274)	(19)	(24)	(24)		
Food and beverages prepared out of home	168	-17^{*}	−17	−18	0.96	1,062
	(145)	(10)	(11)	(13)		
Clothing, footwear, other apparel	197	−17	−7	-28^{**}	0.20	1,062
	(160)	(11)	(14)	(13)		
Temptation goods	22	-5^{**}	-6^{**}	−3	0.40	1,062
	(39)	(2)	(3)	(3)		
Health insurance costs	50	−9	-16^{*}	−1	0.18	1,062
	(105)	(8)	(9)	(10)		
Cost of elderly and child care	39	-18^{***}	−11	-27^{***}	0.09*	1,062
	(105)	(6)	(8)	(7)		
Education	46	−9	−6	−12	0.57	1,062
	(111)	(8)	(9)	(10)		
Car/vehicle expenses	369	−29	−48	−9	0.35	1,062
	(322)	(25)	(30)	(35)		
Other transport expenses	19	-6^{**}	-8^{***}	−4	0.23	1,062
	(43)	(3)	(3)	(4)		
Internet and phone bills	173	-21^{**}	-25^{**}	−18	0.59	1,062
	(112)	(8)	(10)	(11)		
Panel B:						
Housing expenditure, \$	1,404	-142^{**}	−79	-212^{***}	0.15	1,062
	(810)	(59)	(74)	(75)		
Mortgage, associated fees	1,507	−20	−140	128	0.11	272
	(564)	(100)	(129)	(128)		
Rent	1,096	-80^{*}	−83	−76	0.93	773
	(522)	(47)	(59)	(60)		
Utilities	204	-43^{***}	-45^{***}	-42^{***}	0.86	1,062
	(178)	(12)	(15)	(14)		

Notes: All amounts are in US dollars. Food and beverage consumption at and out of home excludes alcohol. The estimates in Column (2) are from Equation 1, whereas Columns (3) and (4) are from Equation 2. The models for housing expenditure all control for whether the respondent moved since January 2021. All regressions control for baseline household income, baseline labor supply, Biden Child Tax Credit amount, number of people and number of children in the household, and respondent characteristics: Hispanic indicator, Black or African American indicator, age, and sex. We also control for if the respondent received any reminders and/or bonuses to complete the survey, as well as if they live in the same household with another respondent. Finally, we control for if the respondent was re-randomized in February 2021. All outcome variables are winsorized at the 5^{th} and 95^{th} percentiles. All regressions are weighted using sampling weights.
Standard errors in parentheses.
a. Pre-registered primary outcome.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Treatment Effects on Assets and Debt

	(1) Control mean (std. dev)	(2) Treatment effect: overall	(3) Treatment effect: bimonthly	(4) Treatment effect: quarterly	(5) p-value: bimonthly vs. quarterly	(6) N
Net assets (non-housing), $[a]	17,229	2,498	5,079	−337	0.13	1,074
	(29,631)	(2,258)	(3,190)	(2,488)		
		[0.78]	[0.28]	[1.00]		
Panel A:						
Assets (non-housing), $	36,371	308	3,102	−2,761	0.04	1,074
	(27,955)	(1,817)	(2,392)	(2,154)		
Cash, checking, savings, cds, stocks, bonds	1,354	−339**	−469**	−196	0.26	1,074
	(2,717)	(171)	(184)	(233)		
Retirement accounts	1,777	−139	−705**	484	0.05*	1,074
	(4,991)	(385)	(355)	(600)		
Loans to relatives	0	0	0	0	0.99	1,074
	(0)	(0)	(0)	(0)		
Gifts from relatives	6	1	3	−1	0.19	1,074
	(23)	(2)	(2)	(2)		
Total value of durable goods	33,234	784	4,273*	−3,049	0.01	1,074
	(24,962)	(1,717)	(2,316)	(1,986)		
Panel B:						
Debt (non-housing), $	19,142	−2,190	−1,977	−2,424	0.85	1,074
	(22,797)	(1,595)	(2,162)	(1,824)		
Student loans	6,900	−549	68	−1,226	0.37	1,074
	(13,945)	(933)	(1,274)	(1,058)		
Credit card debt	4,449	−460	−1,074**	214	0.06*	1,074
	(6,155)	(442)	(510)	(598)		
Medical debt	402	−86	−104	−67	0.73	1,074
	(1,055)	(75)	(98)	(85)		
Other debt	7,353	−1,103	−877	−1,351	0.74	1,074
	(12,344)	(912)	(1,210)	(1,085)		
Loans from relatives	2	0	−1**	1	0.05**	1,074
	(9)	(1)	(1)	(1)		
Gifts to relatives	38	9	12	5	0.61	1,074
	(95)	(9)	(11)	(11)		

Notes: All amounts are in US dollars. The estimates in Column (2) are from Equation 1, whereas Columns (3) and (4) are from Equation 2. The models for housing expenditure all control for whether the respondent moved since January 2021. All regressions control for baseline household income, baseline labor supply, Biden Child Tax Credit amount, number of people and number of children in the household, and respondent characteristics: Hispanic indicator, Black or African American indicator, age, and sex. We also control for if the respondent received any reminders and/or bonuses to complete the survey, as well as if they live in the same household with another respondent. Finally, we control for if the respondent was re-randomized in February 2021. All outcome variables are winsorized at the 5^{th} and 95^{th} percentiles. All regressions are weighted using sampling weights.
Standard errors in parentheses
a. Pre-registered primary outcome.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Treatment Effects on Broader Well-being and Temptation Goods

	(1)	(2)	(3)	(4)	(5)	(6)
	Control mean (std. dev.)	Treatment effect: overall	Treatment effect: bimonthly	Treatment effect: quarterly	p-value: bimonthly vs. quarterly	N
Psychological well-being index[a]	0.00	0.05	0.01	0.10	0.46	1,072
	(1.00)	(0.07)	(0.09)	(0.10)		
		[0.92]	[0.98]	[0.83]		
Financial security index[a]	0.00	0.03	0.07	0.00	0.54	1,074
	(1.00)	(0.07)	(0.09)	(0.09)		
		[0.95]	[0.83]	[1.00]		
Food security index[a]	0.00	0.01	0.11	−0.10	0.09*	1,071
	(1.00)	(0.07)	(0.10)	(0.10)		
		[0.99]	[0.69]	[0.83]		
Housing security index[b]	0.00	0.29***	0.27***	0.30***	0.80	1,074
	(1.00)	(0.07)	(0.09)	(0.08)		
		[0.00]***	[0.02]**	[0.01]***		
IPV index[b]	0.00	−0.04	−0.07	−0.01	0.66	1,074
	(1.00)	(0.07)	(0.08)	(0.11)		
		[0.69]	[0.73]	[0.87]		
Any physical violence by an intimate partner last 6 months†	0.07	−0.02	−0.03*	0.00	0.35	1,074
	(0.26)	(0.02)	(0.02)	(0.03)		
Any forced sex by an intimate partner last 6 months†	0.03	0.00	0.00	−0.01	0.34	1,074
	(0.18)	(0.01)	(0.02)	(0.01)		
Alcohol expenditure last 30 days, $[b]	17.20	−2.97	−3.86	−1.99	0.56	1,062
	(32.90)	(2.11)	(2.67)	(2.61)		
		[0.50]	[0.48]	[0.87]		
Cigarettes/tobacco expenditure last 30 days, $[b]	4.95	−1.95**	−2.46**	−1.38	0.36	1,062
	(14.28)	(0.88)	(1.01)	(1.11)		
		[0.18]	[0.17]	[0.72]		
List Experiment‡						
IPV last 6 months	0.10	−0.20**	−0.15	−0.26**	0.40	1,070
	(0.07)	(0.09)	(0.11)	(0.11)		
Drink more than five alcoholic drinks in one occasion last 30 days?[b]	0.25	−0.10	−0.18*	−0.01	0.17	1,073
	(0.06)	(0.08)	(0.10)	(0.11)		
Smoke cigarettes or other tobacco or nicotine products last 30 days?[b]	0.06	0.15*	0.13	0.17	0.74	1,072
	(0.06)	(0.09)	(0.11)	(0.12)		

Notes: The estimates in Column (2) are from Equation 1, whereas Columns (3) and (4) are from Equation 2. All regressions control for baseline household income, baseline labor supply, Biden Child Tax Credit amount, number of people and number of children in the household, and respondent characteristics: Hispanic indicator, Black or African American indicator, age, and sex. We also control for if the respondent received any reminders and/or bonuses to complete the survey, as well as if they live in the same household with another respondent. Finally, we control for if the respondent was re-randomized in February 2021. All outcome variables are winsorized at the 5^{th} and 95^{th} percentiles. All regressions are weighted using sampling weights.

The expenditure for alcohol and cigarettes/tobacco products are reported for the entire household.

† This is a binary variable created based on a Likert-scale, taking value of 1 if the respondent expressed any intimate partner violence or forced sex in the past 6 months (rarely, occasionally, frequently, very frequently), and 0 if they answered "Never" or that they haven't had a partner in the last 6 months.

‡ For the list experiment results, the first column shows the difference in average number of activities between the "long" and "short" lists in the control group, which can be interpreted as the share of respondents in that group who experienced the outcome in question.

Standard errors in parentheses. FDR-corrected p-values for index variables are shown in brackets.

a. Pre-registered primary outcome.

b. Pre-registered secondary outcome.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Heterogeneous Treatment Effects by Sex of Respondent

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Main regression (values from Tables 4–7)			Heterogeneity regression Treatment effect: overall				
	Control mean (std. dev.)	Treatment effect: overall	Control mean if Female (std. dev.)	Female × Treated	Male × Treated	p-value: female vs. male	Number of Females	N
Participated in labor market	0.73	-0.05	0.72	-0.06	0.01	0.30	835	1,074
	(0.44)	(0.03)	(0.45)	(0.04)	(0.06)			
Weekly labor supply in hours	26.41	0.03	25.16	-0.04	0.26	0.93	835	1,074
	(19.65)	(1.48)	(19.60)	(1.71)	(2.79)			
Weekly labor supply in hours, if > 0	35.96	1.97	35.07	2.73*	-0.10	0.32	574	765
	(13.49)	(1.38)	(13.70)	(1.63)	(2.40)			
Total income in the last 30 days including cash transfer, $	3,341	92	3,259	212	-308	0.09*	830	1,069
	(2,201)	(126)	(2,140)	(135)	(283)			
Total income in the last 30 days without cash transfer, $	3,341	-333***	3,259	-231*	-675**	0.14	830	1,069
	(2,201)	(123)	(2,140)	(133)	(274)			
Total expenditure in the last 30 days, $	2,945	-302***	2,918	-189*	-682***	0.03**	825	1,062
	(1,378)	(93)	(1,334)	(100)	(212)			
Assets (non-housing, $)	36,371	308	35,278	808	-1,361	0.61	835	1,074
	(27,955)	(1,817)	(26,861)	(2,029)	(3,764)			
Debt (non-housing, $)	19,142	-2,190	18,874	-1,478	-4,569	0.40	835	1,074
	(22,797)	(1,595)	(22,684)	(1,832)	(3,159)			
Net assets (non-housing, $)	17,229	2,498	16,404	2,286	3,207	0.85	835	1,074
	(29,631)	(2,258)	(29,045)	(2,649)	(4,273)			
Psychological well-being index	0.00	0.05	-0.02	0.07	0.01	0.77	833	1,072
	(1.00)	(0.07)	(0.98)	(0.08)	(0.16)			
Financial security index	0.00	0.03	-0.04	0.14*	-0.31**	0.00***	835	1,074
	(1.00)	(0.07)	(0.98)	(0.08)	(0.14)			
Food security index	0.00	0.01	0.03	-0.00	0.04	0.81	832	1,071
	(1.00)	(0.07)	(0.99)	(0.09)	(0.14)			
Housing Security Index	0.00	0.29***	-0.02	0.27***	0.34***	0.64	835	1,074
	(1.00)	(0.07)	(1.03)	(0.08)	(0.12)			
IPV Index	0.00	-0.04	0.01	0.01	-0.20**	0.08*	835	1,074
	(1.00)	(0.07)	(1.02)	(0.09)	(0.09)			
IPV (from list experiment)‡	0.10	-0.20**	1.32	-0.16	-0.36**	0.31	831	1,070
	(0.07)	(0.09)	(0.97)	(0.10)	(0.17)			

Notes: Columns (1) and (2) are from Tables 4–7, showing the overall control mean and the treatment effect for each outcome, respectively. Column (3) presents the control mean of the outcome for the subsample of female respondents. The estimates in columns (4) and (5) follow from the Equation 3. Column (6) reports the p-value obtained from the t-test where the null hypothesis is such that the estimates in columns (4) and (5) are equal. All regressions control for baseline household income, baseline labor supply, Biden Child Tax Credit amount, number of people and number of children in the household, and respondent characteristics: Hispanic indicator, Black or African American indicator, age, and sex. We also control for if the respondent received any reminders and/or bonuses to complete the survey, as well as if they live in the same household with another respondent. Finally, we control for if the respondent was re-randomized in February 2021. All outcome variables are winsorized at the 5^{th} and 95^{th} percentiles. All regressions are weighted using sampling weights.

Labor market participation is equal to one if hours of work is positive, and zero otherwise (regardless of reported earnings).

‡ For the list experiment results, the first column shows the difference in average number of activities between the "long" and "short" lists in the control group, which can be interpreted as the share of respondents in that group who experienced the outcome in question.

Standard errors in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Heterogeneous Treatment Effects by Single Motherhood

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Main regression (values from Tables 4–7)			Heterogeneity regression Treatment effect: overall				
	Control mean (std. dev.)	Treatment effect: overall	Control mean if single mother (std. dev)	Single Mother x Treated	All Others x Treated	p-value: single mother vs. others	Number of Single Mothers	N
Participated in labor market	0.73	-0.05	0.68	-0.00	-0.06	0.47	240	1,074
	(0.44)	(0.03)	(0.47)	(0.07)	(0.04)			
Weekly labor supply in hours	26.41	0.03	21.69	6.43*	-1.84	0.03	240	1,074
	(19.65)	(1.48)	(18.49)	(3.39)	(1.59)			
Weekly labor supply in hours, if > 0	35.96	1.97	31.94	9.57***	0.05	0.01	160	765
	(13.49)	(1.38)	(13.21)	(3.12)	(1.47)			
Total income in the last 30 days including cash transfer, $	3,341	92	2,668	831***	-106	0.00	239	1,069
	(2,201)	(126)	(1,446)	(231)	(141)			
Total income in the last 30 days without cash transfer, $	3,341	-333***	2,668	317	-503***	0.00	239	1,069
	(2,201)	(123)	(1,446)	(228)	(138)			
Total expenditure in the last 30 days, $	2,945	-302***	2,670	-43	-366***	0.12	238	1,062
	(1,378)	(93)	(1,262)	(176)	(108)			
Assets (non-housing, $)	36,371	308	23,994	4,284	-273	0.22	240	1,074
	(27955)	(1817)	(23618)	(3151)	(2120)			
Debt (non-housing, $)	19,142	-	14,094	5,604	-4,351***	0.01	240	1,074
	(22,797)	(1,595)	(17,866)	(3,746)	(1,646)			
Net assets (non-housing, $)	17,229	2,498	9,900	-1,321	4,077	0.31	240	1,074
	(29,631)	(2,258)	(24,922)	(4,757)	(2,488)			
Psychological well-being index	0.00	0.05	-0.11	0.16	0.03	0.43	240	1,072
	(1.00)	(0.07)	(0.93)	(0.15)	(0.08)			
Financial security index	-0.00	0.03	-0.23	0.04	0.05	0.93	240	1,074
	(1.00)	(0.07)	(0.87)	(0.12)	(0.08)			
Food security index	-0.00	0.01	0.15	-0.32**	0.10	0.02	240	1,071
	(1.00)	(0.07)	(1.01)	(0.16)	(0.08)			
Housing Security Index	0.00	0.29***	-0.19	0.45***	0.24***	0.23	240	1,074
	(1.00)	(0.07)	(1.22)	(0.15)	(0.08)			
IPV Index	-0.00	-0.04	0.09	-0.00	-0.06	0.78	240	1,074
	(1.00)	(0.07)	(1.13)	(0.19)	(0.08)			
IPV (from list experiment)‡	0.10	-0.20**	1.32	-0.24	-0.19*	0.80	239	1,070
	(0.98)	(0.09)	(1.04)	(0.18)	(0.10)			

Notes: Variable definition follows the Pre-Analysis Plan. Columns (1) and (2) present the overall control mean for each outcome and the control mean of the outcome for the subsample of single mothers in the baseline, respectively. The estimates in columns (3) and (4) follow from the Equation 3. All regressions control for baseline household income and labor supply, whether the household received the Biden Child Tax Credit, number of people in the household, number of minors in the household, an indicator for the respondent being Hispanic, an indicator for the respondent being black or African American, an indicator for the respondent being a single mother, age, and sex. We also control for if the respondent received any reminders and/or bonuses to complete the survey as well as if they live in the same household with another respondent. Finally, we control for if the respondent was re-randomized in February 2021. All outcome variables are winsorized at the 5^{th} and 95^{th} percentiles. All regressions are weighted using sampling weights.

Labor market participation is equal to one if hours of work is positive, and zero otherwise (regardless of reported earnings).

‡ For the list experiment results, the first column shows the difference in average number of activities between the "long" and "short" lists in the control group, which can be interpreted as the share of respondents in that group who experienced the outcome in question.

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 10: Heterogeneous Treatment Effects by Above and Below Median Income

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Main regression (values from Tables 4–7)			Heterogeneity regression Treatment effect: overall				
	Control mean (std. dev.)	Treatment effect: overall	Control mean if Below Median Income (std. dev.)	Below Median Income × Treated	Above Median Income × Treated	p-value: below vs. above median income	Number of Respondents with Below Median Income	N
Participated in labor market	0.73	-0.05	0.63	-0.04	-0.05	0.89	534	1,074
	(0.44)	(0.03)	(0.48)	(0.05)	(0.04)			
Weekly labor supply in hours	26.41	0.03	20.78	1.03	-1.04	0.48	534	1,074
	(19.65)	(1.48)	(19.68)	(2.40)	(1.71)			
Weekly labor supply in hours, if > 0	35.96	1.97	32.94	3.73	0.67	0.28	329	765
	(13.49)	(1.38)	(14.59)	(2.55)	(1.30)			
Total income in the last 30 days including cash transfer, $	3,341	92	2,350	-44	28	0.78	534	1,069
	(2,201)	(126)	(1,657)	(165)	(199)			
Total income in the last 30 days without cash transfer, $	3,341	-333***	2,350	-469***	-398**	0.78	534	1,069
	(2,201)	(123)	(1,657)	(161)	(195)			
Total expenditure in the last 30 days, $	2,945	-302***	2,573	-461***	-226*	0.20	527	1,062
	(1,378)	(93)	(1,321)	(142)	(123)			
Assets (non-housing, $)	36,371	308	26,624	1,207	-2,581	0.30	534	1,074
	(27,955)	(1,817)	(23,042)	(2,642)	(2,653)			
Debt (non-housing, $)	1,9142	-2,190	13,466	-1,772	-3,701	0.53	534	1,074
	(22,797)	(1,595)	(19,970)	(1,807)	(2,626)			
Net assets (non-housing, $)	17,229	2,498	13,158	2,979	1,120	0.69	534	1,074
	(29,631)	(2,258)	(26,933)	(2,999)	(3,481)			
Psychological well-being index	0.00	0.05	-0.08	0.14	-0.03	0.22	534	1,072
	(1.00)	(0.07)	(1.05)	(0.11)	(0.09)			
Financial security index	0.00	0.03	-0.17	0.08	-0.03	0.44	534	1,074
	(1.00)	(0.07)	(0.93)	(0.09)	(0.10)			
Food security index	0.00	0.01	-0.01	-0.04	0.05	0.53	534	1,071
	(1.00)	(0.07)	(1.02)	(0.11)	(0.10)			
Housing Security Index	0.00	0.29***	-0.06	0.26**	0.30***	0.75	534	1,074
	(1.00)	(0.07)	(1.08)	(0.11)	(0.08)			
IPV Index	-0.00	-0.04	0.05	-0.09	0.00	0.52	534	1,074
	(1.00)	(0.07)	(1.07)	(0.10)	(0.10)			
IPV (from list experiment)‡	0.10	-0.20**	1.24	-0.23*	-0.20*	0.89	534	1,070
	(0.07)	(0.09)	(1.06)	(0.13)	(0.11)			

Notes: Columns (1) and (2) are from Tables 4–7, showing the overall control mean and the treatment effect for each outcome, respectively. Column (3) presents the control mean of the outcome for the subsample of respondents with below-median income. The estimates in columns (4) and (5) follow from the Equation 3. Column (6) reports the p-value obtained from the t-test where the null hypothesis is such that the estimates in columns (4) and (5) are equal. All regressions control for baseline household income, baseline labor supply, Biden Child Tax Credit amount, number of people and number of children in the household, and respondent characteristics: Hispanic indicator, Black or African American indicator, age, and sex. We also control for if the respondent received any reminders and/or bonuses to complete the survey, as well as if they live in the same household with another respondent. Finally, we control for if the respondent was re-randomized in February 2021. All outcome variables are winsorized at the 5^{th} and 95^{th} percentiles. All regressions are weighted using sampling weights.
Labor market participation is equal to one if hours of work is positive, and zero otherwise (regardless of reported earnings).
‡ For the list experiment results, the first column shows the difference in average number of activities between the "long" and "short" lists in the control group, which can be interpreted as the share of respondents in that group who experienced the outcome in question.
Standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix

A Tables

Table A1: Treatment Assignment by Date

Date	Total Treatment	Quarterly	Bimonthly	Control	Total respondents
February 5, 2021	254	129	125	180	435
February 19, 2021	207	104	103	132	340
March 15, 2021	190	96	94	684	874
March 25, 2021	44	21	23	406	451
Total	695	350	345	1,402	2,097

Notes: We present the total number of treated participants in each recruitment round. Overall, we allocate 66.76% of our sample to the control group and 33.24% to the treatment group. The shares differ somewhat across the four rounds of recruitment due to requirements by the CCDC to expedite the launch of cash transfers.
To guarantee the comparability of the enrollments, we assign weights in each round so we have the same target proportion of treated and control households after weighting.

Table A2: Implementation Table

Status	Control	Bimonthly	Quarterly	Total data
No longer want to participate in the study[†]	44	4	3	51
Did not interact with the survey	497	91	89	678
Did not consent for the study in the survey[‡]	9	1	4	14
Consented for the study in the survey but did not start the survey[‡]	7	1	0	8
Answered some of part 1 of the survey	116	52	52	220
Finished part 1 (did not begin part 2)	20	6	5	32
Finished part 1 (began part 2 but did not finish it)	9	4	9	22
Finished part 1 and part 2	700	186	188	1,074
Total[§]	1,402	345	350	2,097

Notes: The survey is divided into two parts to minimize respondent fatigue. We only consider complete surveys, respondents who finished both parts, in our study.

[†] The participants explicitly stated they do not want to engage with the study and would not like any communication from us.

[‡] The survey module included a consent form at the beginning.

[§] Three participants were in our pre-pilot program and was included in our survey sample by mistake. Since they received a different payment amount with a different payment schedule, we do not include them in the total.

The data is based on the final survey numbers on August 14, 2022, after the closure of the endline survey.

Table A3: Differential Attrition—Overall Treatment Status

	Completed Endline	Completed Endline
Overall treatment	0.00	
	(0.03)	
Bimonthly		0.03
		(0.04)
Quarterly		−0.02
		(0.04)
p-value (bimonthly vs. quarterly)		0.33
Control mean	0.51	
Control std. dev.	0.50	
Number of completed endline surveys — control group	700	
Number of completed endline surveys — treatment group	374	
Total number of observations	2,097	

Notes: We test whether attrition is correlated with treatment by regressing an indicator variable for whether a participant attrited on the treatment indicators taking value of 1 for the treatment group, 0 otherwise.

The survey is divided into two parts to minimize respondent fatigue. We only consider complete surveys, respondents who finished both parts, in our study.

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Predicting Attrition from Demographics

	Attriter Mean (std. dev.)	Completed Endline
Respondent is Hispanic	0.70	-0.03
	(0.46)	(0.03)
Respondent is African-American	0.22	0.08^{***}
	(0.42)	(0.03)
Respondent is female	0.70	0.10^{***}
	(0.47)	(0.03)
Number of minors in the household	1.87	-0.01
	(1.67)	(0.01)
Number of people in the household	4.46	0.00
	(1.85)	(0.01)
Age of the respondent	35.72	-0.004^{***}
	(9.44)	(0.00)
Annual household income, $	23,894.32	0.00^{**}
	(21,554.61)	(0.00)
F-test of joint significance (F-stat)		57.98^{***}

Notes: We test whether attriters differ from non-attriters by asking whether attrition status can be predicted from baseline outcomes and stratification variables.
The survey is divided into two parts to minimize respondent fatigue. We only consider complete surveys, respondents who finished both parts, in our study.
Standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Differences between Attriters in the Treatment and the Control Group

	Control mean (std. dev.)	Treatment dummy: overall	N
Respondent is Hispanic	0.69	0.03	1,023
	(0.46)	(0.03)	
Respondent is African-American	0.23	−0.03	1,023
	(0.42)	(0.03)	
Respondent is female	0.69	0.03	1,023
	(0.46)	(0.04)	
Number of minors in the household	1.81	0.18	1,023
	(1.52)	(0.14)	
Number of people in the household	4.50	−0.15	1,023
	(1.93)	(0.13)	
Age of the respondent	35.44	0.85	1,023
	(9.61)	(0.71)	
Annual household income, $	24,400.68	−1,535.90	1,023
	(17,365.80)	(1,572.72)	

Notes: We test whether baseline characteristics of attriters in the treatment group are different from those of attriters in the control group by restricting the sample to attriters and regressing baseline variables on treatment assignment taking value of 1 for the treatment group, 0 otherwise.

The survey is divided into two parts to minimize respondent fatigue. We only consider complete surveys, respondents who finished both parts, in our study.

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Balance Check—Overall Sample

	(1) Control mean (std. dev.)	(2) Treatment dummy: overall	(3) Treatment dummy: bimonthly	(4) Treatment dummy: quarterly
Respondent is Hispanic	0.68	0.01	0.02	0.00
	(0.47)	(0.02)	(0.03)	(0.03)
Respondent is Black	0.26	−0.02	−0.02	−0.01
	(0.44)	(0.02)	(0.03)	(0.03)
Respondent is female	0.74	0.01	0.01	0.00
	(0.44)	(0.02)	(0.03)	(0.03)
Number of children in household	1.82	0.04	−0.02	0.11
	(1.50)	(0.09)	(0.11)	(0.13)
Number of people in household	4.45	−0.04	−0.09	0.01
	(1.90)	(0.10)	(0.13)	(0.12)
Age of the respondent	34.93	0.59	0.51	0.67
	(9.17)	(0.50)	(0.65)	(0.66)
Annual household income, $	26,308	-1,730	-2,104	-1,361
	(41,491)	(1,450)	(1,753)	(1,768)
N	1,402	698	347	351

Notes: The estimates in Column (2) are from Equation 1, whereas Columns (3) and (4) are from Equation 2 where the dependent variable is the baseline characteristics presented in each row. All regressions are weighted using sampling weights. Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Balance Check—Survey Sample

	(1)	(2)	(3)	(4)
	Control mean (std. dev.)	Treatment dummy (overall)	Treatment dummy (bimonthly)	Treatment dummy (quarterly)
Respondent is Hispanic	0.68	−0.01	0.01	−0.03
	(0.47)	(0.04)	(0.05)	(0.05)
Respondent is African-American	0.29	−0.01	−0.02	0.00
	(0.45)	(0.03)	(0.04)	(0.04)
Respondent is female	0.78	−0.01	0.00	−0.02
	(0.41)	(0.03)	(0.04)	(0.04)
Number of minors in the household	1.83	−0.09	−0.07	−0.11
	(1.48)	(0.12)	(0.16)	(0.16)
Number of people in the household	4.40	0.04	0.04	0.04
	(1.88)	(0.15)	(0.20)	(0.18)
Age of the respondent	34.44	0.34	0.37	0.31
	(8.72)	(0.69)	(0.91)	(0.93)
Annual household income, $	28,103.50	−1,650.17	−3,823.09	712.22
	(55,267.64)	(2,385.45)	(2,462.48)	(3,067.36)
Total income in Jan 21, $	4,181.39	−308.28	−414.38	−192.21
	(2,979.14)	(211.23)	(251.76)	(298.40)
N	700	374	186	188

Notes: The estimates in Column (2) are from Equation 1, whereas Columns (3) and (4) are from Equation 2 where the dependent variable is the baseline characteristics presented in each row. All regressions are weighted using sampling weights.
Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A8: Treatment Effects on Primary Outcomes—Different Specifications†

	(1) Control mean (std. dev.)	(2) Treatment mean (std. dev.)	(3) Treatment effect: overall	(4) Treatment effect: bimonthly	(5) Treatment effect: quarterly	(6) p-value: bimonthly vs. quarterly	(7) N
Participated in labor market							
Using robust standard errors	0.73	0.67	-0.05	-0.03	-0.06	0.63	1,074
	(0.44)	(0.47)	(0.03)	(0.04)	(0.04)		
Using clustered standard errors	0.73	0.67	-0.04	−0.03	−0.05	0.66	1,074
	(0.44)	(0.47)	(0.03)	(0.04)	(0.04)		
Weekly labor supply in hours							
Using robust standard errors	26.41	25.57	0.03	−0.21	0.29	0.85	1,074
	(19.65)	(21.32)	(1.48)	(1.76)	(2.17)		
Using clustered standard errors	26.41	25.57	0.11	−0.20	0.45	0.80	1,074
	(19.65)	(21.32)	(1.46)	(1.74)	(2.14)		
Weekly labor supply in hours, if > 0							
Using robust standard errors	35.96	38.00	1.97	0.83	3.25*	0.30	765
	(13.49)	(14.22)	(1.38)	(1.74)	(1.85)		
Using clustered standard errors	35.96	38.00	1.83	0.63	3.14*	0.27	765
	(13.49)	(14.22)	(1.37)	(1.73)	(1.82)		
Total income last 30 days including cash transfer, $							
Using robust standard errors	3,341.44	3,270.18	92.05	69.62	116.69	0.81	1,069
	(2,200.99)	(1,962.05)	(125.87)	(,155.37)	(,164.22)		
Using clustered standard errors	3,341.44	3,270.18	82.91	62.67	104.75	0.83	1,069
	(2,200.99)	(1,962.05)	(122.26)	(,154.92)	(,159.52)		
Total income last 30 days without cash transfer, $							
Using robust standard errors	3,341.44	2,848.14	-333.03***	−375.02**	−286.90*	0.64	1,069
	(2,200.99)	(1,937.17)	(122.77)	(,148.46)	(,162.44)		
Using clustered standard errors	3,341.44	2,848.14	-340.72***	−380.44**	−297.88*	0.67	1,069
	(2,200.99)	(1,937.17)	(119.27)	(,147.70)	(,158.20)		
Total expenditure last 30 days, $							
Using robust standard errors	2,945.45	2,577.69	-302.13***	−228.91**	−382.59***	0.30	1,062
	(1,378.05)	(1,375.00)	(93.41)	(116.10)	(123.09)		
Using clustered standard errors	2,945.45	2,577.69	-290.22***	−212.75	−373.81***	0.32	1,062
	(1,378.05)	(1,375.00)	(98.25)	(,129.24)	(,124.11)		
Assets (non-housing), $							
Using robust standard errors	36,370.91	35,848.53	308.27	3,102.43	−2,761.30	0.04	1,074
	(27,954.53)	(27,480.18)	(1816.63)	(2,391.80)	(2,153.59)		
Using clustered standard errors	36,370.91	35,848.53	-78.65	2,815.35	−3,200.72	0.03	1,074
	(27,954.53)	(27,480.18)	(1748.00)	(2,274.20)	(2,130.98)		
Debt (non-housing), $							
Using robust standard errors	19,142.35	15,521.27	-2190.01	−1,976.62	−2,424.43	0.85	1,074
	(22,796.89)	(20,888.27)	(1594.70)	(2,162.47)	(1,824.32)		
Using clustered standard errors	19,142.35	15,521.27	-2297.88	−2,186.38	−2,418.17	0.93	1,074
	(22,796.89)	(20,888.27)	(1551.38)	(2,177.52)	(1,759.59)		
Net assets (non-housing), $							
Using robust standard errors	17,228.56	20,327.26	2498.28	5,079.05	−336.87	0.13	1,074
	(29,631.44)	(30,405.97)	(2258.36)	(3,190.48)	(2,487.72)		
Using clustered standard errors	17,228.56	20,327.26	2219.23	5,001.72	−782.55	0.11	1,074
	(29,631.44)	(30,405.97)	(2216.01)	(3,181.94)	(2,445.51)		
Psychological well-being index							
Using robust standard errors	0.00	0.08	0.05	0.01	0.10	0.46	1,072
	(1.00)	(0.96)	(0.07)	(0.09)	(0.10)		
Using clustered standard errors	0.00	0.08	0.06	0.02	0.11	0.44	1,072
	(1.00)	(0.96)	(0.07)	(0.09)	(0.10)		
Financial security index							
Using robust standard errors	0.00	0.03	0.03	0.07	0.00	0.54	1,074
	(1.00)	(0.96)	(0.07)	(0.09)	(0.09)		
Using clustered standard errors	0.00	0.03	0.03	0.06	−0.01	0.57	1,074
	(1.00)	(0.96)	(0.07)	(0.09)	(0.09)		
Food security index							
Using robust standard errors	0.00	−0.03	0.01	0.11	−0.10	0.09*	1,071
	(1.00)	(0.96)	(0.07)	(0.10)	(0.10)		
Using clustered standard errors	0.00	−0.03	-0.01	0.08	−0.11	0.11	1,071
	(1.00)	(0.96)	(0.08)	(0.10)	(0.10)		

Notes: The estimates in Column (3) are from Equation 1, whereas Columns (4) and (5) are from Equation 2. All regressions control for baseline household income, baseline labor supply, Biden Child Tax Credit amount, number of people and number of children in the household, and respondent characteristics: Hispanic indicator, Black or African American indicator, age, and sex. We also control for if the respondent received any reminders and/or bonuses to complete the survey, as well as if they live in the same household with another respondent. Finally, we control for if the respondent was re-randomized in February 2021. All outcome variables are winsorized at the 5^{th} and 95^{th} percentiles. All regressions are weighted using sampling weights.

†The specification defined above assumes robust standard errors. For each primary outcome, we report the results using clustered standard errors at the household level in the second row. In that specification, we do not control for the indicator showing if the respondent live in the same household with another respondent.

Labor market participation is equal to one if hours of work is positive, and zero otherwise (regardless of reported earnings).

Standard errors in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A9: Treatment Effects—Housing Security Breakdown

	(1) Control mean (std. dev)	(2) Treatment effect: overall	(3) Treatment effect: bimonthly	(4) Treatment effect: quarterly	(5) p-value: bimonthly vs. quarterly	(6) N
Housing Security Index	0.00	0.29***	0.27***	0.30***	0.80	1,074
	(1.00)	(0.07)	(0.09)	(0.08)		
Likelihood of eviction	0.00	−0.37***	−0.38***	−0.36***	0.86	1,074
	(1.00)	(0.07)	(0.11)	(0.07)		
Number of months mortgage or rent is behind	0.00	0.06	0.06	0.06	0.96	1,074
	(1.00)	(0.07)	(0.09)	(0.09)		
Anyone in HH behind on mortgage or rent?	0.19	0.01	0.03	−0.01	0.41	1,074
	(0.39)	(0.03)	(0.04)	(0.03)		
Homeowners						
Housing Security Index	0.00	0.61***	0.90***	0.35	0.15	278
	(1.00)	(0.23)	(0.17)	(0.37)		
Likelihood of eviction	0.00	−0.52***	−0.52***	−0.53***	0.98	278
	(1.00)	(0.14)	(0.19)	(0.18)		
Number of months mortgage or rent is behind	0.00	0.36**	0.24	0.45	0.51	278
	(1.00)	(0.17)	(0.15)	(0.29)		
Anyone in HH behind on mortgage or rent?	0.08	0.10*	0.08	0.11	0.79	278
	(0.27)	(0.05)	(0.06)	(0.07)		
Renters						
Housing Security Index	0.00	0.31***	0.28**	0.35***	0.57	658
	(1.00)	(0.08)	(0.11)	(0.10)		
Likelihood of eviction	0.00	−0.36***	−0.35***	−0.38***	0.89	658
	(1.00)	(0.09)	(0.13)	(0.09)		
Number of months mortgage or rent is behind	0.00	−0.06	−0.07	−0.05	0.89	658
	(1.00)	(0.08)	(0.09)	(0.10)		
Anyone in HH behind on mortgage or rent?	0.26	−0.01	0.01	−0.04	0.47	658
	(0.44)	(0.04)	(0.06)	(0.04)		

Notes: The likelihood of eviction is based on a 4-point Likert scale. It is, along with the number of months mortgage or rent is behind, standardized based on the control group. For the breakdown for homeowners and renters, the standardization is redone for each subsample separately. The third component of the housing security index is a dummy, taking the value of 1 if there is anyone in the household behind on mortgage or rent payment and 0 otherwise. The estimates in Column (2) are from Equation 1, whereas Columns (3) and (4) are from Equation 2. All regressions control for baseline household income, baseline labor supply, Biden Child Tax Credit amount, number of people and number of children in the household, and respondent characteristics: Hispanic indicator, Black or African American indicator, age, and sex. We also control for if the respondent received any reminders and/or bonuses to complete the survey, as well as if they live in the same household with another respondent. Finally, we control for if the respondent was re-randomized in February 2021. All regressions are weighted using sampling weights.
Standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A10: Heterogeneous Treatment Effects by Single Motherhood

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	
			Heterogeneity regression Treatment effect: overall		Heterogeneity regression Treatment effect: bimonthly and quarterly					
	Control mean (std. dev)	Control mean if single mother (std. dev)	Single Mother x Treated	All Others x Treated	Single Mother x Bimonthly	Single Mother x Quarterly	All Others x Bimonthly	All Others x Quarterly	Number of Single Mothers	N
Participated in labor market	0.73	0.68	-0.00	-0.06	-0.01	0.00	-0.04	-0.08	240	1,074
	(0.44)	(0.47)	(0.07)	(0.04)	(0.07)	(0.10)	(0.05)	(0.05)		
Weekly labor supply in hours	26.41	21.69	6.43*	-1.84	3.37	10.30*	-1.27	-2.49	240	1,074
	(19.65)	(18.49)	(3.39)	(1.59)	(2.93)	(6.07)	(2.14)	(2.01)		
Weekly labor supply in hours, if > 0	35.96	31.94	9.57***	0.05	5.25**	15.72***	-0.22	0.26	160	765
	(13.49)	(13.21)	(3.12)	(1.47)	(2.60)	(5.03)	(2.17)	(1.58)		
Total income in the last 30 days including cash transfer, $	3,341	2,668	831***	-106	695**	1,002***	-100	-114	239	1,069
	(2,201)	(1,446)	(231)	(141)	(282)	(328)	(178)	(180)		
Total income in the last 30 days without cash transfer, $	3,341	2,668	317	-503***	160	515	-514***	-494***	239	1,069
	(2,201)	(1,446)	(228)	(138)	(278)	(318)	(169)	(180)		
Total expenditure in the last 30 days, $	2,945	2,671	-43	-366***	11	-111	-288**	-449***	238	1,062
	(1,378)	(1,262)	(176)	(108)	(182)	(277)	(143)	(135)		
Assets (non-housing, $)	36,371	23,994	4,284	-273	4,978	3,427	3,299	-4,062	240	1,074
	(27,955)	(23,618)	(3,151)	(2,120)	(3,799)	(4,295)	(2,877)	(2,484)		
Debt (non-housing, $)	19,142	14,094	5,604	-4,351***	7,192	3,602	-4,695**	-3,965*	240	1,074
	(22,797)	(17,866)	(3,746)	(1,646)	(5,758)	(3,317)	(1,991)	(2,114)		
Net assets (non-housing, $)	17,229	9,900	-1,321	4,077	-2,214	-176	7,994**	-97	240	1,074
	(29,631)	(24,922)	(4,757)	(2,488)	(7,318)	(4,240)	(3,313)	(2,974)		
Psychological well-being index	0.00	-0.11	0.16	0.03	0.07	0.28	0.01	0.06	240	1,072
	(1.00)	(0.93)	(0.15)	(0.08)	(0.19)	(0.18)	(0.10)	(0.11)		
Financial security index	0.00	-0.23	0.04	0.05	0.02	0.05	0.10	-0.01	240	1,074
	(1.00)	(0.87)	(0.12)	(0.08)	(0.16)	(0.14)	(0.10)	(0.11)		
Food security index	0.00	0.15	-0.32**	0.10	-0.21	-0.45**	0.20*	-0.00	240	1,071
	(1.00)	(1.01)	(0.16)	(0.08)	(0.21)	(0.18)	(0.11)	(0.11)		
Participation in unpaid work	0.64	0.75	0.11	-0.00	0.06	0.17**	0.01	-0.01	240	1,074
	(0.48)	(0.43)	(0.06)	(0.04)	(0.08)	(0.07)	(0.05)	(0.05)		
IPV Index	0.00	0.09	-0.00	-0.06	-0.15	0.19	-0.05	-0.07	240	1,074
	(1.00)	(1.13)	(0.19)	(0.08)	(0.17)	(0.35)	(0.10)	(0.09)		
Housing Security Index	0.00	-0.19	0.45***	0.24***	0.30	0.64***	0.27**	0.21**	240	1,074
	(1.00)	(1.22)	(0.15)	(0.08)	(0.20)	(0.15)	(0.11)	(0.10)		
Alcohol	17.20	9.43	0.39	-3.53	-1.23	2.43	-4.13	-2.91	238	1,062
	(32.90)	(24.79)	(3.43)	(2.52)	(3.93)	(4.78)	(3.26)	(3.08)		
Cigarettes/tobacco products	4.95	5.04	-2.44	-1.81*	-1.31	-3.86**	-2.86***	-0.69	238	1,062
	(14.28)	(14.76)	(1.83)	(0.97)	(2.41)	(1.82)	(1.02)	(1.31)		

Notes: Variable definition follows the Pre-Analysis Plan. Columns (1) and (2) present the overall control mean for each outcome and the control mean of the outcome for the subsample of single mothers in the baseline, respectively. The estimates in columns (3) and (4) follow from the Equation 3. All regressions control for baseline household income and labor supply, whether the household received the Biden Child Tax Credit, number of people in the household, number of minors in the household, an indicator for the respondent being Hispanic, an indicator for the respondent being black or African American, an indicator for the respondent being a single mother, age, and sex. We also control for if the respondent received any reminders and/or bonuses to complete the survey as well as if they live in the same household with another respondent. Finally, we control for if the respondent was re-randomized in February 2021. All outcome variables are winsorized at the 5^{th} and 95^{th} percentiles. All regressions are weighted using sampling weights.
Labor market participation is equal to one if hours of work is positive, and zero otherwise (regardless of reported earnings).
Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A11: Heterogeneous Treatment Effects by Race

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Main regression (values from Tables 4–7)			HET regression (single treatment dummy)				
	Control mean (std. dev.)	Treatment effect: overall	Control mean if Black (std. dev.)	Black × Treated	Non-Black × Treated	p-value: Black vs. Non-Black	Number of Blacks	N
Participated in labor market	0.73	-0.05	0.71	-0.05	-0.04	0.88	324	1,074
	(0.44)	(0.03)	(0.46)	(0.05)	(0.04)			
Weekly labor supply in hours	26.41	0.03	24.93	-0.64	0.29	0.76	324	1,074
	(19.65)	(1.48)	(20.29)	(2.46)	(1.80)			
Weekly labor supply in hours, if > 0	35.96	1.97	35.35	0.82	2.39	0.59	222	765
	(13.49)	(1.38)	(14.63)	(2.45)	(1.63)			
Total income in the last 30 days including cash transfer, $	3,341	92	3,043	75	99	0.93	322	1,069
	(2,201)	(126)	(2,292)	(214)	(152)			
Total income in the last 30 days without cash transfer, $	3,341	-333***	3,043	-326	-336**	0.97	322	1,069
	(2,201)	(123)	(2,292)	(208)	(149)			
Total expenditure in the last 30 days, $	2,945	-302***	2,598	-147	-363***	0.27	318	1,062
	(1,378)	(93)	(1,482)	(160)	(114)			
Assets (non-housing, $)	36,371	308	31,288	-1,394	982	0.52	324	1,074
	(27,955)	(1,817)	(27,448)	(2,957)	(2,258)			
Debt (non-housing, $)	19,142	-2,190	20,320	1,853	-3,790**	0.08*	324	1,074
	(22,797)	(1,595)	(23,786)	(2,761)	(1,866)			
Net assets (non-housing, $)	17,229	2,498	10,968	-3,247	4,772*	0.10	324	1,074
	(29,631)	(2,258)	(31,939)	(3,951)	(2,738)			
Psychological well-being index	0.00	0.05	-0.06	-0.09	0.11	0.18	324	1,074
	(1.00)	(0.07)	(1.05)	(0.13)	(0.09)			
Financial security index	0.00	0.03	-0.13	-0.17	0.11	0.04**	324	1,074
	(1.00)	(0.07)	(0.96)	(0.12)	(0.08)			
Food security index	0.00	0.01	0.02	-0.13	0.06	0.24	323	1,071
	(1.00)	(0.07)	(1.03)	(0.14)	(0.09)			
Housing Security Index	0.00	0.29***	-0.19	0.11	0.36***	0.21	324	1,074
	(1.00)	(0.07)	(1.19)	(0.18)	(0.06)			
IPV Index	0.00	-0.04	0.24	-0.26*	0.04	0.07*	324	1,074
	(1.00)	(0.07)	(1.30)	(0.14)	(0.09)			
IPV (from list experiment)‡	0.10	-0.20**	1.45	-0.50***	-0.08	0.03**	322	1,070
	(0.07)	(0.09)	(1.04)	(0.17)	(0.10)			

Notes: Columns (1) and (2) are from Tables 4–7, showing the overall control mean and the treatment effect for each outcome, respectively. Column (3) presents the control mean of the outcome for the subsample of Black respondents. The estimates in columns (4) and (5) follow from the Equation 3. Column (6) reports the p-value obtained from the t-test where the null hypothesis is such that the estimates in columns (4) and (5) are equal. All regressions control for baseline household income, baseline labor supply, Biden Child Tax Credit amount, number of people and number of children in the household, and respondent characteristics: Hispanic indicator, Black or African American indicator, age, and sex. We also control for if the respondent received any reminders and/or bonuses to complete the survey, as well as if they live in the same household with another respondent. Finally, we control for if the respondent was re-randomized in February 2021. All outcome variables are winsorized at the 5^{th} and 95^{th} percentiles. All regressions are weighted using sampling weights.
Labor market participation in the first row is based on working hours only without any imputation, i.e., even if the respondent's earned income is positive, if the respondent put "0" hours for their working hours in the survey, we keep it as is.
The estimates for weekly labor supply in hours are conditional on labor market participation.
There are 324 Black respondents in our sample.
‡ For the list experiment results, the first column shows the difference in average number of activities between the "long" and "short" lists in the control group, which can be interpreted as the share of respondents in that group who experienced the outcome in question.
Standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A12: Heterogeneous Treatment Effects Among Respondents with Children, by CTC Receipt

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Main regression (values from Tables 4–7)			HET regression (single treatment dummy)				
	Control mean (std. dev.)	Treatment effect: overall	Control mean if CTC Recipient (std. dev.)	CTC Recipient × Treated	CTC Non-Recipient × Treated	p-value: CTC recipient vs. non-recipient	Number of CTC recipients	N
Participated in labor market	0.73	-0.05	0.75	-0.04	-0.05	0.86	387	813
	(0.44)	(0.03)	(0.43)	(0.05)	(0.06)			
Weekly labor supply in hours	26.41	0.03	26.92	2.40	0.43	0.58	387	813
	(19.65)	(1.48)	(19.52)	(2.32)	(2.76)			
Weekly labor supply in hours, if > 0	35.96	1.97	35.96	5.17**	3.40	0.59	278	565
	(13.49)	(1.38)	(13.52)	(2.04)	(2.62)			
Total income in the last 30 days including cash transfer, $	3,341	92	3,571	184	86	0.73	384	810
	(2,201)	(126)	(2,047)	(202)	(206)			
Total income in the last 30 days without cash transfer, $	3,341	-333***	3,571	-332*	-362*	0.91	384	810
	(2,201)	(123)	(2,047)	(196)	(205)			
Total expenditure in the last 30 days, $	2,945	-302***	3,164	-273*	-381**	0.63	384	806
	(1,378)	(93)	(1,292)	(147)	(164)			
Assets (non-housing, $)	36,371	308	37,509	-1,847	2,343	0.30	387	813
	(27,955)	(1,817)	(26,787)	(2,561)	(3,273)			
Debt (non-housing, $)	19,142	-2,190	18,732	-1,043	-6,196***	0.16	387	813
	(22,797)	(1,595)	(21,386)	(2,994)	(2,165)			
Net assets (non-housing, $)	17,229	2,498	18,777	-804	8,539**	0.07*	387	813
	(29,631)	(2,258)	(29,419)	(3,682)	(3,672)			
Psychological well-being index	0.00	0.05	0.06	0.06	0.11	0.74	386	812
	(1.00)	(0.07)	(0.97)	(0.12)	(0.12)			
Financial security index	0.00	0.03	-0.04	0.09	0.07	0.91	387	813
	(1.00)	(0.07)	(0.96)	(0.12)	(0.10)			
Food security index	0.00	0.01	0.06	0.05	-0.04	0.57	386	812
	(1.00)	(0.07)	(1.00)	(0.12)	(0.13)			
Housing Security Index	0.00	0.29***	0.06	0.32***	0.31***	0.93	387	813
	(1.00)	(0.07)	(0.89)	(0.08)	(0.12)			
IPV Index	0.00	-0.04	-0.04	0.05	-0.03	0.64	387	813
	(1.00)	(0.07)	(0.94)	(0.13)	(0.12)			
IPV (from list experiment)‡	0.10	-0.20**	1.34	-0.34**	-0.01	0.10	385	811
	(0.07)	(0.09)	(0.98)	(0.14)	(0.14)			

Notes: Columns (1) and (2) are from Tables 4–7, showing the overall control mean and the treatment effect for each outcome, respectively. Column (3) presents the control mean of the outcome for the subsample of respondents who are CTC recipients. The estimates in columns (4) and (5) follow from the Equation 3. Column (6) reports the p-value obtained from the t-test where the null hypothesis is such that the estimates in columns (4) and (5) are equal. All regressions control for baseline household income, baseline labor supply, Biden Child Tax Credit amount, number of people and number of children in the household, and respondent characteristics: Hispanic indicator, Black or African American indicator, age, and sex. We also control for if the respondent received any reminders and/or bonuses to complete the survey, as well as if they live in the same household with another respondent. Finally, we control for if the respondent was re-randomized in February 2021. All outcome variables are winsorized at the 5^{th} and 95^{th} percentiles. All regressions are weighted using sampling weights. Labor market participation in the first row is based on working hours only without any imputation, i.e., even if the respondent's earned income is positive, if the respondent put "0" hours for their working hours in the survey, we keep it as is.

The estimates for weekly labor supply in hours are conditional on labor market participation.

Among respondents with children ($N = 813$), 387 are CTC recipients.

‡ For the list experiment results, the first column shows the difference in average number of activities between the "long" and "short" lists in the control group, which can be interpreted as the share of respondents in that group who experienced the outcome in question.

Standard errors in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A13: Treatment Effects on Home Equity for Homeowners

	(1)	(2)	(3)	(4)	(5)	(6)	
	Control mean (std. dev.)	Treatment mean (std. dev.)	Treatment effect: (overall)	Treatment effect: bimonthly	Treatment effect: quarterly	p-value: bimonthly vs. quarterly	N
Home equity	349,029.08	376,814.29	44,820.95	34,789.75	53,709.40*	0.66	268
	(166,283.97)	(169,850.49)	(27,621.16)	(39,923.59)	(30,552.00)		
Home value	473,387.84	485,778.61	26,848.23	3,297.39	47,716.16*	0.20	268
	(139,428.80)	(130,731.27)	(21,470.38)	(29,464.25)	(25,788.69)		
Mortgages or loans	124,358.76	108,964.32	−17,972.72	−31,492.36	−5,993.24	0.34	268
	(125,709.79)	(120,815.94)	(17,183.19)	(23,248.35)	(19,981.43)		

Notes: The estimates in Column (3) are from Equation 1, whereas Columns (4) and (5) are from Equation 2. All regressions control for baseline household income, baseline labor supply, Biden Child Tax Credit amount, number of people and number of children in the household, and respondent characteristics: Hispanic indicator, Black or African American indicator, age, and sex. We also control for if the respondent received any reminders and/or bonuses to complete the survey, as well as if they live in the same household with another respondent. Finally, we control for if the respondent was re-randomized in February 2021. All outcome variables are winsorized at the 5^{th} and 95^{th} percentiles. All regressions are weighted using sampling weights.
Out of 278 homeowners in our survey, 10 of them who declared "0" as their home value and reported positive rent are considered being renters. Them as well as the two respondent with both no rent and no home value are included when estimating the overall treatment effect for net and total assets but not when estimating the treatment effect on the home value and mortgages as the table presents these estimates only for homeowners with positive home value.
Standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A14: Treatment Effects—Follow-up Survey (Endline Survey Sample)

	(1) Control mean (std. dev.)	(2) Treatment mean (std. dev.)	(3) Treatment effect: overall	(4) Treatment effect: bimonthly	(5) Treatment effect: quarterly	(6) p-value: bimonthly vs. quarterly	N
***Tried to** spend more than before March 2021*							
Food & beverage	0.06	0.05	0.01	0.02	0.00	0.46	819
	(0.24)	(0.23)	(0.02)	(0.03)	(0.02)		
Alcohol & tobacco	0.00	0.00	0.00	0.00	0.00	0.85	819
	(0.07)	(0.06)	(0.00)	(0.01)	(0.01)		
Health insurance expenses	0.04	0.05	0.00	0.02	−0.02	0.07*	819
	(0.20)	(0.21)	(0.02)	(0.02)	(0.02)		
Elderly & child care	0.04	0.03	−0.02	0.00	−0.03*	0.21	819
	(0.18)	(0.18)	(0.02)	(0.02)	(0.01)		
Vehicle expenses	0.06	0.05	−0.01	0.02	−0.04**	0.04**	819
	(0.24)	(0.22)	(0.02)	(0.02)	(0.02)		
Rent, utilities & bills	0.05	0.09	0.04**	0.08**	0.00	0.02**	819
	(0.22)	(0.29)	(0.02)	(0.03)	(0.02)		
Overall more spending after March 2021	0.03	0.03	0.00	0.01	−0.01	0.32	819
	(0.17)	(0.18)	(0.01)	(0.02)	(0.01)		
***Ended up** spending more than before March 2021*							
Food & beverage	0.49	0.45	−0.03	−0.04	−0.01	0.73	819
	(0.50)	(0.50)	(0.05)	(0.06)	(0.06)		
Alcohol & tobacco	0.06	0.03	−0.03**	−0.04**	−0.03	0.49	819
	(0.24)	(0.17)	(0.01)	(0.02)	(0.02)		
Health insurance expenses	0.17	0.12	−0.06**	−0.04	−0.08**	0.36	819
	(0.38)	(0.33)	(0.03)	(0.04)	(0.04)		
Elderly & child care	0.16	0.09	−0.08***	−0.09***	−0.07**	0.61	819
	(0.36)	(0.29)	(0.03)	(0.03)	(0.03)		
Vehicle expenses	0.42	0.38	−0.02	−0.01	−0.04	0.65	819
	(0.49)	(0.49)	(0.04)	(0.05)	(0.05)		
Rent, utilities & bills	0.48	0.45	−0.04	−0.02	−0.06	0.55	819
	(0.50)	(0.50)	(0.05)	(0.06)	(0.06)		
Ended up spending more overall	0.54	0.54	0.01	0.01	0.01	0.96	819
	(0.50)	(0.50)	(0.05)	(0.06)	(0.06)		
Tried to pay down debt	0.52	0.50	−0.03	0.02	−0.08	0.20	819
	(0.50)	(0.50)	(0.05)	(0.06)	(0.06)		
How successful paying down debt	0.23	0.28	0.04	0.07	0.02	0.45	819
	(0.42)	(0.45)	(0.04)	(0.05)	(0.05)		
Increase in respondent's weekly work hours	0.17	0.22	0.02	0.02	0.03	0.94	819
	(0.38)	(0.41)	(0.04)	(0.05)	(0.05)		
Increase in other HH members' weekly work hours (mean)	0.14	0.13	−0.04	−0.04	−0.03	0.89	537
	(0.31)	(0.32)	(0.03)	(0.05)	(0.05)		
Increase in other HH members' weekly work hours (dummy)	0.20	0.15	−0.07*	−0.08	−0.06	0.75	537
	(0.40)	(0.36)	(0.04)	(0.05)	(0.05)		
Increase in respondent's weekly earnings	0.24	0.29	0.05	0.09	0.01	0.25	819
	(0.43)	(0.45)	(0.04)	(0.06)	(0.05)		
Increase in other HH members' weekly earnings (mean)	0.15	0.13	−0.03	−0.01	−0.06	0.44	537
	(0.33)	(0.31)	(0.04)	(0.05)	(0.05)		
Increase in other HH members' weekly earnings (dummy)	0.21	0.17	−0.06	−0.04	−0.08	0.56	537
	(0.41)	(0.37)	(0.04)	(0.05)	(0.06)		

Table A14: Treatment Effects—Follow-up Survey (Endline Survey Sample) (continued)

	(1) Control mean (std. dev.)	(2) Treatment mean (std. dev.)	(3) Treatment effect: overall	(4) Treatment effect: bimonthly	(5) Treatment effect: quarterly	(6) *p*-value: bimonthly vs. quarterly	N
Importance of the following factor in determining HH spending							
Getting out of debt	0.58	0.59	0.00	0.00	0.01	0.96	819
	(0.49)	(0.49)	(0.05)	(0.06)	(0.06)		
Attaining more reasonable work hours	0.50	0.52	0.00	−0.01	0.02	0.69	819
	(0.50)	(0.50)	(0.05)	(0.06)	(0.06)		
Having more time with family	0.68	0.62	−0.06	−0.05	−0.07	0.73	819
	(0.47)	(0.49)	(0.04)	(0.06)	(0.06)		
Attaining a higher level of education	0.49	0.54	0.04	0.06	0.02	0.60	819
	(0.50)	(0.50)	(0.05)	(0.06)	(0.06)		
Attaining a higher material standard of living	0.51	0.48	−0.07	-0.10^{*}	−0.04	0.43	819
	(0.50)	(0.50)	(0.05)	(0.06)	(0.06)		
Having more free time	0.53	0.42	-0.12^{***}	-0.14^{**}	−0.10	0.53	819
	(0.50)	(0.49)	(0.05)	(0.06)	(0.06)		
Taking care of family members	0.63	0.60	−0.05	−0.04	−0.05	0.84	819
	(0.48)	(0.49)	(0.05)	(0.06)	(0.06)		
Other	0.29	0.29	−0.03	−0.08	0.02	0.18	819
	(0.45)	(0.45)	(0.04)	(0.05)	(0.06)		
Tried to *spend less than before March 2021*							
Food & beverage	0.78	0.79	−0.01	0.01	−0.03	0.50	819
	(0.42)	(0.41)	(0.04)	(0.04)	(0.05)		
Alcohol & tobacco	0.58	0.58	−0.01	−0.04	0.02	0.45	819
	(0.49)	(0.49)	(0.05)	(0.06)	(0.06)		
Health insurance expenses	0.52	0.45	−0.06	−0.07	−0.06	0.93	819
	(0.50)	(0.50)	(0.05)	(0.06)	(0.06)		
Elderly & child care	0.51	0.45	−0.05	−0.06	−0.04	0.70	819
	(0.50)	(0.50)	(0.05)	(0.06)	(0.06)		
Vehicle expenses	0.77	0.70	-0.08^{*}	-0.10^{*}	−0.05	0.43	819
	(0.42)	(0.46)	(0.04)	(0.05)	(0.05)		
Rent, utilities & bills	0.72	0.65	-0.08^{*}	-0.12^{**}	−0.02	0.14	819
	(0.45)	(0.48)	(0.04)	(0.06)	(0.06)		
Overall less spending after March 2021	0.85	0.85	0.01	0.00	0.02	0.85	819
	(0.36)	(0.36)	(0.03)	(0.04)	(0.05)		
Ended up *spending less than before March 2021*							
Food & beverages	0.32	0.29	−0.06	−0.06	−0.06	0.91	819
	(0.47)	(0.45)	(0.04)	(0.06)	(0.05)		
Alcohol & tobacco	0.63	0.67	0.01	−0.03	0.06	0.19	819
	(0.48)	(0.47)	(0.04)	(0.06)	(0.05)		
Health insurance expenses	0.36	0.33	−0.02	−0.03	0.00	0.67	819
	(0.48)	(0.47)	(0.04)	(0.06)	(0.05)		
Elderly & child care	0.42	0.36	−0.06	−0.08	−0.04	0.57	819
	(0.49)	(0.48)	(0.04)	(0.06)	(0.06)		
Vehicle expenses	0.35	0.32	−0.03	0.00	−0.07	0.31	819
	(0.48)	(0.47)	(0.04)	(0.06)	(0.05)		
Rent, utilities & bills	0.23	0.20	−0.02	−0.03	−0.01	0.65	819
	(0.42)	(0.40)	(0.04)	(0.05)	(0.04)		
Ended up spending less overall	0.27	0.23	−0.06	−0.06	−0.06	0.94	819
	(0.44)	(0.42)	(0.04)	(0.05)	(0.05)		
Did not try to pay down debt	0.28	0.29	0.02	0.00	0.03	0.64	819
	(0.45)	(0.46)	(0.04)	(0.05)	(0.06)		
How unsuccessful paying down debt	0.49	0.45	−0.05	−0.07	−0.03	0.53	819
	(0.50)	(0.50)	(0.04)	(0.06)	(0.06)		

Table A14: Treatment Effects—Follow-up Survey (Endline Survey Sample) (continued)

	(1) Control mean (std. dev.)	(2) Treatment mean (std. dev.)	(3) Treatment effect: overall	(4) Treatment effect: bimonthly	(5) Treatment effect: quarterly	(6) p-value: bimonthly vs. quarterly	N
Decrease in respondent's weekly work hours	0.37	0.32	−0.02	−0.03	−0.02	0.90	819
	(0.48)	(0.47)	(0.04)	(0.05)	(0.05)		
Decrease in other HH members' weekly work hours (mean)	0.27	0.18	−0.09**	−0.09*	−0.08*	0.74	537
	(0.40)	(0.36)	(0.04)	(0.05)	(0.04)		
Decrease in other HH members' weekly work hours (dummy)	0.37	0.24	−0.13***	−0.16***	−0.10	0.34	537
	(0.48)	(0.43)	(0.05)	(0.06)	(0.06)		
Decrease in respondent's weekly earnings	0.37	0.37	0.02	0.00	0.05	0.54	819
	(0.48)	(0.48)	(0.04)	(0.05)	(0.06)		
Decrease in other HH members' weekly earnings (mean)	0.26	0.19	−0.09**	−0.10**	−0.07	0.54	537
	(0.40)	(0.36)	(0.04)	(0.05)	(0.05)		
Decrease in other HH members' weekly earnings (dummy)	0.36	0.25	−0.13**	−0.17***	−0.07	0.16	537
	(0.48)	(0.44)	(0.05)	(0.06)	(0.06)		
Insignificance of the following factor in determining HH spending							
Getting out of debt	0.09	0.09	0.01	0.02	−0.01	0.44	819
	(0.29)	(0.29)	(0.03)	(0.04)	(0.03)		
Attaining more reasonable work hours	0.13	0.11	0.00	0.01	−0.01	0.49	819
	(0.34)	(0.32)	(0.03)	(0.04)	(0.03)		
Having more time with family	0.06	0.08	0.01	0.01	0.02	0.68	819
	(0.23)	(0.26)	(0.02)	(0.03)	(0.03)		
Attaining a higher level of education	0.22	0.21	−0.02	−0.02	−0.01	0.77	819
	(0.42)	(0.41)	(0.04)	(0.05)	(0.05)		
Attaining a higher material standard of living	0.17	0.18	0.02	0.00	0.05	0.32	819
	(0.37)	(0.39)	(0.03)	(0.04)	(0.05)		
Having more free time	0.15	0.16	0.02	−0.01	0.05	0.27	819
	(0.36)	(0.37)	(0.03)	(0.04)	(0.04)		
Taking care of family members	0.07	0.10	0.03	0.03	0.04	0.74	819
	(0.25)	(0.30)	(0.03)	(0.03)	(0.04)		
Other	0.25	0.24	−0.02	−0.06	0.03	0.13	819
	(0.43)	(0.43)	(0.04)	(0.04)	(0.05)		
Total monthly housing costs during last survey, $	2,028.01	2,033.31	47.18	35.58	61.33	0.80	819
	(817.15)	(782.30)	(69.58)	(86.70)	(86.65)		

Notes: All variables except for the total monthly housing costs are originally 5-point Likert scales, 1 representing a lot of decrease in spending effort, actual spending, weekly working hours etc., 2 a little decrease, 3 no change, 4 a little increase and 5 a lot of increase. All dummies representing an increase are defined such that they take the value of 1 when the Likert scale is equal to 4 or 5 and 0 otherwise. Similarly, the dummies representing a decrease are defined such that they take the value of 1 when the Likert scale is equal to 1 or 2 and 0 otherwise.

The estimates in Column (3) are from Equation 1, whereas Columns (4) and (5) are from Equation 2. All regressions control for baseline household income, baseline labor supply, Biden Child Tax Credit amount, number of people and number of children in the household, and respondent characteristics: Hispanic indicator, Black or African American indicator, age, and sex. We also control for if the respondent received any reminders and/or bonuses to complete the survey, as well as if they live in the same household with another respondent. Finally, we control for if the respondent was re-randomized in February 2021. All regressions are weighted using sampling weights.

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A15: Descriptive Statistics by Household Structure

Household Category	(1) Female	(2) Male	(3) Hispanic	(4) Black	(5) CTC recipient	(6) Not CTC recipient	(7) Below-median income	(8) Above-median income	(9) Part-time worker	(10) Full-time worker
Without minors	20.12%	38.91%	20.14%	33.02%	0.00%	37.99%	26.22%	22.10%	25.74%	26.12%
With minors (not recipient's child)	28.26%	32.22%	31.27%	24.69%	26.61%	30.57%	28.46%	29.78%	35.44%	30.85%
Single-parent (recipient's child)	28.74%	8.37%	20.56%	32.10%	35.14%	18.05%	26.78%	21.72%	18.99%	21.89%
Two-parent (recipient's child)	22.87%	20.50%	28.03%	10.19%	38.24%	13.39%	18.54%	26.40%	19.83%	21.14%
Total	100.00%	100.00%	100.00%	100.00%	100.00%	100.00%	100.00%	100.00%	100.00%	100.00%

Notes: The table reports the distribution of the survey sample across household categories for gender, race, CTC-receipt status, income and employment status. Each row gives the values for every household category defined based on the number of children in the household and whether there is a spouse or not.