How effective is (more) money? Randomizing unconditional cash transfer amounts in the US

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Global Priorities Institute | November 2024

GPI Working Paper No . 28-2024

Please cite this working paper as: Jaroszewicz, A., Hauser, O. P., Jachimowicz, J. M. and Jamison, J. How effective is (more) money? Randomizing unconditional cash transfer amounts in the US. *Global Priorities Institute Working Paper Series, No. 28-2024*. Available at https://globalprioritiesinstitute.org/how-effective-is-more-money-randomizing-unconditional-cash-transfer-amounts-in-the-us-jaroszewicz-hauser-jachimowicz-and-jamison

GLOBAL PRIORITIES INSTITUTE

UNIVERSITY OF OXFORD

How Effective Is (More) Money? Randomizing Unconditional Cash Transfer Amounts in the US*

Ania Jaroszewicz, Oliver P. Hauser, Jon M. Jachimowicz, and Julian Jamison

Abstract

We randomized 5,243 Americans in poverty to receive a one-time unconditional cash transfer (UCT) of $2,000 (two months' worth of total household income for the median participant), $500 (half a month's income), or nothing. We measured the effects of the UCTs on participants' financial well-being, psychological well-being, cognitive capacity, and physical health through surveys administered one week, six weeks, and 15 weeks later. While bank data show that both UCTs increased expenditures, we find no evidence that (more) cash had positive impacts on our pre-specified survey outcomes, in contrast to experts' and laypeople's incentivized predictions. We test several explanations for these unexpected results. The data are most consistent with the notion that receiving some but not enough money made participants' (unmet) needs more salient, which caused distress. We develop a model to illustrate how receiving cash can sometimes also highlight its absence. (JEL: C93, D91, I30)

Keywords: Cash Transfers, Poverty, Welfare, Behavioral, Field Experiments

Living in poverty has been linked to a wide range of detrimental outcomes, including worse psychological well-being (Haushofer and Fehr, 2014; Ridley et al., 2020), poorer physical health (Braveman et al., 2010), and more limited cognitive capacity (Mani et al., 2013; Schilbach, Schofield and Mullainathan, 2016). Whether explicitly or implicitly, many researchers and policymakers have argued that providing people with more money—e.g., in the form of unconditional cash transfers (UCTs), "no strings attached" financial payments—should help address these issues and generally improve the recipients' outcomes. Indeed, there is good reason to think that this should be the case: Prior research in low-income

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countries has shown that cash transfers often (though not always) improve individuals' outcomes, for instance increasing consumption and food security.[1]

However, there is also reason to think that UCTs may not have uniformly positive effects, especially if they are of the relatively modest amounts often provided in high-income countries.[2] First, lacking financial resources can produce complicated and persistent issues, such as isolation and limited access to opportunities, which may be difficult to quickly address. Second, increases in objective wealth may not correspond to increases in subjective wealth and well-being, both because the correlation between these is only moderate (Gasiorowska, 2014), and because opportunities for upward mobility may at times generate their own social and psychological challenges (Sorokin, 1959; Friedman, 2016; Präg, Fritsch and Richards, 2022).[3] This opens the possibility for somewhat more nuanced effects. For instance, perhaps UCTs cannot address deep problems (e.g., depression), but are suitable for simpler ones (e.g., paying for groceries); or perhaps they do not have measurable short-term benefits, but help people avoid costly debts in the long run.

To better understand the effects of UCTs on low-income individuals, we collaborated with a non-profit organization to run a preregistered longitudinal field experiment in the US.[4] This study, conducted between July 2020 and May 2021, randomized 5,243 low-income Americans to receive either (1) \$0 (hereafter: "Control"; $N = 3,170$), (2) a one-time UCT of \$500 (roughly half a month's worth of median total household income; $N = 1,374$), or

[1] For largely positive effects, see e.g. Baird, McIntosh and Özler (2011); Miller, Tsoka and Reichert (2011); Robertson et al. (2013); Blattman, Fiala and Martinez (2014); Haushofer and Shapiro (2016); Hidrobo, Peterman and Heise (2016); Blattman, Jamison and Sheridan (2017); Handa et al. (2018); Baird, McIntosh and Özler (2019); Christian, Hensel and Roth (2019); Haushofer et al. (2019); Brooks et al. (2022); Karlan et al. (2022); Londoño-Vélez and Querubín (2022); Banerjee et al. (2023); Cañedo, Fabregas and Gupta (2023); Haushofer, Mudida and Shapiro (2023); Richterman et al. (2023); Wollburg et al. (2023); Aggarwal et al. (2024) and Gupta et al. (2024). Some mixed and null outcomes have also been reported—see, e.g., Berge, Bjorvatn and Tungodden (2015); Andersen, Kotsadam and Somville (2022); Banerjee et al. (2022); Bartos et al. (2022); Hussam et al. (2022) and Aiken et al. (2023).

[2] For instance, in 2020-2021, the US government gave most Americans UCTs in the form of three "economic stimulus checks," totaling \$1,200, \$600, and \$1,400. While substantive for a widespread policy, these amounts are just a fraction of what is often given in low-income countries in terms of purchasing power (Dwyer, Stewart and Zhao, 2023).

[3] This work, largely in sociology, has argued that such opportunities may generate tensions about which community one belongs to (Lee and Kramer, 2013; Curl, Lareau and Wu, 2018), uncertainty about one's identity (Hurst, 2010; Destin and Debrosse, 2017), and guilt that one received opportunities that others did not (Covarrubias, Romero and Trivelli, 2015).

[4] Preregistration and preanalysis plan: socialscienceregistry.org/trials/6149

(3) a one-time UCT of \$2,000 (two months' worth of income; $N = 699$). All participants took a baseline survey before being randomized and were invited to take post-treatment surveys one week, six weeks, and 15 weeks after the cash transfer. These surveys measured the participants' financial well-being (e.g., subjective financial stress, liquidity constraints), psychological well-being (e.g., depression, agency), cognitive capacity (e.g., a fluid intelligence measure, the extent to which the participant thought about money), and physical health (e.g., food security, sleep quality). We summarize these four outcome categories as four indices, which serve as our primary outcome variables. We also observe bank account balances and financial transactions during the trial for the 43% of participants who opted into providing that data. These data allow us to measure when and how the cash transfers were spent, as well as how much money was saved and for how long.

To measure people's priors on the potential effects of these cash transfers, we conducted an incentivized prediction study (Dreber et al., 2015) concurrently with data collection for the main study.[5] Importantly, at the time that we collected this data (November 2020 to January 2021), one-time UCT policies were fairly widespread (e.g., the US government's economic stimulus checks of 2020-2021) but there was still very little research on their effectiveness in high-income countries, allowing us to capture beliefs at a time before more recent research (e.g., Kluender et al. (2024); Vivalt et al. (2024)) became known. We recruited two samples, one of social scientists and policymakers ("experts"; $N = 477$) and another that was representative of the US population on standard demographics ("laypeople"; $N = 971$). Both groups made incentivized predictions about the outcomes of the field experiment, estimating the standardized effect sizes of both treatments (relative to Control) on each of the four survey outcome indices at each of the post-treatment surveyed time points. We find that both experts and laypeople predicted positive effects of both cash amounts on each of the indices at each time point, believing that the \$500 group would outperform the Control group, and that the \$2,000 group would outperform the \$500 group. Average effect size predictions (relative to Control) ranged from 0.16 to 0.65 *SDs*, depending on the treatment, index, and time point. For instance, experts predicted an effect size of 0.49 *SDs* for the

[5] At the time we launched the prediction study, we had collected only 1% of the post-treatment surveys from the main study. See Appendix Section A for details on the prediction study methods and results.

$2,000 group on the financial index one week after cash receipt.

In reality, however, our field experiment results reveal no positive effects of either cash amount on any of the preregistered survey outcomes. This is despite the fact that bank data show that participants spent their money well within the survey time periods, and seemingly primarily on bills and other necessities. In fact, at every post-treatment survey time point, both cash groups reported significantly *worse* outcomes than the Control group on the financial, psychological, and health survey indices (and no significant differences on the cognitive capacity index). We also find no differences between the $500 and $2,000 groups for any of the indices at any time point, and generally find few differences across the post-treatment time periods. These non-positive effects of either cash amount are robust to a wide range of alternative specifications. Subsequent analyses further reveal that the negative effects of cash appear to primarily be concentrated among self-reports of more subjective experiences of outcomes (e.g., how the participant evaluated a certain element of their lives) rather than self-reports of the more objective outcomes themselves (e.g., dollar amounts or the number of days in the past week that an event occurred). This pattern suggests that the cash did not actually produce worse outcomes in some objective sense, but nevertheless made some recipients *feel* worse.

What can explain the lack of positive effects of cash? First, we examine attrition carefully. We observe relatively high response rates for a UCT trial in a high-income country, with some variation across conditions: 80% of Control group participants, 90% of the $500 group participants, and 88% of the $2,000 group participants took at least one post-treatment survey. One could argue that, if the unobserved Control group participants had particularly bad post-treatment outcomes and/or the unobserved cash group participants had particularly good outcomes, our estimates could be biased downwards, masking a potentially more positive effect of cash. (Arguably, the opposite could be true instead, in which case the true effect of cash would be less positive than it already appears to be.) To test how attrition could have affected the direction and magnitude of our effects, we conduct a wide range of analyses, imputation exercises, and bounding exercises. We find that, in specifications where we observe negative effects of receiving cash, attrition could have made them appear more negative than they might otherwise have been. However, these analyses also demonstrate that it is *highly unlikely* that the effects of cash could have been meaningfully positive. That is, even once we take into account attrition, we are confident that the effects of receiving (more) cash are not positive on the outcomes we study.

We then investigate seven potential mechanisms to explain why we may not have seen positive effects. We test for strategic distortion of responses, reference dependence, harmful spending, mismatched expectations, negative inferences about the self, and declining social relationships. Although some of the tests provide possible evidence consistent with some of these mechanisms, we rule out most with reasonable confidence.

Instead, our data are most consistent with the following mechanism. Receiving cash may have made participants consider the ways in which they could spend that cash—i.e., think more deeply about existing financial obligations and potentially uncover new ones. This, in turn, could have caused distress, particularly if they discovered that these obligations were larger than previously thought and the windfall was insufficient to address them. In support of this mechanism, we find that participants who received cash thought more about money and how to spend it, reported needing more money to meet their household's obligations across a wider range of spending categories, and felt more overwhelmed by the needs of people outside their household. Depending on the analyses, these variables either partially or fully statistically mediate the effect of cash on the survey outcomes. This mechanism is consistent with literature documenting information aversion, and in particular that focusing attention on one's bad financial state can be unpleasant (Karlsson, Loewenstein and Seppi, 2009).

We rationalize these findings through an economic model that takes as a starting point an agent who chooses to optimally pay down obligations (e.g., debt) over two periods. Agents choose to be passive or active in managing their obligations, where passively managing these obligations avoids the psychological and economic costs of active management but prevents agents from noticing any financial shocks. We model the agent's best response to an exogenous windfall—akin to our cash treatments—and find that they are more likely to choose to actively manage more of their obligations when receiving the windfall. This, however, leads agents to experience lower utility in the first period because, for a non-trivial range of model parameters, the obligations they uncover are larger than they expect and the windfall is insufficient to address all obligations. Our model further hypothesizes that while agents initially feel worse, they experience higher lifetime utility from having reduced obligations earlier. The model also predicts that larger windfalls may attenuate or even reverse negative utility experienced in the first period.

Our results allow us to make several contributions to the literature. The first relates to providing more rigorous evidence on the effectiveness of UCTs in high-income countries (as opposed to low-income countries; see Footnote 1), particularly during challenging economic conditions. In high-income countries, existing studies have often been limited by non-experimental methods, relatively small samples, and/or outcome measures that are restricted in frequency or scope.[6] Moreover, although most research in high-income countries has suggested positive effects of cash, particularly for children, other studies have documented mixed or no detectable effects.[7] Further complicating interpretations, prior work has used a wide range of methods, with different trials varying whether the transfers are one-time (e.g., Pilkauskas et al. (2023)) or repeated (e.g., Vivalt et al. (2024)), whether the transfers are positive or intended to eliminate debt (e.g., Kluender et al. (2024)), the sample populations, and the outcome measures. These methodological differences and constraints, as well as the mixed results, have left open various questions about which outcomes different UCTs may affect, when, and why (or why not).

Our study administered one-time as opposed to frequently-repeated (e.g., monthly) transfers, distinguishing our work from other recent trials that provided smaller payments continuously (e.g., Agarwal, Cook and Liebman (2024); Gennetian et al. (2024)). Such one-time payments are important to study not only because they are pervasive (e.g., economic stimulus checks, the Earned Income Tax Credit, oil-state rebates), but also because—holding

[6]Non-experimental studies include those examining the effects of receiving government benefits or payments (e.g., Akee et al. (2010); Milligan and Stabile (2011); Dahl and Lochner (2012); Watson, Guettabi and Reimer (2019); Erten, Keskin and Prina (2022); Kovski et al. (2023); Pignatti and Parolin (2023) and Silver and Zhang (2023)) and the effects of winning a lottery (e.g., Kuhn et al. (2011); Cesarini et al. (2016); Lindqvist, Östling and Cesarini (2020) and Golosov et al. (2024)). Experimental studies include Salkind and Haskins (1982); Persaud et al. (2021); Dwyer and Dunn (2022); Troller-Renfree et al. (2022); Liebman et al. (2022); Dwyer et al. (2023); Agarwal, Cook and Liebman (2024), and Kluender et al. (2024).

[7]Those identifying mixed or no effects include Gardner and Oswald (2007); Evans and Moore (2011); Persaud et al. (2021); Jacob et al. (2022); Pilkauskas et al. (2023); Dwyer et al. (2023); Silver and Zhang (2023); Aizer et al. (2024); Bartik et al. (2024); Gennetian et al. (2024); Kluender et al. (2024), and Miller et al. (2024).

the amount of money constant—recipients may prefer them to smaller and more regular payments (Kansikas, Mani and Niehaus, 2023), which could plausibly increase the UCTs' effectiveness. Our study is also distinct from many others because rather than evaluating a single amount of a cash transfer versus no transfer, as most studies do,[8] we randomized UCT amounts, allowing us to test how effects may vary as a function of UCT size.

Among the studies on one-time payments, the two most similar (and concurrent) to ours provided low-income US households $1,000 UCTs during the pandemic (Jacob et al., 2022; Pilkauskas et al., 2023). In those studies, participants reported material hardship and mental health (and, in the case of Pilkauskas et al. (2023), parenting, child behavior, and partner relationships) one to three months later. Neither study finds an average treatment effect of cash, though Pilkauskas et al. (2023) find evidence of reduced material hardship among the poorest participants. It is plausible that somewhat more money, measuring outcomes on a shorter time frame, and/or measuring more outcomes would have uncovered positive results. Our experiment—which tested a UCT amount twice as large and measured a particularly wide range of outcomes over time using both surveys and administrative data—allows us to test such possibilities. We find that doubling the size of UCT payments (relative to past work) and measuring a wide range of outcomes only one week later shows no positive, and sometimes potentially even negative, effects. Overall, these findings suggest a narrower range of possible circumstances under which one-off UCTs could have detectably positive effects in similar contexts. Moreover, when viewing these results in conjunction with the prediction study, in which even experts were quite optimistic about the effectiveness of cash, our data challenge what may have been widespread and overly optimistic priors about the effects of UCTs in high-income countries. The results suggest that our posteriors about the effectiveness of similar cash transfer policies in similar settings should be somewhat tempered.

The second contribution relates to the psychology of poverty and scarcity, the feeling that one has fewer resources than one needs (Shah, Mullainathan and Shafir, 2012). While prior research has argued that having insufficient money can impose a range of emotional

[8]One exception is the negative income tax experiments conducted between 1968 and 1982 in the US and Canada (Widerquist, 2005).

and cognitive burdens (Schilbach, Schofield and Mullainathan, 2016; Shah, Mullainathan and Shafir, 2019; Ridley et al., 2020; Kaur et al., 2021), we find that providing additional resources does not necessarily alleviate these adverse effects and may in fact actually produce additional psychological strain for some.[9] Moreover, our work offers one potential explanation for when and why this may be the case: receiving (insufficient) money may in some cases bring to mind not only the needs and obligations that it *can* address, but also those that it *cannot*. Our results suggest that people's baseline perceptions of their obligations may at times capture only a subset of their actual obligations, and receiving a cash transfer may prompt them to engage with their finances more deeply and uncover more obligations. Viewed through the lens of our model, one way to interpret the experimental results is that, even though people were seemingly able to use the cash to address their needs, the psychological and transactional costs of uncovering a fuller but potentially unexpectedly bad view of their finances also caused some psychic disutility. Indeed, our findings and model are consistent with a contemporaneous paper testing the effects of medical debt relief, and awareness of that relief, on a range of outcomes (Kluender et al., 2024). The authors find no average effect of debt relief on mental health and even observe detrimental effects for those randomly assigned to receive phone calls drawing attention to the treatment, which (as noted by the authors) is consistent with our proposed mechanism. Our findings contribute to the literature on the psychology of poverty by beginning to uncover how and why poverty-relief policies (e.g., UCTs to low-income households or debt relief) may in some cases generate unintented negative effects on well-being.

2 Methods

The study was conducted in the US from July 2020 to May 2021, during the COVID-19 pandemic. It was run in close collaboration with a national non-profit organization that provides low-income people cash transfers. Figure 1 shows a timeline of the study.

[9] Other work finding little evidence for some of the hypothesized effects of scarcity include Carvalho, Meier and Wang (2016); Camerer et al. (2018); O'Donnell et al. (2021); de Bruijn and Antonides (2022) and Szaszi et al. (2023).

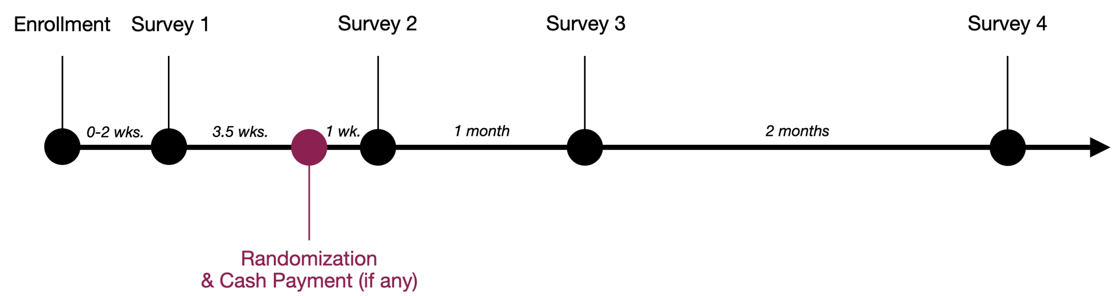

Figure 1: Timeline for the field experiment.

2.1 Enrollment

All participants had previously applied to the non-profit for COVID-19 relief funds. About 43% of participants received funds from the non-profit before the trial began. Of that 43%, 99% had received $500, with a median of 243 days between their pre-trial payment and their trial start date. The non-profit recruited participants from among this pool of applicants by email. The email advertised up to $100 in survey participation payments and a chance of winning $1,000 after the study was over (and thus, after all outcome measures were collected). The recruitment email did not mention the possibility of receiving $500 or $2,000, although some participants seemed to suspect or hope that they might receive money; see Appendix Section H.1. Enrollment was conducted across seven different "waves," such that participants in each wave began the study on different dates, but the treatment group randomization ratios and the time gaps between intervention points did not change. All participants provided informed consent.

Within a few weeks of enrolling, participants were sent the first of four surveys: the baseline or "t1" survey (see Section 2.3) Only participants who completed this survey were subsequently randomized. Column "t1" of Appendix Table C.1 shows enrollment by wave.

2.2 Randomization and treatment

Roughly 3.5 weeks after completing the baseline survey, participants were randomized into one of three treatment arms. The Control group ($N = 3,170$) did not receive any additional funds beyond the survey completion payments (and potentially payments for providing their bank data; see Section 2.4). The $500 group ($N = 1,374$) received $500 as a one-time UCT—

the equivalent of half of monthly median total household income (i.e., earned plus unearned income). The \$2,000 group ($N = 699$) received \$2,000: approximately two months' of median total household income. Funds were non-taxable and recipients could use the money however they wanted. In addition, at the non-profit's request, participants were cross-randomized to either receive or not receive access to certain new design features of the non-profit's online platform, which were separate from the financial aspects of the platform. As specified in our preanalysis plan, we do not analyze the effects of access to these additional features, but control for it in our regressions. (In any case, it had no effect on any of our outcomes.)

Participants were notified of UCT and survey payments by email. The UCT email additionally informed participants that they had been randomly chosen to receive the money. All payments were sent in one of two ways. For 90% of payments, the non-profit placed the money onto its online platform and allowed the participant to "pull" that money into their own external account (e.g., checking account). Initiating such a pull required just a few clicks, an indication of how much money they wanted to pull, and (optionally) what they intended to use the money for. Pull requests were typically fulfilled within a couple of days. For the remaining 10% of payments, the money was "pushed" directly to participants' external accounts.[10]

2.3 Surveys

We administered four surveys. As mentioned above, the "t1" or "baseline" survey occurred 3.5 weeks before the cash transfers. The next three (the "post-treatment surveys") occurred roughly one week ("t2"), one month and one week ("t3"), and three months and one week ("t4") after the cash transfer. This schedule meant that participants were always taking the surveys at the same time of the month, which we believed would minimize noise in survey responses by ensuring that regular monthly financial flows (e.g., rent payments or

[10]In some cases, the non-profit sent payments but the participant did not receive it in their external account, for instance if the participant was unaware of the money, if they wanted to keep it on the platform (e.g., as a "rainy day fund"), or there were errors (e.g., bank account information was incorrectly inputted). Although our primary analysis uses an intent-to-treat, we can verify that the non-profit sent participants the correct UCT amounts and that the cash groups received 87-89% of those payments in their external accounts. See Appendix Figure I.2 and Appendix Section H.3. Section 4.2.1 shows that our primary results are similar when conducting an analysis akin to a treatment-on-the-treated.

welfare benefit receipts) were always at the same time relative to the survey. Appendix Table C.1 shows completion rates for the post-treatment surveys (columns "t2" through "t4") by wave and treatment group, for a total of 16,747 survey responses after cleaning (see Appendix Section E.2 for details on the data cleaning). Participants were invited to complete all surveys; thus, non-response to an earlier post-treatment survey (e.g., t2) does not necessarily mean that participants did not respond to a later post-treatment survey (e.g., t3 or t4).[11] Participants received the surveys by email and completed them online. They typically had eight days to complete the survey, though these windows were extended at times, for instance when the deadline was on a weekend or public holiday.[12]

The four surveys had substantial overlap in content.[13] Each survey included the same questions on participants' financial, psychological, cognitive capacity, and health outcomes; following Anderson (2008), we constructed an index for each of these four categories. The variables within the indices were standardized and weighted, with higher values being "better" (i.e., indicating higher participant well-being).[14]

Specifically, the financial index is composed of savings stock, employment (Vivalt et al., 2024), work performance (if employed) (Kaur et al., 2021), work satisfaction (if employed; Leana and Meuris (2015)), earned income, subjective financial well-being (e.g., whether the participant felt behind on their finances; CFPB (2017)), and liquidity constraints (plausibility of securing $500 in the next three days; WorldBank (2015)).[15,16] The psychological index captures the participant's sense of agency (Lachman and Weaver, 1998); the extent to which

[11]On average, participants took 2.0 out of the three post-treatment surveys they received, with the Control group being less responsive than the cash groups. In Section 4.5 and Appendix Section F, we discuss the correlates of responsiveness and detail the potential role of attrition in explaining the results.

[12]Due to an implementation error, participants in the sixth wave received the UCT payment late, when the t2 survey window was already underway. To ensure that we could still measure the effects of cash one week after receipt, participants in this wave were sent a second t2 survey a week after they received the UCT (column "t2b" in Appendix Table C.1). While we do not include the first t2 survey in our main analysis, we return to analyzing this quasi-random variation in survey and intervention timing in Section 4.4.

[13]See the Social Science Registry (socialscienceregistry.org/trials/6149) for the study materials.

[14]See Appendix Section E.1 for details on the index contents and construction. Index items that are highly correlated with other items in the index receive relatively little weight, while items that are not highly correlated (and thus contain additional information) receive comparatively more weight. Results are robust to using Z-scored indices or examining variables individually; see Section 4.2.1.

[15]Our preanalysis plan specified using 95% winsorization on top values for unbounded variables (e.g., savings, income), which in this data is equivalent to using 90% winsorized values on the top and bottom.

[16]Self-reported measures are prevalent in this literature and our survey results come with the usual caveats. However, our administrative bank data allows us to partly validate at least the savings and income measures. We find they are generally well aligned; see Appendix Figure I.3 and Appendix Section H.2.

they were "living their best life" (Kahneman and Deaton (2010)); positive mental health (e.g., life satisfaction, how carefree they felt; Lukat et al. (2016)); how happy, anxious, and lonely they felt; and depression (PHQ-9; Kroenke, Spitzer and Williams (2001)). The cognitive capacity index (Mani et al., 2013; Shah, Mullainathan and Shafir, 2019) is composed of a measure of fluid intelligence (nine Raven's matrices (Bilker et al., 2012)), the participant's sense of their "everyday memory" (Royle and Lincoln, 2008), and the extent to which the participant had "money on the mind"[17] (Shah et al., 2018). The health index is constructed of self-reported general physical health, sleep quality, food security (USDA, 2012), nutrition (Gallup, 2017), and exercise (Gallup, 2017).

In addition, we included various exploratory measures, including time and risk preferences (Falk et al., 2022), social well-being (Gallup, 2017), relationship with one's partner or spouse, and self-assessed parenting quality (FragileFamilies, 2011). While we focus on the preregistered outcomes in the manuscript, Appendix Figures I.4 and I.5 display additional results for these exploratory measures.

2.4 Financial data

Our survey data is complemented by several rich sources of financial administrative data. The first dataset captures 23,357 payments the non-profit organization sent to all 5,243 participants. These payments include UCTs, survey payments, bonuses related to our study (e.g., the lottery earnings), and occasionally (2.8% of the time) payments unrelated to our study. When participants of any treatment group chose to "pull" money from their online accounts and indicated what they intended to use this money for, we observe these responses (8,438 responses).

We also observe whether participants received the money in their external accounts (17,646 attempted receipts across 5,135 participants). The vast majority (99.4%) of the

[17]Participants read hypothetical vignettes that were plausibly, but not necessarily, related to money, then rated how much they thought about money. For instance, one vignette describes a scene in which the participant needs to take an unexpected cab ride. They are asked to what extent they would have non-financial thoughts (e.g., "Should I have tried running instead?"), as well as one financial thought ("How much will this unexpected cab ride cost me?"). Participants responded to two different vignettes in each time period, one developed by Shah et al. (2018) and one developed by us. The mechanism results (Section 5.1) are robust to analyzing only the former.

attempted receipts were successful. For each successful money receipt, we observe the date, amount, and the money's final destination—that is, whether the participant chose to receive it in their bank account (95.9%), a virtual payment card (2.2%), or a physical payment card (1.9%). See Appendix Section E.3.

In addition, participants were invited to provide access to their bank account data; 43% of participants ($N = 2,261$) did so.[18] About 79% of these accounts are checking accounts, 10% are savings accounts, and 10% are Paypal accounts. These data capture two key outcomes. First, they show bank account balances, typically as one "snapshot" per bank account per day (after cleaning, we observe 357,134 bank-account-balance-days). Second, they show all transactions from the account, including both credits and debits (after cleaning, we observe 850,396 total transactions). For each transaction, we observe the amount, date, category (e.g., "Food and Drink," "Healthcare," "Bank fees"), and a more detailed description (e.g., "McDonald's," "Kids Dental Place," "Overdraft fee"). Our main analyses use 90% winsorized values (top and bottom) for the bank data, but results are robust to 95% winsorization (top and bottom). For details about these datasets, cleaning procedures, and data preparation, see Appendix Sections E.4–E.7.

3 Participants and evidence from financial data

3.1 Participants

As Table 1 demonstrates, we achieved balance across treatment groups for key demographics (joint F-test for differences across all variables shown: 1.25, p=0.152). The participant sample is majority female, majority non-White (74% of participants who identify as not exclusively White identify as Black or African-American), majority high school graduate, majority parents, and majority without a spouse or partner. Most lived in urban areas. Together, they represented 45 states and the District of Columbia.

[18] Participants were paid $10 for each account linked and were promised an increased probability of receiving cash transfers after the study was over. The likelihood of providing data for at least one bank account is positively associated with being in the $500 or $2,000 group, answering more post-treatment surveys, being younger, being a parent, and having a lower baseline financial index score. Baseline psychological index, cognitive capacity index, and health index scores do not predict the likelihood of providing bank data. See Appendix Table C.2.

Participant Baseline Demographics and Baseline Index Values (Means).

	Control	$500 Group	$2,000 Group	F	p-value
% Female	86	86	87	0.09	0.915
Age	35	35	36	1.68	0.187
% Non-White	77	80	78	2.78	0.062
% More than high school	59	61	60	0.43	0.654
Household size	3.8	3.7	3.7	2.99	0.051
% Parent	82	80	82	1.41	0.243
% Married/partner	43	42	44	0.32	0.728
% Employed	43	43	45	0.93	0.394
Savings stock ($)	405	470	373	1.87	0.155
Debt stock ($)	18,750	18,259	17,513	0.59	0.554
Earned income last mo. ($)	859	910	920	1.52	0.219
Unearned income last mo. ($)	517	535	517	0.34	0.713
% Under Federal Poverty Line 2019	51	49	49	1.05	0.351
Financial index at t1	0	0.055	-0.008	1.58	0.206
Psychological index at t1	0	0.054	0.013	1.39	0.248
Cognitive capacity index at t1	0	0.053	0.009	1.37	0.253
Health index at t1	0	0.079	0.054	3.25	0.039

Table 1: The F test statistic and p-value refer to a one-way ANOVA testing for differences across treatment groups. The index values for the Control group are 0 by construction.

Most participants were living in poverty. Our calculations indicate that about half were under the federal poverty line in 2019. From self-reports, the median earned household income in the month before the t1 survey was $414, median total household income was $1,028 (i.e., earned plus unearned income) and debt stock was on average 44 times larger than savings stock. Median savings reports were $0, and 81% reported having under $100. These numbers are largely corroborated by the bank account data. The median sum of all bank inflows (which can proxy for income) over the 30 days before t1 was $1,126; see Appendix Section H.2 and Appendix Figure I.3. Median bank balances at t1 were $0.86.

Table 1 also displays baseline (pre-randomization) index values for each group. We achieved balance on the financial, psychological, and cognitive capacity indices. For the health index, the cash groups had slightly but significantly higher baseline values than Control, which—if anything—makes the negative values we observe in the cash groups in the post-treatment surveys more notable.

3.2 Bank account balances and spending

For the subset of participants who provided access to their bank accounts, we can examine whether the cash transfers can be observed in their bank account balances and, more intriguingly, how long they stay there. Appendix Figure B.1 illustrates the bank account balances over time by treatment group. As expected, there were no differences in average daily bank account balances across treatment groups before the UCT. Immediately after the UCT was paid, it was reflected in daily bank account balances. In the first two weeks following payment, the $500 group on average had $43 more in their bank accounts, while the $2,000 group had $213 more (controlling for pre-UCT daily balances; see Appendix Table C.3).[19] However, the differences were short-lived. By the second two-week period, there was no statistically significant difference between the $500 group and Control; by the third two-week period, there was also no difference between the $2,000 group and Control.[20]

What explains the rapid closing of the gap between treatment and Control group bank account balances? One possibility is that UCT recipients spent the money; another is that they transferred it to bank accounts we do not observe. To disentangle these explanations, we turn to the financial transactions data. First, we find that while the Control group spent an average of $68.70 per day in the first two weeks following the UCT payment date (not controlling for pre-UCT spending), the $500 group spent an additional $26.33 per day ($p < 0.001$) and the $2,000 group spent an additional $81.67 per day ($p < 0.001$). That is, the $500 and $2,000 UCTs resulted in the recipients spending 138% and 219% of what would have been their regular daily spending amounts, respectively. These numbers correspond to a 74% and 57% marginal propensity to consume out of the $500 and $2,000 windfalls,

[19] The increase in bank balances was less than $500 for the $500 group and less than $2,000 for the $2,000 group because participants occasionally provided data access to bank accounts other than those into which they received their UCTs. We are thus averaging in a number of zeroes.

[20] Results are robust to controlling for the number of days between Wave 1's measurement and the participant's measurement instead of wave number, as well as to controlling for bank account type (e.g., depository, loan, credit) and subtype (e.g., checking, savings). Without controlling for average pre-UCT balance, the $2,000 group retains a significantly higher balance than Control through the fourth fortnight.

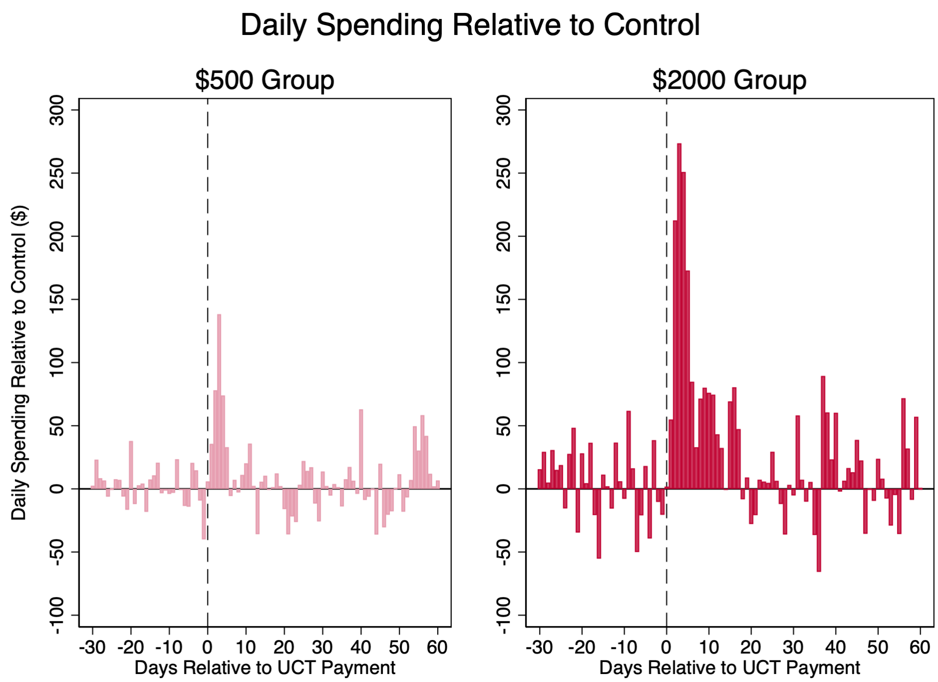

Figure 2: Daily spending of the $500 group (left) and $2,000 group (right), relative to Control, for participants who provided access to bank data. Positive values indicate that the given group spent more than Control. X-axis denotes days relative to when the participant's wave received its UCT.

respectively, just in the first two weeks following cash receipt.[21] By the second two-week period, however, the difference in spending drops substantially and loses significance (see Figure 2 and Appendix Table C.4).[22] We observe a similar pattern for transaction volume, debit volume, and net expenditures (debits minus credits); see Appendix Section H.4.

Because we observe bank data for only 43% of the sample, and the sample is self-selected, it is important to question whether these results also apply to the unobserved participants. When we restrict the sample to just the 43% and run the primary survey analyses, we find that our conclusions about the survey outcomes are virtually unchanged. This is consistent with the notion that the bank balance and financial transaction results would generalize to

[21]Empirical work has shown that marginal propensity to consume tends to be higher among low- (vs. high-) income recipients of cash transfers (Johnson, Parker and Souleles, 2006; Baker et al., 2023). See Egger et al. (2022) and Karger and Rajan (2020) for similar marginal propensity to consume figures among low-income UCT recipients.

[22]Results are robust to controlling for the number of days between the Wave 1 measurement and the particpant's wave's measurement instead of wave number, controlling for bank account type and subtype, and not controlling for pre-UCT spending.

the remainder of the sample.

3.2.1 Spending patterns

What did participants spend the study money on? To answer this question, we use two data sources. First, recall that participants who withdrew money from the online platform (whether the UCTs, survey payments, or both) were asked how they intended to use that money. Two coders blind to the hypotheses and treatment groups coded each of these responses (8,438 responses across 3,331 participants; see Appendix Section E.8). Figure 3 illustrates those codes. The cash groups were most likely to report intending to spend the money on general "bills" (which could not be further categorized into more specific bills, such as utilities or credit cards), groceries, and transportation, and were far more likely than the Control group to intend to use the money for bills and housing. While this data covers the majority of participants and provides insights into how participants may have earmarked the money, it also has some disadvantages. Participants may have felt uncomfortable reporting their true intended usage (Godoy, Karlan and Zinman, 2021), or they may have intended to use the money for one thing and ultimately used it for another. We thus complement this data with the more objective financial transactions data.

To analyze this data, we take the categories the financial services company generated to describe each transaction and regress daily spending in each of those categories (from the UCT date to the final day of the t4 survey) on treatment group dummies. Consistent with prior work (Misra, Singh and Zhang, 2022), the results reveal that much of the UCT money was spent on "transfers," a fairly broad category that includes digital payment vehicles (e.g., Venmo, Paypal), ATM withdrawals, and loading of pre-paid debit cards. Cash groups also spent significantly more on shops (e.g., Dollar Tree, Amazon), food and drink (grocery stores, restaurants), and travel (e.g., gas, parking fees); see Figure 4. We do not detect differences in the amount of bank fees charged (Stango and Zinman, 2014).[23] These results are robust to including a range of covariates, including daily spending before the UCT. When restricting to just the two weeks following the UCT payment, results are similar, although more poorly

[23]This is also true when restricting to what we call "bad" bank fees (insufficient fund fees, overdraft charges, cash advance fees, late payment fees, and generic bank fees)—as opposed to fees that are less likely to be an indication of financial stress, such as ATM withdrawal fees and foreign transaction fees.

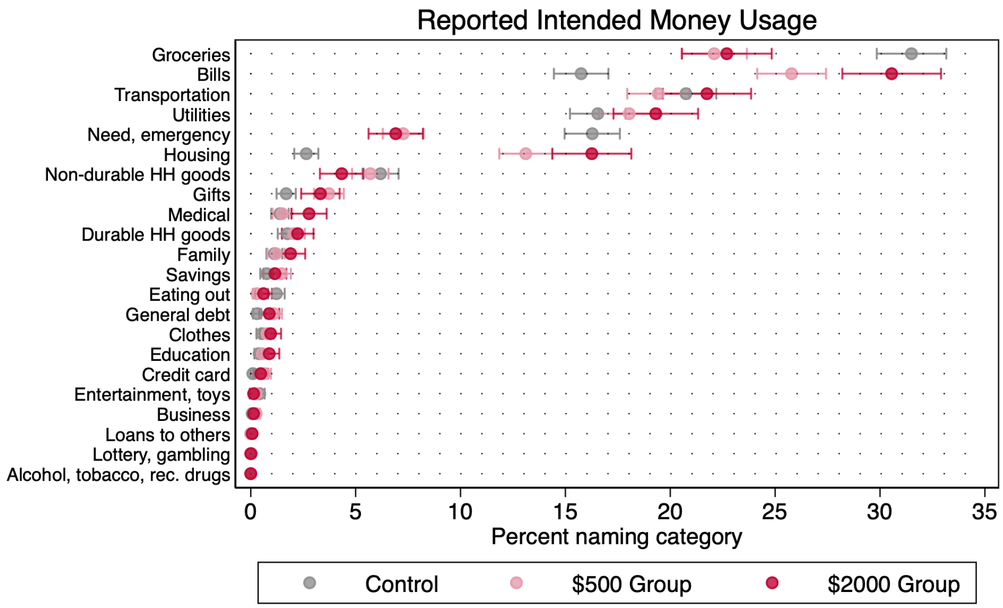

Figure 3: Participants' reported intentions of how they would use the money they were withdrawing. Each response could be coded as multiple categories. Excludes the "Other" category and missing values. HH=household, rec. drugs=recreational drugs. Circle denotes mean value, bars denote 95% CIs.

specified, given the rarity of transactions in certain categories (e.g., healthcare, taxes).

4 Evidence from survey responses

4.1 Analytical approach

We now turn to our preregistered survey outcomes. Our identification strategy is based on random assignment to treatment group and we use an intent-to-treat approach in our primary analyses. Our primary specification to estimate treatment effects is:

$$y_{i,t>1} = \beta_0 + \beta_1 500_i + \beta_2 2000_i + \beta_3 OP_i + \delta y_{i,t=1} + \epsilon_i \tag{1}$$

where y is one of four composite indices (financial, psychological, cognitive capacity, and health; Anderson (2008)) for individual i at time t and $t = 2$, 3, and 4 are the post-treatment surveys. "500" and "2000" are indicator variables that equal 1 if the participant was in the $500 or $2,000 group, respectively. The omitted category is the Control group. By construc-

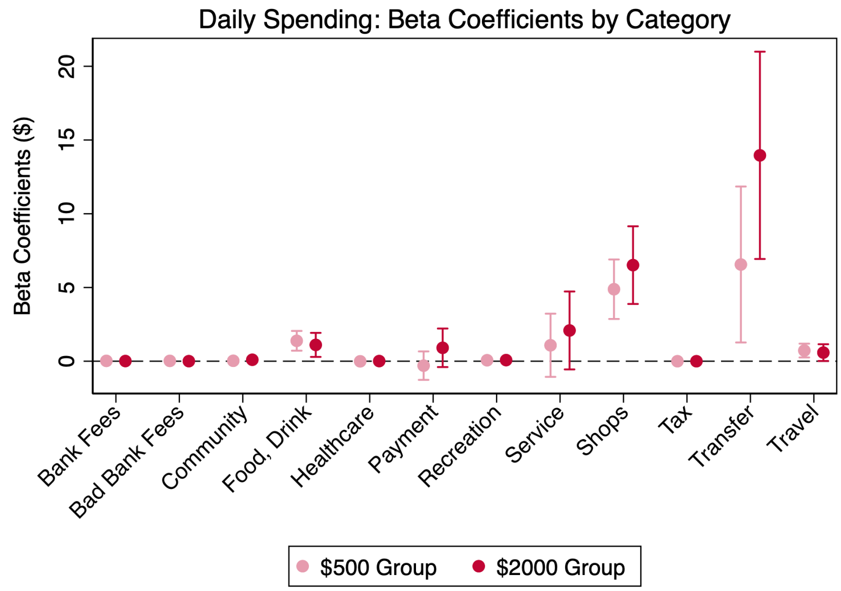

Figure 4: How much more the cash groups spent per day relative to Control, by spending category, for participants who provided access to bank data. Beta coefficients are from regressing average daily spending over the trial period on treatment group indicators with robust SEs. Error bars denote 95% CIs.

tion, the Control group average is zero for each index at each time point. OP is an indicator variable equaling 1 if the participant had access to the additional non-financial components of the non-profit organization's online platform (see Section 2.2). Finally, $y_{i,t=1}$ is the baseline measure of the index, included to improve statistical power (McKenzie, 2012).[24] Our primary analyses collapse across post-treatment time periods (i.e., the data are structured such that each row corresponds to a participant-time period, with up to three post-treatment rows per participant) and cluster robust standard errors at the participant level.

We conduct multiple hypothesis testing corrections in two ways. First, we control the false discovery rate using the Benjamini-Hochberg approach ("BH") (Benjamini and Hochberg, 1995). Second, we control the family-wise error rate using the Westfall-Young approach ("WY"; Westfall and Young (1993)), where the "family" of statistical tests are the eight parameters in our primary analyses (2 treatment groups $\times$ 4 indices). We apply BH and WY corrections to our preregistered survey outcome analyses (Section 4.2) and each robust-

[24]We did not administer Raven's matrices at baseline by design, to avoid a participant becoming more familiar with, and perhaps learning how to efficiently solve, the puzzles in the future. Thus, the baseline cognitive capacity index is constructed using only the other variables in this index.

ness check (Section 4.2.1). The WY corrections result in fewer hypotheses being rejected and the BH corrections yield very similar rejection conclusions as the unadjusted p-values. Because the primary conclusions are fairly similar with or without either set of corrections, for each analysis, we report the unadjusted p-values in the text for brevity and consistency. When the unadjusted p-values reach significance at $\alpha = 0.05$ but the WY- and/or BH-adjusted values do not, we indicate this.[25] Appendix Table C.5 reports the unadjusted, BH adjusted, and WY adjusted p-values for analysis when we aggregate across post-treatment time periods; Appendix Table C.6 reports the three sets of p-values when we disaggregate by post-treatment time period.

4.2 Survey results

Against our expectations, we find no evidence that (more) cash had a positive effect on self-reported survey outcomes for any of our predetermined specifications. In fact, both cash groups reported experiencing worse outcomes than the Control group on the financial ($\beta_{\$500} = -0.096, p < 0.001$; $\beta_{\$2000} = -0.058, p = 0.047$, not significant with WY correction), psychological ($\beta_{\$500} = -0.109, p < 0.001$; $\beta_{\$2000} = -0.130, p < 0.001$), and health indices ($\beta_{\$500} = -0.122, p < 0.001$; $\beta_{\$2000} = -0.143, p < 0.001$). On the cognitive capacity index, there were no statistically significant differences between the Control group and the cash groups, although the coefficients were negative, as well ($\beta_{\$500} = -0.049, p = 0.092$; $\beta_{\$2000} = -0.070, p = 0.061$). There were no statistically significant differences between the two cash groups for any of the indices (all $p \geq 0.228$). Figure 5 and Table 2 summarize our main results. Appendix Figures B.2 through B.5, I.4, and I.5 plot the results by individual variable (standardized as Z-scores and not weighted), visualizing both treatment group differences and how variable values changed over time. As explained in Section 4.5, attrition could have played some role in making the effects appear more negative than they might have otherwise been; however, we also show that positive effects are highly unlikely. We therefore describe our results as "non-positive."

Indeed, the effect sizes we observe are non-negligible. Appendix Section H.5 offers a back-

[25] There are a few cases where the BH adjusted values reach significance but the unadjusted values do not. We do not flag these in the text, but all values are available in Appendix Tables C.5 and C.6.

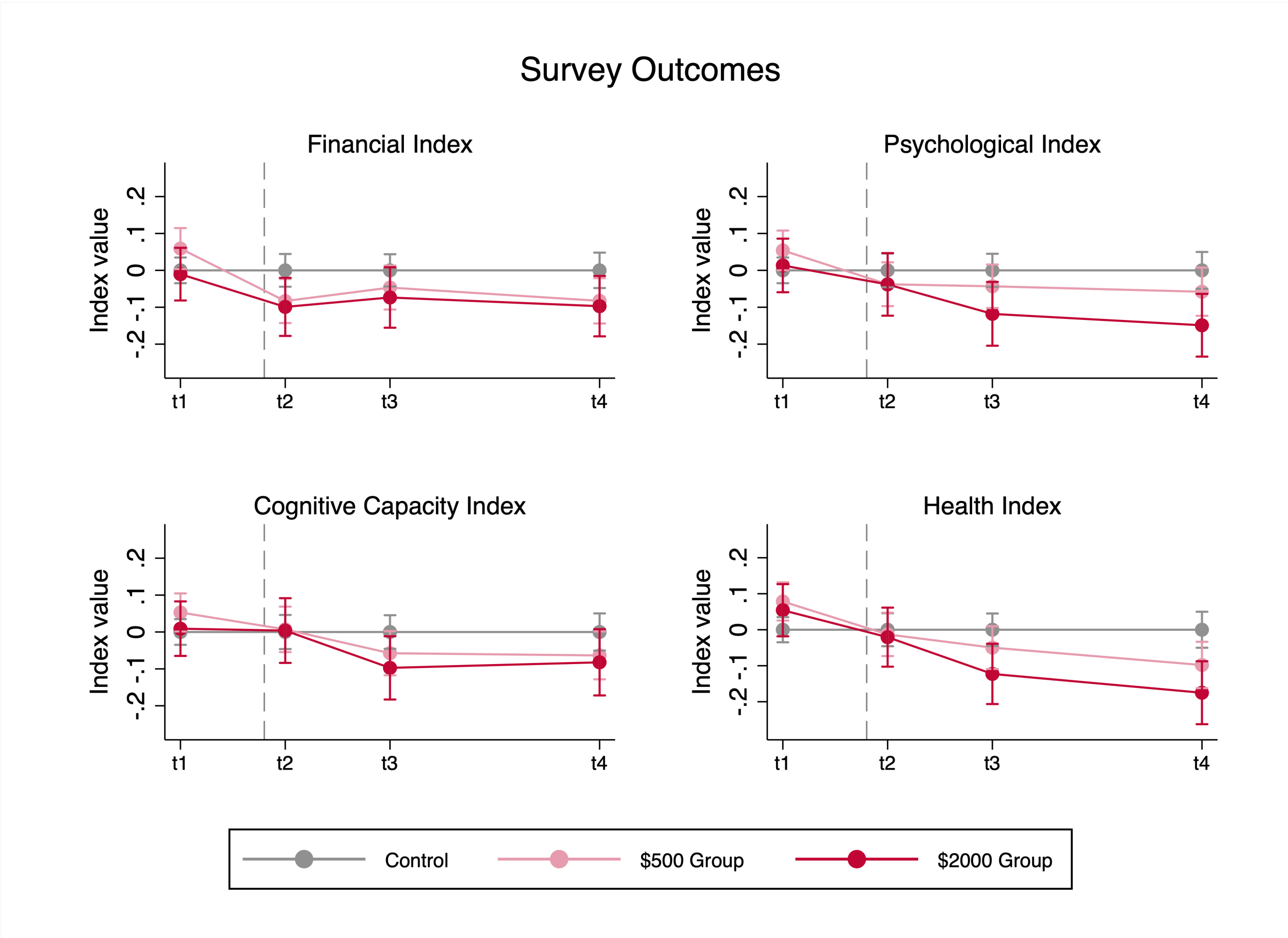

Figure 5: Main survey outcomes for the four prespecified indices. Vertical dashed line indicates intervention; t1 was the baseline survey before randomization and before receiving cash. Error bars denote 95% CIs.

of-the-envelope calculation to benchmark these index effects. Compared to losing one's job due to the pandemic (up to 14 months earlier), the relative impact of the UCTs in our study ranged from about one-quarter for the financial and psychological indices, to two-thirds for the health index, to unity for the cognitive capacity index.

It is also instructive to compare the observed treatment effects with the results of the prediction study, which captured experts' and a nationally representative sample's prior expectations about the effects of these treatments (for details, see Appendix Section A). Figure 6 shows how the 95% CIs of the preregistered RCT analyses compare to experts' predictions. The figure illustrates that many experts were too optimistic about the effectiveness of the

Effect of UCTs on Survey Indices.

	Fin.	Psych.	Cog. Cap.	Health
\$500 Group	-0.096	-0.109	-0.049	-0.122
	(0.022)	(0.024)	(0.029)	(0.024)
\$2,000 Group	-0.058	-0.130	-0.070	-0.143
	(0.029)	(0.031)	(0.037)	(0.030)
Fin. Index at t1	0.635			
	(0.012)			
Psych. Index at t1		0.623		
		(0.011)		
Cog. Cap. Index at t1			0.294	
			(0.013)	
Health Index at t1				0.611
				(0.012)
Online Platform	-0.014	-0.025	-0.024	-0.012
	(0.020)	(0.021)	(0.026)	(0.021)
Constant	0.016	0.043	0.019	0.030
	(0.017)	(0.018)	(0.022)	(0.018)
Observations	10271	9774	9582	9704
R^2	0.415	0.399	0.087	0.382

Table 2: OLS regressions. Collapsing across all post-treatment time points. Standard errors (in parentheses) are clustered at the participant level and robust.

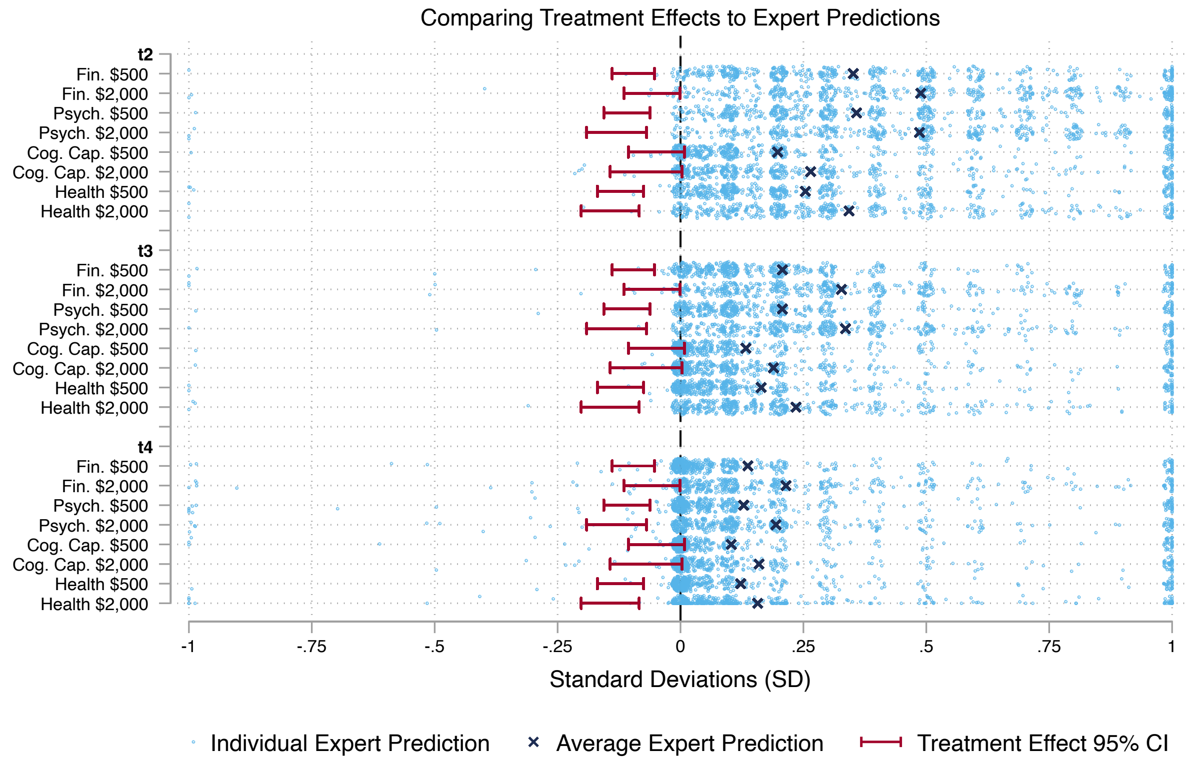

Figure 6: Comparison of the observed, preregistered treatment effects in the RCT with the incentivized predictions of experts in the prediction survey.

cash transfers, particularly for the t2 measurements (i.e., one week after treatment). The equivalent predictions of the laypeople—not plotted here—are even more optimistic; see Appendix Figure B.6 and Appendix Section A.2. The mismatch between predictions and the reality of the treatment effects holds when taking into account attrition; see Section 4.5.

4.2.1 Robustness

The non-positive effects of cash on the survey outcomes are robust to a wide range of checks. These include: (1) an analysis akin to a "treatment on the treated" where we retain only participants who correctly identified how much cash they had received from the non-profit ($N_{Control} = 1,951$; $N_{\$500} = 650$; $N_{\$2000} = 292$); (2) only retaining people who both correctly identified how much cash they received and answered all four surveys ($N_{Control} = 1,148$; $N_{\$500} = 485$; $N_{\$2000} = 215$); (3) controlling for additional covariates (wave number, gender, age, race, education, household size, parent status, partner status, employment at t1, savings at t1, debt at t1, last month's earned income at t1, last month's unearned income at t1, and a binary indicator for being under the federal poverty line in 2019); (4) dropping Wave 6 entirely; and (5) using Z-scored indices instead of the Anderson (2008) constructions. In all cases, neither treatment resulted in positive effects for any index; see Appendix Table C.5.

When running the analysis described in Section 4.1 but not controlling for the relevant baseline index value, we find that all treatment β values are still negative and half are statistically lower than 0. When splitting the data by time period rather than examining all post-treatment values together, we overwhelmingly still see negative effects but reject the null less often; see Appendix Table C.6.

In a final robustness exercise, we disaggregate the indices by variable and time, examining each of the 40 prespecified variables separately at each time point. We regress each of these variables (oriented such that higher values are better) on dummies for the two treatment groups separately at each of 3 post-treatment time points. Of the 240 β coefficients, 142 are not significantly different from 0, two are statistically significantly positive (using $\alpha = 0.05$), and 96 are statistically significantly negative (see Appendix Figures B.2 to B.5). This analysis suggests that the results observed at the level of aggregated indices were not driven by a few outlier variables.

4.3 How did receiving cash make participants feel?

To better understand how cash affected participants' life events—and, importantly, their experiences of those life events—we explore whether there were systematic differences in how participants responded to more objective versus subjective survey questions (Ackerman and Paolucci, 1983; Perrig-Chiello, Perrig and Stähelin, 1999). To this end, we conduct the following exercise (see Appendix Section H.6 for additional details): We first take all the survey questions where one end of the response scale would generally unambiguously indicate higher participant well-being (e.g., we include happiness and health, but not risk or time preferences). We then orient them such that higher values are better. Two independent coders then categorized each question as being more "objective" or "subjective." Objective questions measure quantifiable or countable outcomes that could in theory be verified if data were available (e.g., housing status, the number of days that an event occurred), while subjective questions capture how a participant felt about or experienced something in their life (e.g., how they rated their sleep quality in the past week, how anxious or stressed they felt).

We conduct two analyses to test whether the cash differentially affected participants' reports of their objective and subjective outcomes. The first focuses on effect size, regressing "objective" and "subjective" index outcomes on treatment group dummies. The second analysis focuses on significance testing, regressing each of the objective and subjective survey variables on binary indicators for each treatment group and counting the fraction of β coefficients where the cash recipients were significantly worse than Control.

The two analyses converge to the same conclusion: the negative effects are concentrated among the subjective outcomes. When examining the objective outcomes, the effects cannot be distinguished from 0. See Appendix Figure B.7. We interpret this as suggestive evidence that the cash had no effect (or a positive effect, if one considers the increase in bank account balances and spending) on the more objective outcomes, but a more negative effect on the more experienced, subjective outcomes. That is, although cash made people better off—or at least no worse off—objectively, it made them *feel* relatively worse off.

4.4 Heterogeneity by participant characteristics and time

We examine heterogeneity by participant characteristics, testing whether effects differ as a function of participants' demographics, financial position at t1, and/or psychological characteristics. We find very little evidence for any such heterogeneity; see Appendix Section H.7.

We also examine heterogeneity by survey time. The effects of cash appear relatively stable across our three post-treatment time periods for the financial and psychological indices (all $AnyCash \times t$ dummies: $p \geq 0.138$). Cash appears to produce weakly more negative effects over time for the cognitive capacity index (relative to $AnyCash \times t = 2$: $\beta_{AnyCash \times t=3} = -0.077, p = 0.044$, $\beta_{AnyCash \times t=4} = -0.076, p = 0.053$) and health index ($\beta_{AnyCash \times t=3} = -0.058, p = 0.061$, $\beta_{AnyCash \times t=4} = -0.109, p = 0.002$).

As shown in Appendix Table C.6, we find no evidence of cash improving outcomes even when we restrict our analyses to just t2 measurements, i.e., one week after UCT payment.

Of course, it is possible that cash had even shorter-lived effects in the first few days after cash receipt that then dissipated or changed by the seventh day. To test this possibility, we exploit the natural variation in timing caused by the Wave 6 treatment administration error (see Footnote 12), in which some participants received their UCTs *during* a t2 survey window. We conduct this analysis in two ways. First, we test UCT effects only for Wave 6 participants who responded to the (first) t2 survey one to six days after the UCT was sent. Second, we test the extent to which survey outcomes changed for Wave 6 participants who answered the two t2 surveys only about nine days apart with a UCT payment inbetween. Although the samples are self-selected and analyses are likely underpowered, neither analysis reveals positive effects of cash even on these shorter time frames. See Appendix Section H.8.

4.5 Can attrition explain the lack of positive effects?

Forty-six percent of participants responded to all three post-treatment surveys, 23% responded to two, 15% responded to one, and 17% did not respond to any. Of these 17%, about 18% provided bank account data. In total, thus, we have post-treatment survey data for 83% of our participants and other post-treatment bank data for an additional $0.17 \times 0.18 = 3$ percentage points of participants, for a total of 86%. See Appendix Table C.1. We only briefly summarize the potential role of attrition here but refer interested readers to more extensive analyses and discussion in Appendix Section F.

Could differential attrition explain our results? Post-treatment responsiveness was lower among the Control group (80%) than the $500 group (90%) and the $2,000 group (88%), but there was no significant difference between the two cash groups' responsiveness. It is plausible that differential attrition across the cash and Control groups could have biased estimates of the effects of receiving any cash—relative to no cash—downwards. That is, if the unobserved Control group participants had particularly bad post-treatment outcomes and/or the unobserved cash group participants had particularly good post-treatment outcomes, the real effects of cash could be more positive than we observe.[26] We consider several scenarios

[26]But note that the opposite could be true instead: if the unobserved Control group participants had particularly good outcomes and/or the unobserved cash group participants had particularly bad outcomes, the real effects of cash could be even more negative than we observe. See Appendix Section F.3 for a more detailed discussion.

that could produce such a pattern of results. These include variation in participants' baseline financial need, macroeconomic or public health conditions that affect survey uptake (e.g., COVID conditions or unemployment in their area), financial shocks after baseline, reciprocity or trust towards the experimenter, and differing beliefs about whether responding would yield financial support. In Appendix Section F.4, we statistically evaluate the merits of these scenarios. Overall, we find some limited evidence for the final scenario, but little to no evidence for the others.

Absent differential attrition, are there positive effects of more cash? While the Control group's survey uptake is lower, the response rates are similar and not statistically different between the two cash groups. This allows us to examine the effect of receiving more cash in the *absence* of differential attrition. As Figure 5 and Table 2 show, even quadrupling the cash amount—\$500 versus \$2,000—does not reveal positive effects. Thus, to the extent that differential attrition could explain the lack of positive effects between the Control and cash groups, no such argument can explain the lack of positive effects between the two cash groups.

Are certain types of participants more likely to attrit, leading to lack of positive effects? We examine a large set of potential predictors of responsiveness, operationalizing it in both a binary and continuous way. Importantly, Appendix Table C.7 and Appendix Section F.1 reveal that none of the indices at baseline predict responsiveness, nor do additional baseline financial characteristics. Age, race, and household size carry some predictive power. However, as shown in Section 4.2.1 and Appendix Table C.5, the results are unchanged when controlling for these and many other variables at baseline.

Is this response rate atypical? We observe one or more post-treatment survey responses from 83% of participants. If the response rate were substantially lower than in comparable studies, it could imply that we did not hear from a specific subset of participants who would have responded in another trial, which could affect differences in conclusions. However, our response rate is comparable to many recent cash transfer trials in North America, including Dwyer et al. (2023) (52%), Liebman et al. (2022) (95%), and Yoo et al. (2022) (93%), as well as the two studies most comparable to ours, Jacob et al. (2022) (65%) and Pilkauskas et al. (2023) (42% to 61%). Thus, to the extent to which our results differ from those of other trials, it is unlikely that the differences are driven by an unusual response rate.

Are participants who respond to the surveys systematically different across treatments? We conduct a selective attrition test to identify whether, conditional on response status, observable characteristics are balanced across treatment groups (Ghanem, Hirshleifer and Ortiz-Beccera, 2023). The null is a joint hypothesis of the equality of baseline outcome distributions between respondents in the three treatment arms, as well as attritors in the three treatment arms. Regardless of whether we focus just on the baseline index values or the baseline index values plus an array of covariates collected at baseline, the tests reveal that observable characteristics are balanced (Appendix Section F.2), helping to alleviate such concerns.

Are positive effects (mechanically) possible? To identify whether our data could, in theory, support the possibility of (more) positive effects, we calculate Lee bounds (Lee, 2009). While Lee bounds are quite conservative estimates and we do not assume that the upper or lower bounds are likely to be observed in the real world, for completeness, we present them here before turning to the question of how likely positive effects might actually be. As Appendix Section F.5 shows, it is indeed possible that attrition made the effects appear negative in certain specifications, when they might in fact be positive or indistinguishable from 0. When examining just the $2,000 group, the Lee bound 95% CIs rule out effect sizes higher than 0.09 SDs for the financial index, 0.16 SDs for the psychological index, 0.23 SDs for the cognitive capacity index, and 0.15 SDs for the health index (see Appendix Table C.8). Notably, the upper bounds of the 95% CI Lee bounds are still lower than 9 out of 12 (4 indices $\times$ 3 time periods) average expert predictions from our prediction study. This suggests that even making fairly extreme assumptions about missing data yields estimates that are only marginally positive and considerably more pessimistic than most experts believed.

Are positive effects likely? Importantly, the bounding exercises also reveal that is highly unlikely that the true effect of cash could have been meaningfully positive for all the indices. Specifically, using an inverted Horowitz-Manski bounding exercise (Horowitz and Manski, 2000; Baird, McIntosh and Özler, 2019), we calculate how extreme the missing participants' outcomes would have needed to be for us to conclude that cash had positive effects. We find that the gap between the missing cash and missing Control group participants would have needed to be between 0.4 and 0.7 SDs (depending on the index)—in the *opposite* direction of what we observe on average—for us to conclude that cash had a positive effect. To put these numbers into context, depending on the index, the 0.4 to 0.7 SDs is equivalent to 1.0–4.7× the (non-causal) "effect" of moving from below pre-treatment median income to above it. See Appendix Section F.5.

What effects are likely based on reasonable imputation? Per our pre-analysis plan, we employ a widely-used multiple imputation approach designed for time-series cross-sectional data (Honaker and King, 2010) to identify an outcome we would be likely to see if we were not missing any data. As Appendix Table C.9 and Appendix Section F.6 show, these analyses continue to reveal (weakly) negative effects for all four indices. Given that this approach suggests broadly negative results, and given the various robustness checks above that provide no compelling evidence or rationale to suggest positive results, overall we remain confident in the general conclusion that cash did not have positive effects.

5 Mechanisms

Why did (more) cash not have observable positive effects, as predicted by experts and laypeople? We examine seven possible explanations. We find relatively little (though not no) evidence for six of them: cash group participants strategically trying to sound needy to get more funds, cash group participants comparing their lives to a time when they still had money and feeling badly, cash group participants spending money in ways that harmed them, participants' expectations of how much money they would receive, receiving money from a "charity" made cash group participants feel poor, and cash group participants' social relationships declining. See Appendix Section G for our evidence on these mechanisms.

The final mechanism we explore—and the one that is most consistent with our data—relates to the saliency of financial obligations. If receiving cash made participants think about the ways in which they could spend that cash—i.e., if it led them to think about existing financial obligations and potentially uncover new ones—they could have been distressed by this, particularly if they found that these obligations were larger than expected and the cash windfall was insufficient to address them. Below, we review our empirical evidence for this mechanism (Section 5.1) and present a model that formalizes it (Section 5.2).

Although on net, our data are best aligned with this final mechanism, we note two caveats. First, this trial was not initially designed to test why cash may have no or negative effects on well-being; the analyses below, and those in Appendix Section G, are exploratory and not preregistered. Second, while we focus on the one mechanism that is most consistent with our data, we believe the effect could be multiply determined.

5.1 Empirical evidence

Money "on the mind." Shah et al. (2018) found that income was negatively associated with the extent to which people thought about money in hypothetical scenarios that were plausibly, but not necessarily, related to money. Based on this work, we expected that providing poor individuals with a positive shock to their finances through a UCT would decrease the extent to which they thought about money. However, we in fact find the opposite: both cash groups thought about money more rather than less (collapsing across scenarios in post-treatment surveys: $\beta_{\$500} = 0.152, p < 0.001$; $\beta_{\$2000} = 0.144, p = 0.001$).

Needs over the next 30 days. In t2, we asked participants to indicate whether they had enough money to pay for everything their household needed to pay for over the next 30 days and, if not, how much more money they needed. The cash groups indicated that they would need substantially more money than the Control group (Control mean=\$828, $\beta_{\$500} = \$120, p < 0.001, \beta_{\$2000} = \$192, p < 0.001$). These results might indicate that, relative to the Control group, the cash groups believed they had greater needs or obligations.[27]

Hypothetical stimulus check spending. A third piece of evidence comes from a different t2 question: "Imagine that the government decided to give everyone a \$500 stimulus check. If you got this money today, what are the MAIN thing(s) you would spend the money on?" Participants were then shown 18 categories (e.g., rent, groceries, paying off debts), of which they could select one or more. We find that the cash groups chose significantly more

[27] If the cash groups took on more debt in the week between the cash transfer and when they answered this question, they may have actually had greater obligations. However, we find no evidence of this (all $p \geq 0.496$).

spending categories than Control: $M_{Control} = 2.8, M_{\$500} = 3.0, M_{\$2000} = 3.4$ (both $p \leq 0.009$; see Appendix Figure I.6). One possible interpretation (though not the only one) is that the cash groups had a larger number of financial obligations salient to them.

Overwhelmed by others' needs. Participants were asked the extent to which they agreed with the statement, "Over the past week, I have felt overwhelmed or burdened by the financial needs of people outside my household." Relative to Control, the cash groups reported feeling more overwhelmed or burdened by others' needs post-treatment ($\beta_{\$500} = 0.101, p = 0.014$; $\beta_{\$2000} = 0.214, p < 0.001$). The effect sizes are similar when restricting to the t2 survey ($\beta_{\$500} = 0.129, p = 0.038$; $\beta_{\$2000} = 0.205, p = 0.008$). One interpretation of these results is that cash participants viewed supporting friends and family outside the household as a financial responsibility or domain to which they would like to contribute, but perhaps did not feel financially able to do so.

Spending decision stress. Finally, participants who had received UCTs were more likely to agree with the statement, "Over the past week, I have felt stressed by needing to decide how to spend the money I have" (for all post-treatment surveys: $\beta_{\$500} = 0.201, p < 0.001; \beta_{\$2000} = 0.163, p = 0.001$). These results are consistent with the notion that UCT recipients were thinking about their finances and how to optimally allocate their cash windfall. This finding is also consistent with prior work documenting negative effects of choice, e.g., through choice overload (Iyengar and Lepper, 2000) and regret (Sugden, 1985).

Mediation analysis. The five aforementioned variables (money on the mind, needs over the next 30 days, stimulus check spending, overwhelmed by others' needs, and spending decision stress) either partially or fully statistically mediate the effect of the treatment on the indices, depending on the analysis. Receiving a UCT significantly increased the values for all five variables (all $p \leq 0.014$), which in turn have either a negative or no relationship with the four indices when controlling for treatment group (for 15 out of 19 coefficients: $\beta < 0$, $p \leq 0.021$; for 4 out of 19: NS; "money on the mind" is not included as a mediator for the cognitive capacity index because of collinearity with the index). Finally, when we add the mediators to the regressions of index on UCT indicators, the effects of the UCTs on the indices weaken and sometimes lose significance. Appendix Table C.10 shows the mediation using the three variables that were measured in all post-treatment time periods.

These analyses suggest that the saliency of financial obligations, which may have stemmed from a deeper engagement with one's finances, could have played a role in explaining the non-positive treatment effects (though we urge caution in viewing these as necessarily causal paths given the inherent limitations of mediation analyses (Celli, 2022)).

5.2 Model

To better elucidate the mechanism that is most consistent with our data and to explain how and when it could lead to lower well-being for some people (at least in the short run), we propose a relatively simple discrete time model that captures how an agent allocates scarce resources to manage their finances. The basic structure of the model focuses on the fact that (re-)optimizing one's financial decisions in the face of new information and financial shocks has benefits, but also incurs costs, and hence that there is a tradeoff between "passively" following a predetermined plan and "actively" engaging with a potentially altered financial portfolio.

In the context of our field experiment, the model formalizes how receiving a positive financial windfall—referred to as a "bonus" below—can lead to an improved reallocation of resources new and old, but also to a realization that obligations that were previously (rationally) neglected are more serious than anticipated. As a result, agents in our model may initially experience net negative utility after receiving a windfall payment, despite the fact that the money has a positive direct effect on their cashflow and debt repayment.[28]

In addition to capturing key features and findings from our empirical study, the model also makes predictions that go beyond our current experiment and can help inform future studies. Below, we describe the model setup, the decision problem the agent is facing, and the intuition that we can derive from this model; the details are in Appendix Section D.

Setup. We study the financial management strategy of an agent who can choose to take a passive or active approach towards the repayment of a stock of debt and obligations, denoted by D. Taking an active approach towards some debt and obligations involves paying

[28] A similar effect can be found in the attention model of (Bolte and Raymond, 2024), although in their case agents exogenously receive lower emotional utility from focusing on dimensions with low payoffs, whereas in our case it is endogenous and—key to matching our empirical results—unexpected when it occurs.

an associated cost a to be able to observe any changes that might occur involving these debts and obligations. We assume a three-period setting, where the agent earns income 0, M_1, and M_2 in periods 0, 1, and 2, respectively; all variables are real and non-negative.

In period 0, which can be thought of purely as a planning phase, no money is earned and no payments are made. The agent provisionally assumes that they will be passive in period 1—it is generally optimal for the agent not to deal with their finances every period but rather to optimize an initial plan and then carry it out. In period 0, they decide what payment they would like to make towards the debt, denoted by $\overline{d}$. Unpaid debt or obligations may accrue additional costs over time, which—when dealt with passively—occur without the agent observing these accumulating costs (we will capture the idea of these negative financial developments more generally through a "shock" below). These accumulating costs could come, for instance, in the form of late fees, interest payments, or small problems becoming more serious over time (due to, e.g., deferring maintenance on car repairs or delaying preventative healthcare).

Period 1 stands in for a typical financial cycle (e.g., a month). In the background, Nature introduces a (negative) shock S at the beginning of period 1, which can take a value of either 0 or $s > 0$, with probabilities $1 - q$ and q, respectively. S captures negative events such as individual economic conditions worsening (e.g., late fees), but also general economic downturns (e.g., interest rates rising). Independently and more rarely, Nature also introduces a possible monetary bonus subsequently in period 1, which can take a value of either 0 or $b > 0$, with probabilities $1-p$ and p respectively. The bonus represents a positive windfall the agent might experience, such as the (probably somewhat rare) unconditional cash transfer from our treatment. The agent always observes the realization of B, but not necessarily the existence or true magnitude of the shock S. The associated probabilities p and q, respectively, are known to the agent.

Decision problem. After observing B at the start of period 1, the agent must choose whether to continue with this passive strategy, in which case the initial debt payment $\overline{d}$ will be implemented and prior expectations about the size of the shock remain in place, or to switch to an active strategy. If the agent chooses the active strategy, there is an associated fixed cost a, which may reflect economic costs such as time and effort, but may also include psychological costs such as facing potentially aversive information (Karlsson, Loewenstein and Seppi, 2009; Golman, Hagmann and Loewenstein, 2017). In choosing the active strategy, they also learn the true realized value of S and subsequently have the opportunity to re-optimize the payment towards the total debt, now choosing d^*.

The agent derives utility from consumption, with a per-period utility function $u(\cdot)$, which is assumed to be concave. As detailed in Appendix Section D, for tractability the utility function is assumed to be isoelastic with parameter η; however, any concave function will lead to similar dynamics. The agent's objective is to maximize their expected utility, and hence to equalize consumption across periods, ceteris paribus. Their problem is characterized by the initial choice of $\overline{d}$ in period 0, the decision to take an active or passive stance in debt management in period 1, and the choice of d^* if they decided to be active, also in period 1. Period 2 occurs much later and, during this final period, all agents are forced to pay a and any remaining debt. Essentially, period 2 is the point of reckoning, when all payments come in and go out and must balance.

Assumptions. Our model uses as a starting point a fully rational decision-maker who optimizes their expected utility over their lifetime. We keep our model classical in almost all regards with only one exception: we introduce the possibility of a behavioral type of agent that differs from the "benchmark" type only insofar as they mispredict the magnitude of the shock. While the benchmark agent correctly believes $\widetilde{s} = s$, the behavioral type optimistically believes $\widetilde{s} < s$. Our model makes minimal structural or behavioral assumptions about this systematic deviation from s. We introduce this behavioral type building on a wide range of behavioral patterns documented in the financial attention and decision-making literature. Behavioral patterns that could explain why an agent might have $\widetilde{s} < s$ include overoptimism of avoiding a negative shock (Brunnermeier and Parker, 2005; Howard et al., 2022) and mispredicting the ability to repay (growing) debt and obligations (Stango and Zinman (2009); Leary and Wang (2016); although see Allcott et al. (2022)).

Numerical approach. The model posits three periods with multiple decision points over time that may be contingent on the (potentially unobserved) realizations of B and S. We solve this decision problem with a numerical approach, using backward induction. To do so, we fix the value of several parameters in the model (across a spectrum of feasible levels) and only vary one key parameter at a time, including a (the cost the agent has to pay to actively manage their otherwise passive obligations) and $\widetilde{s}$.

We focus on the natural range of parameter values for which it is in the agent's interest to actively manage more of their obligations only after receiving a windfall $B = b$. (By construction, $a > 0$ implies that agents will not choose an active debt management strategy if $B = 0$ since they can already plan a maximizing value of $\overline{d}$ for precisely that scenario.)

Solution. In solving the model, we focus on the utility experienced by agents from the perspective of period 1—after receiving, or not receiving, the cash windfall—as that is the most pertinent comparison to our field experiment results. Note that our model also enables us to speak to later experienced utility (in period 2), which is outside the scope of our empirical setting but may prove useful for future research.

The findings from the model can be summarized as follows. For many values of a, both the benchmark and behavioral types choose to actively manage their debt when they receive the windfall, but not otherwise. Following their observation of the realized S, any agent who chose to be active re-optimizes their debt payments to d^*. However, while the benchmark type expected to find S to be the true magnitude s, the behavioral type expected $\widetilde{s} < s$. For a nontrivial range of reasonable parameters, this unhappy surprise more than offsets the positive impact of receiving $B = b$, causing this agent to have lower utility in period 1 than their counterpart who did not get a windfall (and hence also continues to expect a smaller shock). The benchmark type, on the other hand, is accurate in their perception of S and therefore does not experience an unhappy surprise after paying a; consequently, their utility is not negatively affected in period 1. Appendix Section D, and Appendix Figures D.10 and I.7, show the robustness of these results for a wide range of parameter values.

Interpretation and further predictions. Our model rationalizes and offers one possible explanation for why participants in our trial who received a UCT might have reported worse outcomes than participants who did not get a UCT. The model suggests that (behavioral) agents can experience negative utility shortly after receiving the cash windfall because, by choosing to re-optimize their debt management strategy after the windfall, they learn that they have more obligations than they previously thought, and thus they cannot consume as much as planned. Of course, this may not be true of everyone in our sample, but it can rationalize our aggregated empirical findings and shed new light on the potential challenges arising for low-income individuals when receiving cash transfers.

Importantly, our model also makes two predictions that we are not able to test in our current empirical setting, but that offer guidance for future research. First, the model predicts that sufficiently high cash payments will *not* result in negative utility in period 1. This is because, once the cash transfer is large enough, it will be sufficient to pay all obligations and as a result lead to positive utility in period 1 from having received the cash. Second, even if the cash is insufficient to cover all obligations in period 1, our model would still suggest that lifetime utility is optimized through the cash transfer. This means that agents should be better off (or, at the very least, not worse off) in the long run (represented by period 2 in the model) because they settled some of their obligations earlier, and thus those obligations did not get worse over time. Though we do not observe period 2 in our data, a future study may wish to explore this prediction empirically.

6 Discussion

This paper reported on a field experiment that provided people in poverty with \$2,000, \$500, or nothing. Surprisingly, the windfalls did not improve financial, psychological, cognitive capacity, or health survey outcomes, neither between the Control and cash groups, nor between the two cash groups. Our findings should be interpreted in light of several caveats. While we aimed to be relatively comprehensive in our outcome measurements, we may have missed some positive effects of cash, such as time investments into human capital or children's development. In addition, the study was conducted during the COVID-19 pandemic—a feature shared by several contemporaneous studies on this topic. Although this should not affect the study's internal validity, it may affect its generalizability to other contexts. Nevertheless, we believe our findings raise important questions—and begin to provide some answers—on poverty alleviation and cash transfers in high-income countries.

First, on which outcome(s) should poverty alleviation programs be optimizing? In particular, what should be the relative importance of objective financial outcomes (e.g., the ability to pay for pressing needs, pay down debt, or save) versus subjective well-being (e.g., how anxious or stressed a person feels)? If the goal is to increase the former, then simply providing cash to those in need likely accomplishes that goal. If the goal is, at least in part, to increase the latter, then the results from this study suggest that unrestricted one-off UCT payments of this magnitude in such settings may not always be the correct tool.

What, then, might the correct tool be? One option might be to increase the amount of money given (Balboni et al., 2022)—indeed, our model predicts that the observed negative psychological effects should disappear for sufficiently large cash transfers. A different approach might be to couple cash transfers with potentially more cost-effective and/or complementary resources, such as investments to the community or mental health support (Blattman, Jamison and Sheridan, 2017; Little et al., 2021). Furthermore, the way in which cash is delivered (e.g., payment timing) could be varied to better match the recipients' needs and preferences (Kansikas, Mani and Niehaus, 2023). Alternatively, one could support low-income households primarily through other means like in-kind benefits, skill building (Bandiera et al., 2017), or opportunities for rewarding work (Hussam et al., 2022).

Finally, even if insufficiently large cash windfalls produce no positive effects on subjective well-being, their benefits may still outweigh the costs. This may be particularly true if one considers positive externalities on others, such as the recipients' children or friends, and/or the recipients' preferences (Liscow and Pershing, 2022). It is our very strong suspicion is that if a group of low-income people (or, for that matter, any group of people) were given the option to have \$0, \$500, or \$2,000, nearly all would choose the \$2,000—even if they knew that it could have no or negative effects on subjective outcomes.

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