UNIVERSITY OF CALIFORNIA

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Windfalls of Time and Money: Effects on Wellbeing and Decision-Making

A dissertation submitted in partial satisfaction of the

requirements for the degree of Doctor of Philosophy

in Management

by

Colin Richard West

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ABSTRACT OF THE DISSERTATION

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Doctor of Philosophy in Management

University of California, Los Angeles, 2021

Professor Sanford E. DeVoe, Chair

This dissertation investigates how people respond to windfalls of time and money. From vacations and outsourcing, to bonuses and commissions – individuals and organizations frequently experience windfalls of time and money. These moments of temporary abundance provide a critical window into human psychology. I focus on these moments and what they reveal about psychological wellbeing and economic decision-making. In Chapter 1, I investigate the psychological benefits of providing people with large, unexpected windfalls of time or money. In Chapter 2, I examine how experiencing episodic financial windfalls can shape intertemporal preferences – trade-offs between smaller immediate rewards and larger delayed rewards. In Chapter 3, I investigate how people make decisions about whether to spend or save a financial windfall. Chapter 1 compares the benefits of windfalls of time versus money for people living in poverty. Researchers, organizations, and policymakers involved in poverty alleviation

have historically had a narrow focus on material and financial constraints, largely neglecting time poverty – the chronic feeling of having too many things to do and not enough time to do them. While a large number of studies have tested the effects of providing financial windfalls (via cash transfers), often finding significant benefits for psychological and economic wellbeing, there is little understanding of the consequences of time windfalls. To test whether windfalls of time may have overlooked benefits, I conducted a field experiment over six weeks in an urban informal settlement in Kenya with a sample of working mothers (*N* = 1550). I provided participants with weekly 'time transfers' (vouchers for laundry or meal services), which provided an extra 5-7 hours per week, on average. I compared the effect of time transfers against equivalently-valued cash transfers, which increased participants' earnings by an average of 33%, as well as a 'survey-compensation-only' condition, which increased earnings by 14%. I found that each condition led to similar increases in subjective wellbeing, reductions in perceived stress, and decreases in relationship conflict. Mediation analyses showed that cash transfers and time transfer produced these benefits via distinct psychological mechanisms. This work provides the first empirical comparison of windfalls of time versus money.

Chapter 2 investigates the effect of experiencing episodic financial windfalls on intertemporal preferences. Compared to 1970, far fewer people work in jobs that offer a stable, predictable income. Many households now experience highly volatility income streams, primarily due to fluctuations in within-job earned income, including shortfalls (such as cuts to shift work) and windfalls (such as bonuses and commissions). Such episodic windfalls are distinct from the entirely exogenous windfalls examined in Chapter 1. However, people might treat upswings in a volatile income stream similarly to a series of exogenous windfalls.

Past research has examined household strategies for managing income volatility, but there is little evidence on how experiencing income volatility can shape intertemporal preferences. Using individual-level panel data, I found that experiencing greater biannual income volatility over 27-years, from late adolescence through middle age, predicts subsequent financial impatience. This effect holds across the wealth spectrum, from households living paycheck-to-paycheck to those with substantial wealth. In an additional study, I zoom-in to examine the experience of monthly income volatility, which may be especially psychologically impactful due to the ways in which people budget. On this more granular time-scale, I found a stronger association between income volatility and impatience. I also found evidence for a moderating effect, such that income volatility only shapes intertemporal preferences when people feel like they have little control over income fluctuations. Lastly, I conducted a conceptual replication of this effect using a representative sample of Americans, stratified on income. I found a significant association between recent monthly income volatility and impatience, using an incentive compatible decision task. However, the moderating effect of perceived financial control does not replicate. These findings add to our understanding of the psychological consequences of resource scarcity by showing that, controlling for overall wealth, resource fluctuations can shape the formation of economic preferences.

Chapter 3 investigates how people allocate financial windfalls using their personal budget. Financial windfalls often provide an opportunity for individuals to contribute to their savings. Yet, people tend to be especially impatient when allocating a windfall, compared to allocating their typical income. Prior research has focused on budgeting as a tool to curb impulsive spending over time, documenting a wide array of budgeting techniques used to separate and earmark money. In this work, I show that different budgeting procedures can also have large, unintended effects on how people initially allocate a financial windfall. First, I found that under specific budgeting procedures people rely on a naïve diversification heuristic when making financial allocations. As a result, allocation decisions are biased depending on the partitioning of budget categories. I found that partitioning savings into multiple sub-categories led people to allocate significantly more money to saving, versus spending. Second, I separate the effects of budget partitioning from financial goal-setting. I show that partitioning effects are both distinct from and more impactful than setting savings goals. Indeed, I found that setting savings goals had no impact on subsequent windfall allocation decisions, whereas budget partitioning had a large effect.

People across the income spectrum report feeling a scarcity of both time and money. However, this general experience of scarcity is often punctuated by windfalls, whereby people have a temporary abundance of time or money. By examining how people respond to these windfalls, this work contributes to knowledge about how our fundamental resources of time and money shape psychological wellbeing and economic decision-making.

The dissertation of Colin Richard West is approved.

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TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

PREFACE

Chapter 1: Windfalls of Time Versus Money: A Field Experiment

A version of this chapter is under review: West, C. & Whillans, A.V. Alleviating Time Poverty Among the Working Poor. *Nature Human Behaviour*. I am the first author of this chapter with equal contributions from Ashley Whillans. We contributed equally to all aspects of the work including the initial conception, piloting, study design, writing the stage 1 manuscript that was accepted as a Registered Report at *Nature Human Behaviour* in June 2019, experiment implementation, data analysis, and writing the stage 2 manuscript that is now under peer review.

Chapter 2: Episodic Financial Windfalls: Effects of Income Volatility on Impatience

A version of this chapter is being prepared for publication: West, C. & DeVoe, S.E. Income Volatility and Financial Impatience Across the Wealth Spectrum. I designed the studies in collaboration with S. DeVoe; I collected the data, conducted the analyses, and prepared the manuscript. S. DeVoe provided analytic advice and edited the manuscript.

Chapter 3: Allocating Financial Windfalls: Budgeting as Choice Architecture

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West, C. & DeVoe, S.E. Income Volatility Increases Financial Impatience Across the Wealth Spectrum.

Bogard, J., **West, C.**, & Fox, C. Heuristics and Biases in Evaluations of Economic Inequality.

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Chapter 1: Windfalls of Time Versus Money: A Field Experiment

ABSTRACT

Poverty entails more than a scarcity of material resources—it also involves a shortage of time. To examine the causal benefits of reducing time poverty, we conducted a longitudinal field experiment over six consecutive weeks in an urban slum in Kenya with a sample of working mothers (*N*=1550), a population who is especially likely to experience severe time poverty. Participants received vouchers for services designed to reduce their burden of unpaid labor. We compared the effect of these vouchers against equivalently valued unconditional cash transfers (UCTs) and a survey-compensation-only control condition. Using a pre-post design, a pre-registered Bayesian ANCOVA indicated that the time-saving, UCT, and survey-compensation-only conditions led to similar increases in subjective wellbeing, reductions in perceived stress, and decreases in relationship conflict. Exploratory analyses revealed that the time-saving vouchers and UCTs produced these benefits through distinct psychological pathways. We conclude by discussing the implications for economic development initiatives.

Poverty is associated with lower engagement in preventative health care (even when access is available), lower medication adherence, increased spending on 'temptation goods,' reduced productivity at work, and lower adoption of useful new technologies (Katz & Hofer, 1994; Peters et al., 2008; Feehan et al., 2017; Evans & Popova, 2014; Kim, Sorhaindo, & Garman, 2006; Brown et al., 2013). These seemingly disparate behaviors may share a common feature: they may be driven, in part, by the fact that people living in material poverty also tend be 'time poor.' Indeed, poverty is not only a state of material constraints, it also involves temporal constraints. The current study explored whether time poverty reinforces barriers toward economic mobility and contributes to poverty traps.

Consistent with previous research, we refer to individuals as 'time poor' when they engage in long hours of unpaid work and have no choice but to do so (Vickery, 1977; Burchardt, 2008; Goodin et al., 2008). Time poverty severely affects low-income women living in developing countries. A lack of basic household amenities requires poor women to spend far more time on household production tasks like cooking and cleaning as compared to their richer counterparts (OECD, 2014). For example, women in Sub-Saharan Africa spend an average of 4.2 hours on unpaid work each day (Abdourahman, 2017). These unpaid household activities can be conceptualized as a kind of tax that individuals, especially women, must pay before undertaking remunerated work. In this project, we propose that reducing time poverty, thereby lowering this personal 'tax,' could have direct benefits for subjective well-being, perceived stress, and relationship conflict, as well as indirect benefits for economic well-being. Despite these potentially far-reaching consequences, there is little understanding of the psychological and economic consequences of the time poverty that often coincides with financial constraints. Traditional economic measurements of poverty often neglect the fact that households living below the poverty line face substantive time deficits (for a comprehensive review, see Hirway, 2017). Furthermore, aid programs tend to focus on material constraints. Billions of dollars of economic aid have been spent to provide monetary and non-monetary aid to people living in extreme poverty. The most common aid programs include food, livestock, and fertilizer, as well as services such as agricultural training, community health workers, and teachers (Alderman, Gentilini, & Yemtsov, 2018; Currie & Gahvari, 2008; Hidrobo et al., 2014; Das et al., 2005). We suggest that the effectiveness of these aid programs could be increased by considering recipients' time costs, either by adjusting how aid is delivered or by creating programs directly aimed at reducing recipients' temporal constraints (related arguments are made by Khera, 2011; 2014). One reason that aid programs may neglect time poverty is the lack of data on time-use amongst the working poor in developing countries. While richer countries have benefited from extensive survey data on time-use, these data are critically absent from countries where time poverty is the most pervasive (Hirway, 2017). Despite these limitations, there is some evidence that time poverty is an important factor in economic development efforts. A large-scale correlational analysis of the Indian Human Development survey, which included 41,554 households in 1,503 villages and 971 urban neighborhoods, found that women who owned a cookstove and did not have to fetch wood were healthier and spent more time on income generating activities than women who did not own a cookstove (Sheikh, 2014). Of course, this research cannot rule out selection effects, therefore women with higher wealth or status in their communities might also be more likely to own and benefit from appliances such as cookstoves.

One previous study experimentally tested the causal effects of reducing unpaid labor (Whillans et al., 2017). In this experiment, sixty working adults recruited in Vancouver, Canada were assigned to spend a small windfall of money ($40 CAD) during two consecutive weekends.

During one weekend, participants were instructed to spend this windfall in any way that would save them time. During another weekend, participants were instructed to spend this windfall on a material purchase for themselves. After making a time-saving (vs. material) purchase, participants reported greater positive mood, lower negative mood, and lower perceived stress. Yet, this experiment targeted affluent individuals living in North America, provided a small one-time payment, and assessed immediate mood. It is unclear whether these findings would apply to poverty alleviation efforts.

Given the limited causal evidence in this area, we used a randomized control trial to evaluate the benefits of reducing time poverty. We recruited working women living in Kibera, an urban slum near Nairobi, Kenya. We selected this population because women living in this context face significant material and temporal constraints. In Kibera, working women earn an average of 100-200 KSH ($0.99-1.98 USD[1]) per day and spend a median of 42 hours on paid labor and 36 hours on unpaid labor each week (Haushofer & Shapiro, 2016). We randomly assigned women living in this community to receive time-saving vouchers designed to reduce their burden of unpaid labor for three consecutive weeks over the course of a six-week study. These vouchers were redeemable for cooking or cleaning services (details below). Based on our pilot data, we estimated that both time-saving vouchers would provide study participants with an additional 3-7 hours each week. We compared the effect of these time-saving vouchers against equivalently-valued unconditional cash transfers (UCTs). We also compared time-saving vouchers and UCTs against a survey-compensation-only condition in which participants did not receive windfalls of any kind. UCTs have received a great deal of attention as a critical tool for

[1] Conversion rate between Kenyan Shillings (KSH) and US dollars (USD) as of the middle of our study (January 1, 2020)

poverty alleviation in developing countries. Recent research finds that UCT's produce significant welfare benefits. For example, in a large-scale field experiment in Kenya (*N*=1,372), households that received UCTs experienced significant improvements in self-reported happiness, life satisfaction, and perceived stress (Haushofer & Shapiro, 2016). These well-being benefits persisted for up to three years (Haushofer & Shapiro, 2018). Cash transfers have also been shown to increase hours of employment, monthly net earnings, and subjective financial well-being when provided to the unemployed, and to improve monthly cash earnings when provided to micro-entrepreneurs (Blattman & Fiala, 2013; Baird et al., 2018; Blattman et al., 2016). Cash transfers also improve empowerment among adolescent girls and young women, as proxied by increased agency and control over decision-making, greater access to financial resources, improved schooling outcomes, decreased teen pregnancy, and better health (Baird et al., 2016). Furthermore, the administrative and overhead costs of providing unconditional cash transfers are extremely low. Given the well-documented benefits and low administrative costs, UCTs serve as a stringent standard by which to compare the effectiveness of aid programs designed to save time. Using equivalently-valued UCTs as a benchmark, we measured the effectiveness of time-saving services and compared the benefits of reducing time versus financial poverty (Blattman & Niehaus, 2014; Shapiro, 2017). Reducing time poverty directly addresses a critical market failure in urban slums. Time poverty is pervasive in this context due to limited infrastructure and a high cost for basic services (e.g. water, sewage, and electricity; Talukdar, 2018). People in urban slums also cannot afford to purchase time-saving services. In Kibera, there are several small businesses that offer such services, but they are largely unaffordable. For example, a single load (8kg) of laundry costs 500 KSH, on average, which equates to over three times the average daily wage. In our pilot data, 76.5% of working women living in Kibera reported "never" paying for laundry services, and 82.4% reported “never” paying for prepared meals from local vendors. We proposed that providing cash transfers was unlikely to address this market failure because people do not readily spend money on time-saving services, even when they can afford to do so (Whillans et al., 2017).

Policymakers are not systematically addressing this market failure, partially because they also undervalue the possible benefits of time-saving services. In an initial pilot study, we asked thirty current and aspiring policymakers from the Harvard Kennedy School of Public Policy how they would allocate 2100 KSH ($21 USD) of aid per recipient to improve the welfare of working women living in Kibera. Only 6% of respondents spontaneously reported that the 2100 KSH should be used to save time for these women. When we explicitly provided respondents with the choice to fund one of three aid programs (an unconditional cash transfer program, an in-kind goods program, or a time-saving program), only four respondents (13%) selected the time-saving program and twenty-six respondents (87%) chose cash. These findings suggest that both recipients and policymakers undervalue time-saving services.

In contrast, we hypothesized that reducing temporal (vs. financial) poverty would have a positive impact on three critical outcomes: subjective well-being, perceived stress, and relationship conflict. We focused on subjective well-being and perceived stress because these outcomes are linked to economic decision-making (Lerner et al., 2015). For example, greater positive affect is associated with a range of downstream economic benefits including increased productivity, work performance, and higher earnings (Boehm, & Lyubomirsky, 2018; Lyubomirsky et al., 2005). Furthermore, stress caused by poverty is linked to short-sighted economic decision-making and excessive risk aversion (Mani et al., 2013). We focused on relationship conflict based on existing evidence showing that cash transfers can reduce intimate partner violence (Buller et al., 2018). However, there is also data showing that providing cash windfalls to women may lead to arguments with their partner about how to spend this income, possibly increasing domestic violence (Haushofer & Shapiro, 2016). Because gains of time are harder to account for than gains of money (Mogilner et al. 2018) and because time is less fungible than money (Leclerc et al., 1995), we predicted that providing women with time-saving vouchers would be less likely to cause relationship conflict than cash transfers.

As discussed above, recent research finds that receiving cash transfers can have positive benefits for subjective well-being, stress, and intimate partner violence (Haushofer & Shapiro, 2016; Kilburn et al., 2018; Hjelm et al. 2017; Samuels & Stavropoulou, 2016; Haushofer et al., 2019; Buller et al., 2018). Prior research also finds that time-saving services can have positive benefits for subjective well-being, perceived stress, and relationship conflict (Whillans et al., 2017). Building on this research, we pre-registered three primary hypotheses. We predicted that participants who were randomly assigned to receive UCTs or time-saving vouchers would experience positive benefits on each of our three key outcomes at endline compared to participants who were randomly assigned to the survey-compensation-only condition. We also predicted that participants assigned to receive time-saving vouchers would experience greater positive benefits compared to participants receiving UCTs.

H_1: Women who are randomly assigned to receive UCTs for three consecutive weeks will report higher subjective well-being, lower perceived stress, and lower relationship conflict at endline compared to women who are assigned to the survey-compensation-only condition and receive no windfalls of any kind.

H_2: Women who are randomly assigned to receive time-saving services for three consecutive weeks will report higher subjective well-being, lower perceived stress, and lower relationship conflict at endline compared to women who are assigned to the survey-compensation-only condition and receive no windfalls of any kind.

H_3: Women who are randomly assigned to receive time-saving services for three consecutive weeks will report higher subjective well-being, lower perceived stress, and lower relationship conflict at endline compared to women who are assigned to receive equivalently valued UCTs.

PRE-REGISTERED RESULTS

Pre-Processing Checks. Before testing our three primary hypotheses, we conducted pre-registered analyses on patterns of attrition and a manipulation check assessing whether the time-saving condition successfully reduced participants' burden of unpaid labor.

Overall, we observed a low rate of attrition across the six weeks of the experiment (n = 83; 7.2%). Consistent with our pre-registered plan, we first conducted a chi-square analysis to examine whether there was differential attrition for participants assigned to the survey-compensation-only, UCT, or time-saving conditions. In the survey-compensation-only condition 34 participants (8.7%) did not complete the study; in the UCT condition 20 participants (5.2%) did not complete the study; and in the time-saving condition 29 participants (7.7%) did not complete the study. These differences were not significant, X^2 (2, N = 1,153) = 3.86, p = 0.15.

As per our pre-registered analysis plan, we then tested for differential attrition along the following baseline characteristics: age, education, marital status, number of people living in the household, number of children living in the household, the household member responsible for financial decision-making, total hours of paid labor in the past 7 days, total hours of unpaid labor in the past 7 days, total income over the past 6 months, total household spending over the past 7 days, depression, subjective well-being, perceived stress, and relationship conflict. Attrition did not differ across any of these characteristics (see Table A1 for analyses). The final sample at endline included 1,435 participants (M_{age} = 35.89, SD = 9.05; 60% married; $M_{children}$ = 3.03, SD = 1.41; median baseline weekly income = 1,250 KSH; $M_{weekly\ hrs\ of\ paid\ work}$ = 44.64, SD = 20.91; $M_{weekly\ hrs\ of\ unpaid\ work}$ = 40.24, SD = 20.43; see Tables A2-A3 for further detail on sample characteristics). Random assignment was successful in balancing conditions on relevant demographic, employment, well-being, and economic characteristics (Table A4). Given the success of random assignment and the fact that we did not observe selective attrition, it is unlikely that attrition meaningfully impacted our results.

Next, we conducted a pre-registered manipulation check to determine whether the time-saving condition had a meaningful impact on participants' burden of unpaid labor relative to the UCT condition. During each of the treatment weeks, participants reported changes in their burden of unpaid labor in the past 7 days on a scale from -3 = *decreased a lot* to 0 = *no change* to 3 = *increased a lot*. We created a weighted average of participants' responses to this item during the treatment (weeks 3-5). Using this measure, we conducted a Bayesian *t-test* with the null hypothesis that there was no difference between conditions regarding a change in the burden of unpaid labor across treatment weeks (H_0: $\delta = 0$). The one-sided alternative hypothesis in this analysis states that the time-saving condition led to a reduction in the burden of unpaid labor as compared to the UCT condition (H_A: $\delta < 0$). Based on Rouder et al (2009), we assigned δ a Cauchy prior distribution with $r = 1/\sqrt{2}$, truncated to only allow negative effect size values.

Consistent with our a priori predictions, the time-saving condition led participants to experience a reduction in their perceived burden of unpaid labor ($M_{time\text{-}saving}$ = -2.27, SD = 0.87, 95% credible interval [-2.38, -2.17]) compared to participants in the UCT condition (M_{UCT} = -0.67, SD = 1.01, 95% credible interval [-0.78, -0.57]). In these analyses, we observed a $BF_{A,0}$ >

1000, meaning that the data were more than 1000 times more likely under H_A than under H_0. The median resulting posterior distribution for the effect size δ equaled 1.67 (95% credible interval [1.48, 1.86]). This result indicates decisive evidence for H_A (Kass & Raferty, 1995). See Table A5 and A1 for the full results of these analyses.

Primary Analyses. Given these successful pre-processing checks, we conducted our primary pre-registered analyses. Our primary pre-registered analyses examined differences between conditions in endline subjective wellbeing, perceived stress, and relationship conflict. For each of these outcomes, we conducted a Bayesian ANCOVA with the following planned comparisons: UCT versus survey-compensation-only condition, time-saving versus survey-compensation-only condition, and UCT versus time-saving condition. For each comparison we calculated a Bayes factor (B_{10}) comparing the null model (M_0, including the respective baseline measure) against a model (M_1) that included the condition effect. See Methods for further detail.

In these three Bayesian ANCOVAs, we found strong evidence in support of the null hypothesis that there were no differences in endline subjective wellbeing, perceived stress, or relationship conflict across conditions, controlling for the respective baseline measure. For subjective wellbeing, the data were 41 times more likely to occur under the null model compared to the model that included the condition effect ($BF_{10} = 0.025$). For perceived stress, the data were 70 times more likely under the null model ($BF_{10} = 0.014$). Lastly, for relationship conflict, the data were 36 times more likely under the null model ($BF_{10} = 0.028$). Nine planned pairwise comparisons also revealed strong evidence in favor of the null hypothesis as indicated by Bayes Factors ranging from 0.01 to 0.32 (see Tables 1-2 for Bayesian ANCOVA results).

Table 1 (Chapter 1). Bayesian model comparisons

Models	P(M)	P(M|data)	BF_M	BF_{10}	error %
Models predicting endline SWB:					
M_0: Null model (incl. baseline SWB)	0.500	0.976	40.508	1.000	
M_1: Condition + baseline SWB	0.500	0.024	0.025	0.025	2.478
Models predicting endline PSS:					
M_0: Null model (incl. baseline PSS)	0.500	0.986	69.725	1.000	
M_1: Condition + baseline PSS	0.500	0.014	0.014	0.014	1.662
Models predicting endline conflict:					
M_0: Null model (incl. baseline conflict)	0.500	0.973	36.184	1.000	
M_1: Condition + baseline conflict	0.500	0.027	0.028	0.028	2.111

Note: Reporting the prior model probability, P(M); the posterior model probability, P(M|data); the posterior model odds, BF_M; and the Bayes Factor indicating the predictive performance of a given model divided by the predictive performance of the null model (BF_{10}).

Table 2 (Chapter 1). Bayesian pairwise comparisons

		Prior Odds	Posterior Odds	BF_{10}	error %
Pairwise comparisons on endline SWB					
Control	UCT	0.587	0.058	0.099	0.002
Control	Time-saving	0.587	0.112	0.191	0.001
UCT	Time-saving	0.587	0.062	0.105	0.002
Pairwise comparisons on endline PSS					
Control	UCT	0.587	0.049	0.083	0.002
Control	Time-saving	0.587	0.064	0.109	0.002
UCT	Time-Saving	0.587	0.064	0.110	0.002
Pairwise comparisons on endline conflict					
Control	UCT	0.587	0.067	0.114	0.002
Control	Time-saving	0.587	0.189	0.322	< 0.001
UCT	Time-saving	0.587	0.069	0.118	0.002

Note: The posterior odds have been corrected for multiple testing by fixing to 0.5 the prior probability that the null hypothesis holds across all comparisons (Westfall, Johnson, & Utts, 1997). Individual comparisons are based on the default t-test with a Cauchy (0, r = 1/sqrt(2)) prior. Bayes Factors are uncorrected.

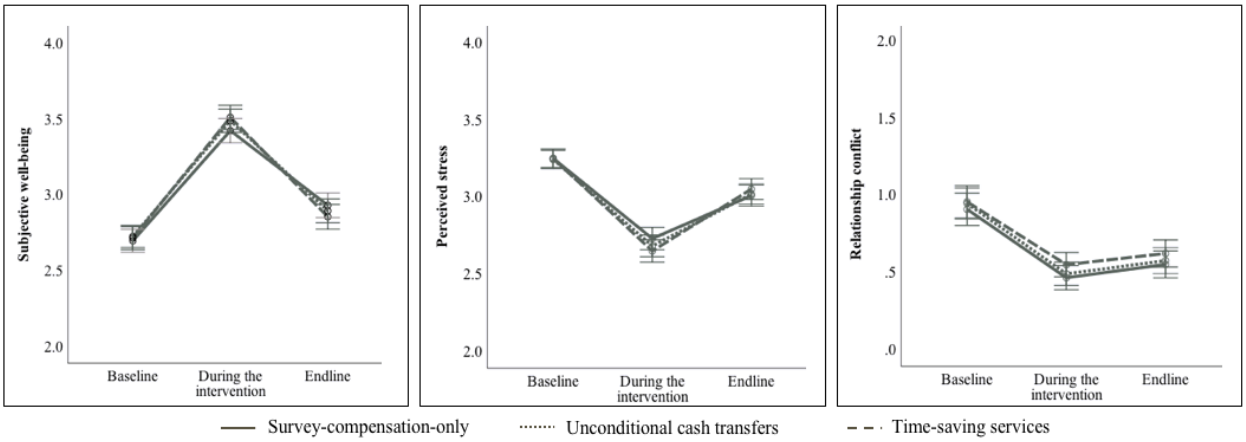
EXPLORATORY RESULTS

Our pre-registered analyses provided evidence that there were no differences between conditions at endline on the three primary outcomes of interest: subjective wellbeing, perceived stress, and relationship conflict. We then conducted exploratory analyses to examine longitudinal trends, mechanisms, and individual differences in treatment effects. In these exploratory analyses, we used data from the three pre-registered conditions (n = 1,070) and an additional exploratory time-saving condition (n = 365). See Appendix A, Section 4 for detail.

Longitudinal analyses. In contrast to our a priori predictions, all three conditions had large effects on each outcome that were similar in magnitude. Collapsing across condition, participants experienced a substantial baseline-to-endline increase in subjective wellbeing (d = 0.22), reduction in perceived stress (d = 0.29), and decrease in relationship conflict (d = 0.35).

To further explore these results, we conducted a repeated measures ANOVA testing for the effect of condition on each outcome: 1) at baseline, 2) during the intervention, and 3) at endline. Consistent with our pre-registered analyses, in this analysis, we found no significant effect of condition on subjective wellbeing, perceived stress, or relationship conflict. However, we did find a significant effect of time for each outcome such that the benefits were greatest during the intervention (Weeks 3-5), but still persisted at endline. Post-hoc pairwise comparisons revealed that, relative to baseline, participants in each condition experienced a significant difference on all three outcomes during the intervention and at endline. We found no significant interaction effects between condition and time. See Figure 1 and Table 3 for descriptives at baseline, during the intervention, and endline. See Tables A6-A8 for results of repeated measures ANOVAs.

Figure 1 (Chapter 1). Effect of condition and time point on subjective well-being, perceived stress, and relationship conflict.



Note. This figure reports estimated marginal means and 95% confidence intervals by condition and time point: baseline (week 1), during the intervention (weighted average of weeks 3-5), and endline (week 6). Outcome measures during the intervention were calculated as a weighted average of participants' responses in weeks 3-5. For subjective well-being, the measure during the intervention was a weighted average of positive affect and negative affect in Weeks 3-5. The satisfaction with life scale was not included in this measure because it required a visual aid and therefore could not be administered via the phone survey during the intervention weeks.

Table 3 (Chapter 1). Descriptives by condition for subjective wellbeing, perceived stress, and relationship conflict at baseline (week 1), during the interventions (weeks 3-5, weighted average), and at endline (week 6).

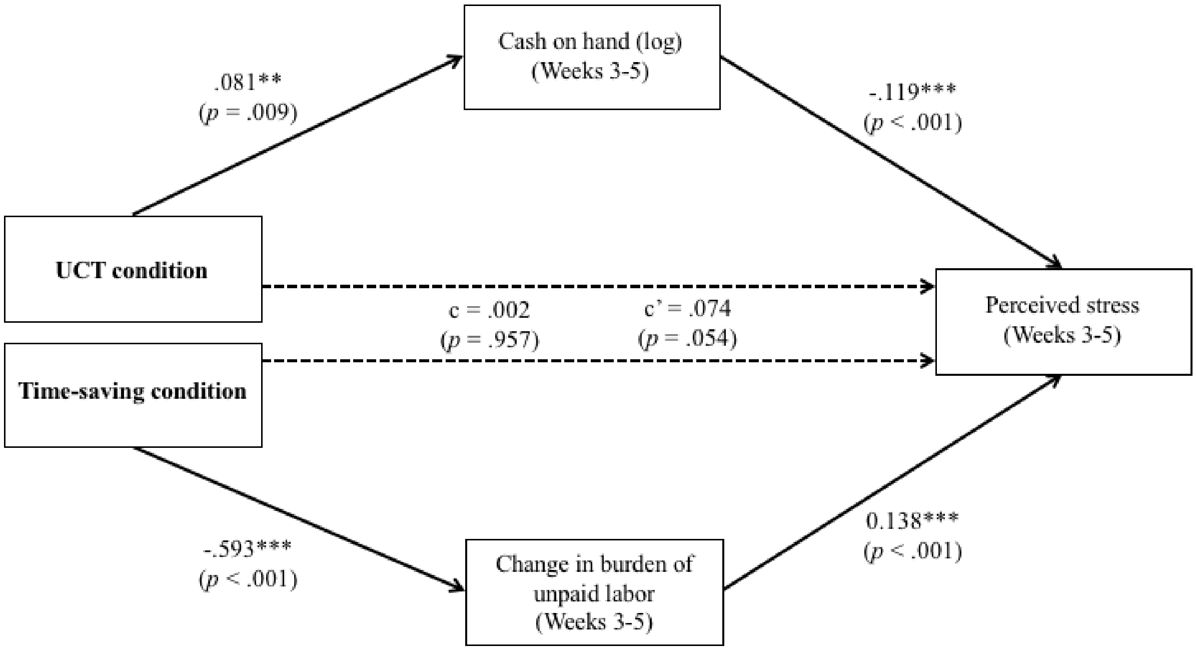
Time	Condition	SWB *M* (*SD*)	PSS *M* (*SD*)	Conflict *M* (*SD*)
Baseline	Survey-compensation-only	2.69 (0.68)	3.25 (0.54)	0.90 (0.93)
	UCT	2.72 (0.73)	3.24 (0.55)	0.94 (0.98)
	Time-saving	2.71 (0.66)	3.25 (0.55)	0.92 (0.91)
During	Survey-compensation-only	3.42 (0.72)	2.73 (0.67)	0.46 (0.62)
	UCT	3.48 (0.71)	2.68 (0.62)	0.49 (0.70)
	Time-saving	3.48 (0.73)	2.66 (0.65)	0.55 (0.77)
Endline	Survey-compensation-only	2.92 (0.74)	3.00 (0.63)	0.55 (0.80)
	UCT	2.89 (0.75)	3.01 (0.60)	0.57 (0.81)
	Time-saving	2.85 (0.73)	3.06 (0.61)	0.59 (0.73)

Note. Reporting means and standard deviations for each condition at each time point: baseline (week 1), during the intervention (weighted average of weeks 3-5), and endline (week 6). Outcome measures during the intervention were calculated as a weighted average of participants' responses in weeks 3, 4, and 5. To be included, participants had to complete 1 or more phone survey. Endline includes in-person endline survey and endlines conducted via phone. Sample size for SWB: survey-compensation-only (*n* = 317), UCT (*n* = 344), time-saving (*n* = 647); for PSS: survey-compensation-only (*n* = 310), UCT (*n* = 333), time-saving (*n* = 637); for relationship conflict: survey-compensation-only (*n* = 317), UCT (*n* = 344), time-saving (*n* = 647). The larger *n* for time-saving is accounted for the fact that we included both the results from our pre-registered time-saving condition as well as data from the additional, exploratory time-saving condition.

Mechanisms. Consistent with the exploratory analyses specified in our pre-registered report, we conducted a series of bootstrapped mediation and moderation analyses to test for three mechanisms by which time-saving vouchers might improve subjective well-being, perceived stress, and relationship conflict during the study. First, time-saving vouchers might produce these benefits by reducing the total amount of time spent on unpaid labor. Second, time-saving vouchers might remove especially disliked tasks. Third, time-saving vouchers might enable recipients to spend more time on welfare-enhancing activities such as paid work or socializing.

We found evidence in support of the first mechanism: Time-saving vouchers conferred psychological benefits by reducing unpaid labor. This finding aligns with research showing a robust negative association between unpaid labor and psychological wellbeing, particularly for working women (Giurge et al., 2021). The effect of the time-saving condition on perceived stress during the intervention was mediated by a reduction in participants' burden of unpaid labor (indirect effect = -0.082, 95%CI[-0.128, -0.036]). Reducing the burden of unpaid labor did not mediate the effect on subjective wellbeing (indirect effect = 0.0361, 95%CI[-0.012, 0.091]) or relationship conflict (indirect effect = 0.019, 95%CI[-0.026, 0.063]). See Tables A9-A10. We also conducted mediation and correlation analyses to test for the second and third possible mechanisms. We did not find evidence for either mechanism. Additional time spent on paid work or socializing and the extent to which participants disliked cooking and doing laundry did not predict the benefits of time-saving vouchers (Tables A11-A14). Participants in the UCT condition experienced benefits through a distinct mechanism: an increase in 'cash on hand' during the treatment weeks ("How much cash [in shillings] does your household have on hand right now?"). Receiving unconditional cash transfers led to an average increase in monthly income of 29% relative to baseline earnings, and as a result, participants in the UCT condition reported having more cash on hand during the treatment weeks as compared to participants in the time-saving condition, (β = 0.067, t(1018) = 2.141, p = .032, 95% CI(β) [0.006, 0.127]). Moreover, increased cash on hand mediated the effect of the UCT condition on subjective wellbeing (indirect effect = 0.010, 95%CI[0.002, 0.023]) and perceived stress (indirect effect = -0.009, 95%CI[-0.021, -0.002]). Cash on hand did not mediate the effect of the UCT condition on relationship conflict (indirect effect = -0.002, 95%CI[-0.010, 0.002]). These findings suggest that the benefits of UCTs were driven by an increase in liquid cash resources, and not from an increase in spending. The important role of 'cash on hand' for psychological well-being aligns with prior research which provides evidence that having readily available cash predicts life satisfaction over and above total income, investments, and debts (Ruberton et al., 2016). We examined these distinct mechanisms for the benefits of the time-saving versus UCT conditions using parallel mediation models. In each model, the burden of unpaid labor, cash on hand (log), and total spending (log) during the treatment weeks were analyzed simultaneously as parallel mediators, while controlling for baseline cash on hand (log), baseline income (log), and the respective baseline outcome measure. See Figure 2 and Tables A9-A10.

Figure 2 (Chapter 1). Parallel mediation analysis: mediating effects of changes in perceived burden of unpaid labor and (log) cash on hand.



Note. Reported are standardized coefficients, controlling for baseline perceived stress, baseline cash on hand (log), and baseline income (log). Cash on hand (log), change in burden of unpaid labor, and total spending (log) during the treatment are were analyzed as parallel mediators. The indirect effect of total spending (log) was not significant, indirect effect = 0.001, 95%CI[-0.003, 0.006]. The total effects of conditions (time-saving=1, UCT=0) are reported as *c*; direct effects are reported as *c'*. See Table S6 for further detail. **p* < .05, ***p* < .01, ****p* < .001.

Individual Characteristics. Consistent with the exploratory analyses specified in our pre-registered report, we also conducted a series of regression analyses to assess whether the benefits of each condition differed based on participant characteristics at baseline. It is possible that the benefits of time-saving vouchers might be strongest for participants who are experiencing the greatest stress at baseline or for participants with work skills that allow them to take on additional paid labor. There might also be greater benefits of time vouchers for participants who are employed in occupations that are flexible and have a greater availability of work, such as occupations that involve short-term contracts or micro-enterprises.

We found no evidence that the following baseline characteristics significantly influenced the benefits of the time-saving services condition relative to UCTs: education level, occupation, household size, income, wellbeing, perceived stress, relationship conflict, or risk of depression. Yet, we did observe a moderating effect of micro-enterprise ownership. Participants who were micro-entrepreneurs (i.e. reported owning a business or a portion of a business; 30%), experienced greater benefits from cash transfers as compared to time-saving vouchers. Specifically, micro-entrepreneurs who were randomly assigned to receive cash transfers experienced a larger decrease in perceived stress at endline, controlling for baseline, as compared to micro-entrepreneurs who received time-saving services. See Tables A15-16.

DISCUSSION

We conducted a pre-registered, highly-powered field experiment over six weeks with a sample of 1,070 working mothers living in Kibera, an urban informal settlement in Nairobi, Kenya. Results from pre-registered Bayesian analyses provide strong evidence that receiving three consecutive weeks of time-saving services, cash transfers, or additional income through survey completion had statistically similar benefits for subjective wellbeing, perceived stress, and relationship conflict. Although these benefits were greatest during the intervention period, they persisted even after the intervention was removed.

Exploratory pathway analyses suggest that UCTs and time-saving services confer welfare benefits through distinct mechanisms. UCT's reduced perceived stress by increasing the total value of available 'cash on hand.' Consistent with past research (Ruberton et al., 2016), immediately available cash appeared to provide a sense of financial safety that was critical for the emotional wellbeing of women in our study, above and beyond the effects of additional spending during the intervention period. In contrast, time-saving services reduced perceived stress by lowering participants' perceived burden of unpaid labor. This finding is consistent with prior research showing that the benefits of time-saving purchases are driven by the perception of reduced daily demands (Whillans et al., 2017; Dunn et al., 2020). We also found initial evidence that female microentrepreneurs with school-aged children living at home derived greater psychological benefits from cash (vs. time-saving vouchers), experiencing lower stress at endline after receiving 1500 KSH. Yet, interestingly, microentrepreneurs in the time-saving condition earned the most money. Microentrepreneurs in the time-saving condition earned 343 KSH more than microentrepreneurs in the UCT condition and 309 KSH more than those in the survey-compensation-only condition. These comparisons were statistically significant. Building on these results, future work should examine the relative effects of cash transfers (vs. time-saving) on the wellbeing and earnings of microentrepreneurs.

Unexpectedly, there was no difference between the UCT and survey-compensation-only conditions with respect to our three primary outcomes. Relative to baseline earnings, participants in the UCT condition experienced a 33% increase in their total income during the intervention period, whereas participants in the survey-compensation-only condition experienced an 14% increase. The similar benefits observed across these conditions could be a consequence of the severe financial deprivation that participants faced at baseline. Prior to the intervention, participants earned a median of 1250 KSH per week and had a median total savings of 0 KSH. These data suggest that under these circumstances, even small cash infusions, the provision of limited additional paid work, and time-saving services may have a large psychological impact.

As the survey-compensation-only condition did not differ from the time-saving or UCT conditions, it is possible that participants in the survey-compensation-only condition derived some psychological benefit from the money itself and additional psychological benefit by *working* for this money; thus, yielding similar psychological benefits to the UCT and time-saving conditions despite receiving less money (and less valuable compensation) in total. This possibility aligns with the results of a field experiment in Kenya showing that working for pay improved wellbeing relative to waiting for an equivalent-valued windfall (Bhanot et al., 2018). Further research is needed to understand the psychological effects of receiving cash transfers of various sizes and the benefits of providing additional work relative to cash and non-cash aid.

Overall, the insights generated from this field experiment have implications for promoting welfare and economic mobility among chronically stressed populations. Participants in this study were not only financially and temporally impoverished, they were also at high risk for clinical depression. Twenty-percent of participants in our study reported depression scores on a validated measure (CES-D) that were indicative of being at risk for clinical depression (Radloff, 1977). These rates are consistent with research on prevalence of depression among people living in poverty (rates ranging from 15-45%; Everson et al., 2002). This chronic stress and risk of depression may perpetuate poverty traps—overlapping economic, environmental, and psychological conditions that make it difficult to escape poverty unless multiple constraints are relieved simultaneously (Ghatak, 2015; Barrett et al., 2016; Barrett et al., 2019; Bedoya et al., 2019). Therefore, reducing stress and depression such as through the provision of repeated payments of cash, time-saving vouchers, or additional work may all be important pathways towards alleviating poverty.

Rigorous testing is needed to develop effective methods of reducing stress and depression for individuals living in severe poverty. For instance, a recent study found that psychotherapy alone had no measurable effect on the psychological well-being of chronically poor individuals living in rural Kenya, whereas cash transfers led to significant improvements (Haushofer et al., 2020). Furthering this work, our results show that freeing up temporal resources may be an overlooked strategy for reducing stress among working mothers living in material poverty.

Future research should apply similar experimental methods to compare the efficacy of cash transfers, time transfers, and other forms of in-kind aid towards reducing stress, with possible downstream consequences for depressive symptomologies and physical health.

One important limitation of this field experiment was the total duration of the intervention. Participants received UCTs, time-saving services, or received survey-compensation for a period three consecutive weeks. It is possible that the cumulative effects over a longer period of time may be substantially different as participants are better able to make plans for upcoming cash and time transfers. In particular, longer-term time transfers may provide increasing marginal returns and more persistent benefits if people spend their additional time on repeated rewarding activities like learning a new skill, applying for new jobs, establishing a new business, or volunteering (Mochon et al., 2008). Future research should examine the long-term benefits of time-saving services and technologies, especially for working mothers living in developing markets.

Another important limitation is that we were unable to test the effect of UCTs or time-saving services against a neutral control condition. Given our focus on subjective well-being, it was necessary to bring participants into the lab to fill out surveys. It was therefore necessary to pay participants for completing the study, to adhere to ethical guidelines and to reduce the possibility of differential attrition across conditions. However, the statistical similarity between the benefits of the UCT and neutral control conditions reveals an open area for future research: the amount of cash or work necessary to create sustainable changes in subjective well-being. Additional research should also explore whether the benefits of time-saving services are greater when these services do not incur any time cost. In our study, participants had to travel short distances to pick up their meals and to pick-up and drop-off laundry. Future research should explore whether reducing these small frictions would bolster the benefits of time-saving services.

CONCLUSION

Policymakers and researchers in economic development tend to focus on alleviating tangible and financial constraints (Alderman et al., 2018; Currie & Gahvari, 2008), neglecting resources of time or, in many instances, exacerbating time poverty by requiring recipients of aid programs to travel long distances, wait in lines, and fill out arduous paperwork (see Giurge, Whillans, & West, 2020 for a review). Despite being neglected and undervalued, our data show that reducing time poverty has psychological benefits that are comparable to reducing financial constraints via cash transfers and receiving survey-compensation. Given the substantial evidence documenting the benefits of cash transfers on psychological wellbeing, economic outcomes, and women's empowerment, these results highlight the importance of conducting additional research on the efficacy of time-saving programs and investments. Future research could involve the provision of time-saving services, time-saving technologies, and infrastructure improvements that save time. The laundry and prepared meal services offered in this study represent two viable time-saving services among many possibilities, including improved transportation services to reduce commute times as well as improved cookstoves and water collection technologies. Other technologies may have time-saving elements that are underappreciated in economic evaluations such as solar-powered appliances that provide off-the-grid households with additional daylight hours in order to be more productive or engage in leisure activities. More extensive time-tracking, especially in developing countries, is an important step towards evaluating the full range of economic and psychological benefits produced by these infrastructure and technological investments (Hirway, 2017). In sum, this research provides initial evidence for the potential benefits of reducing time poverty. We focused on working mothers in an urban slum in Nairobi because this population is especially likely to face both severe financial and temporal constraints. However, time poverty is a pervasive and growing concern around the world, affecting a large and diverse swath of humanity. Data from the Gallup World Poll suggests that stress is rising for people around the world—and it is driven, in part, by the increased demands on our time (Ward et al., 2020). A better understanding of time poverty is needed to find more effective and sustainable ways to improve economic and psychological wellbeing for people living all over the world.

METHODS

Ethics statement. This research was approved by the ethics committee at the Harvard Business School (HBS-IRB18-0905) and the Kenya Medical Research Institute (Protocol No. Non-Kemri 629).

Planned sample. We recruited participants through the Busara Center for Behavioral Economics, a research organization based in Nairobi, Kenya. Busara has a dedicated participant pool of over 15,000 people living in nearby informal settlements, enabling efficient recruitment of working mothers living below the poverty line. The study was conducted at the Kibera Town Center (KTC), a facility located in Kibera and operated by the Human Needs Project. Kibera is the largest informal settlement nearby Nairobi, Kenya, with an estimated 200,000 inhabitants. Based on similar research conducted through the Busara Center, we expected low attrition of around 10%. Consistent with this estimate, 1,550 participants completed our baseline measures before the forced lockdowns caused by COVID in March 2020 required data collection to stop. Of these participants, we collected endline data from 1,435 participants, including 1,070 participants across our three pre-registered experimental conditions and 365 participants in an additional exploratory time-saving voucher condition. Overall, we observed an attrition rate of

7.3%. See Table A1 for details on attrition across the experiment. The final sample size for our pre-registered comparisons (N = 1,070) was lower than the target sample that was specified in the pre-analysis plan (N = 1,200) due to the COVID-19 pandemic. In March 2020, data collection was terminated in accordance with public health guidelines in Kenya.

Women who lived no further than a 30-minute walk from Kibera Town Centre were recruited via text message to participate in a five-minute eligibility phone call. This requirement ensured that accessing KTC did not impose a significant time cost. To participate, respondents had to be 18 years of age or older (the legal age of consent in Kenya), provide informed consent, and work for pay at least twenty-five hours per week. To reduce attrition, we only recruited working mothers with at least one child enrolled in school and living at home. This criterion increased the likelihood that participants would remain in their current residence and complete the study in its entirety. See Table A17 for full details on number of exclusions during the sample recruitment process. See Tables A2-A3 for the demographic and employment characteristic of the sample.

Based on pilot research, we chose two time-saving vouchers for use in our experiment: prepared meal and laundry services. To ensure that these time-saving vouchers reduced participants' existing burden of unpaid labor, we excluded women who reported that they "always" used laundry and/or prepared meal services. Similarly, we excluded women who reported spending fewer than three hours per week cooking and fewer than three hours per week completing laundry. To ensure that the time-saving services meaningfully reduced the burden of unpaid labor, we excluded women with seven or more individuals living in their household. To facilitate data collection, respondents had to have a working cell phone that was not shared with another household member. See Table A17 for details on number of exclusions based on each of these criteria.

Study Timeline. This study included a baseline and endline survey containing identical pre-registered measures. See Figure 3 for study design. To ensure that we could collect endline data from participants who were unwilling or unable to come to the Kibera Town Center to complete the endline measures (e.g., due to forced lockdowns during the COVID-19 pandemic), we provided participants with the option of completing a modified endline survey over the phone. 29% completed the endline measures over the phone. See Table A18 for the demographic characteristics of participants who completed the endline in-person (vs. over the phone). We also collected granular data on participants' affective experiences, stress, time-use, and household consumption in weekly phone surveys beginning in Week 2 and continuing through Week 5. All participants received compensation for completion of the baseline and endline surveys (500 KSH each) as well as phone surveys (250 KSH each).

The baseline survey was conducted in a lab setting at the Kibera Town Center (Week 1). Eligible participants were invited to the KTC to provide consent and complete the baseline survey, including the primary pre-registered measures: subjective well-being, perceived stress and relationship conflict. Participants also completed a variety of exploratory and demographic measures. After completing the baseline survey, participants were randomly assigned between-subjects to one of two treatment conditions or a survey-compensation-only condition (1=time-saving, 2=UCT, 3=control). Using the "sample" function in R, we generated a random integer between the values of 1 and 3 by running the following code for each participant: treat<-sample (1:3,1).

In Week 2, participants began receiving a weekly phone call to complete a survey about their experiences, well-being, time-use and consumption over the past week. Starting in Week 3, participants who were randomly assigned to one of the two treatment conditions received 1) time-saving services or 2) equivalently-valued unconditional cash transfers. Participants received one of these windfalls every week for three consecutive weeks (Week 3-5). The time-saving and UCT conditions were matched in terms of their cost-to-administer, thereby holding constant the total amount of aid that was disbursed (500 KSH per week). In Week 6, all participants were invited back to KTC to complete the endline survey, which included the identical measures of subjective well-being, stress, and relationship conflict. See Figure 3 for study timeline.

For all data collection, trained field officers guided participants through all survey items in Swahili or English (participants chose their preferred language), ensuring that every participant—including those with limited reading, writing, and numeracy skills—could comprehend and complete all instructions and measures. Field officers were blind to the condition and study hypotheses for baseline and endline data collection.

Details on Time-Saving Vouchers. To develop the time-saving vouchers, we selected services that were likely to have the greatest benefits for our target population. We conducted a pilot study to identify local services that met the following criteria for working women in Kibera: the services 1) saved a significant amount of time, 2) replaced chores that were unpleasant, and 3) replaced chores that did not involve significant social interaction (i.e., women typically engaged in these chores alone). Based on these criteria, we selected prepared meals and laundry services. For all three treatment weeks, participants assigned to the time-saving condition received either prepared meals (two meal varieties alternated across weeks) or laundry services.

Condition 1: Time-Saving Vouchers (*n* = 349). The cost to provide each of these time-saving services was 500 KSH per week. Based on our pilot data, 500 KSH worth of these services eliminated a significant amount of unpaid labor among our target population (3-7 hours per week on average). Building on prior research, we sought to amplify the benefits of the time-saving vouchers by reminding participants about the specific amount of time that they would save (Whillans et al., 2018) and by asking them to make detailed plans for this additional time (Liberman & Trope, 2008; Rogers & Bazerman, 2008).

Condition 2: Unconditional Cash Transfers (*n* = 366). The weekly cash transfer was 500 KSH. Therefore, participants received 1500 KSH in cash transfers during the treatment period, which amounted to a median income increase of 33% relative to baseline earnings.

Condition 3: Survey-Compensation-Only (*n* = 355). Participants received no windfalls of any kind but earned money for completing surveys.

In all conditions, participants experienced an increase in their earnings via compensation for completing the baseline survey (KSH 500), weekly phone surveys (KSH 250 per completed survey), weekly text messages (25 KSH per completed survey), and endline survey (KSH 500).

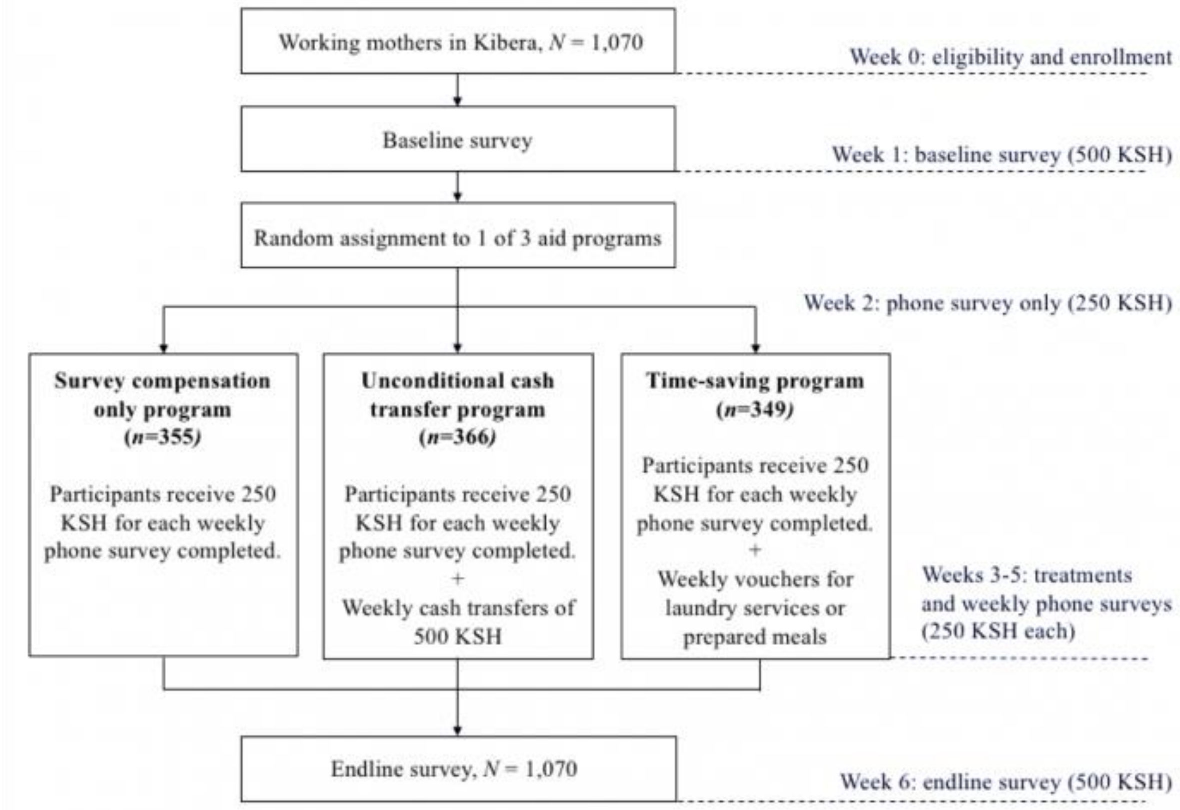
Manipulation Check (phone surveys in Weeks 3-5). To ensure that the time-saving services reduced participants' burden of unpaid labor as compared to UCTs, participants who were assigned to conditions 1 and 2 were asked to complete the following question during phone calls in Weeks 3-5 (weeks in which participants received windfalls of time or money): "Over the PAST 7 DAYS, to what extent did receiving [prepared meals / laundry / cash transfers] affect your burden of unpaid labor?" Participants indicated their response on a scale from -3 = *Decreased my burden of unpaid labor a lot*, 0 = *Did not change my burden of unpaid labor*; 3 = *Increased my burden of unpaid labor a lot*. We calculated a weighted average of responses to this item in weeks 3-5 as a measure of whether the time-saving services effectively reduced participants' burden of unpaid labor. In a Bayesian *t-test*, we found robust evidence that participants in the time-saving condition experienced a greater decrease in their burden of unpaid labor compared to participants in the UCT condition, $BF_{A,0} > 1000$. See Results for full analysis.

Primary Measures (baseline and endline). To measure subjective well-being at baseline and endline, participants completed (a) the 12-item Schedule of Positive Affect and Negative Affect (SPANE; Deiner et al., 2009: PA_{T1} $\alpha = 0.91$; NA_{T1} $\alpha = 0.91$; PA_{T2} $\alpha = 0.92$; NA_{T2} $\alpha = 0.92$), and (b) the 5-item Satisfaction with Life Scale (SWLS; Deiner et al., 1985). Based on past research, we defined subjective well-being (SWB) as a combination of high positive affect (PA), low negative affect (NA) and high satisfaction with life (SWL) (Diener 1994; Diener et al., 1999; Sheldon, 2013). Each of these measures were highly correlated at baseline ($rs_B > 0.51$, $ps \leq 0.01$) and endline ($r_E > 0.50$, $ps < 0.01$) therefore, as per our pre-registered analytic plan, we created a composite measure at both time points by combining PA (averaged), SWL (averaged) and NA (averaged and reverse-coded). Due to Covid-19 or other constraints on returning for in-person endline surveys, 29% of our sample completed the endline measures over the phone. As part of these phone calls, we were unable to complete the SWL measure because it required a visual aid. For these participants, we created an alternative SWB composite comprised of PA and NA only. We report treatment effects on SWB both on the full sample ($N = 1070$) and on the sub-sample who completed the endline in-person and therefore completed all three sub-components on SWB ($n = 764$). Results are substantially unchanged. See Tables A19-A20.

To measure perceived stress, participants completed the 10-item Perceived Stress Scale (PSS; Cohen, Kamarck, Mermelstein,983; Cohen, 1988) at baseline and endline. We created a composite measure of perceived stress at both time points by taking the average of all items of the PSS (PSS_B $\alpha = 0.77$; PSS_E $\alpha = 0.82$). The PSS conceptualizes perceived stress as a lack of control over important life outcomes. Research suggests that both time-saving services and UCTs can increase perceived control over daily events (Whillans, Pow, & Norton, 2020). Thus, our focus on this definition of stress dovetails with recent research and addresses recent calls from researchers to focus on specific elements of stress (Kagan, 2016).

To measure relationship conflict at baseline and endline, participants completed the 9-item negative interaction scale of the network of relationship inventory (Furman & Buhrmester, 2009). We created a composite of relationship conflict at both time points by taking the average of all nine items ($RelationshipConflict_B$ $\alpha = 0.95$; $RelationshipConflict_E$ $\alpha = 0.95$). For participants who reported that they were not married or in a marriage-like relationship, they completed these measures by responding about their closest social relationship (e.g., sister, mother). We report treatment effects on the combined measure of relationship conflict (collapsing across reports of conflict with romantic partners and close social relationships) as well as effects on the sub-sample who are married or in a marriage-like relationship ($n = 604$). Results are substantially unchanged. See Tables A21-A22.

Figure 3 (Chapter 1). Study design and timeline



Chapter 2: Episodic Financial Windfalls: Effects of Income Volatility on Impatience

ABSTRACT

We investigate whether income volatility is associated with financial impatience—the preference to receive a small sum of money immediately over a larger sum of money later. We find that experiencing more income volatility predicts greater subsequent financial impatience across the wealth spectrum. Using 27-years of longitudinal data on biannual income, Study 1 (*N* = 5,106) demonstrates that this effect operates above and beyond individual differences in income-risk preferences and total wealth. Study 2 (*N* = 326) conceptually replicates these findings with recent month-to-month income volatility and observe that this effect occurs primarily among people who have little control over their finances. In Study 3 (*N* = 841), we collected a representative sample of working Americans, stratified on income, to test the effects of recent monthly income volatility on a spending versus saving decision with real stakes; but the moderation as a function of financial locus of control does not replicate. At all income levels, greater income volatility is associated with a preference for more immediate spending and less saving. We conclude by discussing the implications for employers and policymakers.

Around the world, households are facing increasingly volatile incomes as fewer jobs offer consistent working schedules and predictable salaries (Federal Reserve, 2018; OECD, 2019; Morduch & Schneider, 2017). For instance, the average American household now experiences 30% more variability in year-to-year income compared to 1970 (Dynan et al., 2012; Gottschalk & Moffitt, 2009). Income volatility is also rising in other countries where detailed household-level data is available, including Germany (Burkhauser et al., 1997), Great Britain (Dickens, 1996), Sweden (Gustavsson, 2004), and Canada (Baker & Solon, 2003; Beach et al., 2008; 2010). In this research, we examine the relationship between income volatility and financial decision-making. Across three studies, we demonstrate that income volatility leads people to make more impatient decisions, irrespective of income level and overall wealth.

Past research has linked the experience of poverty to a pattern of impatient decision making, including over-borrowing, more impulsive spending, less saving, and poor planning (Lea et al., 1993; Haushofer & Fehr, 2014; Zhao & Tomm, 2018; Ong et al., 2019; for reviews see: Banerjee & Mullainathan, 2010; Pepper & Nettle, 2017). The evidence indicates that this pattern of myopic behaviors is not the result of individual failures or pathology, but rather a psychological consequence of experiencing chronic financial deprivation. However, the psychological effects of poverty and income volatility are often confounded because poor households typically experience the most volatile income streams. For instance, in developing countries, income uncertainty is a fundamental feature of poverty due to a variety of economic risks such harvest failure, health shocks, crime, precarious employment, and a lack of adequate insurance or other risk-sharing arrangements (Morduch, 1999; Dercon, 2002; Baulch & Hoddinott, 2000). Similarly, data from the US Financial Diaries Project and JP Morgan Chase find that income volatility is highest for households below the poverty line, primarily due to within-job pay fluctuations (Hannagan & Morduch 2015; Morduch & Siwicki 2017; Morduch & Schneider 2017; Farrell & Greig 2016). We theorize that part of the relationship between poverty and impatience may actually be driven by income volatility, rather than an absolute lack of resources. Therefore, rising income volatility may help to explain patterns of impatient financial decision-making across the income spectrum. For instance, even high-income households are spending beyond their means and under-saving – a recent survey found that 18% of households making more than $100,000 annually are living paycheck to paycheck (Census Bureau, 2020; Willis Towers Watson, 2020). Rising household income volatility represents a fundamental change in labor markets (Gottschalk & Moffitt, 2009; Hacker & Jacobs, 2008). It is critical to better understand how income volatility shapes financial decision-making and intertemporal trade-offs.

Psychological consequences of income volatility

In the United States and around the world, a growing percentage of people work in jobs that do not offer a stable salary. For instance, compared to 1980, more people work part-time, as contractors and freelancers, and in the gig economy doing crowdwork and work-on-demand (Federal Reserve, 2018; OECD, 2019; Manyika et al., 2016; De Stefano, 2016). Full-time workers are also facing increasing income volatility. Union membership has declined, and companies are demanding more flexible labor, leading to more irregular working schedules and less predictable income streams (Bureau of Labor Statistics, 2019; McMenamin, 2007; Golden, 2015). In addition, compensation packages have shifted towards lower guaranteed salaries and more incentive-based pay, leading to further volatility (e.g. tips, bonuses, commissions, profit-sharing, stock options, etc.; Federal Reserve 2018; Lemieux et al., 2009; Lazear & Shaw, 2008; Lazear, 2018).

The effects of income volatility are most severe for people living paycheck-to-paycheck, since an unexpected negative income shock can lead to housing instability, utility disruptions, food insecurity, and cycles of increasing debt (Bania & Leete, 2009; Leete & Bania, 2010; Collins et al., 2014). However, wealthier households are also affected. Prior survey data suggests that, across the economic spectrum, households with more volatile incomes accumulate less savings, incur more debt, are more likely to default on their debts, and report greater overall financial strain compared to households with stable incomes (Diaz-Serrano, 2005; Schneider & Harknett, 2017; Pew Charitable Trusts, 2017; Fisher, 2010; TD Bank Group, 2017). These patterns of impatient behavior may be caused, in part, by the psychological experience of income volatility. Economic research has examined household strategies for managing income volatility and sharing risk (Morduch 1995; 1999; Fafchamps, 1999; 2003; Dercon, 2002), but there is currently little evidence on individuals' psychological responses to income volatility.

Economic models of life-cycle consumption predict that people will respond rationally to income volatility by making more patient economic choices such as increasing precautionary savings and reducing discretionary spending (Leland, 1978; Skinner, 1988; Kimball, 1990; Weil 1993; Carroll, 1997). When individuals have little control over the future incomes and incomplete knowledge about the distribution of their possible earnings, rational economic theory predicts that they will be motivated to engage in precautionary savings to protect against future income risk (Kimball, 1990). In contrast to these models, descriptive research from consumer psychology suggests that people are more likely to make short-sighted decisions in response to income volatility. When people face a high degree of uncertainty about the future, prioritizing the present can be viewed as a 'contextually adaptive response' (Pepper & Nettle, 2017; Fawcett et al., 2012). That is, if people believe their future financial outcomes are mostly out of their control, they may invest less effort and attention towards this future state. Indeed, low perceived control over future outcomes has been associated with impatient consumer financial decisions, including over-spending and under-saving (Perry & Morris, 2005; Shapiro & Wu, 2011; Cobb-Clark et al., 2016). In the current work, we test these two alternative theories on how income volatility shapes intertemporal decision-making.

Measurement and Analytic Approach

Defining and measuring income volatility. Income volatility is defined as fluctuations in earnings away from a general trend (Gottschalk & Moffitt, 1994; Congressional Budget Office, 2008). Previous research has used a wide variety of methods to measure and model income volatility. For instance, several studies use parametric models of income dynamics, often with the aim of separating fixed and variable income components, or distinguishing income mobility from transitory shocks (Moffitt & Gottschalk, 2002; Haider, 2001; Baker & Solon, 2003; DeBacker et al., 2013; Moffitt & Zhang, 2018). For testing our hypotheses, we prefer statistics that reflect psychologically salient fluctuations in income. People tend to be insensitive to gradual changes in their income, rather, they are attentive to significant fluctuations from one year or one month to the next. For instance, Mitra and colleagues (Mitra et al., 1997) found that people are largely insensitive to annual pay raises less than 7%. Given our aim of understanding psychological responses to income volatility, we use a measure that captures meaningful percentage changes in income. Specifically, following past research, we measure income volatility as the standard deviation of percentage change in income from one period to the next (Dynarski & Gruber, 1997; Cameron & Tracy, 1998; Congressional Budget Office, 2008; Dynan et al., 2012; Latner, 2018). This measure can be calculated across any time horizon, but we focus on year-to-year fluctuations (Study 1) and month-to-month fluctuations (Studies 2 and 3) since these are common intervals at which people evaluate their income and make financial decisions.

Defining and measuring impatience. Aligning with past research, we define impatience as a preference to receive a small sum of money sooner over a larger sum of money later (Samuelson, 1937; Frederick et al., 2002). People are constantly faced with choices involving trade-offs between costs and benefits that are realized at different times. When making these intertemporal choices, people often succumb to temptations and undervalue future outcomes, choosing smaller-sooner rewards over larger delayed rewards (Ainslie & Haslam, 1992; Cohen, et al., 2020). This pattern of impatient decision-making, often described as 'delay discounting,' has a profound effect on many important life outcomes, particularly financial wellbeing.

Impatience has been conceptualized and measured in several of ways across different disciplines including psychology, public health, and economics. For instance, impatience is often viewed as a personality trait that can be captured using self-reported scales (Barratt, 1965; Patton et al., 1995; Dickman, 1990). However, we focus on 'choice-based' measures of impatience because they tend to be the most predictive of real-world financial behavior (Burks et al., 2012; Hamilton et al., 2015). Choice-based measures define impatience as a personal discount rate for outcomes realized in the future (Mazur, 1985; 1987; Laibson, 1997; Frederick et al., 2002; Andreoni et al., 2015). Discounts rates can be calculated using a 'matching' elicitation method whereby people report an indifference point between money received today versus in the future (Thaler & Shefrin, 1981; Hardisty et al., 2013). In Studies 1 and 2, our key outcome of interest is a matching elicitation of impatience in which participants are asked to indicate an amount of money that would convince them to wait 1 month to receive a cash prize of $1000, rather than claiming it immediately. Whereas in Study 3, we use a related incentive compatible choice-based method in which people can choose to receive $1000 immediately or set aside a portion of this money to be received in 6 months, plus 10% interest.

Individual differences in discount rates, as measured through matching elicitation procedures, have been directly linked real-world behaviors. People with higher discount rates (indicating greater impatience), tend have less savings, higher credit card debt, and lower overall lifetime earnings (Angeletos et al., 2001; Chabris et al., 2008; Nyhus & Webley, 2001; Meier & Sprenger, 2010; 2012; Golsteyn et al., 2013; Sutter et al., 2013). Indeed, discount rates have been identified as one of the strongest predictors of household financial behavior, controlling for a wide range of demographic and household characteristics (Klawitter et al., 2012).

However, delay discounting is not a fixed preference. Rather, it is both a state and trait variable, in that discount rates can be influenced by situational factors, yet people also have predisposed tendencies that they bring to each situation (Soman et al., 2005; O'Donoghue & Rabin, 2015; Odum & Baumann, 2010; Odum, 2011). For instance, experiencing poverty, psychological stress, and associated stress hormones has been shown to increase discount rates (Haushofer & Fehr, 2014; 2019; Bernheim et al., 2015; Riis-Vestergaard et al., 2018). In the current research, we test whether discount rates can also be shaped by the experience of income volatility.

Testing the relationship between income volatility and impatience. Rational economic models predict that income volatility will lead more patient economic preferences, driven by a motivation to protect against future income uncertainty. Research in consumer behavior suggests the opposite prediction – income volatility will lead to greater impatience, as uncertainty about the future causes people to narrow their attention towards more near-term outcomes at the expense of long-term planning. These competing theories have not been directly tested. Past research has sought to measure the extent of precautionary saving in response to income uncertainty, however, these studies have not measured the effects on underlying time preferences (Leland, 1968; Sandmo, 1970; Carroll, 1997; Mody et al., 2012). Furthermore, much of this empirical analysis on precautionary savings has either focused on the role of risk aversion in precautionary savings behavior (Kimball, 1990; Bommier & Grand, 2019), or on the impact of discrete income risks in a two-period setting – such as income shocks, risk of an economic downturn, and unemployment risk (Eeckhoudt & Schlesinger, 2008; Storesletten et al., 2004; Parker & Preston, 2005). In the current research, we measure income volatility experienced over time: 27-years of income in Study 1; 6 months of income in Study 2; and 12 months of income in Study 3. Furthermore, we focus directly on the psychological construct of impatience as our outcome of interest. We predict that income volatility will shape discount rates for people at all income levels, even after accounting for risk aversion.

Overview of studies. In Study 1, we use data from the National Longitudinal Study of Youth to investigate the effects of individual historical experiences of income volatility on financial impatience. We examine the effects of within-person biannual income volatility experienced over a 27-year period from 1980-2006 on a subsequent impatience, captured in the 2006 survey wave. In Study 2, we examine intra-year income volatility since most people experience more volatility within-years than across-years (Hannagan & Morduch, 2015; Farrell & Grieg, 2015). We test the effects of within-person monthly income volatility over the previous half-year on present-day financial impatience. In Study 3, we examine the effects of month-to-month income volatility over a full-year on an incentive compatible measure of impatience. In all of these studies, we control for risk aversion.

STUDY 1: effects of income volatility over 27-years

Data and Measures

Study 1 uses data from the National Longitudinal Study of Youth (NLSY79), a cohort survey conducted by the Bureau of Labor Statistics. The NLSY79 followed 12,686 individuals beginning in 1979 through 2016. The original sample included Americans born between 1957-1964, therefore they completed the first survey when they were 14-22 years old. Surveys were conducted every year from 1979 to 1994, and then every other year thereafter. In each survey wave, participants reported their annual income, allowing for a longitudinal analysis of income volatility from early adulthood through middle age. The NLSY79 includes a measure of financial impatience in only one survey wave (2006). Therefore, our analysis focused on the biannual income fluctuations experienced between 1980-2006 as a predictor of impatience in 2006. To distinguish the effects of volatility from overall wealth accumulation, we used a detailed measure of net worth captured in the 2004 survey wave as a control variable. Accounting for attrition across the total study period, the resultant sample in 2006 includes 7,653 individuals (51% women, ages 41-49, median net worth in 2004 was $65,000, median income in 2006 was $35,000).

Dependent variable. The key outcome of interest was financial patience measured in the 2006 survey wave using a matching elicitation method:

> *"Suppose you have won a prize of $1000, which you can claim immediately. However, you can choose to wait 1 month to claim the prize. If you do wait, you will receive more than $1000. What is the smallest amount of money in addition to the $1000 you would have to receive 1 month from now to convince you to wait rather than claim the prize now?"*

Following past research that has examined impatience in the NLSY79 (DeVoe et al., 2013; Courtemanche et al., 2015), we calculated a personal discount factor, k, for each individual such that k=V/A, where V is the immediate gain ($1000) and A is the total amount needed in order to wait one month. Therefore, a value of k=1 reflects total patience and values lower than 1 reflect successively greater impatience (method based on Mazur, 1987). We observed discount factors ranging from 0.001 to 1. Consistent with past research, we dropped participants who reported being perfectly patient (k = 1; *n* = 800), since this indicates a possible misunderstanding of the question, as well as observations lower than 3 standard deviations below the mean (*n* = 10). See Table A23 for robustness checks using no exclusion criteria for the measure of impatience.

Independent variables

Income Volatility. The NLSY79 includes a measure of individual earnings from salary, wages, and incentive-pay in every survey wave from 1980-2006. In order to rule out periods of unemployment as a source of income stability, we marked any year with $0 in reported income as missing data. We did not need to exclude high outliers with respect to income because the Bureau of Labor Statistics uses a top-code of $216,200 for annual income data in order to ensure respondents' anonymity. We calculated income volatility as the standard deviation of percentage change in year-to-year income based on methods used in Dynan et al. (2012) and Shin and Solon (2011). Biannual percentage change is calculated as follows: Percent Change $_{t-2 \text{ to } t}$ = $100*(Y_t - Y_{t-2}) / [(Y_t + Y_{t+2})/2]$, where Y indicates a respondents' annual income in a given year. This method is useful because it naturally bounds the range from -200% to 200%. We then calculated the standard deviation of biannual percentage changes as a summary measure of the income volatility an individual experienced between 1980-2006. By analyzing percentage changes, rather than absolute income levels, we did not need to use any transformations (such as adjustments for yearly Consumer Price Index) to maintain comparability over time. In order to be included in the analyses, a respondent must have reported income data in two consecutive biannual surveys at least five times between 1980-2006 (i.e. must report at least ten years of income data within the 27-year study period in order to calculate a meaningful measure of variation).

Control variables. We included two primary control variables: total net worth and risk-seeking for income. The NLSY79 captures highly detailed information on participants' assets and debts. This includes respondents' estimates of their home value, details on their mortgage, the market value and debt for all vehicles owned, the total value of investments (including stocks, bonds, mutual funds, and certificates of deposit), the value of IRAs and 401k accounts, the value of any cash savings and other assets (i.e. jewelry, art), and all other outstanding debts, such as credit cards and student loans. NLSY79 uses all of this information to compute a variable depicting total net worth. Since this information on assets and debts is not captured in the 2006 survey wave, we used the total net worth variable from the nearest preceding survey wave (2004). Total net worth includes respondents who are in debt and therefore have a negative net worth (9.3% of respondents) as well as those with $0 in net worth (7% of respondents).

We also included a measure of 'risk-seeking for income' as a control variable. It is possible that especially risk-seeking individuals self-select into jobs with more volatile incomes. Furthermore, rational economic models predict that income volatility will motivate people to engage in precautionary financial behaviors in proportion to their risk aversion (Kimball, 1990; Bommier & Grand, 2019). Therefore, we included risk-seeking for income as a control variable to examine whether the link between income volatility and impatience is explained by individual differences in risk-seeking. The NLSY79 includes a useful measure of risk-seeking that is specific to job choices and income. In the 2006 survey wave, respondents answered “yes” or “no” to two of the following three questions:

1. “Suppose you are the only income earner in the family, and you have a good job guaranteed to give you your current income every year for life. You are given the opportunity to take a new and equally good job, with a 50-50 chance that it will double your income and a 50-50 chance it will cut your income by a third. Would you take the new job?”
2. If the answer was “yes” to question 1, respondents are asked: “would you take a new job with a 50-50 chance that it would double your income and a 50-50 chance it would cut your income in half?”
3. If the answer was “no” to question 1, respondents are asked: “would you take a new job with a 50-50 chance that it would double your income and a 50-50 chance it would cut your income by 20%?”

These three questions can be used to create a 1-4 score for risk-seeking. Individuals are defined as *1=very risk averse* if they answer “no” to questions 1 and 3 (question 2 not asked); *2=somewhat risk averse* if they answer “no” to question 1 and “yes” to question 3 (question 2 not asked); *3=somewhat risk-seeking* if they answer “yes” to question 1 and “no” to question 2 (question 3 not asked); and *4=very risk-seeking* if they answer “yes” to questions 1 and 2 (question 3 not asked).

Study 1: Results and Discussion

Table 4 reports the means, standard deviations and intercorrelations between variables. The raw correlations show that patience is significantly negatively correlated with income volatility ($r = -.06$, $p < .01$). Controlling for total net worth and risk-seeking for income, greater income volatility experienced from 1980-2006 was associated with less patience in 2006, $\beta = -.052$, $t(4846) = -3.546$ $p < .001$, CI(β) = [-.080, -.023]. The regression models are reported in Table 5.

We also found a significant effect of total net worth on patience, such that wealthier individuals tend to be more patient, $\beta = .136$, $t(5014) = 10.078$ $p < .001$, CI(β) = [.109, .162]. This aligns with previous research demonstrating a link between poverty and impatience (Shah et al., 2012; Haushofer & Fehr, 2014; Falk et al., 2018). However, we found no interaction between income volatility and total net worth, $\beta = .008$, $t(5013) = .255$ $p = .726$, CI(β) = [-.056, .072]. That is, income volatility is associated with greater impatience across the wealth spectrum—even people who have accumulated significant wealth tend to be more impatient in response to income volatility. Figure 4 plots the relationship between income volatility and subsequent impatience in each wealth decile, from households at the 5th percentile of wealth (net worth of -$6,325) to households at the 95th percentile of wealth (net worth of $722,125).

Table 4 (Chapter 2). Study 1: descriptive statistics and correlations

Variable	*N*	*M*	*SD*	1	2	3	4
1. Patience (2006)	6,392	.71	.21	--			
2. Income volatility (1980-2006)	7,670	59%	29%	-.06**	--		
3. Total net worth (2004)	7,536	$190k	$400k	.16**	-.05**	--	
4. Risk-seeking for income (2006)	7,292	1.96	1.20	-.04**	.09**	-.03**	--

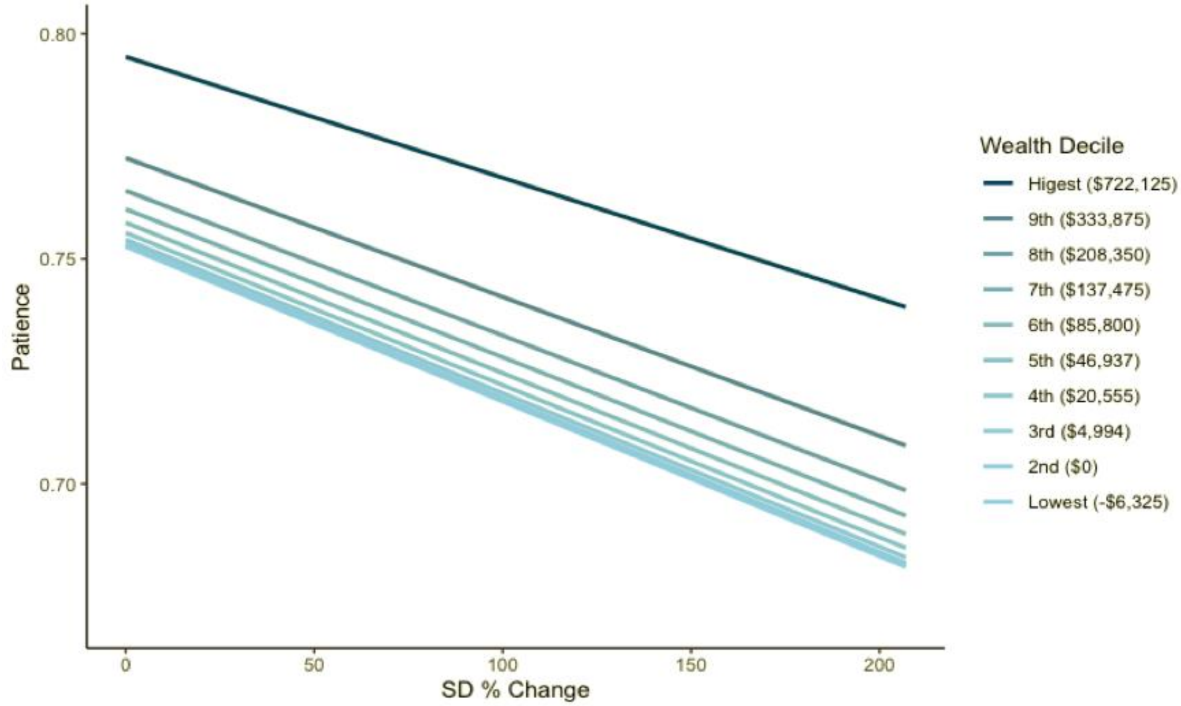
Notes. Reporting means, standard deviations, and correlations. Patience is measured in the 2006 survey and calculated as a monthly discount factor (k=V/A). Income volatility is measured using annual income data from each survey wave between 1980-2006. An overall measure of income volatility is calculated as the standard deviation of percentage change in bi-yearly income between 1980-2006. Total net worth is measured in the 2004 based on a detailed assessment all assets and debts. Risk-seeking for income is calculated based on responses to three questions in the 2006 survey wave.
Valid N (listwise) = 4,831. * $p < .05$, ** $p < .01$

Table 5 (Chapter 2). Study 1: effects of biannual income volatility on patience

DV: monthly discount factors (higher values reflect greater patience)	β	t	p	95% CI (β) Lower bound	95% CI (β) Upper bound
Model 1:					
Income volatility 1980-2006	-.052	-3.673	< .001	-.080	-.024
Net worth 2004	.136	10.078	< .001	.109	.162
Model 2:					
Income volatility 1980-2006	-.052	-3.546	< .001	-.080	-.023
Net worth 2004	.139	10.114	< .001	.112	.166
Risk-seeking for income 2006	-.028	-1.940	.052	-.057	.000
Model 3:					
Income volatility 1980-2006	-.054	-3.417	.001	-.085	-.023
Net worth 2004	.128	3.974	< .001	.065	.192
SD % change × Net worth	.008	.255	.799	-.056	.072

Note. Three OLS regression models predicting monthly discount factors (calculated based on responses in the 2006 survey wave). Income volatility is measured using annual income data from each survey wave between 1980-2006. An overall measure of income volatility is calculated as the standard deviation of percentage change in bi-yearly income between 1980-2006. Total net worth is measured in the 2004 based on a detailed assessment all assets and debts. Risk-seeking for income is calculated based on responses to three questions in the 2006 survey wave. Reporting standardized regression coefficients, *t*-statistics, *p*-values, 95% confidence intervals. Model 1 (N = 5,016), Model 2 (N = 4,848), Model 3 (N = 5,016).

Figure 4 (Chapter 2). Study 1: relationship between income volatility and patience in each wealth decile



Note. Interaction plot for the association between income volatility experienced between 1980-2006 and patience measured in the 2006 survey wave. Plotting simple slopes at the following wealth percentiles (within this sample): 5th, 10th, 15th, 25th, 35th, 45th, 55th, 65th, 75th, 85th, and 95th percentiles. *N* = 5,016.

These findings show that income volatility experienced over a 27-year period can shape current financial impatience, controlling for both risk-seeking and a highly detailed assessment of household assets and debts. The effects hold across the wealth spectrum, indicating that the psychological consequences of income volatility extend to households that are not living paycheck-to-paycheck. Although the results are correlational, the ordering and temporal separation of measurement in this study support our theory for a causal link between experienced income volatility and subsequent impatience. While this design minimizes concerns of a 'same source' bias (Richardson et al., 2009; Burton-Jones, 2009; Podsakoff et al., 2003), it cannot rule out reverse causality. It could be that static preferences for impatience lead respondents to choose income-earning activities that produce more volatile income streams. The ability to control for risk-seeking income preferences is an important step in addressing such an alternative explanation, but it cannot rule it out definitively.

Overall, this study provides evidence that long-run histories with income volatility are an important factor in predicting current intertemporal financial decision-making. However, shorter-term income fluctuations may be even more impactful. Many recurring expenses are paid on monthly cycles (e.g. credit card debts, phone bills, cable bills, rent, mortgages, car leasing payment, utilities, etc.) and therefore, even small deviations in monthly earnings could have significant consequences. As a result, people may be especially attuned to monthly income fluctuations. Therefore, in Study 2 we examine the effects of income volatility on a month-to-month time scale and explore the moderating role of perceived control over one's financial circumstances.

STUDY 2: effects of recent monthly income volatility

Many workers' incomes may *appear* stable when observed on a yearly basis, but when viewed on a more granular time-scale it becomes apparent that they experience substantial volatility (Morduch & Siwicki, 2018; Morduch & Schneider, 2017). Indeed, within-year income volatility is rising in the United States and households now experience more variability within-year than across-years (Bania & Leete, 2009; Farrell & Greig, 2015; 2016; Hannagan & Morduch, 2016). Most of this monthly volatility is due to fluctuations in within-job earned income, including upswings like bonuses and commissions as well as downswings like cuts to shift work and seasonal cuts (Farrell & Greig, 2016; Federal Reserve Survey, 2013). If the experience of income volatility causes greater impatience, we should be able to observe this link at the more granular level of recent income fluctuations across the income spectrum. Therefore, in this study we examine the effects of month-to-month income volatility on financial impatience, controlling for overall earnings across the study period and risk preferences. We used a sample of fully-employed workers in the United States in order to rule out the possibility that the consequences of income volatility are driven by changes to employment status. This sample, recruited via Amazon's Mechanical Turk, is slightly lower-income compared to the overall population of the United States. We are particularly interested in the experience of low-income individuals because they are the most exposed to the harms of income volatility.

In addition, we explore perceived control over financial outcomes as a moderator. The effect of income volatility on impatience may depend on how much personal control people feel over their financial circumstances. That is, income volatility may have a greater influence on impatience when fluctuations are caused by factors outside of one's control. This likely involves both *real* and *perceived* control over income fluctuations. For instance, some income fluctuations are mostly controllable (e.g. many workers in the 'gig' economy treat this work as a secondary source of income that can be increased or decreased at their discretion; see Irwin, 2019 and Collins et al., 2019). Other sources of income volatility involve a lesser degree of control, such as earnings from causal labor, shift work, and incentive-pay. Moreover, psychological predispositions and childhood experiences can lead people to feel a greater or lesser degree of control in uncertain environments, irrespective of actual control. For example, growing up in stressful, low-SES environments can lead people to feel less control when faced with economic uncertainty in adulthood, leading to more impatient decision-making (Griskevicius et al., 2013; Mittal & Griskevicius, 2014; Compas et al., 1991; Frankenhuis et al., 2016).

Data and Measures

We collected monthly income data from a sample of 326 fully-employed workers via Amazon's Mechanical Turk (ages 18-72, M_{age} = 37.046, *SD* = 11.884; 44% women; 43% single, 42% married, 14% living with someone as a couple; 46% have at least one child; median income = \$40,000-\$49,000 per year). Participants reported their income for each of the past six months using any records they had available (e.g. online banking, pay stubs, etc.), and then their monthly incomes were displayed in a line graph. We tested two different line graph formats to investigate whether the display method influenced participants' perceptions of their own income volatility. The graphical display method did not influence participants' perceptions of income volatility, $F(1, 324) = 1.40$, $p = .238$, and had no effect on financial impatience, $F(1, 285) = .002$, $p = .967$. Therefore, we report the effects of income volatility on financial impatience collapsing across display method (see Table 7 and Table A25).

After viewing the graph of their monthly incomes, participants completed the same measure of patience as in Study 1, followed by an assessment of risk preferences (Benjamin et al., 2010). Lastly, participants responded to a scale measuring perceived financial control. To capture perceived financial control, we adapted the general locus of control scale developed by Pearlin and Schooler (1978). The general locus of control scale has been validated across a large number of studies (see Cobb-Clark & Schurer, 2013; Cobb-Clark et al., 2016) and it has been adapted to the domain of personal health (Wallston & Wallston, 1981). In this study, we adapted the items so that they were specific to personal finances. Participants rated their agreement with the following 6 items on a 1 (*strongly disagree*) to 5 (*strongly agree*) scale: "I have little control over my financial circumstances," "There is really no way I can solve some of the financial problems that I have," "There is little I can do to change my financial circumstances," "I often feel helpless in dealing with problems related to money," "Sometimes I feel that I am pushed around in life by my financial circumstances," and "My financial future mostly depends on me" (α = .81). All but the last item on this scale are reverse-coded so that higher scores indicate more perceived control over one's financial situation (*M* = 3.306, *SD* = .931).

As in the previous study, we measured experienced income volatility as the standard deviation of percentage change in income. We calculated the standard deviation of percentage change on a month-to-month time-scale across the 6-month period captured in this study. We also calculated monthly discount factors using the same method as in Study 1 (k=V/A), dropping respondents who indicated perfect patience (*n* = 36) and observations lower than 3 standard deviations below the mean (*n* = 2). We calculated an ordinal measure of risk-seeing (0-10 scale) using the elicitation procedure in Benjamin et al. (2010), and for financial control we calculated the average score (1-5 scale) across the 6 items that were adapted from Pearlin and Schooler (1978) for this study. See Table A26 for robustness checks using no exclusion criteria for the measure of impatience.

Study 2: Results and Discussion

Table 6 reports the means, standard deviations and intercorrelations between variables. The raw correlation shows income volatility is negatively correlated with patience (r = -.15, p < .01). We note the associate between monthly income volatility and patience is stronger than the association observed in the previous study using biannual income volatility (r = -.06). When we controlled for total 6-month earnings and risk preferences, we found a significant negative association between monthly income volatility and patience, β = -.189, t(282) = -3.538 p < .001, CI(β) = [-.295, -.084] (Table 7). Furthermore, as in Study 1, we found no significant interaction between income volatility and total 6-month earnings with respect to effects on patience, β = -.035, t(285) = -.561 p = .575, CI(β) = [-.158, .088]. We did not analyze the effects at each income level, as in Study 1, due to the smaller sample and narrower range of income levels within this sample.

In order to investigate the role of perceived control over financial circumstances, we first tested the interaction between monthly income volatility and perceived financial control with respect to effects on patience. We observed a significant interaction, β = -.515, $t(281)$ = -2.974 p = .003, CI(β) = [-.856, -.174]. To further probe the role of perceived financial control, we conducted a bootstrapped moderation analysis. The results indicate that income volatility had a greater effect on patience when people felt like they had little control over their financial circumstances. Adjusting for total 6-month earnings, income volatility was a significant negative predictor of patience when perceived financial control was low (conditional effect, β = -.314, p < .001, 95% CI(β) = [-.457, -.169]) and at the mean (conditional effect, β = -.d157 p = .005, 95% CI(β) = [-.267, -.047]), but there was no association when perceived financial control was high (conditional effect, β = .001, p = .993, 95% CI(β) = [-.159, .157]. See Figure 5 for a simple slopes plot of the association between income volatility and patience at these three levels of financial control.

We also conducted a Johnson-Neyman floodlight analysis to estimate the effects of income volatility on patience across the entire range of perceive financial control. We found that, at a 95% confidence level, income volatility was associated with significantly greater impatience when perceived financial control was less than 3.59 on a 1-5 scale, which included 56% of our sample.

Table 6 (Chapter 2). Study 2: descriptive statistics and correlations

Variable	*N*	*M*	*SD*	1	2	3	4	5
1. Patience	290	.72	.23	--				
2. Income volatility (Nov – May)	325	27%	31%	-.15*	--			
3. Total 6-month earnings	325	$20k	$40k	-.34**	-.11	--		
4. Risk preferences (0-10)	323	4.19	3.24	.00	.04	.17**	--	
5. Financial control (1-5)	326	3.31	.93	.17**	.05	-.02	.05	--

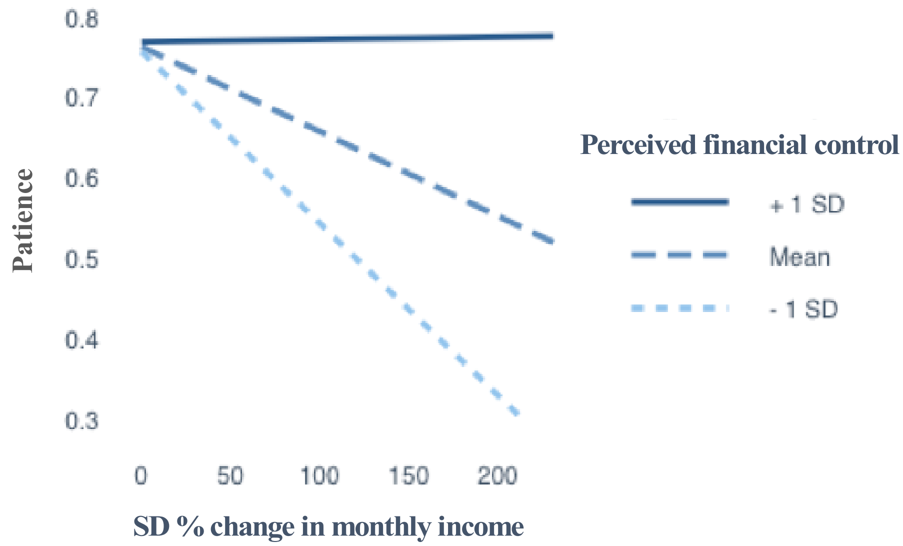
Note. Reporting means, standard deviations, and correlations. Patience is measured in June as a monthly discount factor (k=V/A). Income volatility is measured using monthly income data for each month between from November 2017 through May 2018. An overall measure of income volatility is calculated as the standard deviation of percentage change in month-to-month income from November 2017 through May 2018. Total 6-month earnings is the sum of monthly incomes over this period. Risk preferences is measured as an ordinal scale (0-10) with higher values reflecting greater risk-seeking. Financial control is measured using the 6-item scale adapted from Pearlin and Schooler (1978). Valid *N* (listwise), $N = 283$. * $p < .05$, ** $p < .01$

Table 7 (Chapter 2). Study 2: effects of monthly income volatility on patience

DV: monthly discount factors	β	*t*	*p*	95% CI (β) Lower bound	95% CI (β) Upper bound
Model 1:					
Income volatility (Nov – May)	-.178	-3.277	.001	-.285	-.071
Total 6-month earnings	-.353	-6.537	< .001	-.459	-.246
Model 2:					
Income volatility (Nov – May)	-.189	-3.538	< .001	-.295	-.084
Total 6-month earnings	-.362	-6.737	< .001	-.468	-.256
Risk preferences	.067	1.180	.239	-.045	.179
Model 3:					
Income volatility (Nov – May)	-.164	-2.734	.007	-.2282	-.046
Total 6-month earnings	-.336	-5.482	< .001	-.457	-.215
Income volatility × earnings	-.035	-.561	.575	-.158	.088

Note. Three OLS regression models predicting monthly discount factors. Reporting standardized regression coefficients, *t*-statistics, *p*-values, 95% confidence intervals. Patience is measured in June as a monthly discount factor (k=V/A). Income volatility is measured using monthly income data for each month from November 2017 through May 2018. An overall measure of income volatility is calculated as the standard deviation of percentage change in month-to-month income from November 2017 through May 2018. Total 6-month earnings is the sum of monthly incomes over this period. Risk preferences is measured as an ordinal scale (0-10) with higher values reflecting greater risk-seeking. Model 1 ($N = 289$), Model 2 ($N = 286$), Model 3 ($N = 289$).

Figure 5 (Chapter 2). Study 2: relationship between income volatility and patience at three levels of perceived financial control



Note. Simple slopes plot for the association between income volatility and discount factors at three levels of perceived financial control (-1 *SD* = 2.38, *Mean* = 3.31, and +1 *SD* = 4.24).

This study shows that monthly income volatility predicts subsequent impatience. Comparing these results with the previous study, we observe that monthly income volatility is a much stronger predictor of impatience than biannual income volatility. This may be because month-to-month income fluctuations are more difficult to manage from a practical budgeting standpoint and therefore deviations from the mean are more psychologically impactful. Cross-sectionally, overall income does not appear to buffer against the effects of income volatility. Rather, these findings indicate that people are affected by income volatility to the extent that they feel in control of their financial circumstances. For those who report a high degree of control over their financial lives, income volatility is unrelated to financial impatience. These findings align with past research showing that a sense of personal control is critical in self-regulation, cognitive control, and goal achievement (Bandura & Wood, 1989; Karniol & Ross, 1996; Schmid et al., 2015).

In Study 3, we sought to conceptually replicate the effects of monthly income volatility and perceived financial control on impatience. In order to conduct a rigorous test of our theory, we preregistered our predictions, we collected a representative sample of American adults, stratified on income level, and we used an incentive compatible measure of impatience.

STUDY 3: effects of monthly income volatility across the income spectrum

Data and Measures

We collected a full-year of monthly income data from a sample of 930 participants in the United States recruited via Qualtrics Panels (M_{age} = 43.82, *SD* = 15.85; 56% women; median annual income = $50,000; 60% employed full-time). We used stratified sampling on annual income to recruit participants from across the income distribution and we over-sampled low-income individuals (<$40,000 in annual income in 2019) such that this group comprised at least one third of the overall sample. This sampling approach ensured that we could conduct a highly-powered moderation analysis to examine the effects of income volatility on impatience across the economic distribution.

This study was conducted in December 2020. Participants reported their income for each of the past 11 months (January 2020 through November 2020) using any records they had available (e.g. online banking, pay stubs, etc.). As in the previous study, we measured income volatility as the standard deviation of percentage change in income from month-to-month. As our measure of impatience, we offered participants the chance to receive a real $1,000 cash prize via a check in the mail. They could choose to receive the full sum immediately by allocating the money to a ‘spending’ account (to be sent in a check in 2 days) or set aside a portion of this money in a study-specific ‘savings’ account. Participants were informed that any money allocated towards savings would be sent by mail in a separate check in 6 months, plus 10%

interest. We explained that one person from this study would be randomly selected to receive this money for real: *"If you are selected, you will be asked to provide your mailing address so that we can send you two checks in the mail. The 1st check will be for the amount you allocate to spending, and it will be mailed within 2 days. The 2nd check will be for the amount you allocate to savings (plus 10% interest), and it will be mailed in 6 months."* We also provided three examples to ensure that participants understood the choice they were making: *"If you allocate all of the money to spending, you will receive $1000 in 2 days and nothing in 6 months; If you allocate half of the money to spending and half to savings, you will receive $500 in 2 days and also $550 in 6 months; If you allocate all of the money to savings, you will receive nothing in 2 days and $1100 in 6 months)."* The interest rate of 10% was chosen based on the results of a pilot experiment with participants recruited from the same population.

In our preregistration, we defined our measure of impatience as the amount of money participants allocated to savings (money received in a check sent in 6 months, plus 10% interest) versus spending (money received in a check sent in 2 days). This allocation decision between a sooner-smaller and larger-later cash prize captures a similar measure of intertemporal preferences as in the matching elicitation method used in the previous two studies.

Lastly, we administered the 6-item measure of perceived financial control as in the previous study ($\alpha = .83$) and collected information on demographics and financial literacy.

Preregistered Analysis Plan

We preregistered two predictions for this study. First, we predicted that income volatility would be significantly negatively associated with the amount of money participants chose to save in the financial allocation task, controlling for total 11-month earnings. In our analysis plan, we preregistered that we would regress the amount saved (portion of the $1000 cash prize) onto the variable for income volatility (standard deviation of percentage change in month-to-month income over the 11-month study period), controlling for total 11-month earnings. A significant negative coefficient on income volatility would indicate support for our first prediction. Second, aligning with the results of Study 2, we predicted that the relationship between income volatility and impatience would be moderated by perceived financial control such that the effect would be stronger when people feel little control over income fluctuations. We preregistered that we would test the interaction term between income volatility and perceived financial control. A significant negative coefficient would indicate support for our second prediction.

Results and discussion

Table 8 reports the means, standard deviations, and intercorrelations between variables. Table 9 reports the full regression results. The results indicate support for our first prediction that greater monthly income volatility would predict a lower allocation to savings in the intertemporal decision task, controlling for overall earnings, β = -.083, $t(839)$ = -2.737 p = .018, CI(β) = [-.151, -.014]. Similar to the results of Study 1, the effects of income volatility on savings allocation persist across the income distribution. We find no interaction effect between income volatility and total 11-month earnings, β = .029, $t(838)$ = .703 p = .482, CI(β) = [-.052, .111]. Figure 6 plots the relationship between income volatility and saving allocation in each income decile, from households at the 5th percentile of income (11-month earnings of $701) to households at the 95th percentile of income (11-month earnings of $160,000). However, we do not find support for our second prediction for an interaction effect between income volatility and perceived financial control. This may be because the more representative sample was a full point lower on financial locus of control scale and we were not observing sufficient variance at the high levels for this to be detected as an interaction.

Table 8 (Chapter 2). Study 3: descriptive statistics and correlations

Variable	*N*	*M*	*SD*	1	2	3	4	5
1. Amount allocated to saving	930	$636	$298	--				
2. Income volatility (Jan – Nov)	841	25%	37%	-.09**	--			
3. Total 11-month earnings	882	$53k	$104k	.05	-.12**	--		
4. Risk preferences (0-10)	795	3.41	2.57	.05	.04	-.03	--	
5. Financial control (1-5)	930	2.32	.88	-.19**	.15**	-.17**	.03	--

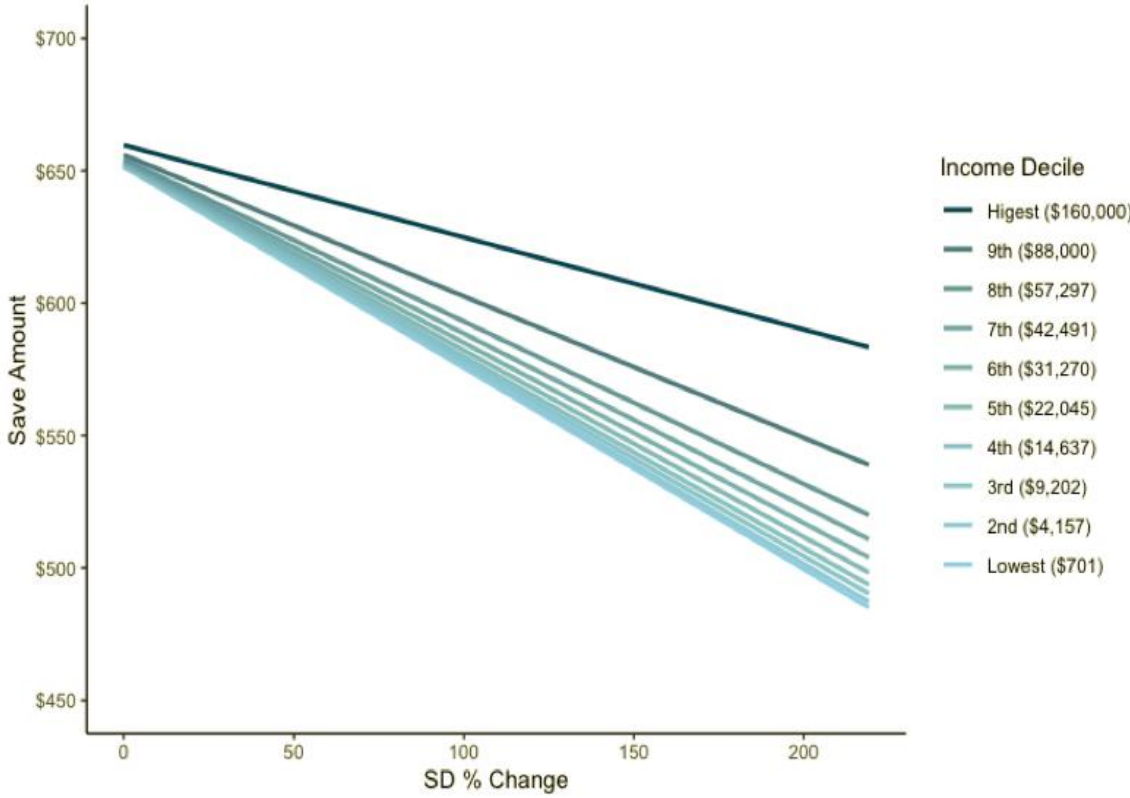
Note. Reporting means, standard deviations, and correlations. Amount allocated to savings was measured on December 3-8. Monthly income volatility is measured as the standard deviation of percentage change in month-to-month income from January to November. Total 11-month earnings represents the sum of monthly incomes over this period. Risk preferences is measured as an ordinal scale (0-10) with higher values reflecting greater risk-seeking. Financial control is measured using the 6-item scale adapted from Pearlin & Schooler (1978). Valid *N* (listwise), $N = 795$. * $p < .05$, ** $p < .01$

Table 9 (Chapter 2). Study 3: effects of monthly income volatility on savings allocation

DV: amount allocated to savings	β	*t*	*p*	95% CI (β) Lower bound	95% CI (β) Upper bound
Model 1:					
Income volatility (Jan – Nov)	-.083	-2.373	.018	-.151	-.014
Total 11-month earnings	.031	.906	.365	-.036	.098
Model 2:					
Income volatility (Jan – Nov)	-.100	-2.580	.010	-.176	-.024
Total 11-month earnings	.044	.960	.337	-.046	.135
Risk preferences	.067	1.428	.154	-.025	.159
Model 3:					
Income volatility (Jan – Nov)	-.095	-2.442	.015	-.171	-.019
Total 11-month earnings	.018	.459	.646	-.059	.096
Income volatility × earnings	.029	.703	.482	-.053	.111

Note. Three OLS regression models predicting amount allocate to saving. Reporting standardized regression coefficients, *t*-statistics, *p*-values, 95% confidence intervals. Monthly income volatility is measured as the standard deviation of percentage change in month-to-month income from January to November. Total 11-month earnings represents the sum of monthly incomes over this period. Risk preferences is measured as an ordinal scale (0-10) with higher values reflecting greater risk-seeking. Model 1 ($N = 841$), Model 2 ($N = 726$), and Model 3 ($N = 841$).

Figure 6 (Chapter 2). Study 3: relationship between income volatility and patience in each income decile



Note. Interaction plot for the association between month-to-month income volatility and savings allocation at each income decile). Plotting simple slopes at the following 11-month income percentiles (within this sample): 5th, 10th, 15th, 25th, 35th, 45th, 55th, 65th, 75th, 85th, and 95th percentiles. $N = 841$.

This study conceptually replicates the central result showing that income volatility is an important predictor of impatience, controlling for overall earnings. This association holds across the income spectrum, demonstrating that both rich and poor households' intertemporal decisions can be affected by the experience of recent income volatility. However, we did not find support for our second prediction regarding the role of perceived financial control. Further research is needed to understand how controllable and uncontrollable income fluctuations affect financial decision-making.

GENERAL DISCUSSION

This research examines the consequences of income volatility for financial decision-making. In Study 1, we use individual-level longitudinal income data collected over a period of 27 years, spanning from late adolescence through middle age. Experiencing greater income volatility over this period predicts increased subsequent financial impatience, controlling for total net worth and a measure of risk-seeking for income. A limitation of this study is that the National Longitudinal Study of Youth only included the measure of impatience in the 2006 survey wave, and therefore we are unable to observe within-person changes in impatience over time.

In Study 2, we zoom-in to examine the relationship between month-to-month income volatility and impatience. Monthly income volatility, which has also been increasingly precipitously in the United States (Bania & Leete, 2009), may be especially psychologically impactful due to the ways in which people budget. For instance, a recent nationally-representative survey examining household budgeting practices found that over 85% of respondents adjust their budget on a weekly or monthly time horizon (Zhang et al., 2020). Consistent with this notion, we find a stronger association between income volatility and impatience when analyzed on this more granular time scale. Furthermore, we found that intertemporal decisions were more shaped by income volatility when people feel like their financial circumstances are largely outside of their control.

In Study 3, we conducted a preregistered, highly-powered conceptual replication of the previous study. We tested the relationship between monthly income volatility on impatience using a representative sample of Americans and an incentive compatible measure of impatience. We found that monthly income volatility predicted impatient intertemporal decision-making for individuals from across the economic distribution. However, we did not replicate the findings with respect to perceived financial control and so caution is warranted in interpreting the significance of the interaction that did emerge in exploratory analyses in Study 2.

A limitation that persists across these studies is the inability to rule out liquidity constraints as a possible alternative mechanism that could partially explain the link between income volatility and impatience. There is some research indicating that the proportion of liquidity-constrained households has risen slightly in the United States between 1983 to 2007, alongside rising income volatility (Dogra & Gorbachev, 2016). Even wealthy households may face liquidity constraints due high costs of living and social pressures to maintain their lifestyle. Households that have more volatile incomes may simultaneously face liquidity constraints, and therefore may exhibit more impatience economic preferences because they are less able to wait for larger, delayed financial rewards.

Despite these limitations, these studies highlight an important psychological link of income volatility with impatience. The increase in impatience associated with income volatility is poised to undermine the compensatory responses likely needed to effectively manage a volatile income over time. While these consequences are no doubt experienced most severely by poor households, our findings show that income volatility is related to increased impatience regardless of income level, which suggests that many of the harmful consequences associated with poverty may extend to households who are not poor but face increasingly volatile income streams. Therefore, these results have important practical implications for the study of labor markets, compensation structures in organizations, and the design of economic aid programs.

Practical Implications

This research provides insights for policymakers and companies. Indeed, a direct implication of this research for policymakers and employers is that they should focus both on reducing the amount of income volatility workers experience as well as increasing workers' control over income fluctuations.

Implications for policymakers. Many policy programs designed to support low-income households inadvertently increase income volatility. For instance, many income assistance programs—unemployment benefits, Supplemental Security Income, Temporary Assistance for Needy Families, and food stamps—are frequently interrupted because recipients are required to re-certify their eligibility. For instance, food stamps recipients are typically required to re-apply every 6 or 12 months (Carlson & Keith-Jennings, 2018). While verifying eligibility (e.g. income-testing) is important for ensuring that the program reaches the target recipients, lengthy renewal processes often exacerbate income volatility as people wait for payments to resume. In addition, benefit amounts often change from one eligibility period to the next based on reported income level. These programs focus on *absolute* levels of poverty and, in doing so, often neglect 'episodic poverty'—periodic dips in monthly income that cause people to temporarily fall below the poverty line (Morduch & Siwicki, 2017). Individuals may lose benefits if they experience income spikes in the months leading up to re-certification and find themselves facing an income dip without the safety net of benefits in the subsequent months. Our results suggest that income assistance programs should be designed to account for episodic poverty by lengthening eligibility periods and providing greater predictability to recipients. In addition, these programs should consider the fact that people with volatile incomes may be especially impatient. As a result, they may drop out of lengthy benefit application and renewal processes, and they may spend benefits more impulsively if they feel like they could lose these benefits in the next re-certification process.

In the United States, some recent state and municipal legislation has been introduced to address irregular working hours, which has the effect of reducing income volatility. Irregular working hours is the largest cause of income volatility, especially for low-income workers in the food services and retail industries (Federal Reserve, 2018). Together, these industries account for 28 million US jobs—19% of the total US workforce (Bureau of Labor Statistics, 2019). Aside from managerial roles, most people are paid hourly and they typically receive just 3-7 days' notice on their working schedule for the following week (Williams et al., 2018). Schedule volatility has been shown to harm employee wellbeing and increase financial strain (Henly & Lambert, 2014). To address these concerns, 'fair workweek' legislation was passed in Oregon in 2017, followed by municipal ordinances in Seattle, San Francisco, Philadelphia, Chicago, and New York City (Wolfe, Jones, & Cooper, 2018). This legislation requires food service and retail companies employing >700 people to post schedules at least 2 weeks in advance and compensate workers for any last-minute changes. On a federal level, fair workweek legislation has been introduced, but not passed. The Schedules That Work Act (S.1772, 2015) would require companies in the food service, cleaning, hospitality, warehouse, and retail industries to provide at least 2 weeks' notice on schedules for all employees. A recent study found that fair workweek legislation can improve family wellbeing (Gassman-Pines & Ananat, 2018). Our findings suggest that more advance notice and predictability in work schedules—and therefore earnings—may also bolster feelings of financial control and help workers make more patient financial decisions.

Implications for employers and financial institutions. Companies can also benefit from stabilizing workers' schedules. In the food service and retail industries, irregular working hours are typically a result of companies seeking to match labor to predicted customer traffic in order to increase profitability (Perdikaki et al., 2012). However, the evidence indicates that irregular scheduling does not increase profits, and it may have downstream consequences on workers' wellbeing, productivity, and retention (Williams et al., 2018; Schneider & Harknett, 2019; Choper et al., 2019). An experiment with Gap stores in San Francisco and Chicago found that stabilizing sales associates' working hours led to an increase in workers' productivity as well as store-level sales (7% increase in sales in the treatment stores relative to control stores during the 10-month intervention period; Williams et al., 2018).

Companies should also consider restructuring their employees' compensation packages so that incentive-pay represents a smaller proportion of overall compensation or disburse incentive-pay more evenly throughout the year. Furthermore, companies should ensure that workers feel a high degree of control over their incentive-pay outcomes. Incentive-pay tends to increase workers' psychological focus on the incentives themselves (Hur & Nordgren, 2016), and can often be effective in increasing workers' productivity (Weibel, Rost, & Osterloh, 2007). However, there may be deleterious downstream effects on financial wellbeing. Our results suggest that, holding total compensation constant and to the extent that incentive-pay schemes feel uncontrollable, workers with a higher proportion of incentive-pay may be more financially impatient. Given the interaction with financial locus of control did not replicate in our nationally representative sample, more research needs to be done before clear policy proscriptions can be made regarding the controllability of financial income.

Companies can also reduce workers' income volatility through income-smoothing initiatives such as subsidized employee loans and early access to paychecks. For instance, Walmart allows their 1.4 million workers to access a portion of their pay between bi-weekly paychecks for hours they have already worked (Corkery, 2017). Walmart's intention was to help their hourly employees—who earn an average of $14.26/hour—avoid payday loans. Our findings indicate that this initiative could increase their employees' financial patience and help them stick to longer-term financial plans. Additionally, employers could help their employees manage income volatility via short-term savings programs. Many employers already offer matched-contribution 401(k) savings programs for retirement. Short-term savings programs with similar employer-matched contributions may be even more beneficial for overall financial wellbeing.

Given rising income volatility, there is a growing need for new financial products and innovations that can help workers from across the socioeconomic spectrum smooth their income and feel a greater sense of control over their financial life. Financial literacy initiatives are largely ineffective (Fernandes et al, 2014) and short-term credit products ('short-term, small-dollar credit' such as payday loans, auto loans, and bank overdraft protection) tend to worsen long-term financial stability (CFPB, 2014). Banks and fintech companies need to develop new financial products tailored to the primary source of income volatility: within-job earnings fluctuations. Existing products tend to focus exclusively on bridging temporary shortfalls, rather than helping consumers smooth their income.

People who experience income volatility should have a strong incentive to be patient, resisting impulse spending and increasing precautionary savings to protect against future income shocks. Our research shows that, despite these incentives, the psychological experience of income volatility is linked with impatience. Policymakers and companies need to consider not only the absolute level of financial constraints facing workers, but the practical and psychological consequences of income volatility.

Chapter 3: Allocating Financial Windfalls: Budgeting as Choice Architecture

ABSTRACT

Most people want to save more money. Yet, they are often unsure how much they can or should allocate towards savings at any given time. In order to simplify these types of financial decisions, people often organize their financial priorities into meaningful categories. Budgeting and earmarking money is a fundamental strategy that people use to make financial allocation decisions and pursue their financial goals. In this research, we investigate how financial allocation decisions are shaped by the structure of consumers' budgets and the budgeting procedure they follow. Across six preregistered studies (N = 3,234), we demonstrate that budget structures and procedures can have large, unintended effects on saving versus spending decisions. Based on these insights we developed and tested a budgeting procedure that increased intended allocations to saving by 29%.

Most people use some form of budgeting to manage their household finances. When people receive a financial windfall – whether from a bonus, commission, stimulus check, or any other unexpected lump sum – they often rely on their budget in order to decide how to allocate this money. For example, a recent survey found that 66% of households refer to a budget when making financial decisions (Zhang et al., 2020).

Budgeting is defined by creating distinct financial categories and earmarking money for specific purposes. For instance, people might divide a financial windfall into categories such as 'bills and expenses,' 'debts,' 'discretionary spending,' 'retirement savings,' and 'emergency savings.' The ways in which people design their budget reflects their priorities, regrets, and aspirations for the future. However, households' budgeting *process* can also shape savings and consumptions decisions (Galperti, 2019; Kosegi & Matejka, 2020). In this research, we investigate how seemingly arbitrary features of household budgets can have large, unintended effects on financial allocation decisions. Across six preregistered experiments (N = 3,234), we demonstrate how different budgeting procedures can influence savings contributions and we investigate the mechanism through which budgets shape financial decision-making.

Deciding whether to spend or save a financial windfall

Decisions about how much to save versus spend at any given time involve trade-offs between financial obligations, immediate desires, and long-term plans as well as predictions about future earnings and expenses. Rational economic theories assume that households approach these decisions as an optimization problem in which saving and spending amounts in each time period are determined based on a desire to smooth consumption over their lifespan (Ando & Modigliani, 1963; Hall, 1978; Browning & Crossley, 2001; Gourinchas & Parker, 2002). However, people deviate from rational economic models of life-cycle consumption for several reasons. People tend to discount future outcomes relative to near-term desires (Laibson, 1997), struggle with self-control (Fujita, 2011), and succumb to social pressures to overspend on conspicuous goods and services (Bagwell & Bernheim, 1996; Charles et al., 2009). In addition, people tend to be over-optimistic about their future financial circumstances, under-estimating their future expenses and overestimating their future discretionary income (Ülkümen et al., 2008; Howard et al., 2018; Berman et al., 2016; Zauberman & Lynch, 2005). All of these psychological tendencies bias people in the same direction – towards over-spending and under-saving in each time period.

People are especially inclined towards immediate consumption when they are allocating a financial windfall, as opposed to a stable income (Bodkin, 1959; Arkes et al., 1994).[2] Financial windfalls, by definition, are unexpected and temporary income boosts. Such windfalls are becoming increasingly common due to changes in modern labor markets. A growing number of people work in jobs with irregular pay schedules, or compensation schemes that include a large proportion of incentive-pay, such as bonuses, tips, commissions, and stock options (Federal Reserve 2018; Lemieux et al., 2009; Lazear, 2018). As a result, income volatility is rising, and households are fronted with periodic financial windfalls (OECD, 2019; Dynan et al., 2012; Gottschalk & Moffitt, 2009; Morduch & Schneider, 2017). Given these trends, decisions about how to allocate financial windfalls have become a crucial determinant of overall financial

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[2] Past literature examining the allocation of windfalls gains has found contradictory evidence. Bodkin (1959) found that people have a higher marginal propensity to consume windfall gains, in contrast to the permanent income hypothesis (Friedman, 1957) and the life-cycle theory of consumption (Ando & Modigliani, 1963). Additional studies also found increased consumption of windfall gains, relative to ordinary income (Jones, 1960). However, Kreinen (1961) and Reid (1962) did not find any increased propensity to consume windfalls. Using a survey of Israeli families receiving lump sum restitution payments from German, Kreinen finds no increase propensity for immediate consumption. More recent evidence indicates that the extent to which windfalls are spent on immediate consumption depends on the size and framing of the windfall (Keeler, James, & Abdel-Ghany, 2012; Epley & Gneezy, 2007). Smaller windfalls are more likely to be spent, as are windfalls that are framed as departures from the status quo, rather than returns to the status quo. This may explain why the stimulus checks during COVID-19 were largely allocated towards saving – these windfalls were relatively large and framed as a partial restitution for lost income.

wellbeing. Windfalls also offer a critical window into the role of budgeting in financial allocation decisions since windfalls gains, by dint of being unexpected, are typically received without predefined plans or earmarks.

Budgeting in financial decision-making

Budgeting is a fundamental strategy that people use to resist the biases that lead to over-consumption. Indeed, household budgeting is ubiquitous across cultures and generations (Zelizer, 1989; Graeber, 2011). Anthropological accounts have documented a wide array of related budgeting strategies, including physically separating cash into multiple envelopes or pitchers, giving cash to multiple friends or family members for safe-keeping, purchasing illiquid assets as a means of protecting and storing wealth for future use, opening multiple bank accounts (e.g. saving in separate accounts for different purposes), and creating labeled accounts using online banking and software applications (Zelizer, 1989; 2017). Across these disparate budgeting strategies there are some common features. Fundamentally, consumers budget by partitioning household finances into multiple categories and allocating money into these categories so that they can be used for different purposes (Heath & Soll; 1996; Zhang et al., 2020).

Past research on budgeting has focused on household budgets as a tool to bolster self-control. A large body of evidence has shown that categorizing and earmarking money are indeed effective strategies to curb impulsive spending (Shefrin & Thaler, 1981; 2004; Henderson & Peterson, 1992; Wertenbroch, 2002; Benabou & Tirole, 2004; Antonides et al., 2011; Beshears et al., 2016). Once money is earmarked towards a specific purpose, people tend to treat it as non-fungible and, therefore, budget partitioning can protect money from being spent on temptation goods or other expenses (Thaler, 1999; Health & Soll, 1996; Hastings & Shapiro, 2013; 2018). For example, in a field study with low-income households in rural India, Soman and Cheema

(2011) found that physically partitioning a predefined portion of weekly income into a sealed envelope reduced the likelihood that this money would be spent.

However, budgets not only affect how money is spent over time, they may also have a profound effect on how money is allocated in the first place. By comparison, much less attention has been given to the role of budgets in initial allocation decisions. In the current research, we focus on the specific features of budgeting partitioning that influence how people allocate financial windfalls. These features include the number of spending and savings categories, the partitioning of these categories, and the sequence with which people allocate money into each category.

Budgeting and heuristic decision-making

Budgets are intended to simplify financial allocation decisions. One simplifying strategy that consumers often use when allocating scarce resources is a *naïve diversification heuristic*, also referred to as a *1/n heuristic* (Messick, 1993). Applying a 1/n heuristic entails the following decision process: use equal division across identified categories, recipients, or causes as a benchmark, and then make adjustments based on the details of the situation (Messick & Schell, 1992). A 1/n heuristic is useful and efficient in many types of allocation decisions – it effectively reduces cognitive complexity and it is easy to justify and explain to others (Messick 1993; Samuelson & Allison, 1994; Fehr & Schmidt, 1999). Therefore, it is possible that people may use a 1/n heuristic in financial windfall allocation decisions.

However, while a 1/n heuristic may be useful and efficient, it can also lead to systematic biases because people tend to make insufficient adjustments from the benchmark of equal division across the option space. As a result, allocations decisions can be influenced by the configuration of the option space (Fox et al., 2005). In the domain of budgeting, the option space is defined by the ways in which financial categories are partitioned. For instance, imagine you were deciding how to allocate a holiday bonus across two categories: spending and savings. How much would you allocate to each category? Many people choose an even allocation, contributing approximately 50% of their bonus to each category, in keeping with a 1/n of heuristic. That is, they ask themselves: "How should I spread this bonus across my budget?" This mindset leads to a decision process in which people anchor on a 1/n heuristic and then adjust based on their current needs, desires, and circumstances. This strategy seems reasonable, but what if your budget was structured differently? Consider a budget in which the savings category is partitioned into discrete sub-categories such that there is a total of five categories: 1) spending, 2) savings for emergencies, 3) savings for retirement, 4) savings for upcoming expenses, and 5) savings for all purposes. Applying the same 1/n heuristic would lead people to allocate 80% of their bonus to savings. This example demonstrates how a 1/n heuristic can lead to resource allocation decisions that are *partition-dependent* such that people are biased by the subjective grouping of the option set (Fox & Rottenstreich, 2003; Langer & Fox, 2005; Tannenbaum et al., 2014; Bardolet et al., 2011).

However, people do not always apply a 1/n heuristic when allocating a financial windfall. More commonly, people rely on defaults, such as those defined by their workplace saving program (Thaler & Benartzi, 2004) or by their family and friends (Lindbeck, 1997). For instance, contributions of 3-6% are common default rates in many 401(k) saving plans, and most people stick with this default. Similarly, peer-effects are an important determinant of financial decisions and people often choose savings contribution amounts based on perceived descriptive or injunctive norms in their social network (Bursztyn et al., 2014). Even when there is no salient default, people do typically consider every budget category when allocating financial windfalls.

Rather, they may ask themselves: “How much money am I *able* to set aside for saving right now?” This mindset tends to lead to low savings contributions as people focus on trade-offs and competing desires. Indeed, in some cases, multiple savings goals can lead to less overall savings accumulation due perceived goal conflict, which tends to increase action deferral (Soman & Zhao, 2011).

Overview of studies

In this research, we examine how different budgeting procedures can shape financial allocation decisions. We theorize that people will rely on a 1/n heuristic under specific budget procedures and, therefore, can be nudged to increase their total saving contributions via a partitioning intervention.

First, we predict that when allocating a fixed amount of money across their household budget, people will rely on a 1/n heuristic. Second, we predict that people will be more likely to rely on 1/n heuristic when allocating money across all budget categories simultaneously. That is, consumers can divide money between spending and savings categories all at once, or they can consider each budget category in isolation, paying particular bills and expenses ahead of making savings decisions, or vice versa. We predict that people will be more likely to rely on 1/n heuristic when following a simultaneous budgeting procedure, compared to a sequential procedure. Third, we predict that these effects are driven by heuristic decision-making, rather than attention to multiple reasons for saving.

We test these predictions across six preregistered experiments. In Study 1, we examined how different budget configurations influenced intentions to save. Participants allocated a hypothetical raise across their household budget with either spending or saving categories partitioned into multiple sub-categories. In Study 2, we conducted a conceptual replication to test the effects of budgeting partitioning on a savings decision with real stakes. In Study 3, we explored the budgeting decision process by varying the number of savings partitions. In Study 4, we tested the conditions under which people rely on a 1/n heuristic by manipulating consumers' budgeting process to be simultaneous or sequential. In Study 5, we explored the mechanism by directly isolating the effects of financial goal-setting versus the effects of budget procedure. Lastly, in Study 6, we combined insights from the preceding studies to measure the relative effects of each feature of budget procedure on savings allocations.

STUDY 1: the effects of budget partitioning on savings allocations

In the first study, we aimed test whether people use a 1/n heuristic in windfall allocation decisions and to provide an initial demonstration of the effects of budget partitioning. Therefore, we randomly assigned participants to make the same financial choice - allocating a hypothetical raise - using three different budgeting procedures.

Method

Study 1 was a preregistered experiment conducted with a sample of 412 online participants (M_{age} = 34.3, *SD* = 10.0, 38% women; M_{income} = $47,891, *SD* = $27,512; 77% employed full-time). In order to be eligible to participate, individuals had to report an annual income greater than USD $10,000 and less than USD $500,000. After providing their personal annual income (in dollars, before taxes), participants received the following instructions: "Imagine you received a 20% raise on your annual income. This amounts to a raise of $[reported annual income * 0.2]. Being as realistic as possible, please indicate how you would allocate this raise into your household budget." Participants were randomly assigned between-subjects to one of three elicitation procedures to decide how much money (in dollars) they would allocate to each budget category. In the 'spending-partitioned' condition, participants were asked to allocate their raise among 7 spending categories (food dining; housing, repairs, purchases for the household; shopping, personal care; transportation, travel; health, fitness; entertainment products, events; and, all other spending) and 1 superordinate savings category. In the 'savings-partitioned' condition, participants allocated money across 4 savings categories (i.e., emergency savings, savings for upcoming expenses or purchases, retirement savings, and all other savings) and 1 superordinate spending category. In the control condition, participants allocated money across between 1 superordinate spending category and 1 superordinate savings category.

In order to hold information constant across conditions, all spending and savings sub-categories were listed in parenthesis alongside the superordinate category. For example, in spending-partitioned condition, the superordinate savings category was presented as follows: *"Savings (emergency savings, savings for upcoming expenses or purchases, retirement savings, and all other savings)."* Additionally, across all conditions, the order of saving and spending categories was counterbalanced. See Appendix C for more detail on experimental stimuli.

As our key dependent variable, we measured the percentage of participants' total raise amount that they allocated to savings. In the spending-partitioned and control conditions, this was defined as a percentage of raise allocated to the superordinate savings category. In the savings-partitioned condition, the percentage saved was defined as the sum allocated across emergency savings, savings for upcoming expenses or purchases, retirement savings, and all other savings.

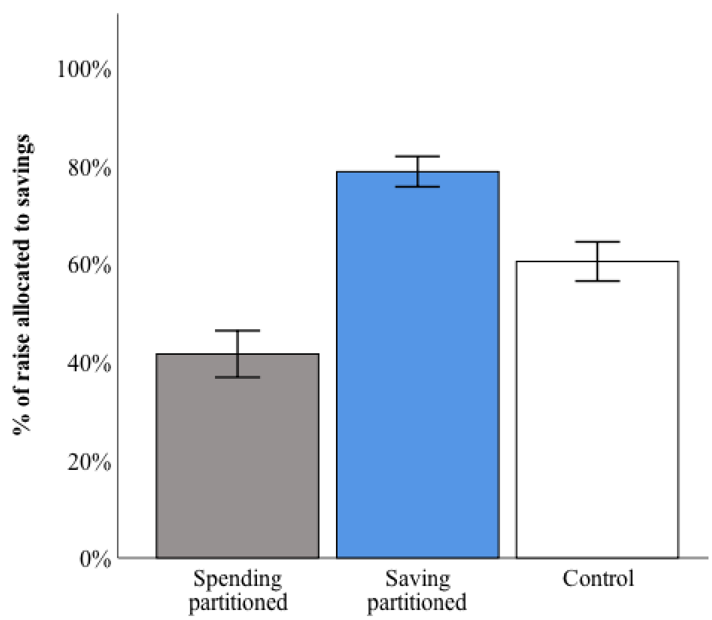
Results and discussion

Participants allocated a significantly larger percentage of their raise to savings in the savings-partitioned condition (M = 78.83%, SD = 18.02%) relative the spending-partitioned (M = 41.61%, SD = 28.04%) and control condition (M = 60.49%, SD = 24.01%), $F(2, 409) = 87.75$, p

< .001. In planned pairwise comparisons, we observe a significant increase in savings between the savings-partitioned condition relative to the control condition, $F(1, 409) = 40.9$, $p < .001$, and relative to the spending-partitioned condition, $F(1, 409) = 165.5$, $p < .001$. We also observe a significant decrease in savings in the spending-partitioned condition relative to the control, $F(1, 409) = 43.9$, $p < .001$. See Figure 7.

As indicated in the preregistered analysis plan, we conducted an ANCOVA to test whether the results hold controlling for annual income (log), age, gender, and education level. After including these controls, we observed no substantive change in the pattern of results (omnibus ANCOVA: $F(6, 403)=80.8$, $p<.001$). We also find no interaction effect between condition and income (log), suggesting the people across the income spectrum use a 1/n heuristic and exhibit partition-dependent preferences in their financial allocation decisions. See Table A28 for robustness checks.

Figure 7 (Chapter 3). Study 1: effects of condition on percentage of raise allocated to savings.



Notes. Displaying means and 95% confidence intervals for each condition.

This study provides evidence that intentions to save (versus spend) are highly malleable depending on the arbitrary partitioning of household budget categories. The results are consistent with a 1/n heuristic, such that participants' decision process involves starting with equal allocation across presented budget categories and then making insufficient adjustments based on their individual preferences.

STUDY 2: an incentive compatible test of budget partitioning

In Study 2, we conducted a conceptual replication of the previous results with an incentive compatible measure of savings versus spending. One limitation of the previous study was that participants allocated a hypothetical raise. It is possible that people will make different financial choices when there are real stakes. That is, people may be less susceptible to arbitrary features of the budgeting procedure. To examine this possibility, we collected a representative sample of US adults and offered them the option to receive a large sum of money immediately or set aside a portion of this money towards 'savings.' Participants made their decisions using two alternative elicitation procedures.

Method

Study 2 was a preregistered experiment conducted with 930 online participants in the United States (M_{age} = 43.82, *SD* = 15.85, 56% women; median annual income = $50,000; 60% employed full-time). We used a sampling strategy stratified on income to ensure that we recruited participants from across the income distribution, including over-sampling individuals who earned less than $40,000 so that this group comprised at least one third of the overall sample.

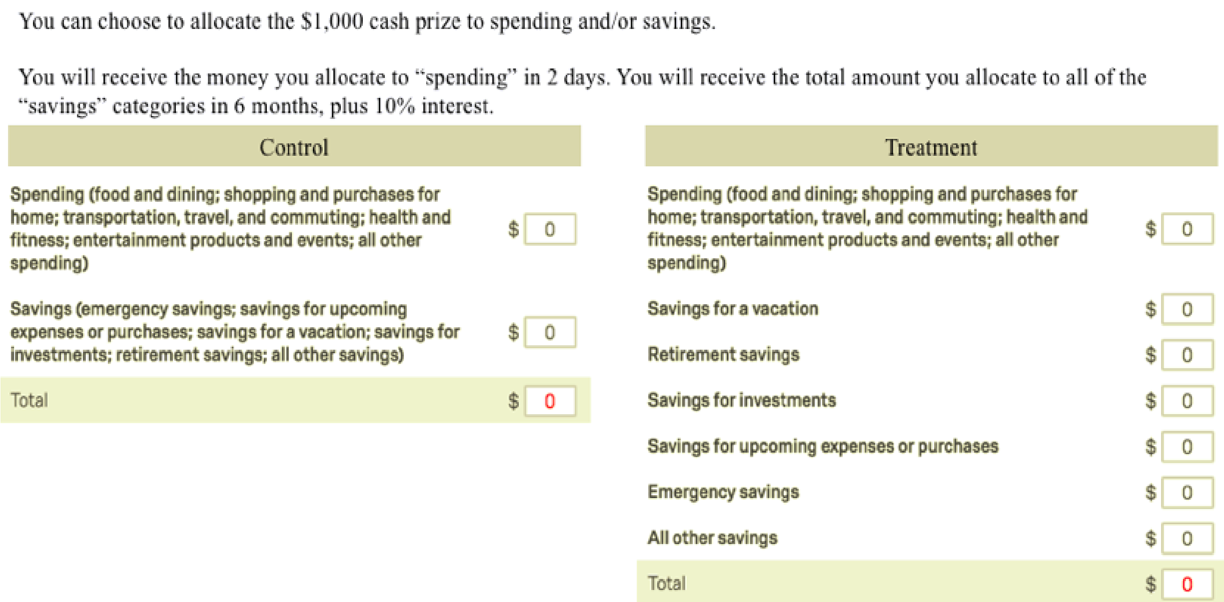
Participants enrolled in this study were offered the chance to receive a real $1,000 cash prize via a check in the mail. They could choose to receive the full sum immediately (to be sent in a check within 2 days) or set aside a portion of this money towards 'savings.' Participants were informed that any money allocated towards saving would be sent by mail in a separate check in 6 months, plus 10% interest. We explained that one person from this study would be randomly selected to receive this money for real: *"If you are selected, you will be asked to provide your mailing address so that we can send you two checks in the mail. The 1st check will be for the amount you allocate to spending, and it will be mailed within 2 days. The 2nd check will be for the amount you allocate to savings (plus 10% interest), and it will be mailed in 6 months."* We also provided three examples to ensure that participants understood the choice: *"If you allocate all of the money to spending, you will receive $1000 in 2 days and nothing in 6 months; If you allocate half of the money to spending and half to savings, you will receive $500 in 2 days and also $550 in 6 months; If you allocate all of the money to savings, you will receive nothing in 2 days and $1100 in 6 months)."* Therefore, this windfall allocation task was incentive compatible such that participants were aware that their decision had real economic consequences.[3] The interest rate of 10% was chosen based on the results of a pilot experiment which showed that this rate was sufficient to incentivize most participants to allocate a portion of the cash prize to savings.

In order to make this decision, participants were randomly assigned between-subjects to one of two elicitation procedures. In the control condition, participants allocated the $1,000 prize across one spending category (amount they wish to put on the immediate check) and one savings category (amount they wish to put on the check in 6 months, plus 10% interest). In the treatment

[3] Past research has demonstrated that paying one randomly selected participant in a decision task is an effective incentive compatible mechanism that produces statistically indistinguishable results from equivalent tasks in which all participants receive incentives with no element of chance (Cubitt et al., 1998; Azrieli et al., 2018; 2020). Therefore, we would expect to observe a similar allocation pattern if we were to provide all participants with a $1,000 windfall, rather than using a lottery to select one participant.

condition, participants allocated the prize across one spending category and 6 savings categories (summed amount to be put on the check in 6 months, plus 10$ interest). In order to hold information constant across conditions, we listed common spending and savings categories in the control condition, which were then partitioned in the treatment condition (see Figure 8).

Figure 8 (Chapter 3). Study 2: control and treatment budget procedures to allocate a $1,000 cash prize



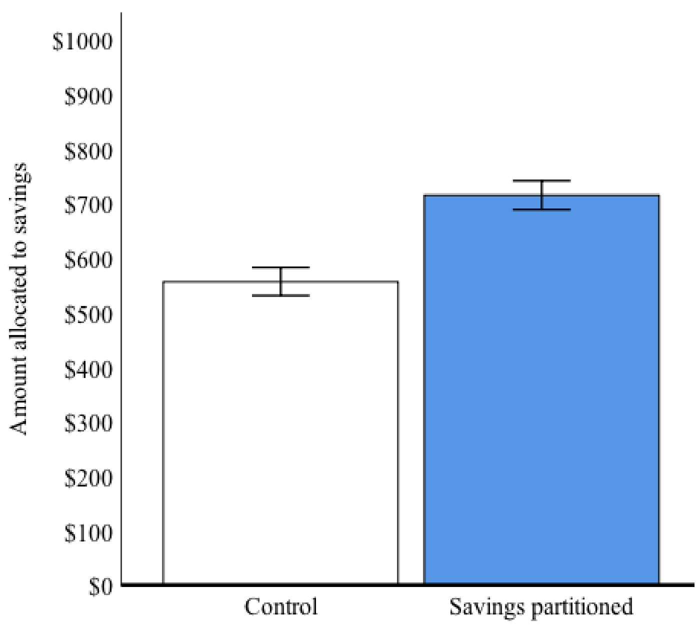
Notes. Example stimuli from Study 2. Participants' responses had to sum to $1,000 in each condition. Order of spending and savings categories was counterbalanced.

Results and discussion

The results of a preregistered ANCOVA showed that, controlling for annual income(log), participants in the treatment condition allocated an extra $179 to savings, compared to those in the control condition ($M_{\text{treatment}}$ = $716.76, *SD* = 285.99; M_{control} = $558.13, *SD* = 288.13; $F(1, 929) = 70.96$, $p<.001$). See Figure 9. As predicted in our preregistration, these results hold in a regression model controlling for age, education level, and financial literacy (see Table A31). Financial literacy was measured by using five questions developed for the Federal Reserve

Survey on Household Economics and Decision-Making (2019), for example: “suppose you had $100 in a savings account and the interest rate was 2% per year. After 5 years, how much money would you have in the account if you left the money to grow?” (More than $102, Exactly $102, Less than $102). We observed a significant main effect of financial literacy, such that participants who scored higher on financial literacy contributed more to savings, $b = 25.53$, $t(928) = 3.72$ $p < .001$, CI(b) = [12.122, 39.15]. We also observed a significant interaction effect between condition and financial literacy on amount saved, $b = -42.90$, $t(926) = -3.24$ $p = .001$, CI(b) = [-68.88, -16.93]. A bootstrapped moderation analysis shows that people at all levels of financial literacy exhibited partition-dependence, but the partitioning effects were stronger for those who were low financial literacy (see Appendix C for results of this moderation analysis). We observed no interaction effect between condition and annual income, $b = 17.94$, $t(926) = .91$ $p = .363$, CI(b) = [-20.72, 56.59].

Figure 9 (Chapter 3). Study 2: effects of condition on the portion of the $1,000 cash prize allocated to savings (check in 6 months, plus 10% interest) versus spending (check in 2 days).



Notes. Reporting marginal means, controlling for annual income (log) and 95% confidence intervals.

The results of this study directly contradict the axiom of *procedure invariance* in rational economic theories. The treatment and control conditions involved an identical decision between a smaller-sooner reward and the option to receive a larger amount in 6 months. All information was held constant, the only difference between conditions was the procedure through which we elicited participants' preference. Taken together, Studies 1 and 2 provide robust evidence that budget partitioning can have a profound influence on both intentions to save and savings decisions with real stakes.

STUDY 3: number of savings sub-categories

In Study 3, we explore the budgeting decision process by varying the number of savings partitions. If people are relying on a 1/n heuristic under this budgeting procedure, then partitioning savings into a greater number of saving sub-categories should increase total allocations to savings.

Method

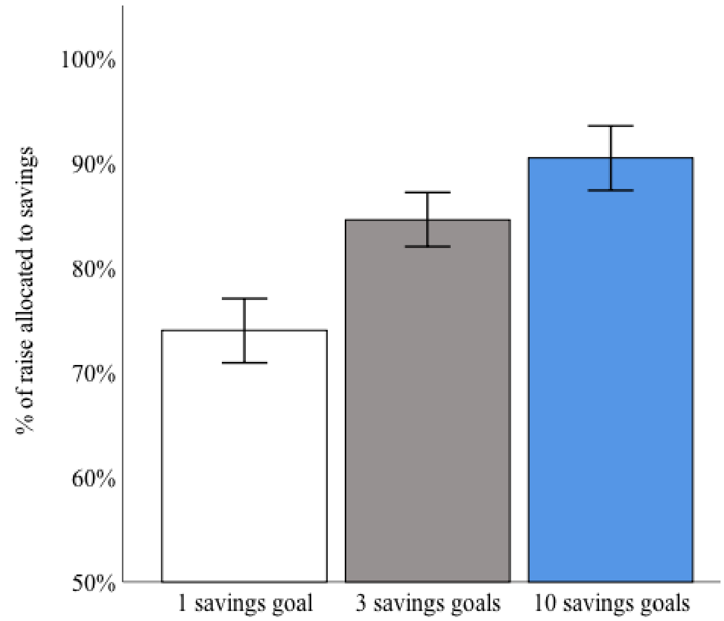
Study 3 was a preregistered experiment conducted with 316 online participants (M_{age} = 34.84, *SD* = 10.06; 37% women; median annual income = $47,000; 93% employed full-time). After entering their annual income for the previous year, participants were randomly assigned to select 1, 3, or 10 savings goals from a list of 12 common goals (e.g., safety net, retirement, education, new home, etc.). Participants were then asked to imagine they received a 20% raise (after tax) on their annual income. We calculated each participant's raise amount based on their reported income, and then asked them to decide how much of their raise to allocate to savings. In the '1-savings goal' condition, participants allocate their raise across 3 budget categories: their chosen savings goal, all other savings, and all spending. In the '3-savings goals' condition, participants allocate their raise across 5 budget categories: their 3 chosen savings goals, all other savings, and all spending. In the ‘10-savings goals’ condition, participants allocate their raise across 12 budget categories: their 10 chosen savings goals, all other savings, and all spending.

As our key dependent variable, we calculated the percentage of the hypothetical raise that participants allocated to all saving categories.

Results and discussion

The results of a preregistered ANOVA show that partitioning savings into a greater number of categories increases total allocations to saving, $F(2, 313) = 32.24$, $p < .001$. Participants allocated the largest percentage of their raise to savings in the 10-savings goals condition ($M = 90.42\%$, $SD = 16.43\%$), followed by the 4-savings goal condition ($M = 84.54\%$, $SD = 12.90\%$), and then the 1-savings goal condition ($M = 73.97\%$, $SD = 16.05\%$). All of the pairwise comparisons showed statistically significant differences (10-savings goals versus 4-savings goals: $F(1, 313) = 7.67$, $p = .006$; 4-savings goals versus 1-savings goal: $F(1, 313) = 24.46$, $p < .001$; 10-savings goals versus 1-savings goal: $F(1, 313) = 63.08$, $p < .001$). See Figure 10.

Figure 10 (Chapter 3). Study 3: effects of condition on percentage of raise allocated to savings



Notes. Displaying means and 95% confidence intervals by condition.

In Study 4, we sought to examine the conditions under which people rely on a 1/n heuristic. We predicted that people would only use this heuristic when it could effectively simplify a financial decision.

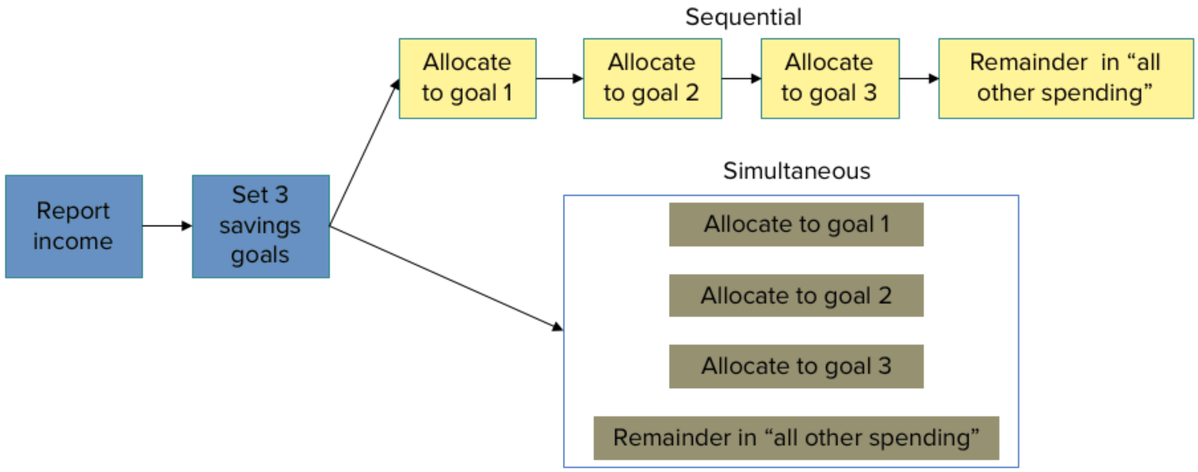
STUDY 4: simultaneous versus sequential budgeting

In Study 4, we examine the specific features of budgeting that might lead people to rely, to a greater or lesser extent, on a 1/n heuristic. A given judgement or decision-making heuristic is adaptive when it reduces the effort involved in a decision task (Shah & Oppenheimer, 2008). In the domain of budgeting, using a 1/n heuristic provides a benchmark that is often useful in reducing the cognitive effort involved in making complex financial decisions, but it can lead to systematic biases depending on the partitioning of budget categories. However, a 1/n heuristic is only effort-reducing when people follow a budgeting procedure in which they allocate a fixed sum of money across budget categories *simultaneously*. Often, people make budgeting choices *sequentially* - for instance, by first allocating money to their primary financial obligations (e.g. non-discretionary expenses like rent, heating, and car payments), then considering secondary expenses (e.g. food and groceries), and finally considering discretionary or hedonic purchases (e.g. clothing, jewelry, entertainment). Regardless of how people rank their financial priorities, a 1/n heuristic does not effectively reduce cognitive effort when used in a sequential budgeting procedure. Therefore, we predicted that people would be more likely to use a 1/n heuristic, and thus exhibit more partition-dependent preferences, in a simultaneous budgeting procedure.

Method

Study 4 was a preregistered experiment conducted with 312 online participants (M_{age} = 33.64, *SD* = 9.35, 38% women; $M_{monthly\ income}$ = \$5,231, *SD* = \$6,182; 85% employed full-time). In order to be eligible, participants had to indicate that they had some discretionary money to save. Participants reported their average monthly income and then selected three savings goals from the same list of 12 goals that was presented in Study 3. Then, we asked participants to construct a realistic household budget for the upcoming months. To do so, participants allocated their average monthly income across four categories: three savings categories labeled with their chosen savings goals and one spending category labeled as “all other uses, including bills, debt repayments, and all spending.” Half of participants allocated their monthly income sequentially, with each of the four categories presented on a separate page. If participants were not satisfied with the final budget after completing the sequential allocation, they were able to repeat the process and make changes. The other half of participants allocated their monthly income simultaneous, with all budget categories presented on a single page. In both conditions, the budgeting procedure was explained to participants ahead of time. See Figure 11 for a study diagram.

Figure 11 (Chapter 3). Study 4: diagram of sequential and simultaneous budgeting procedures



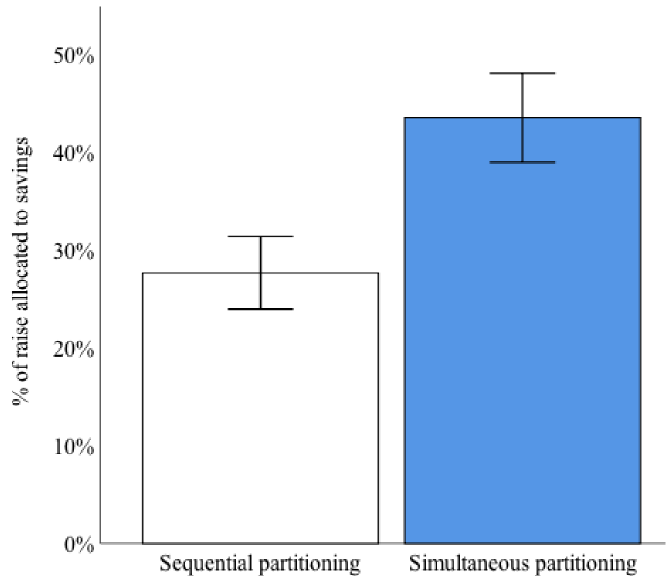
Notes.The 3 specific savings goals were selected by participants from a list of 12 common goals. Order of spending and savings categories was counterbalanced.

Results and discussion

In a preregistered ANOVA, we found that participants in the 'simultaneous' condition allocated an additional 16% of their monthly income to savings, relative to participants in the 'sequential' condition, ($M_{simultaneous}$ = 43.47%, SD = 29.55%; $M_{sequential}$ = 27.63%, SD = 22.74%; $F(1,311) = 27.68$, $p < .001$). See Figure 12.

As in the previous studies, we conducted preregistered robustness checks, controlling for income(log), age, and education. We find that the results are substantively unchanged after including these controls (Table A34). We also observe no interaction effect between condition and income with respect to effects on savings allocation, $b = 9.946$, $t(3,311) = 1.046$, $p = .296$, CI(b) = [-8.757, 28.650].

Figure 12 (Chapter 3). Study 4: effects of condition on percentage of average monthly income allocated to savings



Notes. Displaying means and 95% confidence intervals by condition.

These results suggest that presenting budget categories simultaneously may increase the likelihood that people rely on a 1/n heuristic. Here, the number of budget categories was held constant – all participants allocated their monthly income across 4 categories. The only difference between conditions was whether participants allocated money sequentially or simultaneously. Sequential allocation is not amenable to 1/n of heuristic since this would require participants to either calculate a running tally of total allocations from one page to the next, or plan all of their decisions in advance. It is more likely that, for each savings goal, participants asked themselves: “how much money am I able to allocate to this savings goal right now? In contrast, the simultaneous condition enabled participants to effectively reduce cognitive effort using a 1/n heuristic.

STUDY 5: budget unpacking versus budget partitioning

In all of the previous studies, we examined the effects of partitioning savings into multiple sub-categories. We have proposed that the observed changes in financial allocations are driven by the extent to which people rely on a 1/n heuristic. However, a plausible alternative explanation is that partitioning savings into sub-categories draws attention to multiple reasons for saving. In this attention-based account, the effects are driven by budget 'unpacking' rather than partitioning. Unpacking refers to the process of explicitly listing the components of a given category. In the literature on subjective probability judgment, unpacking a given event into disjoint components tends to increase the perceived likelihood of the event. For instance, unpacking the likelihood of a car crash into "the likelihood of a car crash due to road construction, or due to driver fatigue, or due to break failure" increases the overall perceived likelihood of car crash by drawing attention to the subcomponents (Rottenstreich & Tversky, 1997).[4] To the best of our knowledge, no studies have examined the distinct effects of unpacking versus partitioning in the domain of resource allocation.

Method

We isolated the effects of unpacking versus partitioning in a preregistered experiment with 239 online participants (M_{age} =37.27, SD = 11.87; 37% women; median annual income = $36,000; 79% employed full-time). After providing their monthly income, we asked participants to allocate a hypothetical 20% holiday bonus into their household budget. We calculated each

[4] Unpacking in subjective probability assessments increases the perceived likelihood of a given event because, typically, people do not mentally unpack a hypothesis into its subcomponents and sum across the individual likelihoods of each case. Rather, people tend to form a general impression that is shaped only by cases that are cognitively 'available' in the moment. Therefore, unpacking increases perceived likelihood by reminding people of possibilities they might not have otherwise considered.

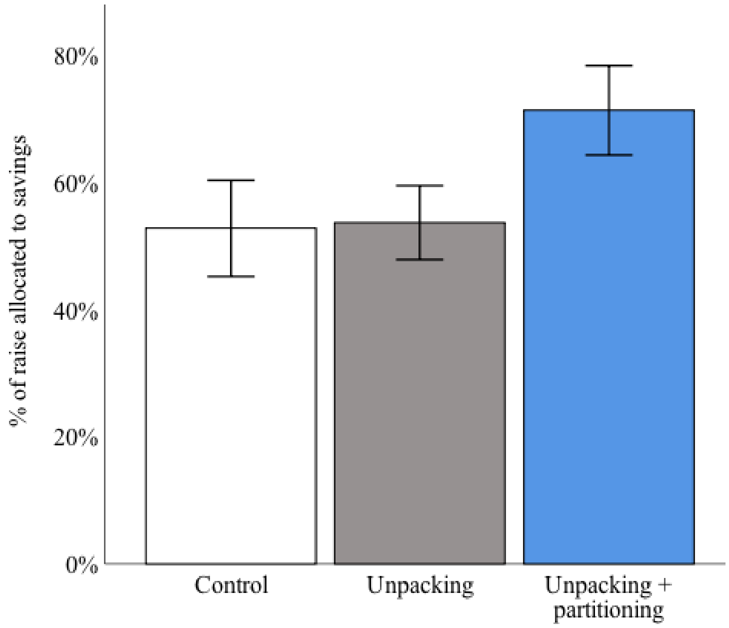
participants' bonus amount (in dollars) and presented them with one of three procedures to decide on how to allocate this money.

In the control condition, participants indicated how much of this bonus they would allocate to savings (using a single savings account) versus spending (using a single checking account). In the 'unpacking' condition, participants first selected three savings goals from the same list of 12 savings goals used in previous studies. Then, participants chose how much of their bonus to allocate into one overall savings account (with chosen savings goals listed in parentheses) and one spending account. In the 'unpacking+partitioning' condition, participants selected three savings goals, then chose how much of their bonus to allocate across one spending account and four separate savings accounts: three accounts earmarked with their chosen goals and one account for "all other savings." As our key dependent variable, we calculated the percentage of participants' bonus amount that was allocated towards savings.

Results and discussion

We found that setting savings goals, and thus unpacking multiple reasons for saving, had no effect on subsequent allocations to savings, whereas partitioning had a large effect. Participants in the unpacking condition allocated 53.6% of their bonus to savings, on average (SD = 28.1%), and participants in the control condition allocated 52.7%, on average (SD = 31.2%). This difference between was not statistically significant ($\text{difference}_{\text{unpacking - control}}$ = -0.89, SE = 4.79, p = .85, 95%CI[-10.32, 8.55]). The unpacking+partitioning condition led participants to allocate 71.3% of their bonus to savings, on average (SD = 31.2%). The difference between unpacking+partitioning and unpacking-only was statistically significant ($\text{difference}_{\text{unpacking\&partitioning - unpacking}}$ = 17.68, SE = 4.61, p < .001, 95%CI[8.59, 26.76]). The difference between unpacking+partitioning and the control condition was also significant (difference$_{unpacking\&partitioning\ -\ control}$ = 17.68, SE = 4.61, p < .001, 95%CI[8.59, 26.76]). See Figure 13.

Figure 13 (Chapter 3). Study 4: effects of condition on percentage of average monthly income allocated to savings



Notes. Displaying means and 95% confidence intervals by condition.

These results suggest that partitioning effects are not a consequence of merely unpacking multiple reasons for saving. In both the unpacking and unpacking+partitioning conditions, participants were asked to take a moment to think about the reasons for saving and select three savings goals from a list of 12. In doing so, unpacking was held constant across these conditions, yet we only observe an increase in savings when participants explicitly allocate money into partitioned savings sub-categories. These results are consistent with participants using a 1/n heuristic, suggesting that budget partitioning influences savings by changing the process by which people make allocation decisions, not by re-directing attention.

STUDY 6: combining each feature of budget partitioning

In Study 6, we combined the insights from previous studies to develop and test a savings nudge based on budget partitioning. We conducted this study in November when many U.S. workers were anticipating a holiday or year-end bonus. We asked participants to allocate their upcoming bonus into their household budget using one of five budgeting procedures.

Based on the results of the previous studies, we preregistered three predictions. First, partitioning savings accounts into multiple sub-accounts will increase allocation to savings relative to a single savings account, even when we keep the number of savings goals constant. Second, people will save more when allocating money across multiple savings accounts simultaneously, as opposed to sequentially. Third, people will allocate more money across multiple savings accounts when these accounts are presented alongside an overall spending account. That is, when people make an explicit allocation to saving versus spending accounts.

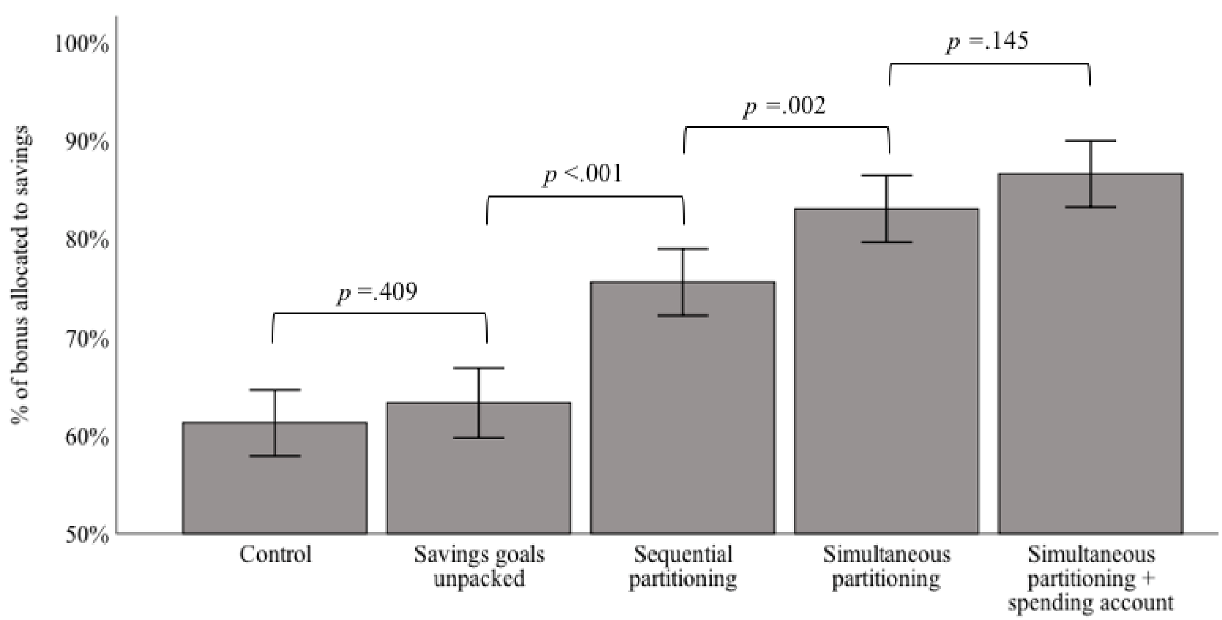
Method

Study 6 was a preregistered experiment conducted with 1,022 online participants (M_{age} = 36.19, *SD* = 12.33, 47% women; median annual = $45,000; 71% employed full-time). In this study participants were asked to allocate a hypothetical 20% year-end bonus into their household budget. Participants indicated their annual income, then we told them the dollar amount of their holiday bonus. We assigned participants to one of five procedures to allocate this windfall into their household budget. In the control condition (1), participants simply decided how much of this bonus to allocate to a single savings category. In the 'savings goals' condition (2), participants chose 4 savings goals from the same list of 12 common saving goals, and then allocated to a single savings category. In conditions (3) and (4) participants chose 4 savings goals and then allocated money across each goal (plus an 'all other savings' category) sequentially or simultaneously. In condition (5), we added an all-purpose spending category so that participants allocated the entire bonus across 4 savings categories and 1 spending category. See Appendix C for diagrams of each budgeting procedure.

Results and discussion

In a preregistered ANOVA, we found a significant omnibus effect of condition on savings allocation, $F(4, 1017)=42.96$, $p < .001$. In planned pairwise comparisons, we found that setting savings goals had no impact on savings allocations whereas partitioning had a large effect, especially simultaneous partitioning (See Figure 14 and Table 10). In preregistered regressions we found that these effects hold including a set of financial controls (income, subjective financial wellbeing, and financial literacy score) as well as a set of demographic controls (age, gender, and education). See Table A37 for regression results.

Figure 14 (Chapter 3). Study 6: effects of condition on percentage of bonus allocated to savings



Note. Displaying means and 95% confidence intervals. P-values correspond to preregistered planned contrasts in a general linear model with LSD corrections for multiple comparisons.

Table 10 (Chapter 3). Study 6: all pairwise comparisons

Pairwise comparison		Mean difference	*p*	95% CI
Savings goals unpacked	Control	2.06	.409	[-2.83, 6.95]
Sequential partitioning	Control	14.34	<.001	[9.56, 19.12]
Simultaneous partitioning	Control	21.78	< .001	[17.00, 26.56]
Simultaneous+spending	Control	25.34	< .001	[20.57, 30.11]
Sequential partitioning	Savings goals unpacked	12.28	< .001	[7.38, 17.19]
Simultaneous partitioning	Savings goals unpacked	19.73	< .001	[14.82, 24.63]
Simultaneous+spending	Savings goals unpacked	23.28	< .001	[18.38, 28.18]
Simultaneous partitioning	Sequential partitioning	7.44	.002	[2.64, 12.24]
Simultaneous+spending	Sequential partitioning	10.99	< .001	[6.21, 15.78]
Simultaneous+spending	Simultaneous partitioning	3.56	.145	[-1.23, 8.34]

Note. Reporting mean differences (measured in percentage points) for each pairwise comparison. P-values and 95% confidence intervals are calculated using an LSD correction for multiple comparisons.

These results support two of our three preregistered predictions. Aligning with the results of Study 5, we found that partitioning savings into multiple sub-categories led to a significant increase in total saving, whereas setting savings goals without partitioning had no effect. Aligning with the results of Study 4, simultaneous partitioning led to more saving than sequential partitioning. However, we did not find evidence in support of our third prediction – adding a spending account to the simultaneous budgeting procedure had no impact on savings allocations. By adding a spending account, participants were explicitly allocating their entire bonus across an exhaustive list of budget categories. We expected that this would further increase the likelihood that participants would rely on a 1/n heuristic. It is possible that we did not observe this increase in savings allocation due to a ceiling effect.

GENERAL DISCUSSION

Many countries are facing a savings crisis (Benartzi & Thaler, 2013). For instance, 37% of U.S. households cannot cover an unexpected $400 expense without borrowing and half of

American workers are not saving enough to maintain their lifestyle in retirement (Federal Reserve, 2018; Munnel et al., 2018). Financial windfalls are a critical opportunity for individuals to contribute to savings. Yet, people tend to be especially impatient when allocating windfalls (Bodkin, 1959; Arkes et al., 1994).

This research examines how people make windfall allocations decisions and tests whether different budgeting procedures can increase allocations to saving. In contrast to rational economic theory, we found that budget procedures had a substantial influence on how people allocated a financial windfall. Under specific budgeting procedures, people used a 1/n heuristic and, therefore, when savings was partitioned into a greater number of categories, people allocated significantly more money to savings.

Past research has documented that people use a 1/n heuristic in many resource allocation decisions including corporate capital allocation (Bardolet, Fox, & Lavallo, 2009), individual investment decisions (Hedesström et al., 2009; Baltussen, & Post, 2011; Avrahami et al., 2014), and consumer purchasing behavior (Fox et al., 2005). The current research is the first to examine whether people use a 1/n heuristic in the domain of household budgeting. More importantly, this study is the first to investigate the specific choice architecture that leads people to use a 1/n heuristic in resource allocation decisions.

The results show that people only use a 1/n heuristic when allocating a fixed sum across partitioned budget categories. Furthermore, people were more likely to use a 1/n heuristic when allocating money simultaneously, versus sequentially. When using a simultaneous budgeting procedure, we observe large effects of partitioning on total allocation to saving (effect sizes range from d = .55 to d = 1.58). We would not expect to find such large effects in a field context. Indeed, we observed the smallest effect size in Study 2, when participants made an incentive compatible choice about how to allocate a $1000 cash prize. However, in this study, we still found that the partitioning manipulation led participants to allocate an extra $159 to saving, representing a medium to large effect size (d = .55). This highly consequential increase in savings behavior was produced by merely changing the *elicitation* of savings decisions. All participants knew they would receive two checks (if they were selected in the lottery): the first check would be sent in 2 days, and the other would be sent in 6 months, plus 10% interest. Therefore, the only difference between conditions was the elicitation procedure through which participants decided how much money they wanted to allocate to each check. From a rational economics perspective, these were identical allocation decisions.

Financial institutions, fintech software companies, and individual consumers tend to think of budgeting as a strategy to reduce impulsive spending. Our findings show that budgeting can also have a profound influence on the initial allocation of a financial windfall. Under specific choice architecture, partitioning savings categories can be an effective, non-coercive nudge to increase saving contributions.

Conclusion

Overall, I sought to answer three fundamental questions about windfalls of time and money. First, how do windfalls of time versus money affect psychological wellbeing? I found evidence that the benefits of time windfalls have been overlooked, relative to windfalls of money. In a field experiment, I found that randomly assigning participants to receive large windfalls of time (15-21 extra hours over 3 weeks) or money (33% boost income for 3 weeks) caused similar increases in psychological wellbeing. Furthermore, micro-entrepreneurs who received time transfers out-earned those who received cash transfers by the end of the study period (even after accounting for earnings from the cash transfers themselves), indicating that windfalls of time may be spent more patiently and productively than windfalls of money.

Second, given rising income volatility, how does the experience of episodic financial windfalls shape economic preferences? Managing a volatile income requires patience and planning, however, the psychological experience of income volatility may make it especially difficult to resist temptations and stick to long-term goals. Indeed, I found that experiencing income volatility is associated with more impatient economic preferences. Across the wealth distribution, people who experienced more volatility were less willing to wait for larger delayed rewards. Since people living in poverty tend to experience the most income volatility, this psychological link between volatility and impatience may help to explain the pattern of impatient behaviors that have been attributed to the experience of poverty.

Third, when people experience a financial windfall, how do they allocate it across their financial needs, desires, and aspirations? Most people refer to their budget when making these decisions, however, people use a myriad of budgeting techniques and there is currently little understanding of how different budgeting techniques may shape financial allocation decisions.

Whether people create their own budget or rely budget procedures designed by savings clubs, banks, financial advisors, or software applications, the specific configuration of their budget may inadvertently shift their financial allocations between saving and spending. I identified the specific budget procedures under which people rely on naïve diversification heuristics in their financial allocation decisions, making them susceptible to biases based on the partitioning of savings and spending categories. This work provides insights on how consumers use simplifying strategies in complex budgeting decisions, and how to design more effective budgeting procedures to nudge savings contributions.

Despite rising global wealth, most people report feeling constrained in terms of both time and money. Furthermore, resources of time and money are becoming increasingly fragmented and volatile. Working schedules have become more irregular and, on an average work day, people now experience more task-switching and interruptions (McMenamin, 2007; Wajcman & Rose, 2011). Incomes have become similarly fragmented as more people face volatile income streams due contract work, shift work, and more incentive-based compensation (OECD, 2019; Lemieux, MacLeod, & Parent, 2009; Lazear & Shaw, 2008; Lazear, 2018). Consequently, people are forced to manage intermittent windfalls and shortfalls of time and money. In our personal and professional lives, we often focus too narrowly on acute shortfalls of money – these deficits quickly consume our attention and make it difficult to plan for the future. Throughout this dissertation, I have argued that researchers, policymakers, and companies need to better understand the importance of windfall moments for psychological and economic wellbeing. I hope this work can provide insights to help individuals and organizations use their windfalls wisely.

Appendix A: Supplemental Information for Chapter 1

Appendix A includes 4 sections:

Section 1: Sample size deviation from preregistered report
Section 2: Baseline sample characteristics
Section 3: Supplementary methods and results for manipulation check
Section 4: Exploratory results

Section 1: sample size deviation from preregistered report

We made one notable change from our pre-registered plan: our data collection stopping point. We specified an endline sample size of *N*=1,200 for the pre-registered Bayesian analyses testing differences in subjective well-being, perceived stress, and relationship conflict across the three pre-registered conditions (Time-Saving Vouchers vs. Unconditional Cash Transfers vs. Control Condition). Additionally, we planned to collect a further 800 participants if we did not observe Bayes Factors < 0.10 or > 10.00 on each of our primary comparisons.

However, after reaching an endline sample size of *N*=1,070 in March 2020 across our three pre-registered conditions of interest, we were forced to terminate data collection due to the COVID-19 pandemic (we collected *N*=1,435 in total when including data from 365 participants assigned to an additional exploratory time-saving voucher condition). We conducted our pre-registered Bayesian ANCOVAs with this sample of *N*=1,070.

The three omnibus tests of condition on subjective well-being (SWB), perceived stress (PSS), and relationship conflict (conflict) all reached our pre-registered threshold of BF < 0.10. However, we did not reach this threshold on 7 of 9 pre-registered pairwise comparisons between individual conditions. In these comparisons, we observed the following Bayes Factors: BF=0.09 (control vs UCT, effects on SWB); BF=0.19 (control vs time-saving, effects on SWB); BF=0.11 (UCT vs time-saving, effects on SWB); BF=0.08 (control vs UCT, effects on PSS); BF=0.11 (control vs time-saving, effects on PSS); BF=0.11 (UCT vs time-saving, effects on PSS); BF=0.11 (control vs UCT, effects on conflict); BF=0.32 (control vs time-saving, effects on conflict); BF=0.12 (UCT vs time-saving, effects on conflict). Well-established Bayesian reference guides indicate that these results provide "substantial" to "strong" evidence in support of the null hypothesis (Kass & Raferty, 1995). Yet, most of our pairwise comparisons did not reach the pre-registered threshold of BF < 0.10 or >10.00 (see Table 2 of the main manuscript).

We pre-registered that we would collect an additional 800 participants if we did not reach the threshold for Bayes Factors of BF < 0.10 or >10.00. However, due to the COVID-19 pandemic, it was not possible or ethical to continue data collection. Therefore, we terminated data collection in March 2020 in accordance with public health guidelines in Kenya.

Section 2: baseline sample characteristics

Table A1. Baseline differences between Attritors (did not completed the full study, (n = 83) and Remainers (n = 1070).

Variable (measured at baseline)	Attritors Mean (SD)	Remainers Mean (SD)	Difference statistics
Age	34.41 (7.70)	36.09 (9.19)	t(1148)=-1.63, p=.10
Education (% completed primary school)	77%	79%	X^2(N=1, 1153)=0.10, p=.42
% married or marriage-like relationship	60%	60%	X^2(N=1, 1153)=0.02, p=1.00
Household size (total # of people)	4.48 (1.60)	4.68 (1.55)	t(1151)=-1.12, p=.27
Number of children in the household	3.01 (1.34)	3.04 (1.43)	t(1151)=-0.179, p=.86
% responsible for household financial decisions	51%	50%	X^2(N=1, 1153)=0.01, p=1.00
Hours of paid labor in past 7 days	44.34 (22.59)	44.23 (20.79)	t(1151)=-0.04, p=.97
Hours of unpaid labor in past 7 days	35.42 (21.89)	40.79 (24.66)	t(1151)=-1.92, p=.06
Personal income in past 6 months	KSH 38,395 (30,003)	KSH 39,920 (47,268)	t(1142)=-0.11 p=.92
Household spending in the past 7 days	KSH 3,794 (5,834)	KSH 3,397 (3,767)	t(1151)=0.88, p=.38
Baseline depression (CES-D; 1- 4 scale)	2.21 (0.50)	2.24 (0.48)	t(1151)=-0.49, p=.62
Baseline SWB (1 – 5 scale)	2.77 (0.74)	2.71 (0.68)	t(1151)=0.85, p=.39
Baseline PSS (1 – 5 scale)	3.24 (0.55)	3.24 (0.54)	t(1151)=-0.08, p=.93
Baseline relationship conflict[a] (0 – 4 scale)	0.95 (1.01)	0.94 (0.96)	t(1151)=0.09, p=.92

Notes. Reporting means and standard deviations for respondent characteristics at baseline. Time spent on paid and unpaid labor is measured as a percentage of total time reported for the past 7 days. Difference statistic for baseline income uses log (baseline monthly income). To adjust for multiple comparisons, we have used Bonferroni correction. Using this correction, the significance level for these comparisons is $p \leq 0.004$. Therefore, we can determine that Attritors did not significantly differ from Remainers on any of these baseline characteristics.

Table A2. Sample characteristics at baseline

Variable	Sample characteristics Mean (SD), Range
Age	*M*=35.89 (9.05), *RG*: 19 - 69
Education (% completed primary school)	79%
% married or marriage-like relationship	60% (14% single, 16% divorced, 10% widowed)
Household size (total # of people)	*M*=4.63 (1.53), *RG*:1 - 12
Number of children in the household	*M*=3.03 (1.41), *RG*:1-11
% responsible for financial decisions	50% (41% joint with spouse) (5% spouse only)
Hours of paid labor in past 7 days	*M*=44.64 (20.91), *RG*: 0 - 115
Hours of unpaid labor in past 7 days	*M*=40.24 (24.43), *RG*: 1 - 150
Personal income in past 6 months	*M*=43,253 KSH (120,769), *RG*: 0 - 3.3M
Household spending in the past 7 days	*M*=3,444 (3,870), *RG*: 0 - 52,100
Baseline depression (CES-D; 1- 4 scale)	*M*=2.24 (0.48), *RG*: 1.05 - 3.75
Baseline SWB (0 – 5 scale)	*M*=2.72 (0.68), *RG*: 1.13 – 4.87
Baseline PSS (1 – 5 scale)	*M*=3.24 (0.54), *RG*: 1.60 - 4.70
Baseline relationship conflict (0 – 4 scale)	*M*=0.93 (0.95), *RG*: 0 - 4.00

Note. 1,000 Kenyan Shillings (KSH)=9.90 USD (conversion rate as of January 1, 2020). Thus, women in this sample reported making approx. $428.20 USD in the past six months. For women who reported that they did not have a romantic partner, they responded to the items measuring relationship conflict with respect to their closest personal relationship.

Table A3. Participant occupations

Occupation category	Total number (% of baseline sample)
All sales jobs	629 (41.6%)
Trades	61 (4.0%)
Personal services	644 (42.6%)
Casual laborer	104 (6.9%)
Childcare, education, and healthcare services	75 (5.0%)

Note. At baseline, participants provided an open-ended response to the question: "What is your primary job?" This question focused on their occupation. How this person was paid (i.e. salary-based, task-based, or hour/daily) and whether or not they owned a share in their business (micro-enterprise ownership) were coded separately. Responses were coded into 5 categories: all sales jobs (includes selling produce, meals, clothing, and other consumer goods; working at a kiosk); trades (includes cooks, tailors, construction workers, carpenters, electricians, artisans, and all other skilled labor); personal services (includes hairdressers, restaurant staff, drivers, house cleaners, and washing clothes); casual laborer (includes temporary workers, wage laborer and kibarua); childcare, education, and healthcare services (includes daycare workers, teachers, school administrators, and community health workers). If participants mentioned more than one job, their job code was determined based on the first job they described. Job code was marked as missing if a participant's response could not be understood (n = 37).

Table A4. Baseline characteristics, by pre-registered condition assignment

	Survey-compensation-only (*n*=389)	UCT (*n*=386)	Time-Saving (*n*=378)	Model Statistics
Age	36.97 (9.64)	35.47 (8.85)	35.57 (8.70)	$F(2, 1147)=3.55$, $p=.03$
Education (% completed primary school)	77%	80%	80%	$X^2(N=2, 1153)=0.94$, $p=.62$
% married or marriage-like relationship	62%	54%	63%	$X^2(N=2, 1153)=7.90$, $p=.02$
Household size (total # of people)	4.71 (1.60)	4.67 (1.57)	4.61 (1.56)	$F(2, 1150)=0.41$, $p=.67$
Number of children in the household	3.17 (1.53)	2.97 (1.39)	2.97 (1.34)	$F(2, 1150)=2.48$, $p=.09$
% responsible for financial decisions	50%	54%	46%	$X^2(N=2, 1153)=4.73$, $p=.09$
Hrs of paid labor in past 7 days	44.62 (21.35)	44.34 (20.31)	44.31 (21.14)	$F(2, 1150)=0.03$, $p=.98$
Hrs of unpaid labor in past 7 days	40.54 (25.46)	39.81 (23.89)	40.86 (24.14)	$F(2, 1150)=0.19$, $p=.83$
Personal income in past 6 months	KSH 41,115 (47,674)	KSH 36,766 (29,569)	KSH 41,597 (57,366)	$F(2, 1141)=1.27$, $p=.28$
Household spending in the past 7 days	KSH 3,380 (3,800)	KSH 3,513 (4,099)	KSH 3381 (3,953)	$F(2, 1150)=0.15$, $p=.87$
Baseline depression (CES-D; 1- 4 scale)	2.24 (0.48)	2.25 (0.50)	2.23 (0.47)	$F(2, 1150)=0.14$, $p=.87$
Baseline SWB (0 – 5 scale)	2.71 (0.67)	2.72 (0.72)	2.71 (0.66)	$F(2, 1150)=0.04$, $p=.96$
Baseline PSS (1 – 5 scale)	3.25 (0.54)	3.23 (0.54)	3.24 (0.55)	$F(2, 1150)=0.09$, $p=.91$
Baseline relationship conflict[a] (0 – 4 scale)	0.92 (0.96)	0.93 (0.99)	0.96 (0.95)	$F(2, 1150)=0.17$, $p=.84$

Note. Reporting means, standard deviations, and statistics testing for differences by condition. Time spent on paid and unpaid labor is measured as a percentage of total time reported for the past 7 days. Difference statistic for baseline income uses log(baseline monthly income). To adjust for multiple comparisons, we used a Bonferroni correction. With this correction, the significance level for each comparison is $p \leq 0.004$. Thus, we find no significant differences by condition at baseline, which supports our assumption that random assignment to condition was successful.

Section 3: supplementary methods and results for manipulation check

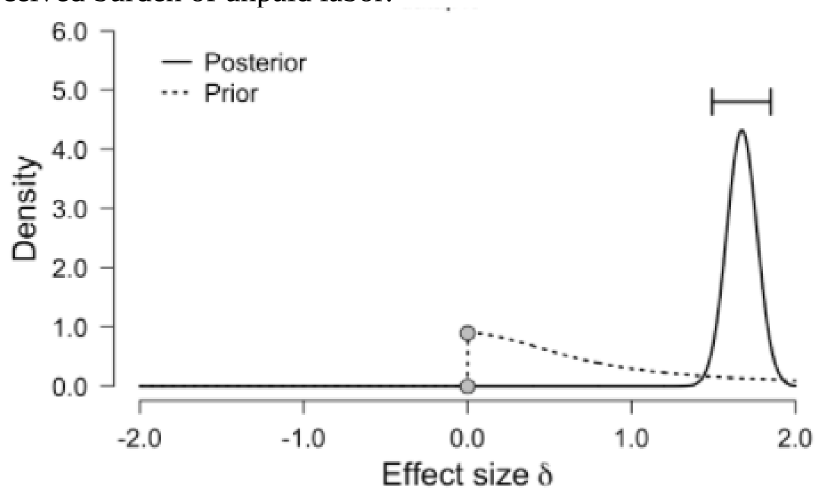
As a manipulation check, we tested for differences between the UCT and time-saving conditions with respect to 'change in perceived burden of unpaid labor' during the intervention period. In three consecutive weekly phone surveys during the treatment (weeks 3-5), participants in the UCT and time-saving conditions were asked: "Over the past 7 days, to what extent did receiving [cash / prepared meals / laundry services] affect your burden of unpaid labor (-3=decreased my burden of unpaid labor a lot, 0=did not change my burden of unpaid labor, 3=increased my burden of unpaid labor a lot)? Participants in the survey-compensation-only condition were not asked this question since they received no windfalls.

A critical assumption of this research is that participants in the time-saving condition reported experiencing a lower burden of unpaid labor as compared to participants in the UCT condition. Therefore, we conducted a Bayesian independent samples *t-test* (one-sided). This assumption was confirmed. We find a Bayes factor of $BF_{10} > 1000$ (error % < 0.001), which is very strong evidence in support the hypothesis that time-saving services reduce participants burden of unpaid labor (Tables S2-S3, Figure S1).

Table A5. Change in perceived burden of unpaid labor, by condition.

					95% Credible Interval	
Group	N	Mean	SD	SE	Lower	Upper
UCT	353	-0.673	1.012	0.054	-0.779	-0.567
Time-Saving	288	-2.274	0.871	0.051	-2.375	-2.173

Figure A1. Prior/posterior distribution density plot: Bayesian independent samples *t-test* results for change in perceived burden of unpaid labor.



Note. Prior and posterior distributions for effect size, δ. Prior uses a one-sided Cauchy distribution with $r = 1/\sqrt{2}$.

Section 4: exploratory analyses

We conducted exploratory analyses to examine the effects of condition assignment over the course of the experiment, including effects during the intervention (weeks 3-5). We explored the mechanisms underlying the observed differences in in subjective well-being, perceived stress, and relationship conflict at three time points: baseline, during the intervention, and endline. Lastly, we examined individual differences in treatment effects based on baseline characteristics including level of education, occupation, microenterprise ownership, household size, income, subjective well-being, perceived stress, relationship conflict, and risk of depression.

For all exploratory analyses, we use data from the three pre-registered conditions (*n*=1,070) as well as from an additional exploratory time-saving condition (*n*=365).

> **Pre-registered time-saving vouchers condition (*n*=349)**. Participants received either prepared meals or laundry services once per week for three consecutive weeks. To possibly amplify the benefits, participants were asked to make a plan for how they would spend the additional time they had as a result of receiving these time-saving vouchers. Prior to teach treatment week, participants provided an open-ended response to the following question: "Next week, you will receive a [prepared meal service / laundry service] designed to save you time. How do you plan to spend this additional free time?" We then asked participants follow-up questions to increase the specificity of their plans: "Where will you complete this activity / these activities?"; "Who will you complete activity / these activities with?"

> **Additional exploratory time-saving vouchers condition (*n*=365).** This condition was identical to the condition described above, except that no planning questions were asked.

Table A6. Repeated Measures ANOVA: effects of condition assignment (including an exploratory time-saving voucher condition) and time point on subjective wellbeing.

Source	df	SS	MS	F-value	*p*	η^2 [95% CI]
Between-subjects						
Condition	2	0.060	0.030	0.125	.883	0.000 [0.000, 0.002]
Error	974	253.332	0.242			
Within-subjects						
Time	2	307.970	153.985	379.994	<.001	0.281 [0.249, 0.311]
Time*Condition	4	2.186	0.547	1.349	.249	0.003 [0.000, 0.007]
Error (Time)	1948	789.390	0.405			

Note. Reporting between- and within-subjects effects of condition and time point (baseline, during the intervention, and endline) on subjective wellbeing.

Table A7. Repeated Measures ANOVA: effects of condition and time point on perceived stress

Source	df	SS	MS	F-value	*p*	η^2 [95% CI]
Between-subjects						
Condition	2	0.039	0.020	0.112	.894	0.000 [0.000, 0.003]
Error	953	166.159	0.174			
Within-subjects						
Time	2	151.331	75.665	265.608	<.001	0.218 [0.187, 0.248]
Time*Condition	4	1.215	0.304	1.066	.372	0.002 [0.000, 0.006]
Error (Time)	1906	542.972	0.285			

Note. Reporting between- and within-subjects effects of condition and time point (baseline, during the intervention, and endline) on perceived stress.

Table A8. Repeated Measures ANOVA: effects of condition and time point on relationship conflict

Source	df	SS	MS	F-value	*p*	η^2 [95% CI]
Between-subjects						
Condition	2	0.735	0.367	0.943	.390	0.002 [0.000, 0.009]
Error	974	379.592	0.390			
Within-subjects						
Time	2	104.072	52.036	120.991	<.001	0.111 [0.085, 0.136]
Time*Condition	4	0.221	0.055	0.129	.927	0.000 [0.000, 0.001]
Error (Time)	1948	837.799	0.430			

Note. Reporting between- and within-subjects effects of condition and time point (baseline, during the intervention, and endline) on relationship conflict.

Table A9. Parallel mediation models estimating the indirect effects of the time-saving condition versus the UCT condition on subjective wellbeing, perceived stress, and relationship conflict via total spending (log), cash on hand (log), and perceived burden of unpaid labor.

Model: Dependent measure	Mediator	Indirect effect	95% CI
Model 1: Subjective wellbeing	Total spending (log)	0.026	-0.027, 0.078
	Cash on hand (log)	-0.011*	-0.024, -0.003
	Burden of unpaid labor	0.037	-0.015, 0.087
Model 2: Perceived stress	Total spending (log)	0.000	-0.003, 0.007
	Cash on hand (log)	0.010*	0.002, 0.023
	Burden of unpaid labor	-0.082*	-0.130, -0.035
Model 3: Relationship conflict	Total spending (log)	-0.002	-0.009, 0.001
	Cash on hand (log)	0.003	-0.002, 0.012
	Burden of unpaid labor	0.020	-0.027, 0.065

Note. Reporting standardized coefficients for 3 parallel mediation models: 1) effects on subjective wellbeing, 2) perceived stress, and relationship conflict. Each of these outcomes are a weighted average of responses during the treatment weeks (weeks 3-5). For each model, the independent variable is condition, where 1=time-saving and 0=UCT. The time-saving condition includes both treatment arms (time-saving and time-saving+planning). The control condition was dropped from these analyses. The mediators are modelled in parallel and the following covariates are included in estimates of both of the mediator and dependent measure: baseline income (log) and respective baseline dependent measure. n = 913, listwise deletion of cases with missing data. 5000 bootstrapped samples. *95% confidence interval around the indirect effect does not include 0.

Table A10. Bootstrapped regressions analyses in parallel mediation analysis estimating perceived stress.

Model: dependent measure	β	t	p	95% CI (β) Lower bound	Upper bound
Model 1: total spending (log)					
Time-saving condition=1 (UCT=0)	-0.048	-1.661	.097	-0.105	0.009
Baseline perceived stress	-0.127	-4.314	< .001	-0.185	-0.069
Baseline monthly income (log)	0.078	2.589	.010	0.019	0.138
Model 2: cash on hand (log)					
Time-saving condition=1 (UCT=0)	-0.081	-2.605	.009	-0.143	-0.020
Baseline perceived stress	-0.158	-4.972	<.001	-0.220	-0.095
Baseline monthly income (log)	0.161	4.953	<.001	0.097	0.225
Model 3: burden of unpaid labor					
Time-saving condition=1 (UCT=0)	-0.593	-22.971	<.001	-0.644	-0.542
Baseline perceived stress	0.010	0.396	.693	-0.041	0.062
Baseline monthly income (log)	-0.005	-0.169	.866	-0.057	0.048
Model 4: Perceived stress (W3-W5)					
Total spending (log)	-0.009	-0.244	.808	-0.078	0.061
Cash on hand (log)	-0.119	-3.584	<.001	-0.184	-0.054
Burden of unpaid labor	0.138	3.552	<.001	0.062	0.215
Time-saving condition=1 (UCT=0)	0.074	1.927	.054	-0.001	0.149
Baseline perceived stress	0.174	5.537	<.001	0.112	0.236
Baseline monthly income (log)	-0.067	-2.085	.037	-0.130	-0.004
Model 5: Perceived stress (W3-W5)					
Time-saving condition=1 (UCT=0)	0.002	0.054	.957	-0.059	0.062
Baseline perceived stress	0.195	6.253	<.001	0.134	0.257
Baseline monthly income (log)	-0.087	-2.736	.007	-0.150	-0.024

Note: Five bootstrapped regression models conducted using Preacher and Hayes Process model 4. Total spending (log), cash on hand (log), perceived burden of unpaid labor, and perceived stress are all measured as the weighted average of responses during the three treatment weeks (weeks 3-5). Reporting standardized regression coefficients, *t*-statistics, *p*-values, and 95% confidence intervals. 5000 bootstrapped samples. n = 913.

Table A11. Parallel mediation models estimating the indirect effects of the time-saving condition versus the UCT condition on subjective wellbeing, perceived stress, and relationship conflict via percentage time spent on paid work and socializing.

Model: Dependent measure	Mediators	Indirect effect	95% CI
Model 1: Subjective wellbeing	Time on paid work	-0.001*	-0.009, 0.006
	Time socializing	0.000*	-0.002, 0.005
Model 2: Perceived stress	Time on paid work	0.000*	-0.004, 0.007
	Time socializing	0.000*	-0.007, 0.004
Model 3: Relationship conflict	Time on paid work	0.000*	-0.004, 0.002
	Time socializing	0.0000*	-0.005, 0.002

Note. Reporting standardized coefficients for 3 parallel mediation models: 1) effects on subjective wellbeing, 2) perceived stress, and relationship conflict. Time spent on paid work and socializing are calculated as a percentage of total time reported in a given week; weighted average of the three treatment weeks (weeks 3-5). For each model, the independent variable is condition, where 1=time-saving and 0=UCT. The time-saving condition includes both treatment arms (time-saving and time-saving+planning). The control condition was dropped from these analyses. The mediators are modelled in parallel and the respective baseline dependent measure is included as a covariate in estimates of the mediator and dependent measure. n = 1043, listwise deletion of cases with missing data. 5000 bootstrapped samples. * 95% confidence interval around the indirect effect does not include 0.

Table A12. Correlation matrix for participants assigned to receive prepared meals (n = 383)

Variable	1	2	3	4
1. Subjective wellbeing (1-5) (endline – baseline difference score)	--			
2. Perceived stress (1-5) (endline – baseline difference score)	-.522**	--		
3. Relationship conflict (0-4) (endline – baseline difference score)	-.084	.031	--	
4. Baseline enjoyment of cooking (1-5)	-.008	.054	.015	--

Notes. Reporting means, standard deviations, and correlations. To measure baseline dislike of cooking, participants responded to the following item: "How much do you enjoy or dislike prepared meals?" Participants provided their response on a 1-5 scale, where 1=very much dislike, 2=somewhat dislike, 3=neither dislike nor enjoy, 4=somewhat enjoy, 5=enjoy very much. Valid N (listwise) = 358. * $p < .05$, ** $p < .01$.

Table A13. Correlation matrix for participants assigned to receive laundry services (n = 392).

Variable	1	2	3	4
1. Subjective wellbeing (1-5) (endline – baseline difference score)	--			
2. Perceived stress (1-5) (endline – baseline difference score)	-.540**	--		
3. Relationship conflict (0-4) (endline – baseline difference score)	-.005	.022	--	
4. Baseline enjoyment of doing laundry (1-5)	-.118*	.059	-.011	--

Notes. Reporting means, standard deviations, and correlations. To measure baseline dislike of cooking, participants responded to the following item: "How much do you enjoy or dislike doing laundry?" Participants provided their response on a 1-5 scale, where 1=very much dislike, 2=somewhat dislike, 3=neither dislike nor enjoy, 4=somewhat enjoy, 5=enjoy very much. Valid N (listwise) = 358. * $p < .05$, ** $p < .01$.

Table A14. Correlation matrix for participants assigned to time-saving conditions (n = 775).

Variable	1	2	3	4
1. Subjective wellbeing (1-5) (endline – baseline difference score)	--			
2. Perceived stress (1-5) (endline – baseline difference score)	-.534**	--		
3. Relationship conflict (0-4) (endline – baseline difference score)	-.046	.026	--	
4. Baseline enjoyment of doing chores (1-5)	.025	.049	-.023	--

Notes. Reporting means, standard deviations, and correlations. To measure baseline dislike of cooking, participants responded to the following item: "How much do you enjoy or dislike completing chores?" Participants provided their response on a 1-5 scale, where 1=very much dislike, 2=somewhat dislike, 3=neither dislike nor enjoy, 4=somewhat enjoy, 5=enjoy very much. Valid N (listwise) = 358. * $p < .05$, ** $p < .01$.

Table A15. Moderation analyses predicting the effects of the time-saving versus UCT condition.

Dependent measure	Moderator	Interaction statistics
Subjective wellbeing	Education	$R^2\Delta$=.001, F(1, 1077)=0.928, p=.336
	Occupation	$R^2\Delta$=.004, F(4, 1049)=1.173, p=.321
	Micro-enterprise ownership	$R^2\Delta$=.004, F(1, 1077)=4.209, p=.040
	Household size	$R^2\Delta$=.002, F(1, 1077)=1.805, p=.179
	Baseline monthly income (log)	$R^2\Delta$=.000, F(1, 1070)=0.003, p=.959
	Baseline subjective wellbeing	$R^2\Delta$=.000, F(1, 1078)=0.532, p=.466
	Baseline perceived stress	$R^2\Delta$=.005, F(1, 1077)=5.601, p=.018
	Baseline relationship conflict	$R^2\Delta$=.000, F(1, 1077)=0.366, p=.546
	Baseline CES-D	$R^2\Delta$=.000, F(1, 1077)=0.291, p=.589
Perceived stress	Education	$R^2\Delta$=.000, F(1, 1075)=0.156, p=.693
	Occupation	$R^2\Delta$=.003, F(4, 1047)=0.894, p=.467
	Micro-enterprise ownership***	$R^2\Delta$=.014, F(1, 1075)=3.866, p<.001
	Household size	$R^2\Delta$=.000, F(1, 1075)=0.490, p=.484
	Baseline monthly income (log)	$R^2\Delta$=.000, F(1, 1068)=0.424, p=.515
	Baseline subjective wellbeing	$R^2\Delta$=.000, F(1, 1075)=0.244, p=.622
	Baseline perceived stress	$R^2\Delta$=.000, F(1, 1076)=0.032, p=.859
	Baseline relationship conflict	$R^2\Delta$=.000, F(1, 1075)=0.421, p=.517
	Baseline CES-D	$R^2\Delta$=.000, F(1, 1075)=0.360, p=.548
Relationship conflict	Education	$R^2\Delta$=.003, F(1, 1077)=4.345, p=.037
	Occupation	$R^2\Delta$=.005, F(4, 1049)=1.503, p=.199
	Micro-enterprise ownership	$R^2\Delta$=.002, F(1, 1077)=2.052, p=.152
	Household size	$R^2\Delta$=.000, F(1, 1077)=0.107, p=.744
	Baseline monthly income (log)	$R^2\Delta$=.000, F(1, 1070)=0.597, p=.440
	Baseline subjective wellbeing	$R^2\Delta$=.000, F(1, 1075)=0.244, p=.622
	Baseline perceived stress	$R^2\Delta$=.000, F(1, 1077)=0.195, p=.659
	Baseline relationship conflict	$R^2\Delta$=.001, F(1, 1078)=0.985, p=.321
	Baseline CES-D	$R^2\Delta$=.001, F(1, 1077)=0.858, p=.354

Note. Reporting 27 moderation analyses, each using Preacher and Hayes Process model 1 with 5000 bootstrapped samples. All models control for the respective baseline dependent measure. Occupation was coded into 4 dummy variables: 1) trades, 2) personal services, 3) casual labor, and 4) childcare, education, and healthcare services; 'all sales jobs' coded as the reference category. To adjust for multiple comparisons, we used a Bonferroni correction. With this correction, the significance level for each comparison is $p \leq 0.002$. Thus, micro-enterprise ownership was the only individual difference that influenced that effect of time-saving vouchers versus UCTs. *** $p < .001$.

Follow-up on significant interaction effects predicting endline perceived stress

Table A16. Moderation analysis conditional effects: effect of time-saving vs UCT on endline perceived stress, controlling for baseline, conditional on micro-enterprise ownership

Conditional effects	β	t	p	95% CI (β) Lower bound	95% CI (β) Upper bound
Micro-enterprise ownership=0	-0.058	-1.569	.117	-0.130	0.014
Micro-enterprise ownership=1	0.188	3.866	$< .001$	0.093	0.283

Note. Reporting standardized coefficients, *t*-statistics, *p*-values, and 95% confidence intervals. 5000 bootstrapped samples. n = 1082.

Table A17. Exclusions based on pre-registered criteria from initial participant pool

Variable	Decision Rule	# of Exclusions	Remaining Eligible
Lives in Kibera	If no, exclude.	360 of 4,286 (8.4%)	3,926[a]
Available to participate	If no, exclude.	475 of 3,926 (12.1%)	3,622[a]
Consent Completed	If no, exclude.	22 of 3,622 (0.06%)	3,600
Gender	If male, exclude.	6 of 3,600 (0.02%)	3,600[b]
Children	If no, exclude.	84 of 3,600 (1.9%)	3,510
Children Living at Home	If no, exclude.	103 of 3,510 (2.3%)	3,407
Children Enrolled in School	If no, exclude.	142 of 3,407 (3.2%)	3,265
25+ hours worked / week	If no, exclude.	549 of 3,265 (16.8%)	2,716
Completed their own laundry	If no, exclude.	9 of 2,716 (0.3%)	2,707
Spent fewer than 3 hours on laundry each week	If yes, exclude.	1 of 2,707 (0.0%)	2,706
"Always" paid money for someone else to do laundry	If yes, exclude.	4 of 2,706 (0.01%)	2,702
Completed own cooking	If no, exclude.	0 of 2,702 (0.0%)	2,702
Spent fewer than 3 hours cooking each week	If yes, exclude.	0 of 2,702 (0.0%)	2,702
"Always" paid money for someone to cook for them	If yes, exclude.	1 of 2,702 (0.0%)	2,701
7 or more people living in the participants' household?	If yes, exclude.	71 of 2,701 (2.6%)	2,630
Travel to Kibera Town Center was 45 minutes or more	If yes, exclude.	21 of 2,630 (0.8%)	2,609[c]
Participant or household member had food allergies	If yes, exclude.	36 of 2,609 (2.4%)	2,573[d]
TOTAL ELIGIBLE:			2,573 (57.7%)

Note: To save time and money, the eligibility survey was designed such that if a participant was excluded on any variable, the survey would be terminated immediately. For example, if a participant did not live in Kibera, they were not asked if they were free to participate. Thus, the number of exclusions reflect participants who were excluded on each variable (after inclusion on the previous variables). [a]A subset of participants (n=171) did not answer the first two questions and were treated as missing in these analyses. [b]The survey did not automatically exclude participants after they reported their gender, explaining the identical denominator for children and gender in this table. [c]We decided to exclude participants only if they lived 45 minutes or farther from KTC (vs. 30 minutes as per our pre-registration) based on recruitment advice from our field officers. Although many participants lived more than 30 minutes away, they passed by Kibera Town Center frequently while commuting to work or running errands, thus KTC was conveniently located for most. [d]A subset of participants did not answer this question (n=358).

Table A18. Characteristics of participants who completed the endline survey in-person (n = 764) at KTC versus over the phone (n = 306).

Variable (measured at baseline)	In-person endline Mean (SD)	Phone endline Mean (SD)	Difference statistics
Age	34.09 (8.16)	36.90 (9.64)	$t(1065) = -4.56$, $p < .001$
Education (% completed primary school)	77%	82%	$X^2(N=1, 1070) = 2.99$, $p = .10$
% married or marriage-like relationship	60%	59%	$X^2(N=1, 1070) = 0.089$, $p = .78$
Household size (total # of people)	4.73 (1.57)	4.56 (1.49)	$t(1068) = -1.59$, $p = .12$
Number of children in the household	3.09 (1.44)	2.92 (1.421)	$t(1068) = -1.82$, $p = .07$
% responsible for financial decisions	50%	49%	$X^2(N=1, 1070) = 0.09$, $p = .79$
Hours of paid labor in past 7 days	43.40 (20.49)	46.99 (21.36)	$t(1068) = 2.56$, $p = .01$
Hours of unpaid labor in past 7 days	39.21 (24.26)	44.72 (25.23)	$t(1068) = 3.31$, $p = .001$
Personal income in past 6 months	KSH 38,696 (46,417)	KSH 43,025 (49,308)	$t(1059) = 1.75$, $p = .08$
Household spending in the past 7 days	KSH 3,199 (3,563)	KSH 3,891 (4,198)	$t(1068) = 2.72$, $p = .01$
Baseline depression (CES-D; 1- 4)	2.24 (0.47)	2.24 (0.51)	$t(1068) = -0.07$, $p = .94$
Baseline SWB (1 – 5)	2.73 (0.68)	2.65 (0.68)	$t(1068) = -1.58$, $p = .12$
Baseline PSS (1 – 5)	3.24 (0.53)	3.24 (0.56)	$t(1068) = 0.11$, $p = .91$
Baseline relationship conflict[a] (0 – 4)	0.95 (0.96)	0.91 (0.97)	$t(1068) = -0.61$, $p = .54$

Notes. Reporting means and standard deviations for respondent characteristics at baseline. Time spent on paid and unpaid labor is measured as a percentage of total time reported for the past 7 days. Difference statistic for baseline income uses log(baseline monthly income). To adjust for multiple comparisons, we have used Bonferroni correction. Using this correction, the significance level for these comparisons is $p < 0.004$. Therefore, participants who completed the endline survey over the phone differ only in terms of age and baseline hours of unpaid labor as compared to those who completed the endline survey in-person at KTC.

Table A19. Bayesian model comparison on the subset of participants who completed the endline survey in-person at KTC (excluding those who completed the over the phone and therefore could not complete the Satisfaction with Life measure).

Models	P(M)	P(M|data)	BF_M	BF_{10}	error %
Models predicting endline SWB:					
M_0: Null model (incl. baseline SWB)	0.500	0.896	8.644	1.000	
M_1: Condition + baseline SWB	0.500	0.104	0.116	0.116	5.247

Note: Reporting the prior model probability, P(M); the posterior model probability, P(M|data); the posterior model odds, BF_M; and the Bayes Factor indicating the predictive performance of a given model divided by the predictive performance of the null model (BF_{10}). $n = 764$.

Table A20. Bayesian pairwise comparisons on the subset of participants who completed the endline survey in-person at KTC.

		Prior Odds	Posterior Odds	BF_{10}	error %
Pairwise comparisons on endline SWB					
Control	UCT	0.587	0.058	0.098	< .001
Control	Time-saving	0.587	0.174	0.297	< .001
UCT	Time-saving	0.587	0.213	0.363	< .001

Note: The posterior odds have been corrected for multiple testing by fixing to 0.5 the prior probability that the null hypothesis holds across all comparisons (Westfall, Johnson, & Utts, 1997). Individual comparisons are based on the default t-test with a Cauchy (0, r = 1/sqrt(2)) prior. Bayes Factors are uncorrected. $n = 764$.

Table A21. Bayesian model comparison on the subset of participants who reported being married or in a marriage-like relationship (and therefore responded to the relationship conflict questions with respect to their romantic partner)

Models	P(M)	P(M|data)	BF_M	BF_{10}	error %
Models predicting endline conflict:					
M_0: Null model (incl. baseline conflict)	0.500	0.927	12.635	1.000	
M_1: Condition + baseline conflict	0.500	0.073	0.079	0.079	2.705

Note: Reporting the prior model probability, P(M); the posterior model probability, P(M|data); the posterior model odds, BF_M; and the Bayes Factor indicating the predictive performance of a given model divided by the predictive performance of the null model (BF_{10}). $n = 604$.

Table A22. Bayesian pairwise comparisons on the subset of participants who reported being married or in a marriage-like relationship

		Prior Odds	Posterior Odds	BF_{10}	error %
Pairwise comparisons on endline conflict					
Control	UCT	0.587	0.247	0.421	< .001
Control	Time-saving	0.587	0.327	0.557	.007
UCT	Time-saving	0.587	0.066	0.112	< .001

Note: The posterior odds have been corrected for multiple testing by fixing to 0.5 the prior probability that the null hypothesis holds across all comparisons (Westfall, Johnson, & Utts, 1997). Individual comparisons are based on the default t-test with a Cauchy (0, r = 1/sqrt(2)) prior. Bayes Factors are uncorrected. $n = 604$.

Appendix B: Supplemental Information for Chapter 2

Appendix B includes supplemental methods and results for Studies 1-3.

Study 1: supplemental methods and results
Effects of income volatility over 27-years

Data for the National Longitudinal Study of Youth is publicly available via the Bureau of Labor Statistics (https://www.nlsinfo.org/content/cohorts/nlsy79).

Robustness checks
We report two robustness checks for the results of Study 1. First, we replicate the results reported in the main text but without using any exclusion criteria for the measure of impatience. We find the same pattern of results (Table A23). Second, we re-run this regression including periods of unemployment in the measure of income volatility. In the main text, we code years with $0 in reported income as missing data to avoid coding periods of prolonged unemployment as stable income. However, some researchers argue that job losses are an important source of volatility since they represent the largest sudden changes in income (Dynan, Elmendorf, Sichel, 2012). In Table A24, we report the effects of income volatility (SD % change in bi-yearly income) on impatience including years with $0 in reported income. In this analysis, we find a stronger association between income volatility from 1980-2006 and impatience in 2006. This suggests that the results reported in the main text may be a conservative estimate since they exclude a large source of income volatility that contributes to further impatience.

Table A23. Study 1: effects of income volatility on impatience, using raw discount factor as the dependent variable

DV: monthly discount factors (raw)	β	t	p	95% CI (β) Lower bound	Upper bound
SD % change 1980-2006	-0.030	-2.203	.028	-0.057	-0.003
Net worth 2004	0.111	8.510	< .001	0.085	0.136
Risk-seeking for income	-0.028	-2.019	.043	-0.054	0.001

Notes: OLS regression predicting monthly discount factors (no exclusions). Reporting standardized regression coefficients, standard errors, *t*-statistics, *p*-values, and 95% confidence intervals. N = 5,412

Table A24. Study 1: effects of income volatility on impatience including periods of unemployment

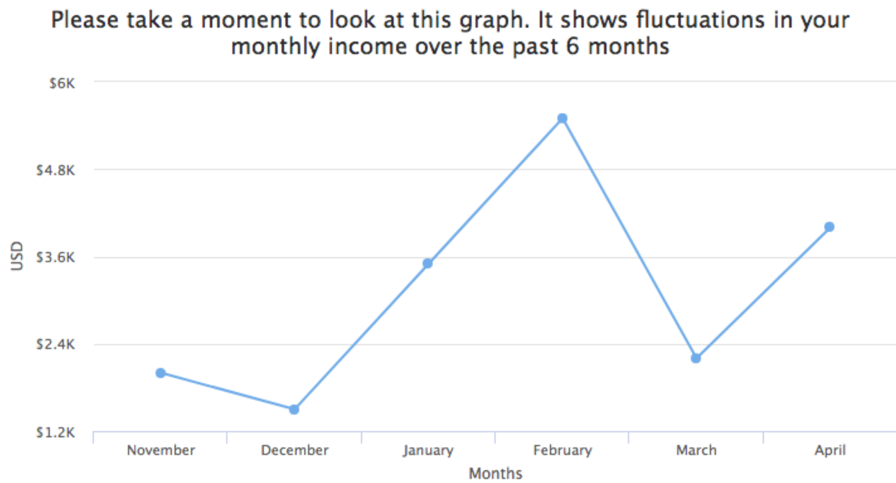
DV: monthly discount factor	(Model 1)	(Model 2)	(Model 3)
SD % change 1980-2006 (Including years with $0 in reported income)	-0.096** (0.013) *p*<.001	-0.091*** (0.014) *p*<.001	0.088*** (0.014) *p*<.001
Net worth 2004		-0.160*** (0.013) *p*<.001	-0.162*** (0.013) *p*<.001
Risk-seeking for income			-0.031* (0.013) *p*=.020
Observations	6,263	5,896	5,688

Notes: TOLS regression predicting patience (monthly discount factors, k=V/A, dropping participants who report perfect patience, k=1, and observations greater than 3 standard deviations below the mean). Reporting standardized coefficients, standard errors in brackets, and p-values. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

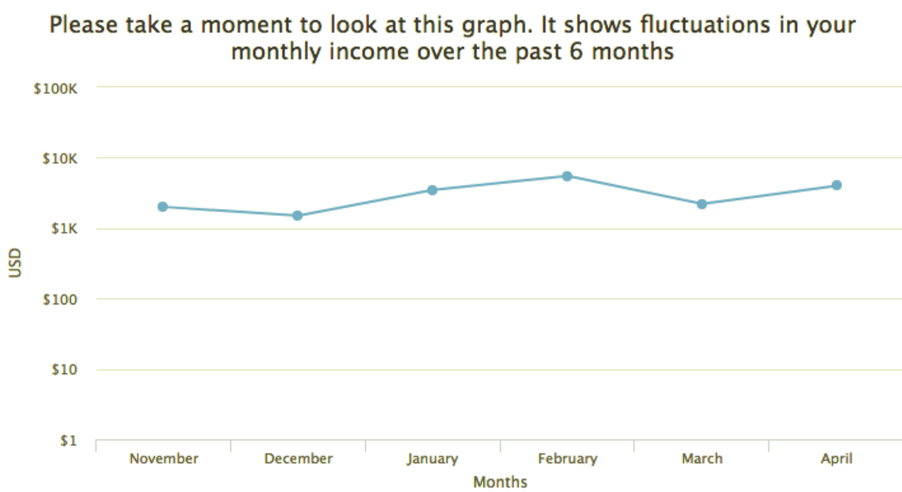
Study 2: supplemental methods and results
Effects of income volatility over 27-years

Additional methodological details
In Study 2, participants were prompted to enter their monthly incomes for each of the past 6 months using any records they have available (e.g. online banking, pay stubs, etc.). The income information they reported was then presented to them in a line graph with either a linear or logarithmic y-axis. As displayed below, the linear display makes income volatility more salient. These two examples show identical income data presented in either the linear or logarithmic display format.

Example line graph with linear y-axis:



Example line graph with logarithmic y-axis:



Robustness checks

We report two robustness checks for the results of Study 2. First, we analyze the associations between income volatility and impatience controlling for the graphical display method. The results are substantively unchanged when including an indicator variable for graphical display method (Table A25). Second, we replicate the results reported in the main text but without using any exclusion criteria for the measure of impatience. We find no substantive differences in the results when using raw discount factors (Table A26).

Table A25. Study 2: effects of income volatility on impatience, controlling for graphical display method (linear vs logarithmic line graph)

DV: monthly discount factors	β	*t*	*p*	95% CI (β) Lower bound	Upper bound
SD % change monthly income	-0.195	-3.459	.001	-0.306	-0.084
Total 6-month earnings	-0.314	-3.462	.001	-0.492	-0.135
Risk preferences	0.065	1.079	.28	-0.053	0.183
Graphical display (linear)	0.009	0.153	.878	-0.103	0.121

Notes. OLS regression model predicting monthly discount factors (dropping participants who report perfect patience, k=1, and observations greater than 3 standard deviations below the mean). Reporting standardized regression coefficients, standard errors, *t*-statistics, *p*-values, 95% confidence intervals. N = 283

Table A26. Study 2: effects of income volatility on impatience, using raw discount factor as the dependent variable

DV: monthly discount factors (raw)	β	*t*	*p*	95% CI (β) Lower bound	Upper bound
SD % change monthly income	-0.178	-3.436	.001	-0.280	-0.076
Total 6-month earnings	-0.341	-6.509	>.001	-0.444	-0.238
Risk preferences	0.111	2.120	.035	0.008	0.215

Notes: Three separate OLS regression models predicting monthly discount factors (no exclusions). Reporting standardized regression coefficients, standard errors, *t*-statistics, *p*-values, 95% confidence intervals. N = 322

Appendix C: Supplemental Information for Chapter 3

Appendix C includes supplemental methods and results for Studies 1-6.

Study 1: supplemental methods and results
Effects of budget partitioning on savings allocations

Preregistered inclusion criteria (resultant sample size)

- Raw sample size (*N*=478)
- Reporting annual income >$10k and <$500k (*N*=437)
- Completed the full survey (*N*=423)
- Passed the captcha (*N*=423)
- Spent at least 3min on the survey in total (*N*=412)

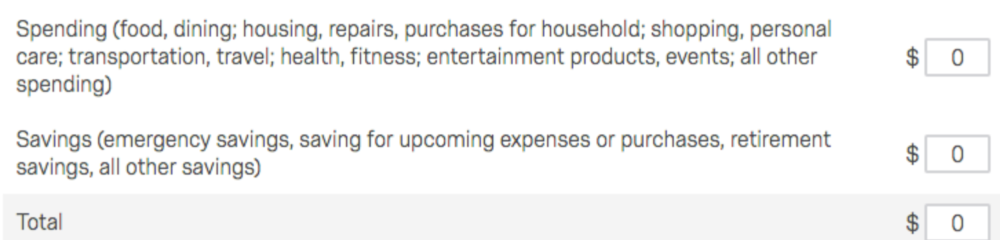
Table A27. Study 1: amount allocated to savings, by condition

Condition		Amount allocated to savings	
	n	*Mean*	*SD*
Spending partitioned	137	41.61%	28.04%
Control	133	78.83%	18.02%
Saving partitioned	142	60.49%	24.01%

Experimental stimuli

"Imagine that you have received a 20% raise (after tax) on your annual income. This amounts to a raise of [reported annual income * 0.2]. Being as realistic as possible, please indicate how you would allocate this raise into your household budget."

Control condition



Savings partitioned condition

Spending (food, dining; housing, repairs, purchases for household; shopping, personal care; transportation, travel; health, fitness; entertainment products, events; all other spending)	$ 0
Emergency savings	$ 0
Savings for upcoming expenses or purchases	$ 0
Retirement savings	$ 0
All other savings	$ 0
Total	$ 0

Spending partitioned condition

Food, dining	$ 0
Housing, repairs, purchases for the household	$ 0
Shopping, personal care	$ 0
Transportation, travel	$ 0
Health, fitness	$ 0
Entertainment products, events	$ 0
All other spending	$ 0
Saving (emergency savings, savings for upcoming expenses or purchases, retirement savings, all other savings)	$ 0
Total	$ 0

Robustness checks

We report three regression models for Study 1. First, we analyze the association between each partitioning condition and savings allocation, relative to the control condition. Second, we examine interaction effects between condition and annual with respect to savings allocations. Third, we add controls for annual income (log), age, education level, and gender.

Table A28. Study 1: effects of partitioning savings and spending on raise allocation

DV: % of raise allocated to savings	*b*	*t*	*p*	95% CI (*b*) Lower bound	95% CI (*b*) Upper bound
Model 1:					
Spending partitioned	-18.88	-6.63	< .001	-24.48	-13.29
Saving partitioned	18.34	2.87	< .001	12.70	23.98
Model 2:					
Spending partitioned	.96	.02	.985	-100.08	101.99
Saving partitioned	-24.29	-.47	.641	-126.69	78.120
Annual income (log)	2.45	.32	.753	-12.87	17.78
Spending partition*income(log)	-4.29	-.39	.700	-26.87	17.78
Saving partition*income(log)	9.28	.82	.410	-12.85	31.39
Model 2:					
Spending partitioned	-18.48	6.53	< .001	-24.05	-12.92
Saving partitioned	18.24	6.40	< .001	12.64	23.83
Annual income (log)	.24	.05	.960	-9.03	9.50
Age	.26	2.17	.031	.02	.49
Education level	3.84	2.23	.027	.45	.72
Gender (women = 1)	-5.98	-2.49	.013	-10.70	-1.25

Note. Three OLS regression models predicting the percentage of the hypothetical raise that participants allocated towards savings. Reporting unstandardized regression coefficients, *t*-statistics, *p*-values, and 95% confidence intervals. Spending partitioned and saving partitioned are dummy-coded variables for condition, where the control condition is the reference group. Model 1 (N = 412), Model 2 (N = 401), Model 3 (N = 412).

Study 2: supplemental methods and results
An incentive compatible test of budget partitioning

Preregistered inclusion criteria (resultant sample size)

- Raw sample (*N*=4,684)
- Passed attention checks (*N*=930)
- Spent at least 2min on the survey (*N*=930)
- Resultant sample (*N*=930)

Table A29. Study 2: amount allocated to savings, by condition

Condition		Amount allocated to savings	
	n	*Mean*	*SD*
Control	474	558.13	288.13
Saving partitioned	456	716.76	285.99

Table A30. Study 2: descriptive statistics and correlations

Variable	*N*	*M*	*SD*	1	2	3	4	5
1. Amount allocated to savings	930	$636	$297	--				
2. Annual income	930	$78k	$109k	.05	--			
3. Age	930	42.82	15.85	.09**	-.07*	--		
4. Education level (1-5)	930	3.82	1.01	.03	.37**	-.08*	--	
5. Financial literacy score (0-5)	930	2.70	1.41	.12**	-.02	-.38**	.05	--

Note. Reporting means, standard deviations, and correlations.

Table A31. Study 2: effects of partitioning savings amount of $1000 check allocated to saving

DV: dollar amount allocated to savings ($0 - $1000)	*b*	*t*	*p*	95% CI (*b*) Lower bound	Upper bound
Model 1:					
Saving partitioned	160.42	8.55	< .001	123.58	197.25
Annual income (log)	27.64	2.81	.005	8.31	46.96
Model 2:					
Saving partitioned	159.64	8.55	< .001	123.01	196.28
Annual income (log)	22.65	2.15	.032	1.93	43.37
Age	.79	1.23	.219	-.469	2.05
Education level	2.92	.29	.77	-16.51	22.35
Financial literacy score	20.52	2.85	.005	6.37	24.67

Note. Three OLS regression models predicting the amount of the $1000 cash prize that participants allocated to savings. Reporting unstandardized regression coefficients, *t*-statistics, *p*-values, 95% confidence intervals. Savings partitioned is a dummy-coded variable for condition, where the control condition is the reference group. $N = 930$.

We conducted a moderation analysis (5000 bootstrapped samples) to analyze the effects of budget partitioning on windfall allocation at different levels of financial literacy. We found that participants who scored poorly in financial literacy (1 SD below the mean) saved an additional $219 when savings categories were partitioned (conditional effect, $b = 219.15$, $p < .001$, 95% CI(β) = [167.47, 270.83]). Participants who scored at the mean allocated an additional $158 (conditional effect, $b = 158.70$, $p < .001$, 95% CI(β) = [122.21, 195.19]), and those with a high financial literacy score allocated an additional $98 (conditional effect, $b = 98.25$, $p < .001$, 95% CI(β) = [46.57, 149.94]). These suggest that people who are low in financial literacy are more likely to rely on a 1/n heuristic in financial allocation.

Study 3: supplemental methods and results
Number of savings sub-categories

Preregistered inclusion criteria (resultant sample size)

- Raw sample (*N*=897)
- Passed attention check (*N*=388)
- Reported annual income >$10k and <$500k (*N*=340)
- Spent at least 3min on the survey (*N*=319)
- An additional 3 participants were excluded because they entered non-numeric characters in the annual income question and therefore their hypothetical raise amount could not be calculated (*N*=316)
- Resultant sample (*N*=316)

Table A32. Study 3: amount allocated to savings, by condition

Condition		Amount allocated to savings	
	n	*Mean*	*SD*
1 savings goal	107	73.97%	16.05%
4 savings goal	98	84.54%	12.90%
10 savings goal	111	90.42%	16.43%

Study 4: supplemental methods and results
Simultaneous versus sequential budgeting

Preregistered inclusion criteria (resultant sample size)

- Raw sample size (*N*=989)
- Passed 2 screener questions and 2 attention checks (*N*=394)
- Reporting annual income >$10k and <$500k (therefore monthly income between $833 and $41,666 (*N*=345)
- Completed the full survey (*N*=343)
- Spent at least 2min on the survey in total (*N*=342)
- Dropped participants who left a negative value for “all other uses” (*n*=12, all in sequential condition; it was not possible to have a negative value in the simultaneous condition) (*N*=330)
- Dropped participant who left $0 for “all other uses” (*n*=16 in sequential; *n*=2 in simultaneous); indicating that they may have misunderstood the task. (*N*=312)
- Resultant sample (*N*=312)

Table A33. Study 4: percentage of monthly income allocated to savings, by condition

Condition		Amount allocated to savings	
	n	*Mean*	*SD*
Sequential partitioning	147	27.63%	22.74%
Simultaneous partitioning	165	43.47%	29.55%

Table A34. Study 4: effects of sequential versus simultaneous partitioning on allocation to saving

DV: % of monthly income allocated to savings	*b*	*t*	*p*	95% CI (*b*) Lower bound	Upper bound
Model 1:					
Simultaneous partitioning	15.84	5.29	< .001	9.96	21.73
Monthly income (log)	11.20	2.38	.019	1.85	20.55
Model 2:					
Simultaneous partitioning	15.73	5.42	< .001	10.02	21.43
Monthly income (log)	8.34	1.78	.076	-.89	17.56
Age	-.636	-4.10	< .001	-.94	-.31
Education level	4.31	2.36	.019	.71	7.91

Note. Three OLS regression models predicting the percentage of the monthly income allocated to savings. Reporting unstandardized regression coefficients, *t*-statistics, *p*-values, 95% confidence intervals. Simultaneous partitioning is a dummy-coded variable for condition, where the sequential partitioning condition is the reference group. *N* = 312.

Study 5: supplemental methods and results
Budget unpacking versus budget partitioning

Preregistered inclusion criteria:

- Raw sample (*N*=718)
- Passed attention check (*N*=360)
- Reported monthly income >$833 and <$41,666 (*N*=291)
- Spent at least 3min on the survey (*N*=239)
- Resultant sample (*N*=239)

Table A35. Study 5: amount allocated to savings, by condition

Condition		Amount allocated to savings	
	n	*Mean*	*SD*
Control	68	52.73%	31.17%
Unpacking	93	53.62%	28.11%
Unpacking + partitioning	78	71.29%	31.24%

Study 6: supplemental methods and results
Combining each feature of budget partitioning

Preregistered inclusion criteria:

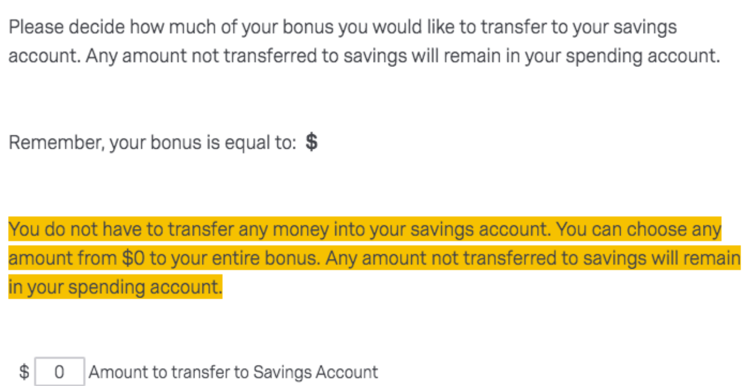
- Raw sample (*N*=1,443)
- Reported annual income >$10k and <$1M (*N*=1,286)
- Spent at least 3min on the survey (*N*=1,258)
- Passed attention checks (*N*=1,022)
- Resultant sample (*N*=1,022)

Table A36. Study 6: amount allocated to savings, by condition

Condition		Amount allocated to savings	
	n	Mean	*SD*
1. Control	210	61.24%	27.55%
2. Savings goals unpacked	189	63.29%	27.88%
3. Sequential partitioning	207	75.58%	24.74%
4. Simultaneous partitioning	207	83.02%	24.03%
5. Simultaneous partitioning + spending account	209	86.58%	19.52%

Experiment stimuli

Condition 1: control

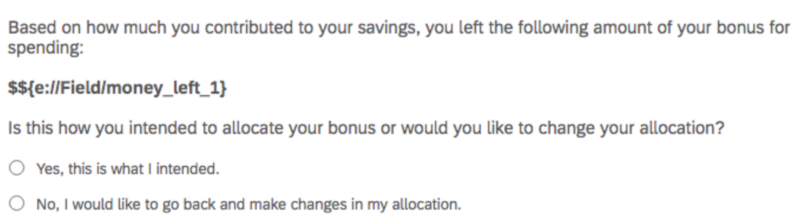

Please decide how much of your bonus you would like to transfer to your savings account. Any amount not transferred to savings will remain in your spending account.

Remember, your bonus is equal to: $

You do not have to transfer any money into your savings account. You can choose any amount from $0 to your entire bonus. Any amount not transferred to savings will remain in your spending account.

$ 0 Amount to transfer to Savings Account

Warning (shown to everyone) – repeat allocation if "no" is selected


Based on how much you contributed to your savings, you left the following amount of your bonus for spending:

$${e://Field/money_left_1}

Is this how you intended to allocate your bonus or would you like to change your allocation?

○ Yes, this is what I intended.

○ No, I would like to go back and make changes in my allocation.

Condition 2: savings goals unpacked

What are your savings goals?

Please take a moment to think about your savings goals and the reasons why you want to save money. From the options below, please select 4 savings goals that are most important to you.

Education	General Investing
Donate to a Cause	Major Purchase
New Car	Retirement
Leaving an Inheritance	Safety Net
Care for Aging Parents	Vacation
New Home	Start a Business

[page break]

You reported the following savings goals:

New Home, General Investing, Major Purchase, Safety Net

Please decide how much of your bonus you would like to transfer to your savings account. Any amount not transferred to savings will remain in your spending account.

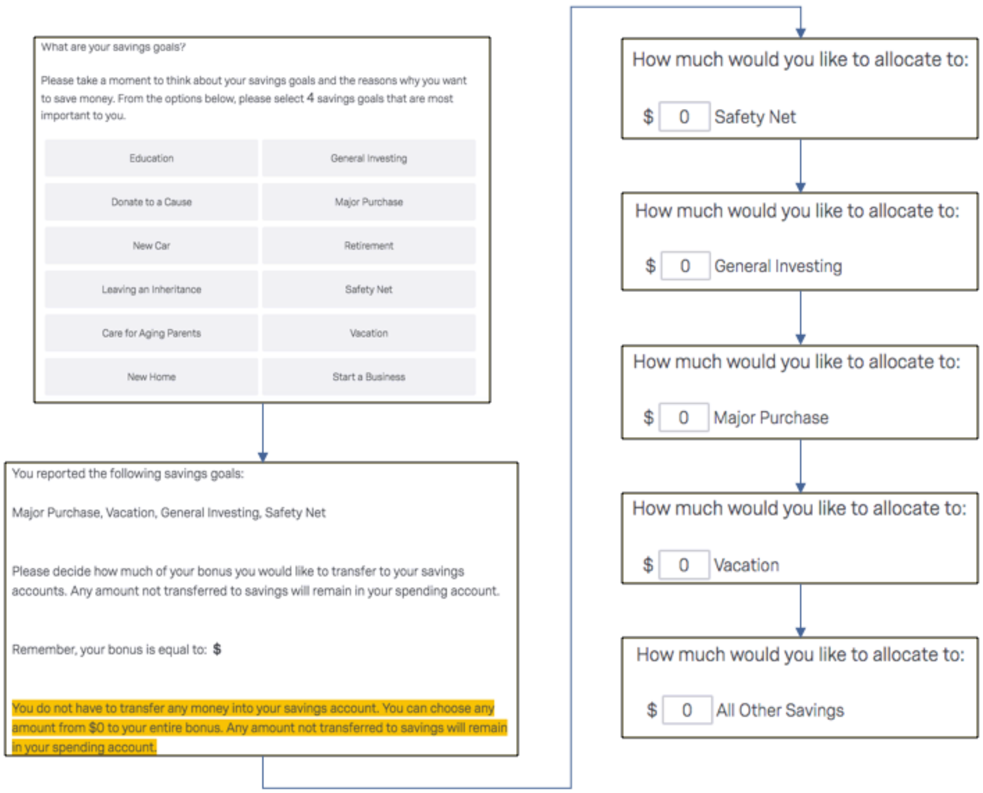
Remember, your bonus is equal to: **$**

You do not have to transfer any money into your savings account. You can choose any amount from $0 to your entire bonus. Any amount not transferred to savings will remain in your spending account.

$ [0] Amount to transfer to Savings Account (New Home, General Investing, Major Purchase, Safety Net, All Other Savings)

Same warning as in condition 1

Condition 3: sequential partitioning



Warning if allocated >100% to savings

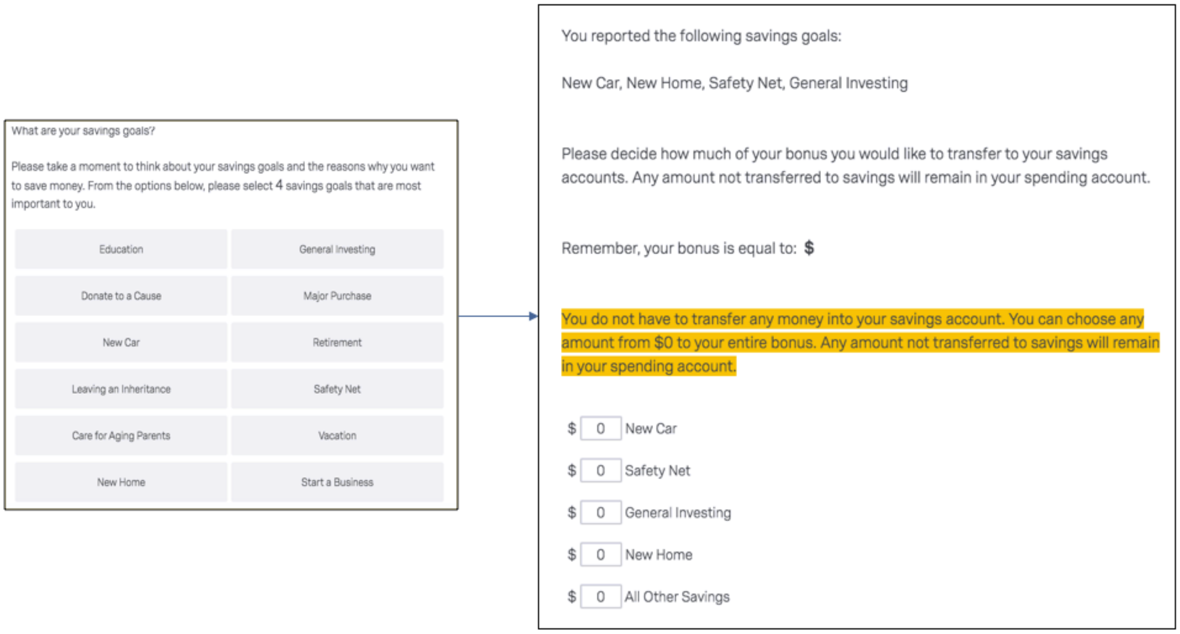
You allocated more than your entire bonus to savings.

The combined amount that you allocate to your savings goals cannot be more than your bonus of **$**

Please proceed to the next page repeat the task. When you repeat the task, remember that you are deciding how to allocate your bonus.

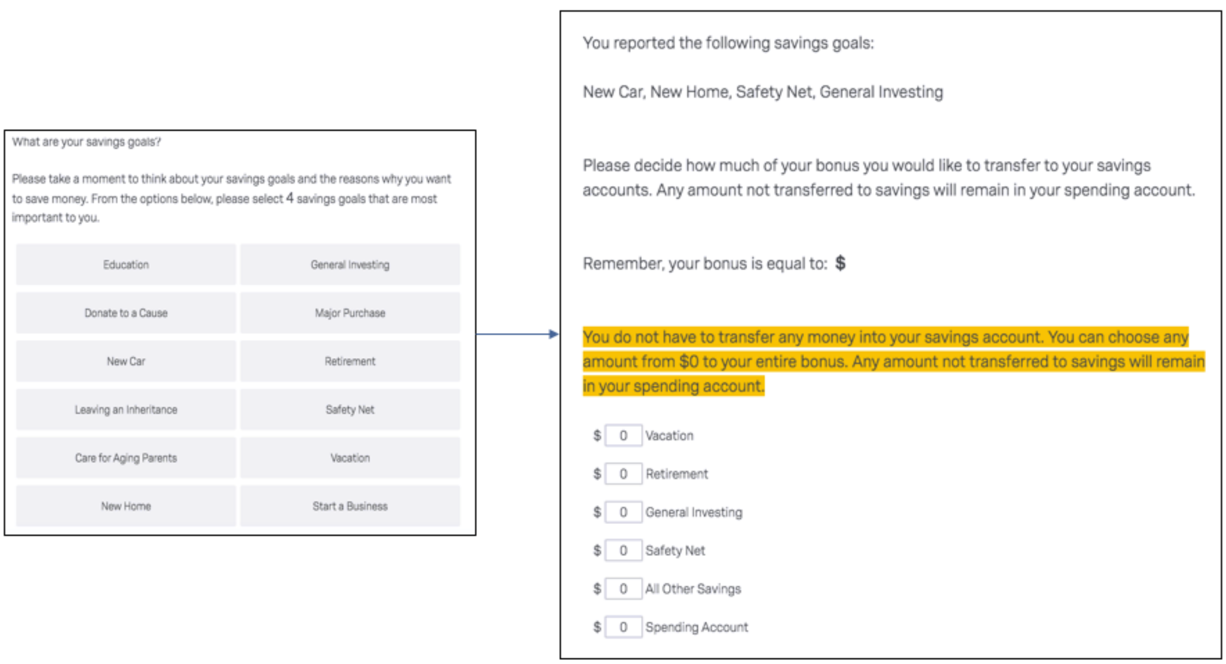
Same warning as in conditions 1 and 2 for participants allocating =<100%

Condition 4: simultaneous partitioning



Same warnings as in condition 3

Condition 5: simultaneous partitioning + spending account present



Same warnings as in conditions 3 and 4

Table A37. Study 6: effects of condition on percentage of bonus allocated to savings

DV: % of bonus saved	Model 1 (financial controls)	Model 2 (demographic controls)
X1 (condition 2 vs 1)	0.021 (*p* = .578)	0.037 (*p* = .325)
X2 (condition 3 vs 2)	0.227*** (*p* < .001)	0.225*** (*p* < .001)
X3 (condition 4 vs 3)	0.125** (*p* = .005)	0.130** (*p* = .003)
X4 (condition 5 vs 4)	0.065 (*p* = .071)	0.051 (*p* = .158)
Log(income in 2019)	0.024 (*p* = .463)	
Subjective financial wellbeing	0.160*** (*p* < .001)	
Financial literacy score	0.021 (*p* = .490)	
Employment (working vs not)	0.041 (*p* = .159)	
Age		-0.073* (*p* = .012)
Gender (women)		-0.064* (*p* = .027)
Education (college educated vs not)		0.075** (*p* = .009)
Observations	1020	1019

Notes: reporting standardized regression coefficients and p-values. Conditions are compared using backward difference coding. X1 compares condition 2 vs 1; X2 compares condition 3 vs 2; X3 compares condition 4 vs 3; X4 compares condition 5 vs 4.

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