Anticipation and Consumption

Neil Thakral
Brown University

Linh T. Tô
Boston University

July 2022*

Abstract

Cash transfer payments are an increasingly widespread policy tool in developed and developing countries, used for both short-term objectives such as boosting consumer spending and long-term objectives such as poverty alleviation. This paper proposes that the marginal propensity to consume out of a windfall depends on a new state variable, the time horizon over which households anticipate receiving a payment. We test this in three different settings arising from a systematic survey of the literature: a natural experiment provided by the randomized disbursement dates of the 2008 U.S. fiscal stimulus payments, and randomized controlled trials on unconditional cash transfers in Kenya and Malawi. Our data show evidence of *excess anticipation-dependence*: Consumption consistently responds more strongly to the receipt of additional income after a shorter anticipation duration, in excess of what standard models predict. While households receiving stimulus payments do not increase spending in advance, the additional consumption expenditure in the month after receiving payment drops by about 30 percent for each additional week that a household waits for their payment. Savings data from Kenya and Malawi show comparable effects. We estimate a model that incorporates this novel form of history dependence to discuss implications for the design of fiscal stimulus policies and cash transfer programs. Our evidence and approach reconcile seemingly conflicting results that consumption responds to anticipated payments in some settings but not others.

*Thakral: Department of Economics, Brown University, Box B, Providence, RI 02912 (email: neil_thakral@brown.edu). Tô: Department of Economics, Boston University, 270 Bay State Road, Boston, MA 02215 (email: linhto@bu.edu). We thank Marshall Drake, Bernadette Hicks, Leah Lam, Steven Lee, and Marcela Mello Silva for excellent research assistance. We are also grateful to Georgios Angelis, Michael Boutros, Kenneth Chay, John Friedman, Peter Ganong, Adam Guren, Tarek Hassan, Stefan Hut, Lawrence Katz, Eben Lazarus, David Laibson, Kevin Lang, Chen Lian, Peter Maxted, Matthew Rabin, Daniel Reck, Alex Rees-Jones, Joshua Schwartzstein, Jesse Shapiro, and David Weil for helpful comments and discussions. Special thanks to James Reisinger for help with the GiveDirectly data. Researchers' own analyses derived based in part on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the NielsenIQ data are those of the researchers and do not reflect the views of NielsenIQ. NielsenIQ is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

1 Introduction

Many policies throughout the world involve directly providing households with cash. Non-contributory cash transfer programs reach over 700 million households in over 130 countries, accounting for the largest share of spending among social safety net programs in developing countries (Honorati, Gentilini and Yemtsov, 2015). Direct cash payments also play an important role in developed economies to restore growth during economic downturns; the United States, for example, spent almost 1 percent of GDP in 2008 to put \$120 billion in the hands of households at the onset of the Great Recession. These policies use the same tool for contrasting goals: long-term objectives such as poverty alleviation, and short-term objectives such as boosting consumer spending.[1] Their effectiveness in achieving these goals has been an active subject of debate by both academics and policymakers (Greenstone and Looney, 2012; Ingram and McArthur, 2018).

The economic intuition that consumers incorporate expectations of income changes in their optimal consumption plans when they learn about such changes suggests an important role for anticipation as a policy instrument. Perhaps surprisingly then, very little empirical evidence characterizes the impulse response of spending to transitory variation in income arriving at different time horizons, which provides a crucial input for evaluating the general equilibrium effects of fiscal shocks and policies (Auclert, Rognlie and Straub, 2018). Motivated by canonical theories of consumption, the literature instead emphasizes "two distinct questions," namely how consumption responds to "anticipated" income changes and how consumption responds to "unanticipated" shocks (Jappelli and Pistaferri, 2010). The dichotomy between anticipated and unanticipated income changes may be misleading if consumption responses depend on the duration between when a household learns about an income change and when the income change occurs.

This paper investigates how the time horizon over which a household anticipates receiving a transfer impacts spending decisions. Canonical tests of consumption theories show that consumption changes too little when information about a future income change arrives (excess smoothness) and changes too much when anticipated income changes occur (excess sensitivity). Our work demonstrates that consumption also exhibits *excess anticipation-dependence*—i.e., the consumption response to an income change depends on the duration of anticipation in excess of what standard models predict. Although a wide range of theories can explain failures of the life-cycle and permanent income models (Modigliani and Brumberg, 1954; Friedman, 1957), those imposing forward-looking consumption decisions predict that the duration of anticipation either does not matter at all or matters only insofar as consumers spend in advance of receiving a windfall. Testing for excess anticipation-dependence thus provides a way to distinguish forward-looking theories of consumption from alternatives.

We systematically survey the literature and arrive at three settings that consist of exogenous

[1]See statements from the U.S. Senate regarding the "goal of increasing consumer spending and providing a short-term boost to the American economy" (Appendix Figure 1); also see statements from the U.K. Department for International Development regarding the role of cash transfers in "escaping poverty traps" (Arnold, Conway and Greenslade, 2011), as well as economic empowerment goals stated as program objectives of the Malawi Social Cash Transfer Program and Ghana's Livelihood Empowerment Against Poverty (Handa et al., 2018).

variation in when households learn about a windfall payment relative to when they receive it.[2] The first consists of a natural experiment provided by the randomized disbursement dates of a U.S. fiscal stimulus payment (Parker et al., 2013). Characterizing how consumption responds before, at the time of, and after income shocks anticipated over different time horizons allows us to test central predictions of theories of consumption. The second and third involve variation induced by randomized controlled trials (RCTs) on unconditional cash transfers in Kenya (Haushofer and Shapiro, 2016) and Malawi (Brune et al., 2017), respectively. Although these settings have been explored in previous work, our empirical findings in each case—greater consumption responses among households that receive payments sooner after announcement—are new.

In our first empirical setting, we use Nielsen Consumer Panel data to study consumption expenditure responses to the tax rebates sent to low- and middle-income American households as part of the Economic Stimulus Act of 2008 (Broda and Parker, 2014; Parker, 2017). Our identification strategy relies on the fact that the last two digits of the recipient's Social Security number (SSN) determined the timing of payment. Previous papers use this strategy to estimate an impulse response function of consumption to the receipt of payment by comparing households a given number of weeks since receiving a stimulus payment with households that will receive payments later. By contrast, our work additionally exploits variation in waiting times across households.

The results show that consumption responds strongly to the receipt of additional income, with a magnitude that depends on the duration of anticipation. First, consistent with previous work, we find no evidence that households increase spending in advance of receiving their stimulus payment. Second, the additional consumption expenditure in the month after receiving payment is largest for households in the earliest payment group and drops by about 30 percent for each additional week that a household waits for their payment. Third, earlier disbursement of stimulus payments leads to a continuing shift in spending behavior: In a given calendar week, spending among households in earlier payment groups exceeds that of households in later payment groups who have received their payment more recently. These patterns emerge for households with different levels of liquidity, financial planning tendencies, and savings habits. Additional tests rule out explanations relying on a direct relationship between waiting time and spending needs or saving ability as well as those relying on intrahousehold or social interactions.

In the next pair of settings, we present new analyses of raw data from published RCTs. The first is an impact evaluation of unconditional cash transfers by a non-governmental organization (GiveDirectly) using a sample of households in Rarieda, Kenya (Haushofer and Shapiro, 2016). The second is a windfall experiment in partnership with a commercial bank (NBS Bank) using a sample of households in villages near Mulanje, Malawi to understand how households manage cash without formal financial products (Brune et al., 2017).

The Kenya study contains a set of treatments to compare lump-sum payments with a series of nine monthly installments. To facilitate that comparison, the lump-sum transfers take place

[2] Appendix A.1 documents that, to the best of our knowledge, other settings with consumption data lack exogenous variation in the timing of information.

at randomly selected but pre-announced times within nine months of enrollment in the program. This previously unexploited random variation in the timing of lump-sum transfers thus provides an ideal experiment for evaluating the role of anticipation. Households with the shortest waiting times exhibit the lowest levels of savings and investments in response to the transfer payments.

The Malawi study contains payment-delay treatments to understand whether time preferences provide scope for financial products such as savings defaults to improve welfare. While the authors find little evidence that delaying payments affects expenditure, our analysis leads to new conclusions. In particular, we find a significant increase in savings in response to receiving a delayed windfall payment. Effects are largely driven by *in-kind savings*, a common form of savings that is incidentally included in measures of expenditure.

A model based on mental accounting explains the four key patterns in our data. We observe (i) a lack of consumption response to information about future payments (excess smoothness) and (ii) a strong response to receiving anticipated payments (excess sensitivity), including among households that do not face binding liquidity constraints, consistent with households treating windfall income differently from their current wealth (Shefrin and Thaler, 1988). In addition, the data show excess anticipation-dependence: (iii) differential consumption response to additional income based on the duration of anticipation, and (iv) greater spending among households in earlier payment groups conditional on calendar week. To describe how consumers mentally categorize windfalls, we incorporate anticipation-dependence into the behavioral life-cycle model of Shefrin and Thaler (1988). Shefrin and Thaler (1988) describe wealth as separated into three mental accounts (current income, current assets, and future income) each having a different MPC, with larger windfalls feeling more "wealth-like" and with a tendency to "leave perceived 'wealth' alone." We highlight how the duration of anticipation also plays an important role in determining how wealth-like a windfall feels to consumers, such that the time dimension matters beyond the classification of income as "future" vs. "current" (or "anticipated" vs. "unanticipated"). As multiple possible channels can operate in conjunction with mental accounting to generate MPCs that exhibit this novel form of history dependence, we use a reduced-form approach (Mullainathan, Schwartzstein and Congdon, 2012) to model the dependence of the MPC on the time dimension, which allows us to characterize robust policy implications of our findings without heavily relying on a specific set of modeling assumptions.[3] The estimates of the model match not only the monthly and weekly spending moments in our data but also the MPCs reported in related work showing that one-time stimulus payments in 2008 boost spending by more than equivalent reductions in income tax withholding in 2009 (Sahm, Shapiro and Slemrod, 2012). We discuss implications for the design of fiscal stimulus policies, focusing on payment frequency, amounts, and targeting.

Our empirical results make several contributions to the extensive literature in household finance, public economics, and macroeconomics on tests of intertemporal consumption models.[4] First,

[3] We illustrate how our approach can capture channels involving reference dependence (Kőszegi and Rabin, 2009; Thakral and Tô, 2021), self-control (Noor, 2007), planning (Gollwitzer, 1993), attention (Bordalo, Gennaioli and Shleifer, 2020), utility from anticipation (Thakral and Tô, 2020), or uncertainty about optimal actions (Ilut and Valchev, forthcoming); see Appendix F.1.

[4] The most closely related papers in this literature to ours are those that use household-level data to estimate the

our work goes beyond the anticipated-unanticipated distinction by positing the importance of the duration over which an income shock is anticipated. Our approach is therefore able to reconcile seemingly conflicting results that consumption responds to anticipated payments in some settings but not others by emphasizing the timing of information and the time horizon over which households anticipate changes in income (see Appendix A.2 for a meta-analysis of consumption responses to "anticipated" payments, which shows greater deviations from consumption smoothing for payments following a shorter anticipation duration). Second, we introduce the notion of excess anticipation-dependence, which distinguishes forward-looking consumption theories from alternatives that incorporate backward-looking elements. Third, we build on existing empirical work methodologically by using a two-step estimation approach.[5] Finally, our findings point toward a novel role for the timing of information in designing effective stabilization policies.

In addition, our paper contributes to existing work that estimates MPCs. Existing explanations for MPC heterogeneity fall into two broad classes (Gelman, 2021): temporary circumstances (e.g., income shocks, liquidity constraints) and persistent characteristics (e.g., impatience, limited attention).[6] Our results suggest that characterizing MPCs requires an additional state variable: the time elapsed since receiving information. Previous research uses hypothetical survey (e.g., Shapiro and Slemrod, 1995, 2003; Jappelli and Pistaferri, 2014; Christelis et al., 2019; Fuster, Kaplan and Zafar, 2021), quasi-experimental (e.g., Parker, 1999; Souleles, 1999, 2002; Johnson, Parker and Souleles, 2006), and structural (e.g., Blundell, Pistaferri and Preston, 2008) approaches to estimate MPCs. The resulting MPCs constitute sufficient statistics for partial equilibrium analysis of fiscal policy (Kaplan and Violante, 2014) and monetary policy (Auclert, 2019). Our work fits within the quasi-experimental approach but provides evidence on *intertemporal* MPCs, which characterize general equilibrium responses to fiscal shocks (Auclert, Rognlie and Straub, 2018) and monetary policy (Wolf, 2021). This also relates to a broader literature in macroeconomics establishing the importance of current consumption responses to future shocks for the effectiveness of fiscal policy (Hagedorn, Manovskii and Mitman, 2019) and monetary policy (Kaplan, Moll and Violante, 2018).[7]

This paper also relates to an expansive body of research in development economics on cash transfers as a tool for alleviating poverty (Hanlon, Barrientos and Hulme, 2012). A systematic review of experiments on cash transfers yields a long list of design features (Bastagli et al., 2016): complementary interventions, conditionality, duration, frequency, main recipient, predictability and reliability, size, and scheduling transfer payments at critical times.[8] We propose a new design

consumption impacts of stimulus payments (e.g., Johnson, Parker and Souleles, 2006; Parker et al., 2013).

[5] Gardner (2021) also proposes this methodology in independent and contemporaneous work.

[6] Lewis, Melcangi and Pilossoph (2021) show that observables such as income, homeownership, age, education, and family status explain less than one-quarter of the variation in MPCs, suggesting an important role for latent characteristics.

[7] See Fagereng, Holm and Natvik (2021) for further references and discussion on how the "dynamics of households' consumption responses to windfall income are essential to address longstanding macroeconomic questions about shock propagation and economic policy."

[8] The notion of timing studied in this literature refers to making funds available to households at specific instances when needs arise, such as the time to pay school fees or to acquire agricultural inputs (Duflo, Kremer and Robinson, 2011).

feature—waiting times—and evaluate its impact in multiple settings.[9] A unique aspect of our work is the use of existing data from published studies that aim to answer a different set of research questions from our paper.

Our work also has significant implications for models of mental accounting. Existing theories leave unresolved the question of how consumers allocate funds to different mental accounts and whether the time dimension matters beyond "future income" and "current income." Correspondingly, research on how consumption responds to changes in income treats anticipated and unanticipated changes as dichotomous (Jappelli and Pistaferri, 2010). Our results shed light on the dynamics of the mental-accounting process by which consumers classify additional income differently based on its source. Theoretical models of mental accounting (Galperti, 2019; Kőszegi and Matějka, 2020; Lian, 2021) shed light on *uses* of income in the form of budgets (e.g., for different goods, categories of goods, or total expenditure); however, these models cannot explain how consumers distinguish between different *sources* of income. Existing empirical models of mental accounting capture the intuition behind violations of fungibility in classifying funds based on their uses ("gas money" in Hastings and Shapiro 2013) or sources ("food money" in Hastings and Shapiro 2018) in static environments but do not consider how consumers set or revise their categorizations. We complement the existing literature by incorporating dynamics and thus enriching the description of the mental-accounting process. Our work also complements lab experiments demonstrating that decision makers exercise some discretion in assigning expenses to different mental accounts, i.e., that mental accounts can be flexible (Soman and Cheema, 2001; Soman and Gourville, 2001; Cheema and Soman, 2006), by contributing policy-relevant evidence of flexibility in how decision makers classify additional income.

Our approach has several notable advantages. First, we make the conceptual contribution of highlighting the connection between disparate areas of work that study fiscal policy in high-income countries and cash transfer programs in developing countries (also see Egger et al. forthcoming) and analyzing them in parallel. Second, analyzing experimental as well as naturally occurring variation in policy-relevant contexts provides greater generalizability (Rodrik, 2009) and makes progress toward mitigating concerns regarding the external validity of laboratory experiments and RCTs (Duflo, Glennerster and Kremer, 2007). Third, and relatedly, our approach exemplifies how experiments can yield more general lessons when combined with theory (Banerjee, 2005; Mookherjee, 2005; Duflo, Glennerster and Kremer, 2007; Deaton and Cartwright, 2018). Fourth, while many studies on MPCs and mental accounting use hypothetical surveys (Shefrin and Thaler, 1988; Fuster, Kaplan and Zafar, 2021), research in psychology suggests that survey-based approaches may fail to capture the effects we study since "mimicry of the passage of time is extremely difficult to accomplish

[9] Several experiments analyze the effect of waiting times in one-time decisions among small, well-defined choice sets involving monetary amounts, specific consumption goods, or effort allocations at different time periods (Dai and Fishbach, 2013; DeJarnette, 2020; Imas, Kuhn and Mironova, forthcoming). These studies find that waiting before making a decision causes subjects to prefer larger delayed payoffs over smaller sooner payoffs. Our analysis of waiting times in the case of cash transfers poses the conceptually distinct question of how receiving a payment with a longer or shorter anticipated delay affects spending and saving decisions.

in a questionnaire" (Arkes et al., 1994).[10] Fifth, relying on strong existing institutions to generate credible variation in payment timing as opposed to running new experiments limits the possible influence of a lack of trust, which can distort conclusions resulting from variation in payment timing (Beam et al., 2022). Finally, the use of existing experiments reduces unintended researcher bias that may arise in designing or implementing new experiments (Rosenthal and Fode, 1963).

The paper proceeds as follows. Section 2 presents an organizing framework for our empirical analyses. Section 3 analyzes consumer responses to the timing of the 2008 Economic Stimulus Payments in the US. Section 4 and Section 5 analyze the timing of payments in cash transfer experiments in Kenya and Malawi, respectively. Section 6 presents and estimates a mental-accounting model to interpret our results and discuss their implications. Section 7 concludes.

2 Framework for interpreting consumption patterns

We summarize predictions about how consumption responds to anticipated transitory income changes from a simple benchmark model in which agents maximize discounted expected utility subject to an intertemporal budget constraint (Deaton, 1992). We introduce the notion of anticipation-dependence, discuss the predictions of alternative models, and provide implications for theory.

2.1 Predictions of benchmark model

The benchmark model captures the basic intuition of the life-cycle and permanent income hypothesis (Modigliani and Brumberg, 1954; Friedman, 1957) that consumption responds little, if at all, when an anticipated income change occurs. Households choose consumption to equate the marginal utility of present consumption with the expected marginal utility of future consumption, given the information they have available (see Appendix B). Any change in the marginal utility of consumption from one period to the next must therefore result from new information. In particular, past information cannot predict changes in marginal utility: Consumers incorporate expectations of income changes in their optimal consumption plans as soon as they learn about such changes, so the marginal utility of consumption does not change when predictable changes in income occur. The benchmark model thus makes a clear prediction: Upon learning about a transitory income shock, consumption changes immediately and remains constant thereafter, with individuals consuming only the annuity value of the income shock (Jappelli and Pistaferri, 2010, 2017). This has straightforward implications for how consumption responds before the shock, at the time of the shock, and after the shock, which we discuss in turn below.[11]

[10]As Arkes et al. (1994) elaborate, "we tried to manipulate anticipation in a questionnaire. This was a very difficult thing to do. We found ourselves writing questionnaires for the anticipated group that contained lines like, 'Two months pass during which you anticipate your rebate check.' While subjects are reading that sentence, only 5 s pass, not 2 months. The point is that the mimicry of the passage of time is extremely difficult to accomplish in a questionnaire study. The actual passage of time may be necessary for anticipated funds to be 'worked into' an account, thereby making them less spendable.... Our recommendation is that "real-time" studies rather than questionnaire studies may be the best way to test the role of anticipation in the spending of windfall gains."

[11]Also see Appendix B for statements of these predictions using the recent framework of intertemporal marginal propensities to consume.

Consumption response to information about future income changes: The benchmark model implies an immediate consumption change upon learning about the shock. The literature uses the term excess smoothness (Deaton, 1987; West, 1988; Campbell and Deaton, 1989) to describe the pattern that consumption responds too little to new information. In fact, recent work using individual-level data provides evidence of no spending response to information about future income changes (McDowall, 2019; Fuster, Kaplan and Zafar, 2021).

Consumption smoothing upon arrival of anticipated income: The benchmark model predicts that consumption does not change from the period preceding the shock to the period when the shock occurs. The literature uses the term excess sensitivity (Hall, 1978; Flavin, 1981) to describe the pattern that consumption responds too much to the arrival of anticipated income. Detailed data from financial accounts provides evidence that households, including those with high levels of liquidity, respond to predictable income changes (Kueng, 2018; Olafsson and Pagel, 2018; Ganong and Noel, 2019; McDowall, 2019; Baugh et al., 2021).

Consumption response after arrival of anticipated income: The benchmark model predicts that the marginal propensity to consume out of the shock in the period of the shock and the periods after the shock does not depend on when the shock occurs. We use the term *excess anticipation-dependence* to describe a failure of this prediction, which is the main subject of this paper. Several factors help explain why this prediction, despite its simplicity, does not receive attention in previous work.[12] First, a large class of alternative models, including models that accommodate excess smoothness and excess sensitivity, also rule out non-trivial levels of anticipation-dependence (see Section 2.2 for further discussion). Second, testing this prediction imposes more demanding data requirements than testing for excess smoothness or excess sensitivity. In particular, a test would require exogenous variation in the duration between when a consumer learns about an income shock and when the shock occurs.

2.2 Implications for theory

A variety of models provide possible explanations for excess smoothness and excess sensitivity. This includes models that incorporate liquidity constraints or buffer-stock savings (Hayashi, 1985; Zeldes, 1989; Aiyagari, 1994; Deaton, 1991; Carroll, 1997), rule-of-thumb behavior (Campbell and Mankiw, 1989), or wealthy hand-to-mouth agents (Kaplan and Violante, 2014).

However, these models do not provide an explanation for excess anticipation-dependence. The benchmark prediction continues to hold under models in which no anticipatory spending takes place (e.g., due to binding liquidity constraints or rule-of-thumb behavior) as long as consumption decisions are forward-looking; such models predict that consumers respond to changes anticipated over any duration the same as they would to an unexpected income change. A similar prediction

[12]As Jappelli and Pistaferri (2017, p. 149) note in their textbook when discussing excess sensitivity, "Another factor that is potentially relevant but neglected in the literature is the time that elapses between the announcement and the actual income change."

holds under forward-looking models in which anticipatory spending changes the amount available for consumers to spend once the additional income arrives (e.g., due to buffer-stock behavior or wealthy hand-to-mouth agents); such models predict that anticipation duration matters only through its effect on anticipatory spending.

Testing for excess anticipation-dependence provides a way to differentiate between classes of consumption models. A key feature of the models discussed above is that consumption decisions are forward-looking (Browning and Lusardi, 1996).[13] Excess anticipation-dependence, by contrast, requires a theory of consumption that incorporates backward-looking elements; we return to this point in Section 6.

We perform two types of empirical exercises to test for anticipation-dependence. First, we conduct a systematic survey of the literature, resulting in three settings with the exogenous variation in payment timing necessary to estimate anticipation-dependence (see Appendix A.1), which we analyze in Sections 3 to 5. Second, we present results from a meta-analysis of the literature to examine how MPCs vary with the time horizon over which households anticipate receiving a payment (see Appendix A.2).

3 Tax rebates in the US

This section analyzes our first empirical setting: the natural experiment provided by the randomized payment of the 2008 tax rebate (Parker et al., 2013; Broda and Parker, 2014; Parker, 2017).

3.1 Setting

In response to the start of the recession in December 2007, the U.S. federal government approved an economic stimulus package in February 2008. All households with positive net income tax liability or at least $3,000 of qualifying income (Social Security, Veterans Affairs, or Railroad Retirement benefits) in 2007 were eligible for the Economic Stimulus Payments (ESPs).

In total, about 130 million U.S. tax filers received approximately $100 billion in tax rebates. Eligible taxpayers received a base payment of $600 ($1,200 for couples filing jointly) if their 2007 federal income tax liability exceeded that amount. Those with tax liabilities between $300 and $600 ($600 and $1,200 for couples) received a base payment equal to their tax liability, and those with tax liabilities of less than $300 ($600 for couples filing jointly) received a base payment of $300 ($600 for couples). Households received an additional $300 for each child that qualified for the Child Tax Credit in 2007. Payments were reduced by 5 percent of the amount by which adjusted gross income exceeded $75,000 ($150,000 for couples).

Payment dates followed a pre-announced timeline. The Internal Revenue Service (IRS) announced a disbursement schedule on March 17, with the earliest payments scheduled for the first week of

[13] This also applies to many models incorporating persistent household behavioral characteristics, such as models of time-inconsistent preferences (Laibson, 1997; Angeletos et al., 2001), temptation (Gul and Pesendorfer, 2004; Bucciol, 2012), and reference dependence (Kőszegi and Rabin, 2009; Pagel, 2017).

May and further batches of payments scheduled for the following weeks. Appendix Table 3 shows the ESP disbursement schedule for on-time filers.[14] Although the payment schedule and amounts were known in advance, households received notification letters from the IRS several days prior to their payment date. Payment dates were staggered because of the infeasibility of mailing all notification letters at the same time. The last two digits of a taxpayer's Social Security Number (SSN), which are effectively randomly assigned, determined their scheduled payment date.[15] On April 25, President Bush stated that the Treasury would start distributing stimulus payments several days earlier than expected.

The 2008 ESPs were the first large tax rebate to use electronic funds transfers (EFTs). About 80 million individual income tax returns were filed electronically in 2007, and tax filers who had provided the IRS with a personal bank account number for their income tax refunds received ESPs through direct deposit into their bank accounts. For tax returns that either provided no bank information or a tax preparer's bank information (e.g., due to a refund anticipation loan, or due to using the refund amount to pay tax preparation fees), the IRS sent paper checks in the mail.

3.2 Data

A multi-wave survey designed by Broda and Parker (2014) provides information about stimulus payments linked with detailed consumer expenditure data from the Nielsen Consumer Panel (NCP, formerly Homescan Consumer Panel).

The NCP data contain information on household demographics (e.g., household size and composition, income, and race) as well as daily spending of about 60,000 active households collected electronically from handheld barcode scanners. NCP households track spending on household items that primarily fall in the grocery, drugstore, and mass-merchandise sectors (see Broda and Weinstein 2010 for additional information), accounting for approximately 30 percent of household expenditure (Coibion, Gorodnichenko and Koustas, 2021). The spending data are aggregated to a weekly level to line up with the frequency of ESP disbursement.

The survey asks households whether they received a tax rebate via direct deposit or check, the dollar amount, the month and day they received their payment, and several questions related to general household financial planning. About 48,000 households provided responses to the survey, of which about 39,000 report receiving a stimulus payment. Among these, Broda and Parker (2014) note that some households do not report a payment date, report a payment date outside the randomized disbursement period, or provide inconsistent responses across multiple waves of the survey. Removing such observations, the remaining sample consists of about 29,000 households. We obtain the same analysis sample thanks to the replication files provided by Parker (2017). We further restrict the sample to households that report receiving a stimulus payment of at least $300.

[14]See Martinez, Meier and Sprenger (2017) for evidence that the 2008 ESPs induce earlier filing in a sample of low-income tax filers.

[15]SSNs assigned prior to June 25, 2011 consist of an area number (first three digits), a group number (middle two digits), and a sequentially assigned serial number (last four digits). The serial number is assigned sequentially within each group.

We interpret our results as internally valid estimates for the subsample of NCP panelists or the population that they represent (Bronnenberg et al., 2015).

To examine the consistency of payment dates in our sample with the randomization, we test whether households receiving ESPs at different times have similar characteristics in Appendix C.1. The sample of households receiving ESPs by direct deposit appears to be randomly distributed across the scheduled payment dates in the first three weeks of May (Appendix Table 4). However, among the sample of households receiving ESPs by paper check, our balance tests (Appendix Table 5) reveal systematic differences by payment date across a wide range of characteristics including rebate amounts (see Appendix C.1 for further discussion). Our analysis therefore focuses on the sample of households receiving payments by direct deposit.

3.3 Estimation

3.3.1 Methodology

The goal of this section is to develop an econometric framework for investigating the relationship between anticipation duration and expenditures induced by the tax rebate.

To facilitate the exposition, we begin by describing our empirical strategy as applied to the standard question in this literature: estimating the impulse response function of consumption to the receipt of payment. This allows for a test of excess smoothness and excess sensitivity as described in Section 2. Credible identification hinges on the presence of not-yet-treated units for constructing counterfactuals: Under random assignment of treatment timing, causal estimates obtain from comparing households a given number of weeks since receiving a stimulus payment with households that will receive payments later. Our analysis therefore focuses primarily on shorter-term impacts.

We use a two-step estimation approach. First we estimate time and household fixed effects independently of the causal effect of treatment by using only pre-treatment data. Then we estimate dynamic treatment effects—i.e., the impact on spending k periods after receiving an ESP for $k \geq 0$—after partialling out the estimated time and household fixed effects.[16]

Formally, denote by E_i the time period of the event that i becomes treated, and define $K_{it} = t - E_i$ to be time relative to treatment. Let Θ be a set of time-invariant household characteristics, and let Y_{it} denote an outcome at time t for household i with time-invariant characteristics $\Theta_i \subset \Theta$.

The first step consists of a regression of the outcome Y_{it} on group-specific time effects $\beta_{\theta t}$ using pre-treatment data:

$$Y_{it} = \alpha_i + \sum_{\theta \in \Theta_i} \beta_{\theta t} + \nu_{it}, \quad \{i, t : K_{it} < -\underline{k}\} \tag{1}$$

where α_i are household fixed effects and $\beta_{\theta t}$ are characteristic-specific time trends. Note that we also exclude data within $\underline{k}$ periods from the treatment date to avoid estimating possible changes in outcomes resulting from the upcoming treatment.

[16] See Gardner (2021) who also proposes this methodology, as well as Borusyak, Jaravel and Spiess (2022) who characterizes its efficiency properties.

In the second step, we model

$$Y_{it} = \widehat{\alpha_i} + \sum_{\theta \in \Theta_i} \widehat{\beta_{\theta t}} + \sum_{k=-\tilde{k}}^{\overline{k}} \gamma_k \mathbb{1}_{\{K_{it}=k\}} + \varepsilon_{it}, \tag{2}$$

where $\widehat{\alpha_i}$ and $\widehat{\beta_{\theta t}}$ are the estimated parameters from Equation (1), γ_k is the effect of treatment k periods after being treated, $\tilde{k}$ is the number of periods of pre-rebate treatment effects to estimate, and $\overline{k}$ is the number of periods of post-treatment effects. We define the cumulative spending impact over a t-week period as $\Gamma_t := \sum_{k=0}^{t-1} \gamma_k$ for $t \geq 1$. Note that the set of post-treatment effects that can be causally identified (i.e., for which $\widehat{\beta_{\theta t}}$ exists to construct a counterfactual) corresponds to periods $\{0, \dots, \max_i E_i - \min_i E_i - \underline{k} - 1\}$. We use a block-bootstrap procedure to compute standard errors adjusted for clustering at the household level.

We proceed to adapt this framework to test whether spending responses vary based on when households receive payments relative to when they are informed. This corresponds to testing for excess anticipation-dependence from the framework in Section 2. Since households in our data receive payments according to a pre-announced disbursement schedule, variation in anticipation duration reduces to variation in treatment time. We therefore incorporate heterogeneous treatment effects as follows by modifying the second step in our estimation:

$$Y_{it} = \alpha_i + \sum_{\theta \in \Theta_i} \beta_{\theta t} + \nu_{it}, \quad \{i, t : K_{it} < -\underline{k}\} \tag{3}$$

$$Y_{it} = \widehat{\alpha_i} + \sum_{\theta \in \Theta_i} \widehat{\beta_{\theta t}} + \sum_{k=0}^{\overline{k}} \gamma_k^{E_i} \mathbb{1}_{\{K_{it}=k\}} + \varepsilon_{it}. \tag{4}$$

The parameter γ_k^τ represents the causal impact of receiving a rebate k periods ago among households treated in period τ. Analogous to before, we define $\Gamma_t^\tau := \sum_{k=0}^{t-1} \gamma_k^\tau$ for $t \geq 1$. To understand whether households receiving rebate payments sooner after the announcement exhibit higher spending responses ($\Gamma_k^\tau > \Gamma_k^{\tau'}$ for $\tau < \tau'$), we test the null hypothesis of no anticipation-dependence.[17]

3.3.2 Assumptions

Operationalizing the two-step econometric procedure from Section 3.3.1 requires making assumptions such as how spending would have evolved over time for treated households in the absence of the stimulus payment. For our main results, the treatment group consists of households that report receiving a stimulus payment by direct deposit within two days of the scheduled payment date, and the comparison group consists of all households that report receiving a stimulus payment within the disbursement period associated with their reported payment method (direct deposit or paper check) as in Broda and Parker (2014) and Parker (2017). We make the following assumptions in estimating Equation (1). First, to determine the counterfactual time trend for spending, the set of

[17] In general, evaluating excess anticipation-dependence requires accounting for anticipatory spending, as Section 2.2 points out. In this setting, however, the null hypothesis of no anticipation-dependence provides the appropriate benchmark because the data show no anticipatory spending (see Section 3.5.1).

characteristics Θ consists of income groups (less than \$15,000; \$15,000–\$30,000; \$30,000–\$50,000; \$50,000–\$70,000; \$70,000–\$100,000; over \$100,000) and deciles of average expenditure by household size in the first quarter of 2008. Second, receiving a rebate check does not affect household spending two weeks in advance ($\underline{k} = 1$). Section 3.4.3 shows that our results are not sensitive to any of the above assumptions.

3.4 Impact of stimulus payments on spending

3.4.1 Average spending impacts

Before presenting our main results on the timing of payments, we discuss the average impact of receiving a stimulus payment on spending as a benchmark. This corresponds to estimating the Γ_t parameters derived from Equation (2). To put the cumulative spending impacts into perspective, note that the NCP data comprise about 30 percent of household expenditure (Coibion, Gorodnichenko and Koustas, 2021), and the average ESP for direct deposit households is about \$1,000.

We find broadly similar magnitudes to those in Broda and Parker (2014) when estimating Equation (2) for three subsamples of EFT households: our main estimation sample consisting of households receiving EFTs near the scheduled payment date, the subset of households receiving EFTs exactly on the scheduled payment date, and all other households that report receiving EFTs. Across these subsamples, our point estimates for Γ_1 range from \$6.67 to \$11.24, and our point estimates for Γ_4 range from \$24.98 to \$44.04, as shown in Figure 1 and Appendix Table 6; we also find insignificant spending responses in the second month after payment receipt, with point estimates for $\Gamma_8 - \Gamma_4$ ranging from $-$\$12.06 to \$11.59.[18] Consistent with their results, we find no spending response in weeks prior to receiving payment.

3.4.2 Impact of timing of stimulus payments

We proceed to test whether households exhibit greater spending responses to payments that arrive earlier. Thus we estimate Equation (4) and test whether the cumulative 4-week spending impacts Γ_4^w vary across groups. Households received EFTs during the 18th, 19th, and 20th weeks of the year, which we denote as periods $w = 1$, $w = 2$, and $w = 3$, respectively (Appendix Table 3). These dates correspond to 6, 7, and 8 weeks after the original IRS announcement, but using the IRS announcement as a point of reference likely understates the extent to which the payments come as a surprise to the first group, especially in light of President Bush's April 25 announcement that the payments would begin sooner than originally stated.

The data show a clear pattern of lower spending impacts for households that wait longer to receive their payments. Figure 2 summarizes our main results for various samples of households.[19]

[18] In estimating the impact of ESPs on spending in the week of receiving payment (Γ_1), Broda and Parker (2014) report point estimates ranging from \$12.8 to \$13.8. They obtain point estimates of the four-week or one-month cumulative increase in spending (Γ_4) ranging from \$27.9 to \$47.6. They also report an insignificant average increase in spending of \$9.3 one month later ($\Gamma_8 - \Gamma_4$) in their preferred specification.

[19] Appendix Figure 4 displays cumulative spending effects during the four weeks following ESP receipt. Also see Appendix Table 7 for the main results in the form of a table.

The left panel displays estimates of Γ_4^w for households receiving payments in different weeks, as well as p-values from testing the null hypotheses that $\Gamma_4^1 = \Gamma_4^2 = \Gamma_4^3$, while the right panel displays the confidence interval for the difference in spending between the first and last groups.

We begin by discussing the full sample of households receiving EFTs near the scheduled payment date. Among households randomly assigned to receive payments in the first week, we estimate a \$65.25 increase in spending during the four weeks after receiving the ESP, about twice as large as the increase in spending for the average household. The monthly spending impact for a household receiving payment in the first week is similar in magnitude to combining the impact on a household receiving payment one week later (\$45.24) with the impact on a household receiving payment two weeks later (\$18.73). This difference in MPCs (about 0.22 for the first group, 0.15 for the second group, and 0.06 for the third group) suggests an important role for the timing of payments in designing effective fiscal stimulus. The remaining rows of Figure 2 examine subsamples based on survey responses to questions pertaining to liquid assets and behaviors related to financial planning and spending as explored by Parker (2017).

To investigate the importance of liquidity, we divide the sample into two groups based on whether the household reports having at least two months of income available in cash, bank accounts, or easily accessible funds in case of an unexpected decline in income or increase in expenses, and we reestimate Equation (1) and Equation (4). Parker (2017) reports point estimates of the marginal propensity to consume NCP goods in the four weeks following ESP receipt ranging from 2.04 to 2.08 percent for households with sufficient liquid wealth and 4.87 to 6.57 percent for households without sufficient liquid wealth. Consistent with these findings as well as other prior literature (Zeldes, 1989; Johnson, Parker and Souleles, 2006; Agarwal, Liu and Souleles, 2007), the results in the second and third rows of Figure 2 show higher spending responses among households without liquidity. In addition, we find significant heterogeneity based on the timing of payment for both constrained and unconstrained households. Among households receiving payments in the third week, we find a spending response of close to zero for those with sufficient liquidity. Randomly assigning more liquid households to receive payments at the beginning of the disbursement period leads to substantial increases in spending of about \$50 over the four weeks after receiving their ESP. We find a similar effect size for the subset of liquidity-constrained households that have to wait until the third week of the disbursement period to receive their payments. Our estimates thus imply an effect of waiting times large enough to close the gap in spending responses between households with and without sufficient liquid wealth.

We next examine heterogeneity in ESP spending responses by financial planning tendencies. We divide households into two groups based on whether they report reviewing their household's financial information in the last few years and formulating a financial plan for their long-term future. Intuitively, we might expect households that formulate consumption plans to exhibit lower propensities to spend out of windfalls (Reis, 2006). Indeed Parker (2017) finds a negative relationship between financial planning and ESP spending responses, and we find a similar relationship on average; however, households that make financial plans and receive ESPs in the first week exhibit the largest spending responses (\$74.58 for planners compared to \$58.06 for non-planners). The finding that the largest spending responses come from households that engage in financial planning highlights the importance of timing and suggests that models in which planning tendencies generically correlate with higher savings may not provide a complete explanation for the evidence.

The last pair of rows in Figure 2 separately consider households that characterize themselves as spending types and saving types, a measure of impatience.[20] We find, consistent with the results in Parker (2017), that more patient households spend less in response to the ESPs. Moreover, self-reported spending and saving types both exhibit stronger responses to payments that arrive earlier. The consistency across these groups corroborates the notion that more time to anticipate future consumption impacts intertemporal decision-making through channels distinct from impatience.

In addition to analyzing spending responses across households with different self-reported financial circumstances, we estimate heterogeneity in spending impacts by objective household characteristics. The relationship between anticipation duration and spending responses persists for households receiving different rebate amounts (see the MPC estimates in Appendix Figure 5 and Appendix Table 8, and see Appendix C.2 for details on estimation). The same pattern also emerges for high- and low-expenditure households as well as high- and low-income households (Appendix Figure 6 and Appendix Table 9).

3.4.3 Robustness

This section explores the sensitivity of our results to the assumptions for determining the counterfactual spending trend in Equation (1), the comparison group of not-yet-treated households, and alternative sample restrictions.

We begin by considering alternative sets of characteristics in the first step of the estimation (Panel A of Figure 3 and Appendix Table 10). In our baseline specification, these characteristics include deciles of pre-rebate average expenditure and six income categories. Removing the income categories from the set Θ does not change the magnitudes of the estimated ESP spending impacts. Instead removing the expenditure deciles leads to slightly smaller estimates, though the differences across households receiving ESPs in different weeks remains equally substantial. The same holds if we remove both sets of characteristics and include only household fixed effects and period fixed effects. Allowing for differential spending trends based on the rebate amount leads to similar magnitudes as our main specification, as does replacing contemporaneous income with lagged values of income (for which the data contain much fewer missing values). Omitting household fixed effects leads to somewhat larger estimates.

We next consider alternative sets of comparison households (Panel B of Figure 3 and Appendix Table 10). The baseline specification uses all households that receive ESPs within the disbursement period associated with their reported payment method to estimate counterfactual

[20] The survey question asks, "In general, are you or other household members the sort of people who would rather spend your money and enjoy it today or save more for the future?" As Parker (2017) notes, the phrasing attempts to elicit a stable household characteristic, though a household's spending response to the stimulus payments could potentially affect this measure.

spending, using only data from at least two weeks before their reported payment weeks. Excluding one, two, or three additional weeks of data preceding ESP receipt slightly increases our estimates of the spending impacts. We also examine the sensitivity of our estimates to alternative specifications of the set of comparison households. Restricting the set of comparison households to only those receiving paper checks, or further restricting to those that receive paper checks near the scheduled payment dates, leads to similar estimates of the ESP spending impacts. We obtain slightly larger point estimates if we use comparison households receiving paper checks in July to ensure that the composition of households used to estimate each of the week fixed effects in Equation (1) remains stable. In our main specification as well as each of these alternative specifications, we find no significant spending responses in the weeks prior to receiving the ESP, providing evidence to support the validity of the estimated counterfactual spending trend (Appendix Table 11).

Lastly, we examine how our estimates change under different sample restrictions (Panel C of Figure 3 and Appendix Table 10). Excluding households that report no spending for a consecutive four-week period does not change the magnitudes of our estimates. Restricting the sample of direct deposit households to those that report receiving their ESP on the exact day specified by the disbursement schedule also leads to similar point estimates.

3.5 Alternative explanations

The fact that liquidity constrained and unconstrained households exhibit similar patterns suggests that households treat windfall income differently from their current wealth, which we explore in more detail in Section 6. In the rest of this section, we assess the plausibility of various alternative explanations for the results. The alternatives naturally fall into two groups: threats to establishing that a longer anticipation duration leads to lower ESP spending, and other reasons why households that face longer waiting times would spend less. In addressing some of these alternatives, we present additional evidence that spending in a given calendar week among households in earlier payment groups significantly exceeds that of households in later payment groups who have received their payment more recently (Figure 4).

3.5.1 Anticipatory spending

Smaller spending responses among households that wait longer before receiving payments may arise if more time allows households to spend more of their ESPs in advance. However, our data show no significant differences in spending prior to ESP receipt, with the total spending response in the month before receiving an ESP ranging from -\$8.10 to \$2.43 across the various specifications in Appendix Table 11.[21]

A simple calibration exercise corroborates our interpretation of the results as evidence of anticipation-dependence in excess of what standard models predict. Explaining the difference in spending we observe between the first and last payment groups would require an average excess

[21] Using daily-level data on 17.2 million households from a large U.S. financial institution, McDowall (2019) also finds highly precise and insignificant anticipatory spending responses in response to tax refunds.

spending prior to ESP receipt of \$3.32 per day (\$46.52 over 14 days) or about \$100 in monthly *NCP* spending. This is over five times as large as the *total* anticipatory spending response implied by the Kaplan and Violante (2014) model, which implies a 6 percent marginal propensity to consume one quarter in advance of receiving a \$500 tax rebate (see their Table IV), and more than an order of magnitude larger than the anticipatory spending response we observe in our data.

3.5.2 Borrowing, debt, and non-Nielsen spending

As our consumption data consist primarily of spending on household items, changes in other forms of spending could potentially occur. Although our data show no evidence of additional spending in advance, households might either increase debt payments or increase non-NCP consumption (e.g., by borrowing, assuming that households have access to credit or are more likely to have access to credit with more time). The former possibility appears inconsistent with previous work on the 2001 and 2008 tax rebates (Agarwal, Liu and Souleles, 2007; Bertrand and Morse, 2009) documenting increases in debt payments upon *receiving* ESPs as opposed to in advance, while evidence on responses to state tax rebates from the Consumer Expenditure Survey (Heim, 2007) rejects the latter. In addition, the Broda and Parker (2014) survey data show no significant difference across payment groups in whether the tax rebate leads households to mostly pay off debt.

Alternatively, we might also observe a relationship between anticipation duration and spending responses if longer waiting times simply lead to a compositional shift toward non-NCP expenditures. The question on self-reported ESP spending from the Broda and Parker (2014) survey provides evidence against this concern. The survey asks households to think about the "extra amount" they are spending because of the tax rebate and report how much of the additional spending falls in the following categories: household products, entertainment, durable goods, clothing, and other. Interpreting these data may present some difficulties because they reflect a combination of spending responses and households' awareness of their spending responses. With this caveat in mind, we find that households in later payment groups do not report higher ESP spending on average than households in earlier payment groups. This holds across each of the five categories of spending, including durables. In total, compared to households receiving ESPs in the first week of May, those receiving ESPs in the second week report spending \$5 to \$45 less and those receiving ESPs in the third week report spending \$35 to \$64 less.

3.5.3 Time effects

Evaluating the role of anticipation duration requires exogenous variation in when households learn about a windfall payment relative to when they receive it. In the context of the 2008 stimulus payments, since households receive information about payments at the same time, the duration of anticipation does not vary independently of calendar time. This could pose a concern if variation in the MPC arises either due to generic week-of-month effects or due to factors specific to the EFT disbursement period.

If the marginal propensity to consume varies over the course of a month with fluctuations in cash on hand, we might expect to find larger spending responses in weeks when households must make rent payments or pay other bills, which tend to occur at the beginning of the month. On the other hand, we might expect to find smaller spending responses in weeks when households receive paychecks, which tends to push in the opposite direction. For a household making rent payments at the beginning of the month and receiving weekly paychecks, this would plausibly lead to larger spending responses to payments received in the first week of May and similar (smaller) responses to payments received in later weeks.[22] We do not find any evidence of larger consumption responses to payments received at the beginning of the month for households receiving ESPs in June and July.[23] The finding that households with different levels of income and liquidity exhibit similar patterns further limits the plausibility of explanations relying on week-of-month effects such as interactions with the paycheck cycle.

Next we address the possibility of variation in MPCs arising due to calendar-time effects specific to the EFT disbursement period. In particular, new information over time about the severity of the financial crisis could lead to smaller absolute spending responses for households in later payment groups. We would expect this channel to be particularly relevant for states that experience higher levels of job loss during the recession. However, when analyzing differences in spending responses across states, we find that those in the top and bottom quartiles of the distribution exhibit similarly strong MPC reductions in response to longer waiting times (Appendix Figure 7). For a more direct test of the relevance of such calendar-time effects, we hold fixed calendar time and analyze differences in spending behavior across groups (to compare households when they would have access to the same information). Comparing the spending response of households in different payment groups *within the same calendar week*, those receiving EFT payments in later weeks spend significantly *less* on average per week in the four weeks after receiving payment compared to those in earlier payment groups (Figure 4). This occurs despite the fact that households in earlier payment groups would have less of their ESPs remaining to spend.

3.5.4 Other mechanisms for anticipation duration to affect spending

We also consider individual- and group-level channels through which greater anticipation could potentially affect spending. Longer waiting times may make it possible for consumers to find ways to save or to find other ways to spend the money. Intrahousehold or social interactions could also potentially explain why anticipation duration matters. We elaborate on why channels based on external commitments, myopia, intertemporal consumption complementarities, rational inattention,

[22] Similarly, for households receiving biweekly paychecks, we would expect a non-monotonic pattern, with the largest response to receiving payments in the second week, and the smallest response to receiving payments in the third week. For households receiving monthly paychecks, we would expect to find larger responses to ESPs received in later weeks of the month.

[23] The spending response among households receiving payments via paper check (i.e., 3–11 weeks after the April 25 announcement that the stimulus payments would begin sooner than originally stated) is flat or decreasing in time since the announcement, depending on the specification. As a caveat, note that this test does not use the ideal source of random variation in payment dates; see Appendix Table 5 and the discussion in Section 3.2.

and rational illiquidity do not suffice to explain the key patterns in our data in Appendix F.2.

The evidence that liquidity unconstrained households exhibit the same effect (Figure 2) suggests that explanations based on waiting times enabling consumers to find ways to save or other external commitments cannot fully explain the patterns in the data. If the effect arises because the passage of time allows households to accumulate or remember expenses that would dampen their spending response (e.g., having more time for long-term needs to arise, having more time to remember high-value investments), then we would expect to find that households in earlier payment groups spend no more than households in later payment groups when holding the calendar week fixed, contrary to the evidence in Figure 4. The results replicate for single individuals, couples, households with and without children (Appendix Figure 8), suggesting that the effects do not reflect specific forms of intrahousehold decision-making. Finally, if the patterns arise from households observing and learning from others' behavior or receiving external advice as time passes, we again would not expect to see the findings in Figure 4.

4 Cash transfers in Kenya

This section analyzes our second empirical setting: an impact evaluation of unconditional cash transfers from the non-profit organization GiveDirectly, which delivers tens of millions of dollars in donations each year via the mobile-phone-based payment service M-Pesa to impoverished households.

4.1 Setting and data

Haushofer and Shapiro (2016) conduct an RCT to evaluate the impacts of unconditional cash transfers by GiveDirectly in rural Kenya from June 2011 to January 2013 on a wide range of outcomes including assets and consumption. The participants consist of 1,008 households from 120 villages in the Rarieda province of Western Kenya who meet the simple means-test criterion of living in a home with a thatched roof. The villages with the highest proportion of thatched roofs in Rarieda were chosen for the study. The average village in the sample consists of 100 households.

The researchers randomized 503 households into treatment arms that vary by whether households receive KES 24,000 (USD 384 PPP) or KES 94,000 (USD 1,505 PPP).[24] Among the 366 households receiving the smaller amount, 193 households received one-time lump-sum transfers.[25] The magnitude of these one-time payments equates to about six months of revenue for the average household.

Households learned of the transfers during a visit from a GiveDirectly representative. During these visits, the representative announced the amount and timing of the payments. Households receiving one-time lump-sum transfers would receive their payment on the first day of a randomly selected month among the nine months following the date of the visit.[26] The outcome measures

[24] As in Haushofer and Shapiro (2016), we report all USD values at purchasing power parity using the World Bank PPP conversion factor of 62.44 KES/USD for private consumption in 2012. The transfer amounts roughly correspond to USD 300 nominal and USD 1,000 nominal.

[25] The remaining 173 households received monthly transfers over a nine month period. The 137 treated households receiving the larger amount received the bulk of their payments at a monthly frequency as well (see Appendix D.1).

[26] Households also received an initial transfer of KES 1,200 immediately following the announcement visit.

come from an endline survey which takes place about 14 months after the baseline survey, and our sample consists of 172 households.[27]

We use random variation in payment dates among households in the lump-sum treatment to estimate the impact of longer waiting times, which we define as more than $k \in \{2, \ldots, 8\}$ weeks from the date of the announcement visit. Since previous research using the GiveDirectly data does not utilize this source of variation in anticipation duration, we conduct balance tests before proceeding. Consistent with random assignment, household characteristics and baseline measures do not significantly differ across households experiencing different waiting times (Appendix Table 12 and Appendix Figure 11). While the Haushofer and Shapiro (2016) experimental design involves randomizing the timing of the lump-sum transfers to facilitate comparability with their monthly-transfer treatment, our paper uses a distinct, previously unexploited source of variation—experimentally induced random variation in the extent to which households anticipated their transfer payments—to examine how anticipation affects decision-making.

4.2 Estimation and results

To estimate the impact of shorter waiting times, we follow the econometric strategy in Haushofer and Shapiro (2016) by conditioning on baseline levels of the outcome variables to improve statistical power. Letting T^k_{vh} indicate a waiting time of $k \in \{2, \ldots, 8\}$ weeks or less since the announcement, we estimate

$$y^E_{vh} = \alpha_v + \beta_k T^k_{vh} + \gamma y^B_{vh} + \varepsilon_{vh}, \tag{5}$$

where y^t_{vh} represents the baseline ($t = B$) or endline ($t = E$) outcome of interest for household h in village v, α_v captures village-level fixed effects, T^k_{vh} indicates treatment with a shorter waiting time, and ε_{vh} is an idiosyncratic error term.[28] The parameter β_k represents the causal impact of a waiting time of k weeks or less relative to a longer waiting time. We test the null hypothesis of no anticipation-dependence from Section 2, which corresponds to $\beta_k = 0$.[29]

We consider four broad outcome measures: savings, assets, durables, and investments. The measure of savings consists of the total value of savings in all savings accounts, including M-Pesa. Assets consist of various types of livestock (cattle; small livestock such as pigs, sheep, and goats; birds such as chicken, turkeys, doves, and quails) and durables. Durables include furniture, agricultural tools, appliances, and other movable assets such as bicycles and cell phones. Investments consist of durable investment (durable assets and non-agricultural business investment in durables) and non-durable investment (agricultural inputs, enterprise expenses, educational expenses, and savings). We present all values in 2012 USD PPP. These measures from Haushofer and Shapiro (2016) capture

[27] The attrition and non-compliance rates in our sample are slightly lower than in the complete sample of 1,008 households. See Appendix D.1 for additional details on the samples.

[28] For the small set of outcomes with a few missing baseline measures, we encode missing values and control for an indicator M^B_{vh} for missing values: $y^E_{vh} = \alpha_v + \beta T_{vh} + \gamma y^B_{vh} + \delta M^B_{vh} + \varepsilon_{vh}$.

[29] Liquidity constraints play an important role in this setting (Haushofer and Shapiro, 2016). As we outline in Section 2, incorporating liquidity constraints into the benchmark models leads to excess smoothness and excess sensitivity but not excess anticipation-dependence. In addition, these data do not contain high-frequency measures of consumption as Section 3 does. Our analysis in this section thus focuses on excess anticipation-dependence.

outcomes at the time of the endline survey, unlike the results in Section 3.4 which constitute an impulse response of spending to windfalls.

Figure 5 displays the main results, which support the hypothesis that shorter waiting times, of a similar duration as in the setting from Section 3, lead to significant reductions in future-oriented decision-making. We present results under a variety of specifications, varying the definitions of the treatment group (shorter waiting times) and comparison group (longer waiting times). Each dot in the figure corresponds to an estimate of the treatment effect from Equation (5), with the associated 95 percent confidence interval shaded vertically, for a given definition of shorter and longer waiting times. We vary the definition of a shorter waiting time between 2 weeks and 8 weeks, and we vary the regression sample to include waiting times between 90 days and 270 days as the comparison group. For example, the first specification compares households receiving transfers within 14 days of the announcement date with households receiving transfers up to 90 days after the announcement date. We find substantial decreases in the probability of having nonzero savings among households randomly assigned to receive cash transfers sooner after the announcement visit. The decrease in savings does not arise due to substitution into other stores of value such as durables or other assets and investments. Households facing the shortest waiting times—those receiving transfers in the first month after the announcement—exhibit the strongest reductions in endline savings, assets, durables, and investments, on the order of about one to two months of average revenue.

We conduct two exercises to ensure that the estimates reflect the impact of differences in waiting times rather than differences in endline survey timing. First, we assess whether the treatment effect estimates change when comparing with a group of households for which the endline survey takes place several months earlier on average. As the various specifications in Figure 5 show, varying the range of waiting times in the comparison group between 3 months and 9 months does not affect our results. Second, we directly examine the shape of the relationship between waiting times and outcomes. If shorter waiting times lead to lower savings solely because households can experience a longer period of elevated consumption before the endline survey takes place, we would expect to see a linear relationship between anticipation duration and the various outcomes. A binned scatterplot of outcomes across the distribution of waiting times (Figure 6) instead shows that households facing the shortest waiting times exhibit especially strong reductions in endline savings, assets, durables, and investments, consistent with a substantive shift in decision-making.[30] We reject the null hypothesis of a linear relationship between waiting times and outcomes (Appendix Figure 12).

We obtain similar results under various alternative estimation approaches. Equation (5) uses an analysis of covariance (ANCOVA) approach (Frison and Pocock, 1992; McKenzie, 2012). We find similar impacts of short waiting times using differences-in-differences, defining the outcome variable as the difference between the endline and baseline measure (Appendix Figure 13). We also

[30]The binned scatterplots use the rule-of-thumb integrated-mean-square-error optimal estimator of the number of bins (Cattaneo et al., 2019); see Appendix Figure 9 for an analogous figure with 9 bins, one corresponding to each month of waiting time. All specifications contain controls for baseline outcomes and village fixed effects. Plotting the difference between endline and baseline outcomes gives the same pattern (Appendix Figure 10). Plotting only baseline outcomes provides evidence of balance (Appendix Figure 11).

obtain similar estimates when altering the ANCOVA approach by adding quadratic controls for baseline outcomes (Appendix Figure 14) or removing village fixed effects (Appendix Figure 15). We also document similar patterns for other outcomes variables: value of savings, durable investment, non-durable investment, and total assets including non-thatched roofs (Appendix Figure 16).

4.3 Alternative explanations

This section considers alternative individual- and group-level factors that may result in anticipation duration influencing savings and investment decisions.[31] First, consumers may find ways to save or to find other ways to spend the money with longer waiting times. Second, intrahousehold or social interactions may result in a role for waiting times.

To address the possibility that finding ways to save or spend as time passes may explain our results, we estimate the impact of short waiting time separately for households that report having no savings at baseline and those that report having no loans at baseline.[32] We investigate the importance of intrahousehold interactions by examining heterogeneity by the gender of the randomly assigned recipient of the transfer, household size, children, and marital status. To evaluate whether receiving external advice or demands from others or observing and learning from others' behavior as time passes might play a role, we re-estimate the model on the following subsamples: households that are net senders of remittances, villages in which an above-median fraction of treated households receive lump-sum transfers, villages in the bottom half of the distribution of the waiting time for the first transfer, and households that receive their lump-sum transfer before the median household in their village.

Estimates of the impact of waiting less than four weeks on savings, assets, durables, and investment for the subsamples described above appear in Figure 7. In each case, we obtain estimates of roughly the same magnitude as the estimates from the full sample, with none of the subsamples showing systematic differences relative to the full sample.

5 Cash transfers in Malawi

This section analyzes our final empirical setting: a field experiment in Malawi among several (orthogonal) interventions in partnership with the commercial bank NBS to encourage savings.

5.1 Setting and data

Brune et al. (2017) conduct an experiment to examine how formal financial products influence consumption decisions by making windfall payments to a sample of 474 randomly selected households living in villages within six kilometers of the NBS bank branch in Mulanje, Malawi. The researchers

[31] Section 3.5.4 discusses these channels in the context of our results on the 2008 stimulus payments in the US. The other possible explanations in Section 3.5 pertain to specific features of the tax-rebate setting.

[32] Moreover, if the effects were driven by having more time for long-term needs to arise, then we would expect the difference between 5 and 6 months of waiting to be the same as the difference between 1 and 2 months of waiting, but Figure 6 shows that the latter is much larger.

randomly vary whether households receive transfer payments of MK 25,000 (USD 176.50 PPP) via cash or direct deposit in March–April 2014.[33] The magnitude of the transfers equates to about four times the baseline average formal savings among households in the sample. The research team informs households during baseline surveying of their eligibility for a cash prize of up to MK 25,000 if they visit the branch exactly two days later, so households have some awareness of the scope of the transfers prior to the visit. During the in-person visit to the bank branch, households receive information about whether and when they will receive transfers.

Participants either receive payments immediately or with a delay, randomized independently of the main treatment arm (i.e., whether the household receives the transfer via cash or direct deposit). The stated goal of the payment delay was to "test the presence of time inconsistency" to shed light on the mechanisms through which formal bank accounts affect spending, though we discuss in Appendix F.3 how consumption responses to payment delays do not necessarily provide a test of time-inconsistent preferences or quasi-hyperbolic discounting.

In the experiment, 160 households receive payments after an eight-day delay, 158 households receive payments after a one-day delay, and the remaining 156 households receive payments immediately. This setting thus allows us to examine the effect of variation in anticipation duration both on a comparable scale to the variation in anticipation for which the settings in Sections 3 and 4 show effects, as well as on a significantly shorter time horizon that the previous settings did not allow us to examine.[34] Consistent with random assignment, baseline characteristics do not significantly differ among households receiving payments with different delays (Appendix Table 13).

We use the experimentally induced variation in payment delays to examine effects on expenditures and savings. All outcome measures derive from a survey containing questions based on Malawi's Third Integrated Household Survey (IHS-3), which each household completes one week after their transfer payment date. The survey includes an expenditure module and a savings module. Focusing on broad categories of expenditures (food, non-food, planned, and unplanned), Brune et al. (2017) find no substantial differences across treatment arms, with the exception of the longest payment delay leading to a significant reduction in unplanned food expenditures (see their Table A3). Our analysis of the data instead focuses on various forms of savings.

5.2 Estimation and results

To obtain the causal impact of anticipated payment delays, we estimate an analog of Equation (5) as in Brune et al. (2017):

$$y^E_{vwh} = \alpha_v + \beta_1 T^1_{vwh} + \beta_8 T^8_{vwh} + \gamma y^B_{vwh} + \delta_w + \varepsilon_{vwh}, \tag{6}$$

[33]We report USD values at purchasing power parity using the conversion factor 141.64 MK/USD as in Brune et al. (2017). The transfer amounts correspond to about USD 60 nominal.

[34]While we report estimates for each treatment arm, we note that Brune et al. (2017) caution that specifications separately estimating the impacts of different payment delays tend not to have enough power to detect small effects.

where y^t_{vwh} represents the baseline ($t = B$) or endline ($t = E$) outcome of interest for household h in village v surveyed in week w, α_v and δ_w capture village and week-of-first-survey fixed effects, T^k_{vwh} indicates treatment with a k-day payment delay, and ε_{vwh} is an idiosyncratic error term. The parameter β_k (for $k \in \{1, 8\}$) represents the causal impact of a k-day delay relative to an immediate windfall. We test the null hypothesis of no anticipation-dependence from Section 2, which corresponds to $\beta_1 = \beta_8 = 0$.[35]

The outcomes consist of various forms of savings. The main results, appearing in Table 1, show that an anticipated eight-day payment delay significantly increases total savings, and this increase arises largely due to *in-kind savings*. In-kind savings consist of advance purchases of farm inputs, business inventory, and bags of maize (see the questionnaire in Appendix Figure 17). The analysis in Brune et al. (2017), by contrast, focuses on expenditure rather than savings and finds little influence of payment delays. As a possible explanation for the discrepancy between the large impact on savings that we observe and the previous results on spending, note that the expenditure survey asks how much households *paid in total* for various consumption goods over the past seven days (Appendix Figure 18); these consumption goods include maize, which households also purchase as a form of in-kind savings.[36] We can rule out large effects on other forms of savings. This includes financial assets, which consist of both formal savings (accounts at NBS or other banks) and informal savings (village savings groups, ROSCAs, cash not for daily living expenses kept at home or in a secret hiding place). Overall the results support the hypothesis that waiting periods cause substantial shifts in household decision-making.

The estimates for eight-day delays corroborate the findings from Sections 3 and 4 that short delays on the order of a week can lead to greater savings. The Malawi setting further allows us to examine the impact of much shorter delays. For all forms of savings, we find insignificant effects of one-day delays in Table 1. While we can rule out large effects of very short delays on savings, the lack of precision presents difficulties in learning more about the shape of the relationship between savings and delay duration using these data.

5.3 Alternative explanations

This section follows Section 4.3 by considering alternative mechanisms that could potentially explain the relationship between waiting times and savings. We find similar point estimates for the impact of a delayed windfall for households receiving direct deposit payments into an account with the NBS Bank rather than cash (Appendix Table 14); this suggests that the results are not driven by waiting

[35] As in Section 4, we focus on anticipation-dependence since liquidity constraints predict excess smoothness and excess sensitivity but not excess anticipation-dependence, and the data do not contain high-frequency measures of consumption.

[36] The effect of payment delays on in-kind savings in Table 1 cannot arise due to maize consumption since the one kilogram of maize in Malawi costs less than 1 USD (Caracciolo et al., 2014). The original question from the Malawi IHS-3 questionnaire asks specifically about how much households *consume* ("food both eaten communally in the household and that eaten separately by individual household members") over the past seven days (Appendix Figure 19). See Browning, Crossley and Winter (2014) for a discussion of the well-known challenges of measuring household consumption using survey data.

times enabling households to find ways to save. Our results hold across married and unmarried households (Appendix Table 15) as well as large and small households (Appendix Table 16), suggesting that the mechanism does not rely on intrahousehold interactions. Finally, the relatively small share of treated households limits the scope for social interactions to provide a plausible explanation in this setting.

6 Mental accounting and timing of windfalls

This section presents a model that accounts for both the lack of anticipatory spending in our data (excess smoothness and excess sensitivity) and the evidence of excess anticipation-dependence. As Section 2 discusses, evidence of excess anticipation-dependence requires incorporating backward-looking elements in modeling consumption decisions.

The behavioral life-cycle model of Shefrin and Thaler (1988) provides a central explanation for excess smoothness and excess sensitivity of consumption by positing that consumers treat their current assets differently from windfall income, even if they do not face binding liquidity constraints.[37] We extend this model to describe how consumers categorize windfalls based on the duration of anticipation, i.e., the time elapsed since learning about the windfall. We discuss how other classes of models, including models based on external commitments, intertemporal consumption complementarities or habit formation, myopia, rational inattention, and rational illiquidity, do not predict the patterns in our data (see Appendix F.2 for more details).

We estimate the model using the weekly spending NCP data from Section 3. The estimated model fits the monthly and weekly spending moments in our data, including the fact that the passage of time leads to smaller decreases in spending among groups receiving payments sooner after learning about them (Figure 4). We show how the model also matches out-of-sample moments and discuss implications for the design of fiscal stimulus policies, focusing on payment frequency, amounts, and targeting.

6.1 Evidence for mental accounting

In our data, households—including those that do not face binding liquidity constraints—treat windfall income as separate from their current wealth, as in models based on mental accounting (Shefrin and Thaler, 1988). Mental accounting provides a broad description of how consumers mentally categorize windfalls, which can be shaped by phenomena such as self-control (Galperti, 2019), internal commitments (Bénabou and Tirole, 2004), goal setting (Koch and Nafziger, 2016), narrow bracketing (Lian, 2021), planning (Kőszegi and Rabin, 2009), and attention (Kőszegi and Matějka, 2020).

Within the literature on consumption smoothing, and more broadly in macroeconomics, various authors invoke mental accounting to explain otherwise puzzling phenomena. This includes the

[37]Consistent with prior work (e.g., McDowall, 2019; Ganong and Noel, 2020; Fuster, Kaplan and Zafar, 2021), our data show no evidence of anticipatory spending in response to a future income shock, including among households with sufficient liquidity.

relationship between liquid wealth and MPCs (Kueng, 2018; McDowall, 2019; Fuster, Kaplan and Zafar, 2021; Boutros, 2022), consumption smoothing in response to losses but not gains (Ganong et al., 2020; Baugh et al., 2021; Massenot, 2021), the relationship between wealth accumulation and capital gains (Fagereng et al., 2019), price stickiness (Angelis, 2021), and the co-holding of savings and debt (Gathergood and Olafsson, 2022).[38]

In our setting, mental accounting provides an explanation for the four key patterns in the data. First, the lack of anticipatory spending in response to information about a future income shock (excess smoothness) among households with liquid wealth suggests that households treat future income differently from current wealth. Second, the high MPC in response to the arrival of predictable windfall gains (excess sensitivity) suggests that households treat current assets differently from current income. These first two facts align closely with the idea from Shefrin and Thaler (1988) that households separate wealth into three mental accounts, each with a different MPC: current income (highest MPC), current assets, and future income (lowest MPC). Third, the differential consumption response to additional income based on the duration of anticipation (excess anticipation-dependence) suggests that households classify additional income based on the time when they learned about the change. Fourth, the larger decrease over time in the spending response among households that wait longer to receive their payments than among households that have already received their payments (Figure 4) suggests that the way households classify additional income depends on the interaction between payment timing and the (remaining) windfall amount. The latter two facts require a dependence of MPCs on payment magnitude and timing.

The intuition behind mental accounting suggests a role for both magnitude and timing. Shefrin and Thaler (1988), in their work on the behavioral life-cycle hypothesis, emphasize that households classify additional income based on *magnitude*: "People tend to consume from income and leave perceived 'wealth' alone. The larger is a windfall, the more wealth-like it becomes." Arkes et al. (1994), in the psychology literature, emphasize the role of *timing*: "unanticipated money may be in no account. Planning for its expenditure takes time. Until some reasonable target is decided upon, the money remains uncommitted and therefore available for extravagant, frivolous, or speculative use. When funds are anticipated, the budgeting process occurs before receipt of the funds. When the funds eventually arrive, they are not available to be spent on some whim." We introduce a simple model that incorporates both of these features and discusses how they interact.

The idea to incorporate the time dimension in modeling mental accounting is new and therefore warrants further discussion. Excess anticipation-dependence in consumption responses to windfalls, combined with a lack of anticipatory spending, necessitates a theory of consumption that incorporates backward-looking elements (see Section 2). We formalize several possible channels through which MPCs may exhibit such history dependence in conjunction with mental accounting in Appendix F.1. First, consumers may form reference-dependent consumption plans as in models of news utility (Kőszegi and Rabin, 2009). Second, temptation may exhibit a greater influence on decision making

[38] Massenot (2021); Lian (2022) also explain high MPCs for liquid consumers by invoking violations of fungibility as in models of mental accounting.

in the presence of sooner opportunities to consume (Noor, 2007; Fudenberg and Levine, 2012). Third, consumers may find deviating from long-held goals or internal commitments more costly (Gollwitzer, 1993, 2015). Fourth, decision makers may put more weight on consumption in response to more recent information that draws their attention (Bordalo, Gennaioli and Shleifer, 2020). Fifth, consumers may experience utility from anticipating future consumption, which results in more forward-looking behavior in response to longer waiting times (Thakral and Tô, 2020; Thakral, 2022). Sixth, the passage of time may also allow decision makers to learn about the optimal action (Ilut and Valchev, forthcoming). As our main conclusions do not rely on a specific interpretation of the relationship between MPCs and timing, we take a reduced-form approach (Mullainathan, Schwartzstein and Congdon, 2012) to model the dependence of the MPC on the time dimension.[39]

6.2 Descriptive model of mental accounting

We model the decision making of a consumer who learns of a windfall and processes it through three mental accounts: a current income account, an intermediate account, and a future income account. For simplicity, assume that the consumer has a positive MPC only for current income and narrowly brackets the windfall separately from other sources of income (Read, Loewenstein and Rabin, 1999). Information about a windfall of magnitude m arrives at time $t = 0$. Before the windfall arrives, the consumer thinks of it as future income. Once the windfall arrives, it enters a separate intermediate or windfall account. In each period t, consumers transfer a fraction $\mu(m, t)$ of the amount that remains in their windfall account to their current income or spending account.[40] If $\frac{\partial \mu}{\partial m} < 0$, then consumers treat smaller windfall amounts as current income to a greater extent than as wealth. If $\frac{\partial \mu}{\partial t} < 0$, then consumers treat windfalls that they learned about more recently as current income to a greater extent than as wealth.

The following expressions describe the model-implied spending out of a windfall of size $m = w_t$ that the consumer anticipates for t periods. Let

$$y_\tau = \mu(w_\tau, \tau) \cdot w_\tau \tag{7}$$

[39] Some attempts to incorporate backward-looking elements fail to account for the main patterns in our data; this includes explanations based on external commitments, myopia, intertemporal consumption complementarities, rational inattention, and rational illiquidity, as Appendix F.2 elaborates. We also note that purely forward-looking theories of consumption, such as models of discounted utility (Laibson, 1997) and temptation disutility (Gul and Pesendorfer, 2004), do not explain the excess anticipation-dependence and lack of anticipatory spending in our data; this also applies to models of expectations-based reference dependence (Kőszegi and Rabin, 2009), unless we additionally impose a mental accounting assumption. See Appendix F.3 for further details.

[40] We could equivalently interpret this as a model in which the consumer classifies the windfall as current income, but some fraction of the windfall gets encoded as wealth as time passes. This heuristic could arise, for instance, if the consumer maximizes in each period a Cobb-Douglas utility function where the expenditure shares depend on the magnitude and timing of the windfall, but the microfoundation of such a model remains a topic for future research. A recent paper by Boutros (2022) makes a similar modeling assumption: "the mental account for the shock is the residual saving after consuming out of the shock in the previous period, not the exogenous shock as in the first period."

denote windfall spending in period $\tau \geq t$. The amount

$$w_{t+k} = w_{t+k-1} - y_{t+k-1} \tag{8}$$

remains in the windfall account in period $t + k$ for $k > 0$. Our main specification assumes that households in the earliest payment group treat the payments as a surprise; in other words, Group 1 anticipates the windfall for 0 periods, Group 2 for 1 period, and Group 3 for 2 periods.[41]

6.3 Nonlinear least squares estimation

To estimate the model, we propose a simple functional form:

$$\mu(m, t) = \beta^m \alpha^t, \tag{9}$$

where β and α parameterize how the propensity to spend out of a windfall varies with the remaining windfall amount and the time duration since learning about the windfall, respectively. While we do not constrain the values of α or β in the estimation, note that if $\alpha, \beta \in (0, 1)$, then the consumer treats smaller windfalls and more recent windfalls as more spendable ($\frac{\partial \mu}{\partial m} < 0$ and $\frac{\partial \mu}{\partial t} < 0$). In addition, under this condition, actual windfall spending (y) will not exceed the amount that remains in the windfall account.

Equations (7) to (9) recursively define windfall spending in each period as a nonlinear function of parameters (α and β) and data (m and t).[42] We assume that idiosyncratic shocks may result in deviations between observed and predicted spending, so in the data we would observe $\widetilde{y_{it}} = y_{it} + \epsilon_{it}$, where $\epsilon_{it} \overset{\text{i.i.d.}}{\sim} \mathcal{N}(0, \sigma^2)$. We use nonlinear least squares to estimate the resulting specification.

Since estimating the model requires high-frequency consumption data, we use the weekly spending NCP data from Section 3. Assuming that non-windfall NCP spending equals pre-rebate average spending, we obtain a measure of total windfall spending (the outcome variable $\widetilde{y_{it}}$) by taking the difference between observed weekly NCP spending and pre-rebate average spending and then applying a scaling factor to convert from NCP windfall spending to total windfall spending. Our main specification uses a scaling factor of 3.33 since the NCP data account for approximately 30 percent of household spending (Coibion, Gorodnichenko and Koustas, 2021).

6.4 Model fit

6.4.1 Parameter estimates

We present estimates of the model in Table 2. In our preferred specification (Column 1, which uses a scaling factor of 3.33) we estimate a magnitude parameter of $\beta = 0.9984$ and a time parameter of $\alpha = 0.5789$. To interpret these magnitudes, note that increasing the size of a windfall by $100

[41]This is consistent with the modeling choice by Kaplan and Violante (2014), which they refer to as the "intermediate informational assumption:" All households learn about the rebate payments upon disbursement of the first set of payments. We obtain similar results under alternative specifications, as Section 6.4.3 shows.

[42]The resulting expression for y_τ has the form $\beta^m \alpha^t m \mathbb{1}_{\{\tau=t\}} + (\beta^{(1-\beta^m\alpha^t)m} \alpha^{t+1} (1 - \beta^m \alpha^t) m) \mathbb{1}_{\{\tau=t+1\}} + \cdots$.

reduces the marginal propensity to consume out of that windfall by 15 percent. We also calculate that the MPC decreases by the same amount from an additional one week of waiting as it would from increasing the size of the windfall by $340, a quantity we refer to as the *waiting equivalent*.[43] The bottom panel of the table shows how closely the estimated model matches the monthly spending moments in the U.S. data (see Figure 2).

6.4.2 Interaction between magnitude and duration

The estimated model also reproduces key features of the weekly spending data. For groups that face shorter waiting times, the estimated model predicts that spending remains somewhat elevated as time passes, as Appendix Table 18 documents. This pattern arises due to the interaction between magnitude and timing within the mental-accounting model: Since a shorter waiting time leads to a higher initial spending response, a smaller amount remains in the consumer's windfall account, and the higher MPC out of a smaller magnitude partly mitigates the MPC reduction in the subsequent period. This provides an explanation for the finding that spending among households receiving stimulus payments in the earliest payment group exceeds that of households receiving payments in later groups even when conditioning on calendar week (Figure 4).

6.4.3 Sensitivity analysis and heterogeneity

To assess the sensitivity of our results, we vary the scaling factor and the informational assumptions. Varying the scaling factor corresponds to making different assumptions about the fraction of spending that the NCP data account for. We consider a range between 1 and 10 to encompass our preferred specification and an alternative from Broda and Parker (2014); they propose to use the share of self-reported ESP spending on household goods (13.7 percent), which would imply a scaling factor of 7.3. Although larger scaling factors result in larger estimates of α, the value of the waiting equivalent remains relatively stable across specifications, as the various columns of Table 2 show. The preferred specification provides the closest fit for the monthly spending moments in the U.S. data, as the bottom panel shows. Varying the informational assumptions corresponds to shifting the number of periods that households anticipate receiving the windfall. In particular, we consider the possibility that households in the earliest payment group anticipate receiving the payment starting at the time of the original IRS announcement, which ignores President Bush's announcement soon before the payment dates that the Treasury would start distributing payments sooner than expected (see Section 3.4.2 for a reminder of the timeline). We find that the waiting equivalents remain stable under the alternative specification, though the baseline informational assumption provides a better fit for the monthly spending moments (see Appendix Table 17).

We also estimate the model separately for the subsamples in Figure 2 and document substantial heterogeneity in waiting equivalents. As Table 3 shows, for households that are liquidity constrained, those that do not make financial plans, and self-classified spenders, an additional week of waiting

[43] We calculate the waiting equivalent by setting $\alpha = \beta^w$ and solving for w.

time reduces the MPC by as much as an additional \$450 to \$750 in the size of the windfall. By contrast, the waiting equivalents for unconstrained households, financial planners, and self-classified savers range between \$150 and \$250.[44] Predicted spending estimates, shown in the bottom panel, generally follow the data reported in Figure 2 (and Appendix Table 7). According to the estimates, households that do and households that do not make financial plans exhibit similar spending responses after two weeks of waiting, as do households that classify themselves as savers or spenders, consistent with the data. Perhaps not surprisingly, our model tends to underpredict the spending response of liquidity-constrained households.

6.4.4 Consumption responses across settings

The parameter estimates enable us to calculate the predicted consumption response to income shocks anticipated over different horizons. To synthesize our results, we plot the model-implied MPC in response to windfalls of different magnitudes as a function of duration in Figure 8 and the total consumption response in Appendix Figure 20. These figures plot the response to a \$1,000 payment (approximately the average size of the U.S. tax rebate from Section 3), a \$404 payment (the size of the cash transfer in Kenya from Section 4), and a \$176.50 payment (the size of the windfall in Malawi from Section 5) over a range of waiting times less than one month to encompass the common domain across all three settings.

The data in each of these settings align with the predictions of the model. The four-week consumption responses estimated using the U.S. data for households receiving payments in the first, second, and third week of May appear alongside the model predictions in Appendix Figure 21. We also produce analogous figures depicting the total consumption response estimated using the Kenya data for households receiving payments less than one month after the announcement (Appendix Figure 22) and the one-week consumption responses estimated using the Malawi data for households receiving payments immediately or after waiting about one week (Appendix Figure 23), along with the predictions of the model.[45] In all cases, the confidence intervals overlap with the predictions of the model. This suggests that the consistency in findings across the three settings extends beyond the qualitative similarities. We note two caveats in applying our estimates to other settings. First, the simple model we estimate does not incorporate liquidity constraints, which surely plays an important role and may lead to higher MPCs and decreased sensitivity to anticipation duration. Second, we caution against extrapolating the estimates beyond the range of waiting times present in the U.S. data due to the functional form assumption in Equation (9).

[44] These characteristics (liquidity constraints, financial planning, and spender/saver tendency) are not highly correlated; their pairwise correlations are less than one-third.

[45] Since the Kenya data do not contain direct measures of the consumption response to receiving the payment, we use the difference between the windfall amount and the change in assets to proxy for consumption.

6.5 Policy implications

We discuss the implications of the model for fiscal stimulus design as well as broader implications for policy and welfare. We also discuss the 2020–2021 stimulus payments in Appendix C.4.

One-time payment vs. flow of payments: Our model provides a possible explanation for the greater effectiveness of one-time payments (e.g., stimulus check) over flows of payments (e.g., reductions in withholding). Sahm, Shapiro and Slemrod (2012) describe arguments from academics and policymakers suggesting that a series of small payments may induce greater spending.[46] Their survey evidence on the 2008 stimulus payments and the 2009 reduction in withholding in the US shows the opposite result, contrary to the prediction of a mental-accounting framework based on the idea of smaller MPCs from larger payment amounts. Our work helps to resolve this tension by pointing out the crucial role of anticipation and timing, which suggests that a lower spending response to a series of smaller payments may result from having more time to anticipate receiving those payments. Quantitatively, the estimated model matches the difference in MPCs between the one-time payment and the reduction in withholding implied by the data from Sahm, Shapiro and Slemrod (2012). Their survey contains data on the fraction of households that use the additional income to mostly spend, mostly save, or mostly pay off debt, and we apply three methods from the literature, following recent work by Feldman and Heffetz (2021), to convert these data to MPC estimates. These methods imply an MPC ranging from 0.22 to 0.41 for the reduction in withholding and ranging from 0.29 to 0.44 for the one-time payment. Despite the wide range across methods, all three approaches imply a difference in MPCs of only 0.03 to 0.07. Consistent with these data, when we model the reduction in withholding as a series of small windfalls of varying levels of anticipation, our estimates imply an MPC of 0.27 for the reduction in withholding compared to an MPC of 0.32 for the one-time payment (Table 4).

Consumption-maximizing payment size: With some caveats, the model also provides guidance on the optimal size of stimulus payments. On net, lower MPCs resulting from larger windfalls may decrease the total spending response. The model implies that the payment amounts that maximize spending are \$757, \$696, and \$664, respectively, for windfalls that arrive one, two, and three weeks after the announcement. For a windfall that arrives completely by surprise, the spending-maximizing amount implied by the model is \$872. These calculations abstract from differences in household characteristics, income, and other financial circumstances which would likely alter these conclusions.[47] In addition, the functional form we assume for μ may constrain substantively important interactions between the time and magnitude of payment, which future work with sufficiently detailed data can investigate using our approach.

[46] Feldman (2010), for example, documents an increase in consumption in response to the 1992 decrease in US federal income tax withholding, which shifts lump-sum tax-refund income to additional monthly income.

[47] The MPC may depend on the size of a windfall relative to income. See Kueng (2018) for evidence of higher MPCs among higher-income households in the context of the Alaska Permanent Fund Dividend payments.

Targeting: Our results echo previous work supporting the common practice of providing broad-based stimulus payments over more narrowly targeted payments to increase aggregate consumption (e.g., see McDowall 2019 and Andreolli and Surico 2021). The addition of the time dimension does not alter this conclusion since MPCs in our model decrease with windfall size for any given waiting time. Extensions of our methodology applied to larger datasets may provide further guidance on targeted payment amounts.

Value of faster or synchronized disbursement: The estimates imply a substantial value for investing in new technologies for the disbursement of fiscal stimulus payments. Consider a \$757 windfall that is anticipated for one week (i.e., the consumption-maximizing payment size). Our estimates imply that a completely unanticipated windfall of \$346 would increase aggregate consumption by the same amount. The model thus implies a willingness to pay for infrastructure to support unanticipated payments on the order of about half of the value of the payments themselves. Under the view that households learn about the rebate payments upon disbursement of the first set of payments (Kaplan and Violante, 2014), investing in solutions that synchronize payment timing provides a channel through which policymakers can implement such unanticipated payments.

Welfare: At an aggregate level, understanding how household spending responds to transitory variation in income at different time horizons (i.e., intertemporal MPCs) provides a crucial input for evaluating the macroeconomic impact of tax and labor-market policies and for designing effective stabilization policies (Auclert, Rognlie and Straub, 2018). At an individual level, failure to perfectly smooth consumption in response to small, transitory income fluctuations entails small utility costs and second-order welfare losses, as a large literature in macroeconomics notes (Akerlof and Yellen, 1985; Cochrane, 1989; Browning and Crossley, 2001). To facilitate a more detailed welfare analysis, we follow Farhi and Gabaix (2020) in computing the "behavioral wedge," a sufficient statistic for the welfare effects of marginal changes in consumption, in Appendix F.4.

7 Conclusion

We document a consistent set of new results across multiple settings using existing observational and experimental data. In the context of both developed and developing countries, additional time spent anticipating a windfall payment leads to lower consumption responses. This robust pattern of excess anticipation-dependence holds across consumers differing by levels of income, liquidity, access to formal financial products, demographic characteristics, and the magnitude of windfall payments. A meta-analysis of the literature on marginal propensities to consume in response to receiving anticipated payments also provides support for these results (Appendix A.2), showing comparably large MPC estimates for studies with the shortest waiting times.

The empirical results suggest a novel role for the timing of information in the design of tax and transfer programs. When policymakers intend to stimulate spending, as in the case of tax rebates, our results highlight the importance of rapid, or possibly synchronized, disbursement of payments.

To encourage longer-term investments, as policymakers may desire when delivering cash transfers to impoverished households, announcing and clearly advertising payments well in advance may lead to more future-oriented decision-making.

Testing the underlying properties of how consumption responds to transitory variation in income and making progress toward obtaining credible intertemporal MPC (iMPC) estimates has significant macroeconomic policy implications. As highlighted in the macroeconomics literature, iMPCs constitute sufficient statistics for general equilibrium responses to fiscal shocks and policies (Auclert, Rognlie and Straub, 2018). Moreover, the properties of iMPCs determine whether lump-sum transfer payments serve as a perfect substitute for conventional monetary policy (i.e., interest rate changes), which has particularly important policy implications when the effective lower bound on nominal interest rates binds (Wolf, 2021). Our finding that a longer anticipation duration can dampen spending suggests that responses to predictable increases in income may look meaningfully different from commonly used model-based extrapolations from the response to unexpected income changes. More broadly, our work highlights the importance of developing richer models of intertemporal preferences and incorporating them into macroeconomic analyses.

Understanding how spending responses vary with time to anticipate receiving a windfall can also inform the design of other public policies that involve payments anticipated over different time horizons. This includes policies such as universal basic income, automatic stabilizers, tax refunds, social security, and unemployment insurance, among many others that would have important welfare implications. Consequences for the structure of compensation within firms, such as bonuses and pay frequency, would also be valuable to explore in future research.

Anticipation likely also has implications for a broader set of outcomes beyond consumption and savings. Our study provides a path for future work in characterizing the effects of time-delayed transfers on a broad range of outcomes such as human capital acquisition, health-related decision making, labor productivity, risk-taking, and business investment. Designing experiments to study these outcomes in addition to consumption would be especially useful.

Finally, our work provides a step toward elucidating the dynamic elements underlying the broad set of phenomena that constitute mental accounting. Developing theoretical frameworks that capture these dynamics seems particularly promising.

References

Agarwal, Sumit, Chunlin Liu, and Nicholas S Souleles. 2007. "The reaction of consumer spending and debt to tax rebates—evidence from consumer credit data." *Journal of Political Economy*, 115(6): 986–1019.

Aiyagari, S Rao. 1994. "Uninsured idiosyncratic risk and aggregate saving." *The Quarterly Journal of Economics*, 109(3): 659–684.

Akerlof, George A, and Janet L Yellen. 1985. "A near-rational model of the business cycle, with wage and price inertia." *The Quarterly Journal of Economics*, 100(Supplement): 823–838.

Andreolli, Michele, and Paolo Surico. 2021. "Less is more: Consumer spending and the size of economic stimulus payments." *Mimeo.*

Angeletos, George-Marios, David Laibson, Andrea Repetto, Jeremy Tobacman, and Stephen Weinberg. 2001. "The hyperbolic consumption model: Calibration, simulation, and empirical evaluation." *Journal of Economic perspectives*, 15(3): 47–68.

Angelis, Georgios. 2021. "Price setting and price stickiness: A behavioral foundation of inaction bands."

Arkes, Hal R, Cynthia A Joyner, Mark V Pezzo, Jane Gradwohl Nash, Karen Siegel-Jacobs, and Eric Stone. 1994. "The Psychology of Windfall Gains." *Organizational Behavior and Human Decision Processes*, 59(3): 331–347.

Arnold, Catherine, Tim Conway, and Matthew Greenslade. 2011. "Cash transfers literature review." *London: Department for International Development.*

Auclert, Adrien. 2019. "Monetary policy and the redistribution channel." *American Economic Review*, 109(6): 2333–67.

Auclert, Adrien, Matthew Rognlie, and Ludwig Straub. 2018. "The intertemporal Keynesian cross." *Mimeo.*

Banerjee, Abhijit V. 2005. "'New Development Economics' and the challenge to theory." *Economic and Political Weekly*, 40(40): 4340–4344.

Bastagli, Francesca, Jessica Hagen-Zanker, Luke Harman, Valentina Barca, Georgina Sturge, Tanja Schmidt, and Lucca Pellerano. 2016. "Cash transfers: what does the evidence say? A rigorous review of programme impact and of the role of design and implementation features." Overseas Development Institute.

Baugh, Brian, Itzhak Ben-David, Hoonsuk Park, and Jonathan A Parker. 2021. "Asymmetric consumption smoothing." *American Economic Review*, 111(1): 192–230.

Beam, Emily A, Yusufcan Masatlioglu, Tara Watson, and Dean Yang. 2022. "Loss Aversion or Lack of Trust: Why Does Loss Framing Work to Encourage Preventative Health Behaviors?" National Bureau of Economic Research.

Bénabou, Roland, and Jean Tirole. 2004. "Willpower and personal rules." *Journal of Political Economy*, 112(4): 848–886.

Bertrand, Marianne, and Adair Morse. 2009. "What do high-interest borrowers do with their tax rebate?" *American Economic Review*, 99(2): 418–23.

Blundell, Richard, Luigi Pistaferri, and Ian Preston. 2008. "Consumption inequality and partial insurance." *American Economic Review*, 98(5): 1887–1921.

Bordalo, Pedro, Nicola Gennaioli, and Andrei Shleifer. 2020. "Memory, attention, and choice." *Quarterly Journal of Economics*, 135(3): 1399–1442.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess. 2022. "Revisiting event study designs: Robust and efficient estimation." *arXiv preprint arXiv:2108.12419.*

Boutros, Michael. 2022. "Windfall Income Shocks with Finite Planning Horizons."

Broda, Christian, and David E Weinstein. 2010. "Product creation and destruction: Evidence and price implications." *American Economic Review*, 100(3): 691–723.

Broda, Christian, and Jonathan A Parker. 2014. "The economic stimulus payments of 2008 and the aggregate demand for consumption." *Journal of Monetary Economics*, 68: S20–S36.

Bronnenberg, Bart J, Jean-Pierre Dubé, Matthew Gentzkow, and Jesse M Shapiro. 2015. "Do pharmacists buy Bayer? Informed shoppers and the brand premium." *The Quarterly*

Journal of Economics, 130(4): 1669–1726.

Browning, Martin, and Annamaria Lusardi. 1996. "Household saving: Micro theories and micro facts." *Journal of Economic literature*, 34(4): 1797–1855.

Browning, Martin, and Thomas F Crossley. 2001. "The life-cycle model of consumption and saving." *Journal of Economic Perspectives*, 15(3): 3–22.

Browning, Martin, Thomas F Crossley, and Joachim Winter. 2014. "The measurement of household consumption expenditures." *Annu. Rev. Econ.*, 6(1): 475–501.

Brune, Lasse, Xavier Giné, Jessica Goldberg, and Dean Yang. 2017. "Savings defaults and payment delays for cash transfers: Field experimental evidence from Malawi." *Journal of Development Economics*, 129: 1–13.

Bucciol, Alessandro. 2012. "Measuring self-control problems: A structural estimation." *Journal of the European Economic Association*, 10(5): 1084–1115.

Campbell, John, and Angus Deaton. 1989. "Why is consumption so smooth?" *The Review of Economic Studies*, 56(3): 357–373.

Campbell, John Y, and N Gregory Mankiw. 1989. "Consumption, income, and interest rates: Reinterpreting the time series evidence." *NBER macroeconomics annual*, 4: 185–216.

Caracciolo, Francesco, Luigi Cembalo, Alessia Lombardi, and Gary Thompson. 2014. "Distributional effects of maize price increases in Malawi." *Journal of Development Studies*, 50(2): 258–275.

Carroll, Christopher D. 1997. "Buffer-stock saving and the life cycle/permanent income hypothesis." *The Quarterly journal of economics*, 112(1): 1–55.

Cattaneo, Matias D, Richard K Crump, Max H Farrell, and Yingjie Feng. 2019. "On binscatter." *arXiv preprint arXiv:1902.09608.*

Cheema, Amar, and Dilip Soman. 2006. "Malleable mental accounting: The effect of flexibility on the justification of attractive spending and consumption decisions." *Journal of Consumer Psychology*, 16(1): 33–44.

Christelis, Dimitris, Dimitris Georgarakos, Tullio Jappelli, Luigi Pistaferri, and Maarten Van Rooij. 2019. "Asymmetric consumption effects of transitory income shocks." *The Economic Journal*, 129(622): 2322–2341.

Cochrane, John H. 1989. "The Sensitivity of Tests of the Intertemporal Allocation of Consumption to Near-Rational Alternatives." *The American Economic Review*, 319–337.

Coibion, Olivier, Yuriy Gorodnichenko, and Dmitri Koustas. 2021. "Consumption inequality and the frequency of purchases." *American Economic Journal: Macroeconomics*, 13(4): 449–82.

Coibion, Olivier, Yuriy Gorodnichenko, and Michael Weber. 2020. "How Did US Consumers Use Their Stimulus Payments?" *Mimeo.*

Dai, Xianchi, and Ayelet Fishbach. 2013. "When waiting to choose increases patience." *Organizational Behavior and Human Decision Processes*, 121(2): 256–266.

Deaton, Angus. 1987. "Life-Cycle Models of Consumption: Is the Evidence Consistent with the Theory?" In *Advances in Econometrics, Fifth World Congress.* Vol. 2, 121–148. Cambridge University Press.

Deaton, Angus. 1991. "Saving and liquidity constraints." *Econometrica*, 59(5): 1221–1248.

Deaton, Angus. 1992. *Understanding consumption.* Oxford University Press.

Deaton, Angus, and Nancy Cartwright. 2018. "Understanding and misunderstanding randomized controlled trials." *Social Science & Medicine*, 210: 2–21.

DeJarnette, Patrick. 2020. "Temptation over time: Delays help." *Journal of Economic Behavior & Organization*, 177: 752–761.

Duflo, Esther, Michael Kremer, and Jonathan Robinson. 2011. "Nudging farmers to use fertilizer: Theory and experimental evidence from Kenya." *American Economic Review*, 101(6): 2350–90.

Duflo, Esther, Rachel Glennerster, and Michael Kremer. 2007. "Using randomization in development economics research: A toolkit." *Handbook of development economics*, 4: 3895–3962.

Egger, Dennis, Johannes Haushofer, Edward Miguel, Paul Niehaus, and Michael W Walker. forthcoming. "General equilibrium effects of cash transfers: experimental evidence from Kenya." *Econometrica*.

Fagereng, Andreas, Martin B Holm, and Gisle J Natvik. 2021. "MPC heterogeneity and household balance sheets." *American Economic Journal: Macroeconomics*, 13(4): 1–54.

Fagereng, Andreas, Martin Blomhoff Holm, Benjamin Moll, and Gisle Natvik. 2019. "Saving behavior across the wealth distribution: The importance of capital gains." National Bureau of Economic Research.

Farhi, Emmanuel, and Xavier Gabaix. 2020. "Optimal taxation with behavioral agents." *American Economic Review*, 110(1): 298–336.

Feldman, Naomi, and Ori Heffetz. 2021. "A Grant to Every Citizen: Survey Evidence of the Impact of a Direct Government Payment in Israel." *Mimeo*.

Feldman, Naomi E. 2010. "Mental accounting effects of income tax shifting." *The Review of Economics and Statistics*, 92(1): 70–86.

Flavin, Marjorie A. 1981. "The adjustment of consumption to changing expectations about future income." *Journal of political economy*, 89(5): 974–1009.

Friedman, Milton. 1957. "The permanent income hypothesis." In *A theory of the consumption function*. 20–37. Princeton University Press.

Frison, Lars, and Stuart J Pocock. 1992. "Repeated measures in clinical trials: Analysis using mean summary statistics and its implications for design." *Statistics in medicine*, 11(13): 1685–1704.

Fudenberg, Drew, and David K Levine. 2012. "Timing and self-control." *Econometrica*, 80(1): 1–42.

Fuster, Andreas, Greg Kaplan, and Basit Zafar. 2021. "What would you do with $500? Spending responses to gains, losses, news, and loans." *The Review of Economic Studies*, 88(4): 1760–1795.

Galperti, Simone. 2019. "A theory of personal budgeting." *Theoretical Economics*, 14(1): 173–210.

Ganong, Peter, and Pascal Noel. 2019. "Consumer spending during unemployment: Positive and normative implications." *American economic review*, 109(7): 2383–2424.

Ganong, Peter, and Pascal Noel. 2020. "Liquidity versus wealth in household debt obligations: Evidence from housing policy in the great recession." *American Economic Review*, 110(10): 3100–3138.

Ganong, Peter, Damon Jones, Pascal Noel, Diana Farrell, Fiona Greig, and Chris Wheat. 2020. "Wealth, Race, and Consumption Smoothing of Typical Income Shocks." *Mimeo*.

Gardner, John. 2021. "Two-stage differences in differences." *Mimeo*.

Gathergood, John, and Arna Olafsson. 2022. "The Co-holding Puzzle: New Evidence from Transaction-Level Data." *Mimeo*.

Gelman, Michael. 2021. "What drives heterogeneity in the marginal propensity to consume? Temporary shocks vs persistent characteristics." *Journal of Monetary Economics*, 117: 521–542.

Gollwitzer, Peter M. 1993. "Goal achievement: The role of intentions." *European review of social psychology*, 4(1): 141–185.

Gollwitzer, Peter M. 2015. "Setting one's mind on action: Planning out goal striving in advance." *Emerging trends in the social and behavioral sciences: An interdisciplinary, searchable, and linkable resource*, 1–14.

Greenstone, Michael, and Adam Looney. 2012. "The Role of Fiscal Stimulus in the Ongoing Recovery." *The Hamilton Project, July*, 6.

Gul, Faruk, and Wolfgang Pesendorfer. 2004. "Self-control and the theory of consumption." *Econometrica*, 72(1): 119–158.

Hagedorn, Marcus, Iourii Manovskii, and Kurt Mitman. 2019. "The fiscal multiplier." National Bureau of Economic Research.

Hall, Robert E. 1978. "Stochastic implications of the life cycle-permanent income hypothesis: theory and evidence." *Journal of political economy*, 86(6): 971–987.

Handa, Sudhanshu, Luisa Natali, David Seidenfeld, Gelson Tembo, Benjamin Davis, and Zambia Cash Transfer Evaluation Study Team. 2018. "Can unconditional cash transfers raise long-term living standards? Evidence from Zambia." *Journal of Development Economics*, 133: 42–65.

Hanlon, Joseph, Armando Barrientos, and David Hulme. 2012. *Just give money to the poor: The development revolution from the global South.* Kumarian Press.

Hastings, Justine, and Jesse M Shapiro. 2018. "How are SNAP benefits spent? Evidence from a retail panel." *American Economic Review*, 108(12): 3493–3540.

Hastings, Justine S, and Jesse M Shapiro. 2013. "Fungibility and consumer choice: Evidence from commodity price shocks." *The quarterly journal of economics*, 128(4): 1449–1498.

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

Hayashi, Fumio. 1985. "The effect of liquidity constraints on consumption: a cross-sectional analysis." *The Quarterly Journal of Economics*, 100(1): 183–206.

Heim, Bradley T. 2007. "The effect of tax rebates on consumption expenditures: evidence from state tax rebates." *National Tax Journal*, 685–710.

Honorati, Maddalena, Ugo Gentilini, and Ruslan G Yemtsov. 2015. "The state of social safety nets 2015." The World Bank.

Ilut, Cosmin L, and Rosen Valchev. forthcoming. "Economic agents as imperfect problem solvers." *Quarterly Journal of Economics.*

Imas, Alex, Michael Kuhn, and Vera Mironova. forthcoming. "Waiting to Choose: The Role of Deliberation in Intertemporal Choice." *American Economic Journal: Microeconomics.*

Ingram, George, and John McArthur. 2018. "From one to many: Cash transfer debates in ending extreme poverty." Brookings Institution.

Jappelli, Tullio, and Luigi Pistaferri. 2010. "The consumption response to income changes." *Annual Review of Economics*, 2(1): 479–506.

Jappelli, Tullio, and Luigi Pistaferri. 2014. "Fiscal policy and MPC heterogeneity." *American Economic Journal: Macroeconomics*, 6(4): 107–36.

Jappelli, Tullio, and Luigi Pistaferri. 2017. *The economics of consumption: theory and evidence.* Oxford University Press.

Johnson, David S, Jonathan A Parker, and Nicholas S Souleles. 2006. "Household expenditure and the income tax rebates of 2001." *American Economic Review*, 96(5): 1589–1610.

Kaplan, Greg, and Giovanni L Violante. 2014. "A model of the consumption response to fiscal stimulus payments." *Econometrica*, 82(4): 1199–1239.

Kaplan, Greg, Benjamin Moll, and Giovanni L Violante. 2018. "Monetary policy according to HANK." *American Economic Review*, 108(3): 697–743.

Koch, Alexander K, and Julia Nafziger. 2016. "Goals and bracketing under mental accounting." *Journal of Economic Theory*, 162: 305–351.

Kőszegi, Botond, and Filip Matějka. 2020. "Choice simplification: A theory of mental budgeting and naive diversification." *The Quarterly Journal of Economics*, 135(2): 1153–1207.

Kőszegi, Botond, and Matthew Rabin. 2009. "Reference-dependent consumption plans." *American Economic Review*, 99(3): 909–36.

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

Laibson, David. 1997. "Golden eggs and hyperbolic discounting." *Quarterly Journal of Economics*, 112(2): 443–478.

Lewis, Daniel, Davide Melcangi, and Laura Pilossoph. 2021. "Latent Heterogeneity in the Marginal Propensity to Consume." *Mimeo.*

Lian, Chen. 2021. "A theory of narrow thinking." *The Review of Economic Studies*, 88(5): 2344–2374.

Lian, Chen. 2022. "Mistakes in future consumption, high MPCs now." *Mimeo.*

Martinez, Seung-Keun, Stephan Meier, and Charles Sprenger. 2017. "Procrastination in the field: Evidence from tax filing." *Mimeo.*

Massenot, Baptiste. 2021. "Pain of Paying in Consumption-Saving Decisions." *Available at SSRN 3820097.*

McDowall, Robert A. 2019. "Consumption behavior across the distribution of liquid assets." *Mimeo.*

McKenzie, David. 2012. "Beyond baseline and follow-up: The case for more T in experiments." *Journal of Development Economics*, 99(2): 210–221.

Modigliani, Franco, and Richard Brumberg. 1954. "Utility analysis and the consumption function: An interpretation of cross-section data." *Post Keynesian Economics*, 1(1): 388–436.

Mookherjee, Dilip. 2005. "Is there too little theory in development economics today?" *Economic and Political Weekly*, 40(40): 4328–4333.

Mullainathan, Sendhil, Joshua Schwartzstein, and William J Congdon. 2012. "A reduced-form approach to behavioral public finance." *Annual Review of Economics*, 4(1): 511–540.

Noor, Jawwad. 2007. "Commitment and self-control." *Journal of Economic theory*, 135(1): 1–34.

Olafsson, Arna, and Michaela Pagel. 2018. "The liquid hand-to-mouth: Evidence from personal finance management software." *The Review of Financial Studies*, 31(11): 4398–4446.

Pagel, Michaela. 2017. "Expectations-based reference-dependent life-cycle consumption." *The Review of Economic Studies*, 84(2): 885–934.

Parker, Jonathan A. 1999. "The reaction of household consumption to predictable changes in social security taxes." *American Economic Review*, 89(4): 959–973.

Parker, Jonathan A. 2017. "Why Don't Households Smooth Consumption? Evidence from a $25 Million Experiment." *American Economic Journal: Macroeconomics*, 9(4): 153–83.

Parker, Jonathan A, and Nicholas S Souleles. 2019. "Reported effects versus revealed-preference estimates: Evidence from the propensity to spend tax rebates." *American Economic Review: Insights*, 1(3): 273–90.

Parker, Jonathan A, Nicholas S Souleles, David S Johnson, and Robert McClelland. 2013. "Consumer spending and the economic stimulus payments of 2008." *American Economic Review*, 103(6): 2530–53.

Read, Daniel, George Loewenstein, and Matthew Rabin. 1999. "Choice bracketing." *Journal of Risk and Uncertainty*, 19(1–3): 171–202.

Reis, Ricardo. 2006. "Inattentive consumers." *Journal of Monetary Economics*, 53(8): 1761–1800.

Rodrik, Dani. 2009. "The new development economics: we shall experiment, but how shall we learn?" In *What Works in Development? Thinking Big and Thinking Small.* Chapter 2, 24–47. Brookings Institution Press.

Rosenthal, Robert, and Kermit L Fode. 1963. "Psychology of the scientist: V. Three experiments in experimenter bias." *Psychological Reports*, 12(2): 491–511.

Sahm, Claudia R, Matthew D Shapiro, and Joel Slemrod. 2012. "Check in the mail or more in the paycheck: does the effectiveness of fiscal stimulus depend on how it is delivered?" *American Economic Journal: Economic Policy*, 4(3): 216–50.

Shapiro, Matthew D, and Joel Slemrod. 1995. "Consumer Response to the Timing of Income: Evidence from a Change in Tax Withholding." *American Economic Review*, 85(1): 274–283.

Shapiro, Matthew D, and Joel Slemrod. 2003. "Did the 2001 tax rebate stimulate spending? Evidence from taxpayer surveys." *Tax policy and the economy*, 17: 83–109.

Shefrin, Hersh M, and Richard H Thaler. 1988. "The behavioral life-cycle hypothesis." *Economic inquiry*, 26(4): 609–643.

Soman, Dilip, and Amar Cheema. 2001. "The effect of windfall gains on the sunk-cost effect." *Marketing Letters*, 12(1): 51–62.

Soman, Dilip, and John T Gourville. 2001. "Transaction decoupling: How price bundling affects the decision to consume." *Journal of marketing research*, 38(1): 30–44.

Souleles, Nicholas S. 1999. "The response of household consumption to income tax refunds." *American Economic Review*, 89(4): 947–958.

Souleles, Nicholas S. 2002. "Consumer response to the Reagan tax cuts." *Journal of Public Economics*, 85(1): 99–120.

Thakral, Neil. 2022. "Anticipatory Utility and Intertemporal Choice." *Mimeo.*

Thakral, Neil, and Linh T Tô. 2020. "Anticipation and Temptation." *Mimeo.*

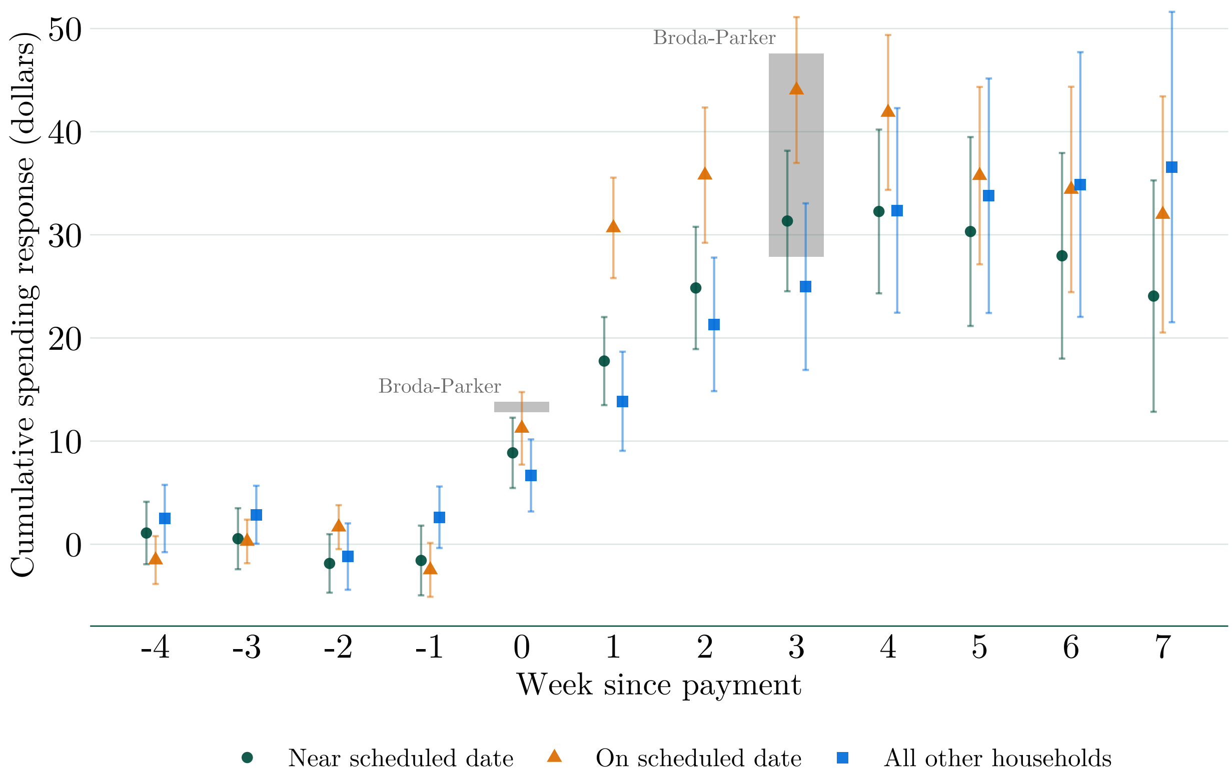
Thakral, Neil, and Linh T Tô. 2021. "Daily Labor Supply and Adaptive Reference Points." *American Economic Review*, 111(8): 2417–2143.

West, Kenneth D. 1988. "The insensitivity of consumption to news about income." *Journal of Monetary Economics*, 21(1): 17–33.

Wolf, Christian K. 2021. "Interest rate cuts vs. stimulus payments: An equivalence result." National Bureau of Economic Research.

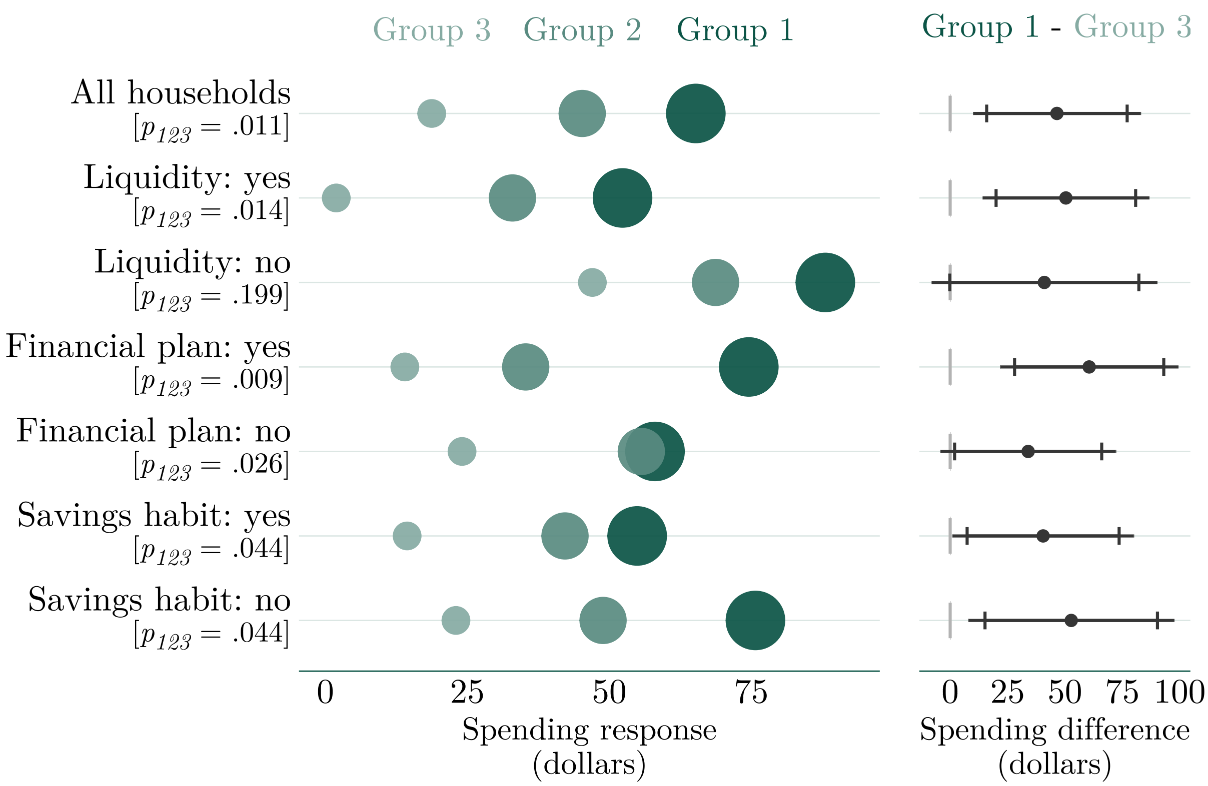
Zeldes, Stephen P. 1989. "Consumption and liquidity constraints: an empirical investigation." *Journal of Political Economy*, 97(2): 305–346.

Figure 1: ESP Spending Responses—Average Impacts (US)



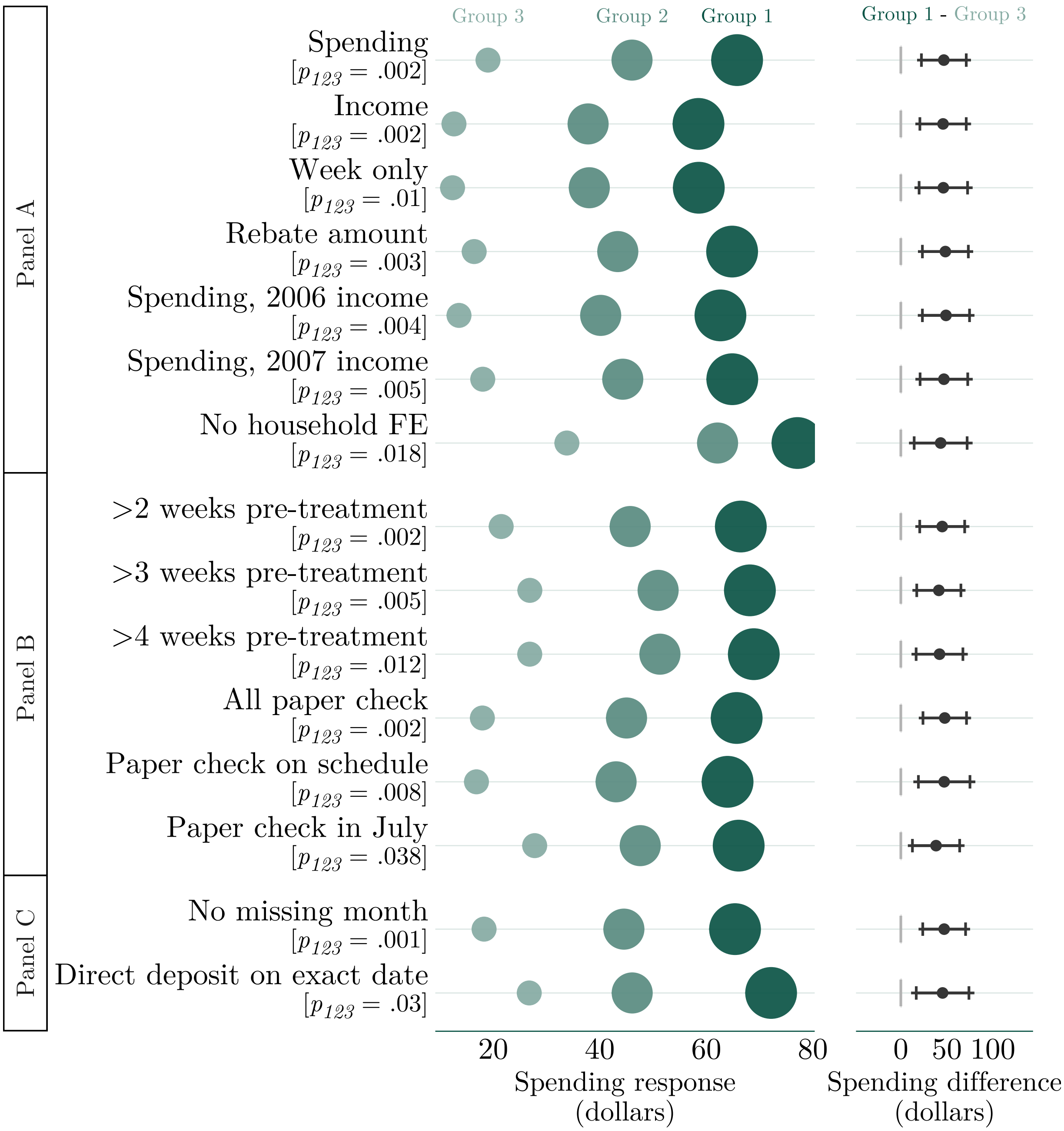
Note: This figure presents estimates of the weekly spending response γ_k (weeks -4 to -1) and the cumulative spending response Γ_k ($k = 1, \dots, 8$) from Equation (2) (using two-stage differences in differences) for various samples. For comparison, the shaded box denotes the range of point estimates for Γ_1 and Γ_4 (using two-way fixed effects) reported by Broda and Parker (2014). The "Near scheduled date" sample consists of households receiving direct deposits three days leading up to the scheduled payment date or the weekend after. The "On scheduled date" sample consists of households receiving direct deposits on the date specified in Appendix Table 3. The "All other households" sample consists of all other households receiving direct deposits. Standard errors reported in parentheses are adjusted for clustering at the household level and obtained from a block-bootstrap procedure with 100 replicates. Calculated based on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business.

Figure 2: ESP Spending Responses by Timing of Payment (US)



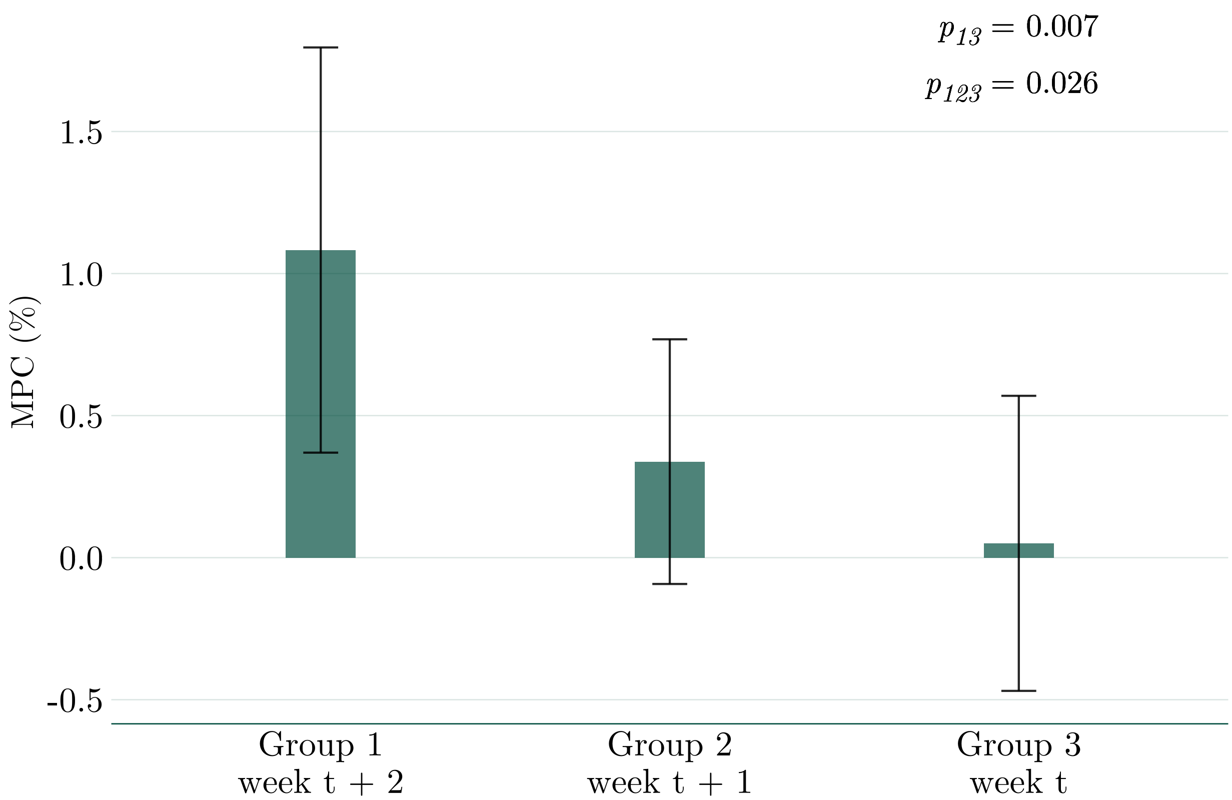
Note: The panel on the left presents estimates from Equation (4) of the four-week cumulative ESP spending response Γ_4^w for households receiving EFTs in the first (Group 1, large-size dot, $w = 1$), second (Group 2, medium-size dot, $w = 2$), and third (Group 3, small-size dot, $w = 3$) week of May, respectively, and the p-value labeled p_{123} corresponds to the null hypothesis of equality across groups. The panel on the right displays the difference in spending between Group 1 and Group 3, along with a 95 percent confidence interval (black line) and 90 percent confidence interval (vertical endpoints). Liquidity is an indicator for reporting that the household has at least two months of income available in easily accessible funds. Financial plan is an indicator for reporting that the household has gathered together its financial information, reviewed it in detail, and formulated a financial plan for the long-term future. Savings habit is an indicator for reporting that household members would rather save more for the future than spend their money and enjoy it today. Standard errors reported in parentheses are adjusted for clustering at the household level and obtained from a block-bootstrap procedure with 100 replicates. Calculated based on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business.

Figure 3: ESP Spending Responses by Timing of Payment—Alternative Specifications (US)



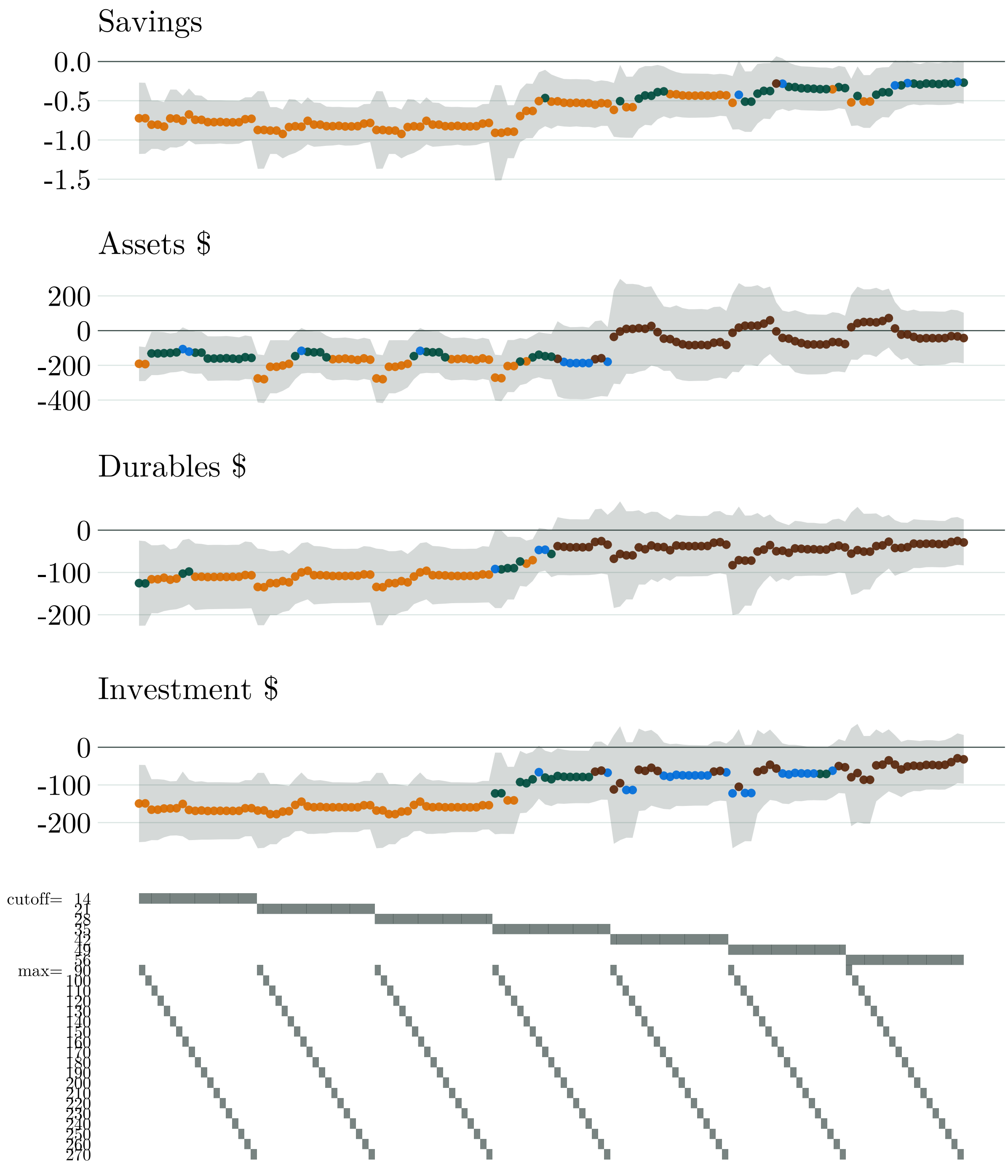
Note: The panel on the left presents estimates from alternative specifications of Equation (4) of the four-week cumulative ESP spending response Γ_4^w for households receiving EFTs in the first (Group 1, large-size dot, $w = 1$), second (Group 2, medium-size dot, $w = 2$), and third (Group 3, small-size dot, $w = 3$) week of May, respectively. Panel A considers alternative sets of characteristics in the first step of the estimation, Panel B considers alternative sets of comparison households, and Panel C considers different specifications of the treatment group. The p-value labeled p_{123} corresponds to the null hypothesis of equality across groups. The panel on the right displays the difference in spending between Group 1 and Group 3, along with a 95 percent confidence interval (black line) and 90 percent confidence interval (vertical endpoints). Standard errors reported in parentheses are adjusted for clustering at the household level and obtained from a block-bootstrap procedure with 100 replicates. Calculated based on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business.

Figure 4: MPC by Timing of Payment—Fixing Calendar Week (US)



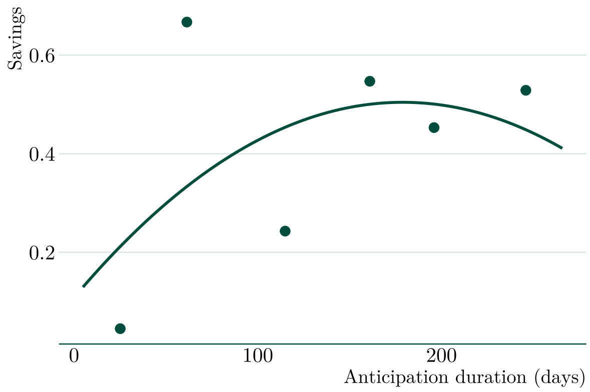
Note: Each bar shows estimates of the weekly marginal propensity to consume, holding the calendar week fixed, for households receiving EFTs in the first (Group 1), second (Group 2), and third (Group 3) week of May, respectively, over a one-month period ($1 \leq t \leq 4$), along with a 95 percent confidence interval. "Week" denotes weeks since the beginning of May; for example, Group 1 week 3, Group 2 week 2, and Group 3 week 1 refer to the MPC in the third week of May for Groups 1, 2, and 3, respectively. The estimates come from Equation (A.2) in Appendix C.2, extended to include fixed effects for the calendar week. The p-value labeled p_{123} corresponds to the null hypothesis of equality across all three groups, and the p-value labeled p_{13} corresponds to the null hypothesis of equality between Group 1 and Group 3. Standard errors are adjusted for clustering at the household level and obtained from a block-bootstrap procedure with 100 replicates. Calculated based on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business.

Figure 5: Impact of Shorter Wait for Cash Transfers (Kenya)

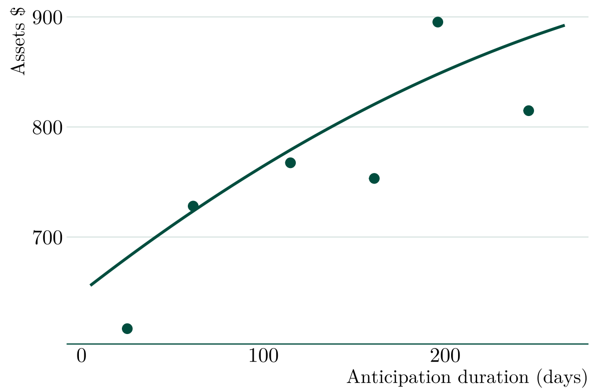


Note: The top four panels correspond to different outcome variables, and the bottom panel contains details on the set of specifications that we estimate. In the top panels, each dot corresponds to an estimate of the treatment effect, β_k, from Equation (5), with the associated 95 percent confidence interval shaded vertically. The bottom panel shows how each specification corresponds to a different definition of the treatment group (short waiting times) and the comparison group (long waiting times), with "cutoff" denoting the threshold for defining a short waiting time and "max" denoting the maximum number of days of waiting time in the comparison group. Savings is an indicator for reporting nonzero savings, and the remaining magnitudes are reported in 2012 USD PPP. Colors denote statistical significance at the 1 percent (orange), 5 percent (green), and 10 percent (blue) levels.

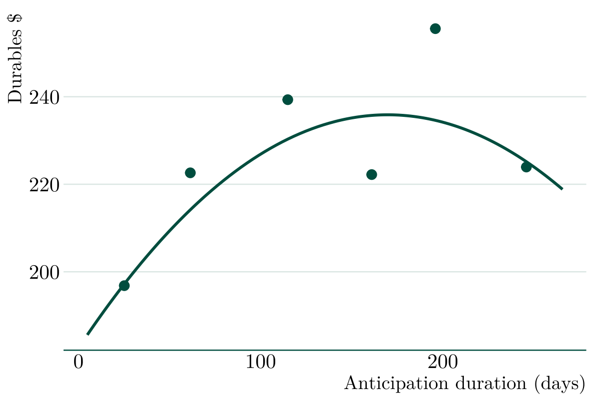
Figure 6: Relationship between Anticipation Durations and Outcomes (Kenya)



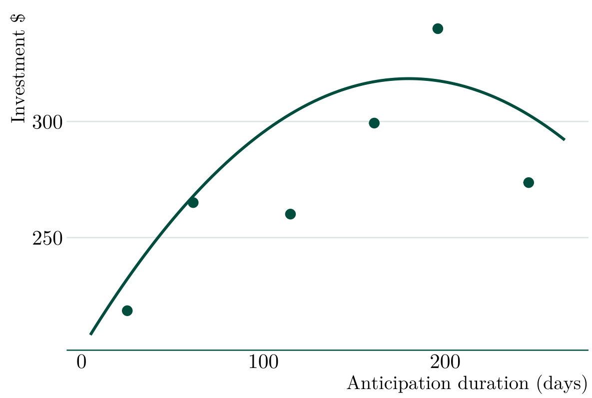
(a) Savings



(b) Assets



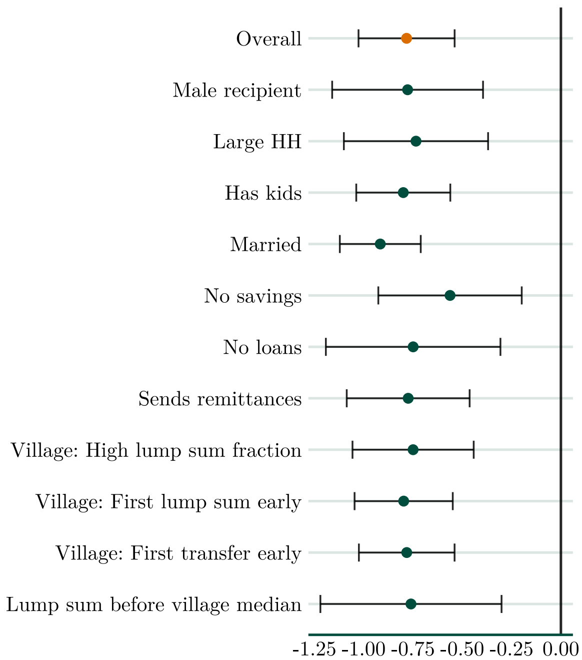
(c) Durables



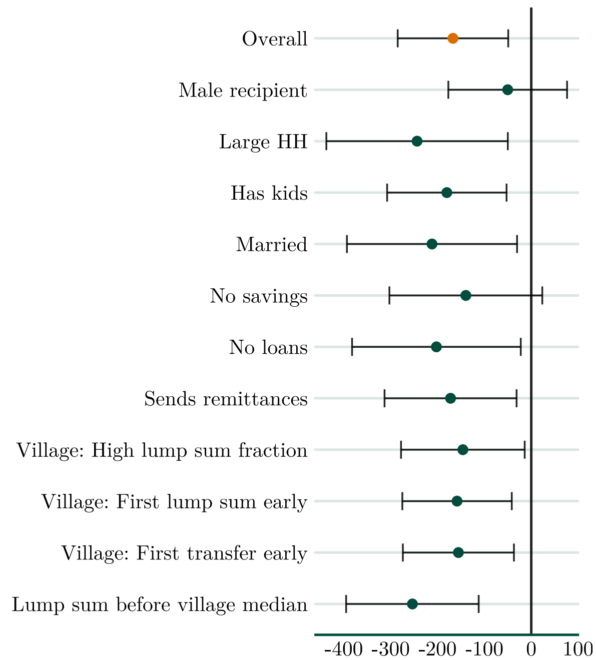
(d) Investment

Note: Each figure depicts the relationship between anticipation duration in days and the specified outcome (savings, assets, durables, and investments) in the form of a binned scatterplot. We use the rule-of-thumb integrated-mean-square-error optimal estimator of the number of bins (Cattaneo et al., 2019). The line shows the fit of a global second-order polynomial. See Section 4.2 for details on the outcomes.

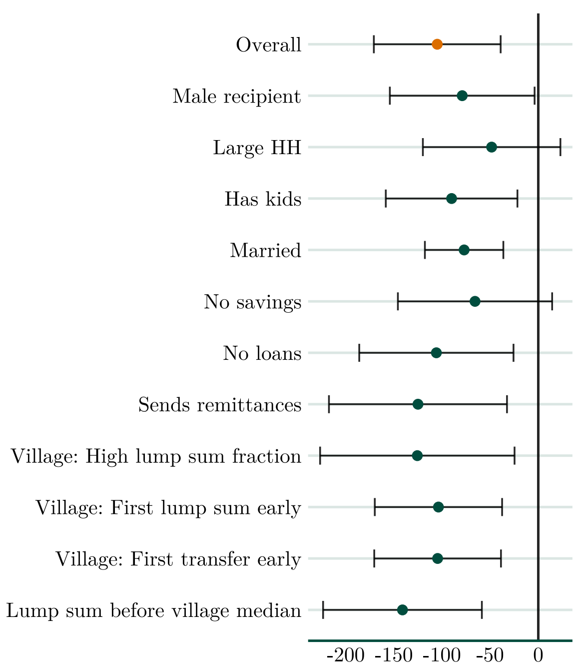
Figure 7: Impact of Shorter Wait for Cash Transfers—Heterogeneity (Kenya)



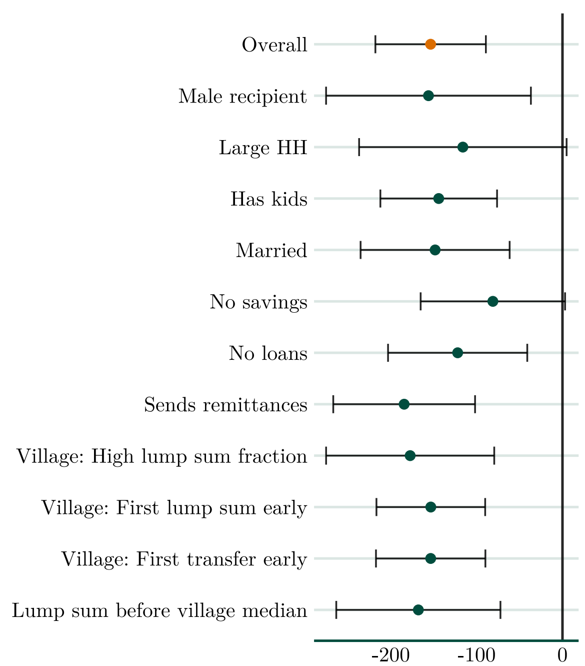
(a) Savings



(b) Assets



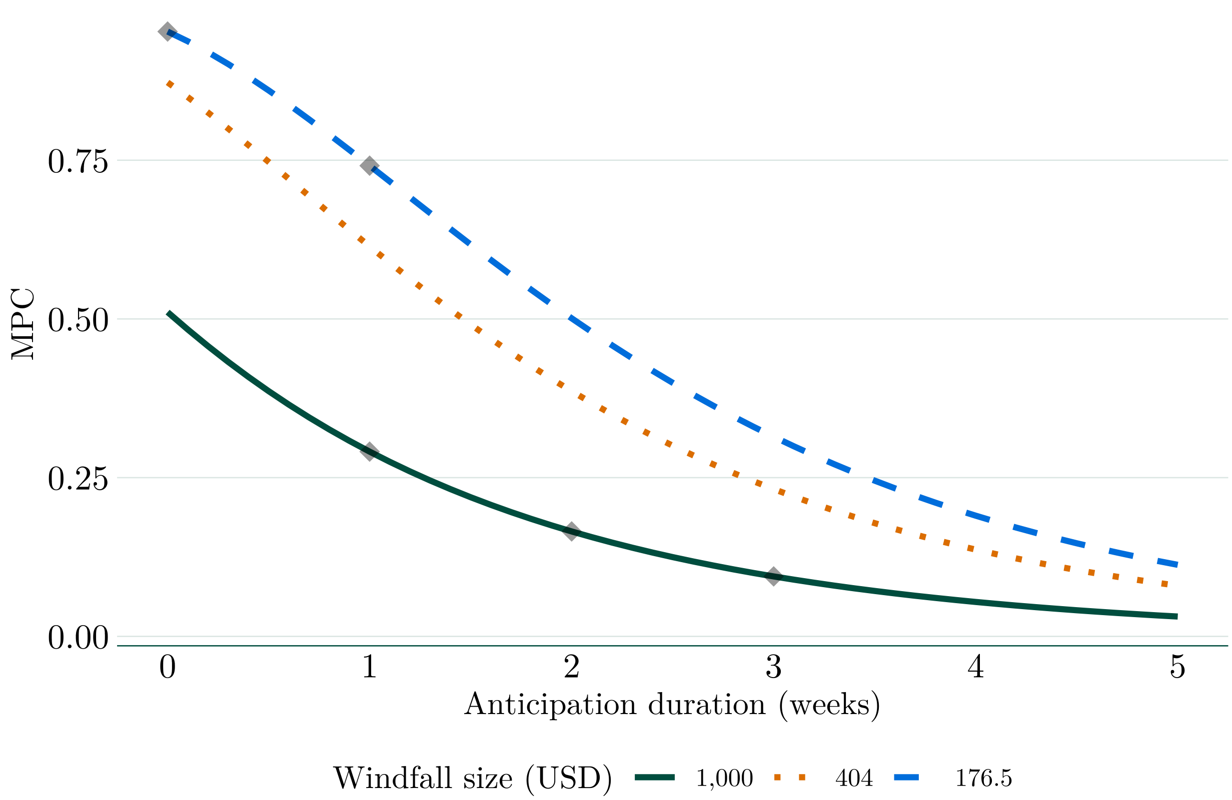
(c) Durables



(d) Investment

Note: Each figure depicts estimates of the treatment effect, β_k, from Equation (5) and the associated 95 percent confidence interval for various samples of households. Each specification uses a cutoff of 4 weeks as the threshold for defining the set of households treated with shorter waiting times and uses households waiting up to 9 months as the comparison group. See Section 4.3 for details on the samples.

Figure 8: Mental Accounting Model—MPC as a Function of Magnitude and Duration



Note: This figure plots estimates of the marginal propensity to consume (MPC) in response to a windfall of the specified magnitude as a function of duration based on Table 2. The MPC is calculated by dividing the model-implied total spending response by the windfall size. The \$1,000 windfall corresponds to the 2008 U.S. stimulus payment (Section 3); the \$404 windfall corresponds to the GiveDirectly cash transfer in Kenya (Section 4); the \$176.50 windfall corresponds to the windfall from the experiment in Malawi (Section 5). The diamonds indicate the range of waiting times present in the data from each setting. See Appendix Figure 20 for an analogous figure plotting the model-implied total spending response, and see Appendix Figures 21 to 23 for a depiction of how well the model fits the data in each setting.

Table 1: Impact of Delayed Windfalls on Savings (Malawi)

	1-day delay	8-day delay	p-value: $\beta_1 = \beta_8 = 0$
NBS account	-11.33	-1.18	0.2034
	(7.32)	(7.16)	
Formal savings	-0.47	12.27	0.5998
	(12.34)	(14.04)	
Informal savings	5.56	17.97	0.2255
	(10.56)	(10.56)	
Total financial assets	8.84	32.50	0.2716
	(18.90)	(20.60)	
In-kind savings	-0.65	137.95	0.0003
	(24.36)	(36.57)	
Total savings	-3.29	159.39	0.0005
	(31.89)	(47.74)	

Note: Each row presents estimates of Equation (6) with the outcome variable as a measure of savings. The sample consists of 474 households receiving MK 25,000 (USD 176.50 PPP) windfalls from the field experiment by Brune et al. (2017). The no-delay treatment consists of 156 households receiving payments via cash or direct deposit without delay. The delay treatments consist of 318 households that receive payments after a one-day delay (158 households) or after an eight-day delay (160 households). The first column presents the estimate of β_1 (the causal impact of receiving the windfall with a one-day delay relative to receiving the windfall immediately), and the second column presents the estimate of β_8 (the causal impact of receiving the windfall with an eight-day delay relative to receiving the windfall immediately). The third column reports the p-value corresponding to the null hypothesis of no difference between the no delay, 1-day delay, and 8-day delay treatments ($\beta_1 = \beta_8 = 0$). Formal savings consist of balances in NBS bank accounts (the bank that facilitated the experiment), other bank or microfinance institution accounts, and employee-based Savings and Credit Cooperatives (SACCOs). Informal savings consist of balances in Rotating Credit and Savings Associations (ROSCAs), village savings clubs (*kalabu yosunga ndalama*), cash that is not for living expenses kept at home or in a secret hiding place or given to someone else for safe keeping. In-kind savings consist of advance purchases of farm inputs, business inventory, and bags of maize. Total financial assets consist of formal and informal savings combined. Total savings consist of total financial assets combined with in-kind savings. All values are reported in USD PPP adjusted using the 2014 exchange rate 141.64 MK/USD. Standard errors are reported in parentheses. The data come from the survey questions displayed in Appendix Figure 17.

Table 2: Mental Accounting Model—Estimates and Fit

Scaling factor	3.33	1	5	7.3	10
Parameter estimates					
α (time)	0.5789	0.4323	0.6447	0.7107	0.7698
	(0.0394)	(0.0445)	(0.0410)	(0.0440)	(0.0478)
β (magnitude)	0.9984	0.9971	0.9986	0.9988	0.9988
	(0.0002)	(0.0003)	(0.0002)	(0.0002)	(0.0002)
Waiting equivalent	340.70	293.16	315.41	274.16	225.53
Predicted monthly NCP spending					
Group 1 (actual: 65.25)	64.82	50.36	63.03	58.57	53.15
Group 2 (actual: 45.24)	38.20	21.28	41.90	43.42	43.03
Group 3 (actual: 18.73)	22.06	9.11	27.12	31.26	33.83

Note: Each column presents estimates of the model defined by Equations (7) and (8) for a different scaling factor. The top panel shows estimates of the parameters from Equation (9), and the the waiting equivalent refers to the magnitude (in dollars) that would result in a decrease in the MPC of the same amount as one additional week of waiting (computed as $\log(\alpha)/\log(\beta)$). The bottom panel displays the excess NCP spending implied by the model (see the data in Figure 2 for comparison). Standard errors reported in parentheses are adjusted for clustering at the household level. Derived based on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business.

Table 3: Mental Accounting Model—Estimates and Fit: Heterogeneity

	Liquidity		Planning		Savings habit	
	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Parameter estimates						
α (time)	0.6016	0.5718	0.6616	0.5304	0.6811	0.5013
	(0.0623)	(0.0499)	(0.0635)	(0.0483)	(0.0576)	(0.0535)
β (magnitude)	0.9979	0.9988	0.9979	0.9988	0.9976	0.9991
	(0.0003)	(0.0002)	(0.0003)	(0.0002)	(0.0004)	(0.0002)
Waiting equivalent	247.48	449.53	191.94	520.76	158.72	746.23
Predicted NCP spending						
Group 1 (monthly)	49.22	84.04	54.39	76.56	47.38	93.03
Group 2 (monthly)	29.42	50.41	35.14	42.19	32.10	48.34
Group 3 (monthly)	17.74	28.87	23.40	22.42	21.68	24.43

Note: Each column presents estimates of the model defined by Equations (7) and (8) for a different subsample of households. The subsamples follow those presented in Figure 2: liquidity unconstrained (Column 1), liquidity constrained (Column 2), those that make financial plans (Column 3), those that do not make financial plans (Column 4), those that classify themselves as savers (Column 5), and those that classify themselves as spenders (Column 6). The top panel shows estimates of the parameters from Equation (9), and the waiting equivalent refers to the magnitude (in dollars) that would result in a decrease in the MPC of the same amount as one additional week of waiting (computed as $\log(\alpha)/\log(\beta)$). The bottom panel displays the excess NCP spending implied by the model (see the data in Figure 2 for comparison). Standard errors reported in parentheses are adjusted for clustering at the household level. Derived based on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business.

Table 4: Mental Accounting Model—One-Time Payment vs. Reduced Withholding

	One-time Payment	Reduced Withholding
Panel A: Survey data from Sahm, Shapiro and Slemrod (2012)		
Percent mostly spend	19	13
Percent mostly save	27	33
Percent mostly pay debt	53	54
Panel B: Methods to convert survey data to MPC estimates		
Shapiro and Slemrod (2003)	0.29	0.22
Parker and Souleles (2019)	0.35	0.32
Coibion, Gorodnichenko and Weber (2020)	0.44	0.41
Panel C: Model prediction		
MPC	0.32	0.27

Note: The top panel reports data from the Thomson Reuters/University of Michigan Surveys of Consumers documented by Sahm, Shapiro and Slemrod (2012, Table 1). The 2008 survey (Column 1) asks respondents how the tax rebates were affecting their spending; the 2009 survey (Column 2) asks respondents how the 2009 reduction in withholding is affecting their spending. The middle panel applies three methods from the literature to convert the survey responses into a measure of the MPC. The bottom panel states the prediction of our model; see Section 6 for additional details. Derived based on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business.