Excess Anticipation-Dependence in Consumption

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Abstract

This paper proposes that the marginal propensity to consume out of windfalls depends on a new state variable, the time horizon over which households anticipate receiving payments. We test this using a natural experiment from the randomized disbursement dates of a U.S. fiscal stimulus payment and assess external validity using randomized controlled trials on unconditional cash transfers in Kenya and Malawi. The data show evidence of *excess anticipation-dependence*: Consumption responds more to receiving additional income after a shorter anticipation duration, beyond what standard models predict. While households receiving stimulus payments do not increase spending in advance, additional consumption expenditure in the month after receiving payment drops over 40 percent for each additional week a household awaits payment. We estimate a mental-accounting model that incorporates this novel form of history dependence and discuss policy implications. Our approach reconciles conflicting results that consumption responds to anticipated payments in some settings but not others.

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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 et al., 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. 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.

[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" (Figure S1); also see statements from the U.K. Department for International Development regarding the role of cash transfers in "escaping poverty traps" (Arnold et al., 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).

We test for excess anticipation-dependence using a natural experiment provided by the randomized disbursement dates of a U.S. fiscal stimulus payment (Parker et al., 2013). The setting uniquely offers natural exogenous variation in when households learn about a windfall payment relative to when they receive it and detailed consumption data. 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.

Specifically, we use NielsenIQ 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 over 40 percent for each additional week that a household waits for their payment. This occurs even though households waiting longer might face greater financial strain and unmet needs, which would push them in the direction of spending a larger portion of their stimulus payment. Third, and perhaps most significantly, earlier disbursement of stimulus payments leads to a continuing shift in spending behavior: In a given calendar week, spending among households in the earliest payment group exceeds that of households in later payment groups who have received their payment more recently. The greater spending for households in the earliest payment group occurs despite these households having a smaller unspent amount remaining but otherwise facing similar conditions to those in later payment groups. These patterns emerge for households with different levels of liquidity, income, financial planning tendencies, savings habits, and income. Additional tests rule out explanations relying on shifts in the timing or allocation of spending, an effect of waiting time on spending needs or saving ability, or intrahousehold and social interactions.

Incorporating the time dimension into mental-accounting frameworks can explain the key patterns in our data. The lack of consumption response to information about future payments (excess smoothness) and strong response to receiving anticipated payments (excess sensitivity), including among households that do not face binding liquidity constraints, is consistent with households treating windfall income differently from their current wealth (Shefrin and Thaler, 1988; Thaler, 1990). In addition, the data show excess anticipation-dependence: (i) differential consumption response to additional income based on the duration of anticipation, and (ii) greater spending among households in the earliest payment group conditional on calendar week. Thus, 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 the psychology literature (Arkes et al., 1994) emphasizes, "the budgeting process occurs before receipt of the funds" but it "takes time. Until some reasonable target is decided upon, the money remains uncommitted and therefore available for extravagant, frivolous, or speculative use." Channels involving reference dependence, self-control, planning, attention, utility from anticipation, or uncertainty about optimal actions can operate in conjunction with mental accounting to generate MPCs that exhibit this novel form of history dependence (see Appendix A.1).

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 in the spirit of Mullainathan et al. (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 particular set of modeling assumptions. 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 et al., 2012). We discuss implications for the design of fiscal stimulus policies, highlighting the importance of faster or synchronized disbursement, broad-based targeting, and quantitative factors such as payment frequency and amounts.

To assess excess anticipation-dependence as a novel state variable in understanding consumption and savings responses to windfalls, we conduct a series of external validity tests. First, surveying the literature, we analyze all other settings we identified (see Supp. Appendix A) with consumption data and exogenous variation in the timing of when households learn about a windfall payment and when they receive it. In particular, we leverage variation induced by randomized controlled trials (RCTs) on unconditional cash transfers in Kenya (Haushofer and Shapiro, 2016) and Malawi (Brune et al., 2017) to show that the results from the U.S. setting extend to understanding consumption and savings responses to cash transfers in developing countries. Second, we conduct a meta-analysis of the literature, extending the work of Havranek and Sokolova (2020), to examine how MPC estimates vary with the time horizon over which households anticipate receiving a payment.

Our work 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 empirical results make several contributions to the extensive literature in household finance and public economics on tests of intertemporal consumption models. First, our work goes beyond the anticipated-unanticipated distinction by positing the importance of the duration over which an income shock is anticipated. Our meta-analysis of consumption responses to anticipated payments corroborates the finding of greater deviations from consumption smoothing for payments following a shorter anticipation duration, reconciling seemingly conflicting results that consumption responds to anticipated payments in some settings but not others. Second, we introduce the notion of excess anticipation-dependence, which distinguishes forward-looking consumption theories from alternatives that incorporate backward-looking elements. 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).[2] 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 et al., 2021), quasi-experimental (e.g., Parker, 1999; Souleles, 1999, 2002; Johnson et al., 2006), and structural (e.g., Blundell et al., 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 et al., 2024) 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 et al., 2019) and monetary policy (Kaplan et al., 2018).[3]

Our approach has several notable advantages. First, while many prominent studies on MPCs and mental accounting use hypothetical surveys (Shefrin and Thaler, 1988; Fuster et al., 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 in a questionnaire" (Arkes et al., 1994).[4] Second, and relatedly, analyzing 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 et al., 2007). Our paper joins a relatively small set of papers in behavioral economics using quasi-experimental policy variation (e.g., DellaVigna et al., 2017; Seibold, 2021). Third, the use of existing experiments reduces unintended researcher bias that may arise in designing or implementing new experiments (Rosenthal and Fode, 1963). In our context, 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

[2] Lewis et al. (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.

[3] See Fagereng et al. (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."

[4] 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."

in payment timing (Beam et al., 2022). Fourth, 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. 2022) and analyzing them in parallel.

The paper proceeds as follows. Section 2 establishes the conceptual underpinning for our empirical analyses by introducing the notion of anticipation-dependence. Section 3 analyzes consumer responses to the timing of the 2008 Economic Stimulus Payments in the US. Section 4 presents and estimates a mental-accounting model to interpret our results and discuss their implications. Section 5 examines related settings both in the US and in developing countries to show the external validity of our findings of excess anticipation-dependence. Section 6 concludes.

2 Benchmark consumption models and excess anticipation-dependence

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 Supp. Appendix B for more formal details). 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.

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 positive income changes, including among households with sufficient liquidity (McDowall, 2019; Fuster et al., 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 financial account data 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. 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." 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. 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 offer 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 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).[5] Excess anticipation-dependence, by contrast, requires a theory of consumption that incorporates backward-looking elements (i.e., history dependence).

2.3 Mental accounting, responses to windfalls, and history dependence

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. Within the literature on consumption smoothing, and more broadly in macroeconomics, various authors invoke mental accounting and violations of fungibility to explain otherwise puzzling phenomena.[6]

Recall that forward-looking theories preclude a direct relationship between time spent waiting for a windfall and consumption responses; moreover, they predict no relationship at all when anticipatory spending does not occur. Excess anticipation-dependence thus requires theories that incorporate such history dependence in modeling consumption decisions.

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, stress 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…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 thus extend the Shefrin and Thaler (1988) model in Section 4 to incorporate history dependence by describing how consumers categorize windfalls based on the duration of anticipation, i.e., the time

[5]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).

[6]This includes the relationship between liquid wealth and MPCs (Kueng, 2018; McDowall, 2019; Fuster et al., 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), the co-holding of savings and debt (Gathergood and Olafsson, 2024), and high MPCs for liquid consumers (Massenot, 2021; Lian, 2023; Mijakovic, 2022); see Table A1 for selected excerpts from these references.

elapsed since learning about the windfall.

The idea to incorporate the time dimension in modeling mental accounting is new and therefore warrants further discussion. Excess anticipation-dependence in responses to windfalls, combined with a lack of anticipatory spending, necessitates a theory of consumption that incorporates backward-looking elements. We formalize several channels through which MPCs may exhibit such history dependence in conjunction with mental accounting (Appendix A.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 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 et al., 2020). Fifth, consumers may experience utility from anticipating future consumption, resulting 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, 2023).

While testing these channels falls outside the scope of this paper, the framework highlights the importance of incorporating backward-looking elements into consumption models to understand how consumers classify additional income differently based on its source.[7]

3 Tax rebates in the US

This section analyzes our main 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

Public announcements and payment timing. In response to the start of the recession in December 2007, the U.S. federal government approved an economic stimulus package in February 2008. 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 May and further batches of payments scheduled for the following weeks. Table S1 shows the ESP disbursement schedule for on-time filers.[8] As

[7]Some attempts to incorporate history dependence fail to account for the main patterns in our data, including explanations based on external commitments, myopia, intertemporal consumption complementarities, rational inattention, and rational illiquidity (Appendix A.2). Purely forward-looking theories of consumption, such as models of discounted utility (Laibson, 1997), temptation disutility (Gul and Pesendorfer, 2004), and expectations-based reference dependence (Kőszegi and Rabin, 2009) similarly do not explain the excess anticipation-dependence and lack of anticipatory spending in our data, unless expectations-based reference dependence additionally imposes a mental accounting assumption (Appendix A.3).

[8]Martinez et al. (2023) show that the 2008 ESPs induce earlier filing in a sample of low-income tax filers.

households may not have been perfectly informed about the disbursement schedule, the IRS sent a notification letter to each household several days prior to their payment date. Payment dates were staggered based on the last two digits of the taxpayer's Social Security Number (SSN) because of the infeasibility of mailing all notification letters at the same time.

Importantly, on April 25, President Bush stated that the Treasury would start distributing stimulus payments several days earlier than expected, after which the news media highly publicized the disbursement schedule. Data from Google Trends reveals that interest in stimulus payments surged when the President announced that that the Treasury would start distributing payments soon, and not beforehand (Figure A1), indicating that the vast majority of households likely began anticipating their payments around the time when the first round of payments started.[9] Similarly, Kaplan and Violante (2014) support the view that "all households learn about the rebate when the first batch of Treasury checks are received."

Stimulus payment amounts and eligibility. 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 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 below $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 distribution channels. 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 NielsenIQ Consumer Panel (NCP, formerly Homescan Consumer Panel).

The NCP data contain information on household demographics (e.g., household size and

[9] Section 4.4 highlights how model-implied spending responses reinforce this informational assumption.

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 spending (Coibion et al., 2021), representing 19 percent of the broader category of total consumption spending or 35 percent of the finer category of spending on broad nondurable goods (Broda and Parker, 2014). 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 aged 18 to 95 that report receiving a stimulus payment of at least $300. We interpret our results as internally valid estimates for the subsample of NCP panelists or the population that they represent (Bronnenberg et al., 2015).

3.3 Estimation

3.3.1 Methodology

Given the staggered timing of the treatment, we use the two-stage estimation approach by Gardner et al. (2024), which allows flexibility in constructing counterfactual trends.[10] 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.

Formally, denote by w_i the time period of the event that i becomes treated, and define $K_{it} = t - w_i$ to be time relative to treatment. Households become partially treated during the week when their payment arrives, corresponding to $K_{it} = 0$. Let Y_{it} denote weekly spending in period t for household i with time-invariant characteristics θ_i.[11]

[10] Gardner et al. (2024) propose this methodology and document its advantages relative to alternative staggered difference-in-differences methods in simulations and applications, even though the alternative methods result in similar point estimates.

[11] Weekly spending refers to expenditure on NielsenIQ goods between Sunday and Saturday of the corresponding week, and week 0 corresponds to the week during which the stimulus payment is received. Households become treated when their reported payment date falls in the Monday to Sunday period that contains the

The first stage consists of a regression of the outcome Y_{it} on household fixed effects α_i and group-specific time effects $\beta_{\theta_i t}$ (where θ_i denotes the group associated with household i) using pre-treatment data:

$$Y_{it} = \alpha_i + \beta_{\theta_i t} + \nu_{it}, \quad \{i, t : K_{it} < -\underline{k}\}. \tag{1}$$

In this stage, we exclude data within $\underline{k}$ periods from the treatment date to account for the possibility that household response starts up to $\underline{k}$ periods in advance.[12]

The second stage recovers the dynamic treatment effects for households that receive payments at different times $w_i \in \{1, 2, 3\}$ relative to the announcement date:

$$Y_{it} = \widehat{\alpha_i} + \widehat{\beta_{\theta_i t}} + \sum_k \gamma_k^{w_i} \mathbb{1}_{\{K_{it}=k\}} + \varepsilon_{it}, \tag{2}$$

where $\widehat{\alpha_i}$ and $\widehat{\beta_{\theta_i t}}$ are the estimated parameters from Equation (1). The parameter γ_k^w represents the causal impact of receiving a rebate k periods ago among households treated in period w. The case of $k < 0$ corresponds to a not-yet-treated week, and the case of $k = 0$ corresponds to a partially treated week. We define the τ-week cumulative spending impact for treatment cohort w as $\Gamma_\tau^w := \sum_{k=1}^{\tau} \gamma_k^w$.

Benchmark predictions. First, the benchmark model predicts that consumption responds to information about future income changes. Excess smoothness corresponds to the null hypothesis that there is no increase in spending in anticipation of receiving the stimulus payment, i.e., $\gamma_k^w = 0$ for $k < 0$, for all groups w.

Second, the benchmark model predicts that households smooth consumption in response to the arrival of anticipated income. Excess sensitivity corresponds to households changing spending after receiving payments, i.e., $\gamma_k^w > 0$ for $k \geq 0$, for all groups w.[13]

Third, the benchmark model predicts that the spending response to an income shock does not depend on the time duration between receiving information about the future income change and the arrival of the shock. Conditional on finding no anticipatory spending, excess anticipation-dependence corresponds to households receiving rebate payments sooner after the announcement exhibit higher spending responses, i.e., $\Gamma_k^w > \Gamma_k^{w'}$ for $w < w'$.

Stronger test of excess anticipation-dependence. Recall that excess anticipation-dependence describes when households receiving a payment earlier exhibit higher spending responses

scheduled date. This ensures that every household is fully treated starting in $k = 1$.

[12] To test for such anticipatory spending responses, we use the pre-event coefficient estimates from the second stage regression; see Gardner et al. (2024) for methodological details. Our data provide evidence of no significant anticipatory spending response (excess smoothness), as Section 3.6.1 shows.

[13] Valid tests for excess smoothness and excess sensitivity do not require distinguishing between consumption responses among households receiving payments at different dates. A weaker test, following the existing consumption literature, would involve pooling households that receive payments at different times in the second stage regression so that the predictions described above pertain to an average spending response across groups.

than households receiving the payment later, evaluated at the same number of periods from their payment date: $\gamma_\tau^w > \gamma_\tau^{w+1}$. Assume that household spending responses decrease as more time elapses from the payment date, as expected based on theory: $\gamma_\tau^w \geq \gamma_{\tau+1}^w$. This allows us to derive a sufficient condition for excess anticipation-dependence: If $\gamma_{\tau+1}^w$ exceeds γ_τ^{w+1}, then group w not only spends more at the same elapsed time (τ) compared with group $w+1$ but also *continues* to have a larger spending response even at a later elapsed time ($\tau + 1$ periods after their own payment), despite the natural decline in spending that typically occurs as time elapses since the payment date.

Assessing spending responses conditional on the same calendar week for households that receive payments at different times relative to the announcement date has several additional advantages. First, since the timing of announcements does not vary by payment group, variation in anticipation duration reduces to variation in treatment time. Holding fixed calendar weeks allows us to distinguish between anticipation-dependence and calendar-week effects. Second, fixing calendar weeks allows us to explore how payment timing and magnitude can both interact to determine MPCs; we revisit this point in Section 4.

3.3.2 Identification

First, under random assignment of treatment timing, we can estimate the causal effect of receiving stimulus payments with different anticipation durations. Second, estimating the causal effect of receiving stimulus payments requires the presence of not-yet-treated units for constructing counterfactuals, with a parallel trends assumption for identification.

Quasi-randomized payment timing. The last two digits of a taxpayer's Social Security Number (SSN), which are effectively randomly assigned, determines their scheduled payment date.[14] To examine the consistency of payment dates in our sample with randomization, we test whether households receiving ESPs at different times have similar characteristics.

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 (Table A2). In particular, out of 33 tests of equality, none are significant at the 5 percent level, and one is significant at the 10 percent level, consistent with random variation. Nevertheless, our analysis accounts for the possibility that small random differences in characteristics could affect the comparison across payment dates by separately analyzing various subsamples of households. We also allow counterfactual time trends to vary with observed characteristics.

Among the sample of households receiving ESPs by paper check, however, our balance tests (Table A3) reveal systematic differences by payment date across a wide range of characteristics including rebate amounts. This occurs because late filers receive payments during

[14] 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.

the later weeks of the paper check disbursement period (see Supp. Appendix C.1 for further discussion). Our main analysis therefore focuses on the sample of households receiving payments by direct deposit. We revisit the sample of households receiving payments by paper check in Section 3.3.3.

Counterfactual spending trend. For our main results, the treatment groups consist of households whose reported payment date falls in the Monday to Sunday period that contains the scheduled date. The comparison group used to construct counterfactual spending 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).

To determine the counterfactual time trend for spending, we allow for differential time trends based on income groups and deciles of average expenditure by household size in the first quarter of 2008.[15] This approach is more flexible than simply using fixed effects.

To allow for the possibility that households respond to the rebate payment before receiving it, we exclude data within two periods from the treatment date in estimating household fixed effects and group-specific time effects (i.e., setting $\underline{k} = 2$ in Equation 1).

3.3.3 Parallel trends and average effects

Estimates of the average spending response to stimulus payments reveal several noteworthy patterns, which we discuss below. This corresponds to estimating the γ_k parameters from Equation (2) when pooling together households receiving payments at different times (Figure A2). We consider subsamples based on the payment method, direct deposit or paper check.

Assessing the parallel trends assumption. First, our data show no significant differences in spending trends more than one week prior to ESP receipt (Figure A2), consistent with the results in Broda and Parker (2014) and Borusyak et al. (2024). Establishing similar trends among treatment and comparison groups in the periods leading up to the treatment date lends support to the identification strategy. In addition, this pattern also holds when analyzing pre-rebate treatment effects for each payment group separately and when using alternative groups of comparison households, further validating the estimated counterfactual spending trend (see Supp. Appendix C.2).

Comparing average effects with existing literature. Second, we find greater spending impacts for households receiving stimulus payments via direct deposit compared to those receiving paper checks (Figure A2). While this does not constitute a test of excess anticipation-dependence due to compositional differences between these two groups, the

[15] The set of characteristics 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.

larger response for direct deposit households (receiving payments 1–3 weeks after the disbursement schedule became highly publicized) compared to paper check households (receiving payments 3–11 weeks after) is consistent with a stronger spending responses for households who receive payments earlier. Related work on the spending response to the 2008 tax rebates masks these differential responses by pooling households receiving payments via direct deposit and paper check (Broda and Parker, 2014; Borusyak et al., 2024; Orchard et al., forthcoming). The original analysis by Broda and Parker (2014) reports relatively large estimates compared to subsequent work by Borusyak et al. (2024) and Orchard et al. (forthcoming) due to methodological differences, with the latter studies accounting for the staggered timing of stimulus payments. Our approach and estimates align with the more recent work (Borusyak et al., 2024; Orchard et al., forthcoming). However, as our analysis primarily focuses on households receiving direct deposits, the magnitudes we report tend to be somewhat larger than than what Borusyak et al. (2024) and Orchard et al. (forthcoming) find.

3.4 Impact of timing of stimulus payments

Our main results pertain to estimates of Equation (2), focusing on whether households exhibit greater spending responses to payments that arrive earlier. 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 among any of the three payment groups (see Section 3.6.1).

We start by testing whether the cumulative 4-week spending impacts Γ_4^w vary across households that receive payments at different times. Households received EFTs in a staggered rollout over three weeks, which we denote as weeks $w = 1$, $w = 2$, and $w = 3$, respectively, relative to President Bush's announcement on April 25 (Table S1). Note that misclassifying how long a household anticipates receiving their payment (e.g., because some households do not pay attention to the news about the upcoming stimulus payment) would attenuate differences in spending responses across these groups.

Figure 1a (also Table S2) summarizes our main results for various samples of households.[16] 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. The remaining rows of Figure 1a 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) as well as household income. These

[16] Figure A3 displays cumulative spending effects during the four weeks following ESP receipt. Households receiving paper checks exhibit smaller spending responses, as discussed in Section 3.3.3.

characteristics are not highly correlated; their pairwise correlations are less than one-third. Finding similar patterns for these subsamples provides evidence that liquidity constraints do not explain our results, that the results from the full sample do not arise due to small differences in the composition of households along these dimensions, and that the pattern of behavior captures a distinct phenomenon from general saving and planning tendencies or other household characteristics. We also conduct a series of robustness checks to assess the sensitivity of our results to alternative assumptions for determining the counterfactual spending trend, the comparison group of not-yet-treated households, and alternative sample restrictions in Figure A4.

Overall spending impacts. We begin by discussing the full sample of households receiving EFTs. The data show a clear pattern of lower spending impacts for households that wait longer to receive their payments. Among households randomly assigned to receive payments in the first week, we estimate a \$53.26 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 exceeds the magnitude of the combined impact on households receiving payments one week later (\$30.89) and those receiving payments two weeks later (\$8.05). To put the cumulative spending impacts into perspective, note that the NCP data comprise about 30 percent of household expenditure (Coibion et al., 2021), and the average ESP for direct deposit households is about \$1,000. The resulting difference in MPCs (about 0.18 for the first group, 0.10 for the second group, and 0.03 for the third group) suggests an important role for the timing of payments in designing effective fiscal stimulus.[17]

Spending impacts by liquidity. 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 (2). 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 (see Table 2 therein). Consistent with these findings as well as other prior literature (Zeldes, 1989; Johnson et al., 2006; Agarwal et al., 2007), the results in the second and third rows of Figure 1a 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

[17]Operationally, we compute these MPCs by dividing the cumulative four-week spending responses in Figure 1a by the average rebate amount (about \$1,000), and then scaling by a factor of 3.33.

sufficient liquidity. Randomly assigning more liquid households to receive payments at the beginning of the disbursement period leads to substantial increases in spending of almost $50 over the four weeks after receiving their ESP. This exceeds the 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.

Spending impacts by financial planning tendencies. 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, our results reveal waiting times as a key driver of this relationship. Households that make financial plans exhibit smaller spending responses only if they receive ESPs in the last week ($2.30 for planners compared to $14.28 for non-planners). The finding that the smallest spending responses come from this group 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.

Spending impacts by spender and saver types. The next pair of rows in Figure 1a separately consider households that characterize themselves as spending types and saving types, a measure of impatience.[18] We find that 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.

Spending impacts by income and other characteristics. In addition to analyzing spending responses across households with different self-reported financial circumstances, we estimate heterogeneity by objective household characteristics. This mitigates the potential for any differences in household characteristics that may arise by random chance to confound differences in spending across payment groups. Importantly, the same pattern emerges for households of different income levels, as the last pair of rows in Figure 1a shows. The relationship between anticipation duration and spending responses also persists for households in states experiencing higher or lower levels of job loss during the recession (Figure A5).

[18]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.

We also find that households of different sizes and compositions exhibit similarly strong spending reductions in response to longer waiting times (Figure A6).

3.5 Stronger test of excess anticipation-dependence: Fixing calendar week

We next examine how spending responses compare during the same calendar week across households in different payment groups, corresponding to the stronger test of excess anticipation-dependence described in Section 3.3.1. This approach eliminates the possibility that calendar-week effects explain the excess anticipation-dependence we observe.[19] It also rules out the potential concern that households only become aware of the payments upon receiving notification letters (or the payments themselves), which would otherwise require differences in calendar week to drive the differences in spending.

The data show that households receiving EFT payments in later weeks spend significantly *less* on average than those in the earliest payment group, consistent with a substantive shift in decision-making. The bars in Figure 1b show estimates of the average weekly spending response for each payment group over the four-week period starting from when the last group receives payments. We reject the null hypothesis of equal responses between the first and last payment groups ($p = 0.039$), as well as the null hypothesis of equal responses across all three groups ($p = 0.046$).

Strikingly, this pattern emerges despite the fact that households in earlier payment groups would have less of their ESPs remaining to spend. Furthermore, by holding fixed the calendar week, this test allows us to compare households when they would have access to the same information, ruling out the possibility that households in later payment groups spend less only because they receive payments during a time when all households respond less (e.g., due to new information over time about the severity of the financial crisis, or media articles highlighting the value of saving).

3.6 Alternative explanations

The fact that liquidity constrained and unconstrained households exhibit similar patterns suggests that households treat windfall income differently from their current wealth, supporting the mental accounting interpretation of the results. We elaborate on why channels based on external commitments, myopia, intertemporal consumption complementarities, rational inattention, and rational illiquidity do not suffice to explain the key patterns in our data in Appendix A.2.

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.

[19] The evidence from Section 5.1 also challenges explanations based on calendar-time effects, as variation in announcement timing results in anticipation duration varying independently of calendar time.

The key pieces of evidence we discuss below to address these alternatives include (1) survey measures of total and category-specific ESP spending; (2) evidence that spending in a given calendar week among households in the earliest payment group significantly exceeds that of households in later payment groups who have received their payment more recently (Figure 1b); and (3) the finding that households exhibit excess anticipation-dependence regardless of whether they face binding liquidity constraints. We also contextualize the magnitude of the effect size to show that it does not require additional explanatory factors beyond waiting time.

3.6.1 Challenges linking longer anticipation to lower ESP spending

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 among any of the three payment groups, with the total spending response in the weeks before receiving an ESP ranging from -\$6.24 to \$2.71 across the various specifications in Table A4.[20] The fact that households with different levels of income and liquidity exhibit similar patterns further limits the plausibility of explanations relying on anticipatory spending.

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 spending prior to ESP receipt of \$3.23 per day (\$45.21 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.

Borrowing and deleveraging. Although our data show no evidence of anticipatory spending, households might either increase debt payments or increase non-NCP consumption in advance (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 et al., 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

[20]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.

leads households to mostly pay off debt ($p = 0.7$).

Non-NielsenIQ spending. 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 report lower ESP spending on average than households in earlier payment groups, consistent with the pattern we find with NielsenIQ spending. 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.

3.6.2 Other factors reducing spending with longer anticipation

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.

Individual-level channels. The evidence that liquidity unconstrained households exhibit the same effect (Figure 1a) 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 Section 3.5.

Group-level channels. The results replicate for single individuals, couples, households with and without children (Figure A6), 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 Section 3.5.

3.7 Contextualizing the effect size

While the evidence rules out a range of alternative explanations, one might still ask whether the differences across payment groups—with additional NCP spending in the month after receiving payment dropping by about $22 for each additional week a household awaits payment—must necessitate some additional, unconsidered factor. Yet prior experimental work corroborates that even modest time delays can meaningfully alter behavior. In laboratory settings, providing three minutes of advance notice about an upcoming choice leads to a 28 percent increase in the probability of choosing fruit over candy (DeJarnette, 2020), while an hour of notice leads to a 35 percent increase in the allocation of real-effort tasks toward the present when task requirements grow over time with a positive interest rate (Imas et al., 2022). In field settings, providing 4–48 hours of notice about a choice between healthy and unhealthy food subsidies increases healthy spending by 61 percent relative to a healthy food subsidy alone (Brownback et al., 2023), and slowing down the delivery of digital loans in Mexico by about ten hours changes the use of funds in such a way that it decreases the probability of default by over 20 percent (Burlando et al., 2025).[21]

To put the magnitude of the consumption effects in perspective, a recent meta-analysis of the micro literature on excess sensitivity (Havranek and Sokolova, 2020) offers several comparisons. An additional week of anticipation, corresponding to a decrease in the MPC of 0.07–0.08, has a smaller effect than liquidity constraints (as our data in Section 3.4 also show) and a slightly larger effect than the difference between food and the entire set of non-durable goods.[22] Moreover, the within-study standard deviation of MPC estimates, after accounting for "features of data and methods that may affect the reported magnitude," is 0.08.[23] We further discuss evidence based on Havranek and Sokolova (2020) in Section 5.3.

4 The role of magnitude and timing in consumption responses

This section presents a descriptive model of mental accounting that accounts for both the lack of anticipatory spending in our data (excess smoothness and excess sensitivity) and the new evidence of excess anticipation-dependence. We further want to account for the interaction between payment timing and the (remaining) windfall amount to explain spending patterns over time.

We estimate the model using the weekly spending NCP data. The estimated model fits

[21] Also see our evidence on cash transfers in Sections 5.1 and 5.2.

[22] In addition, Section 4.4 calculates that an additional week of waiting has the same effect on the MPC as an increase in the size of the payment by $682.

[23] Regressing MPC estimates for total consumption on study fixed effects gives a root mean squared error of 0.28, which drops to 0.08 after controlling for characteristics of the specification (e.g., sample size, frequency of the observations, whether year fixed effects are included), the sample (e.g., liquidty constrained households), and estimation technique (e.g., specific instrumental variables); see Havranek and Sokolova (2020) for the full list of determinants of reported estimates used as control variables.

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 (Section 3.5). We show how the model also matches out-of-sample moments and discuss implications for the design of fiscal stimulus policies.

4.1 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).

Mental accounting provides an explanation for the four key patterns in our 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 1b) 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.

To derive policy implications without imposing strong structural assumptions, we adopt a reduced-form approach as in Mullainathan et al. (2012) to capture how MPCs vary over time. In support of this approach, a variety of theoretical models provide possible channels for history dependence in MPCs, which we discuss in Appendix A.1.

4.2 Descriptive model of mental accounting with magnitude and timing

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 et al., 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.[24] 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

$$y_\tau = \mu(w_\tau, \tau) \cdot w_\tau \tag{3}$$

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

$$w_{t+k} = w_{t+k-1} - y_{t+k-1} \tag{4}$$

remains in the windfall account in period $t + k$ for $k > 0$. Our main specification assumes that households in the earliest payment group start anticipating the payments when the disbursement schedule became highly publicized following President Bush's announcement; in other words, Group 1 anticipates the windfall for 1 period, Group 2 for 2 periods, and Group 3 for 3 periods.[25] We discuss how well the model fits under alternative informational assumptions in Section 4.4.

4.3 Nonlinear least squares estimation

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

$$\mu(m, t) = \beta^m \alpha^t, \tag{5}$$

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,

[24] 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."

[25] This is similar to the "baseline informational assumption" in Kaplan and Violante (2014): All households learn about the rebate payments upon disbursement of the first set of payments.

respectively. We do not constrain the values of α or β in the estimation and therefore do not impose anticipation dependence; however, 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 $\frac{\partial \mu}{\partial t} < 0$).

Equations (3) to (5) recursively define windfall spending in each period as a nonlinear function of parameters (α and β) and data (m and t).[26] 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.

We obtain a measure of total windfall spending (the outcome variable $\widetilde{y_{it}}$) by taking the difference between observed weekly NCP spending and counterfactual spending (estimated using Equation 1) 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 et al., 2021).

4.4 Model fit

Parameter estimates. We present estimates of the model in Table 1. In our preferred specification (Column 1, which uses a scaling factor of 3.33) we estimate a magnitude parameter of $\beta = 0.9988$ and a time parameter of $\alpha = 0.4284$. To interpret these magnitudes, note that increasing the size of a windfall by \$100 reduces the marginal propensity to consume out of that windfall by 12 percent. Furthermore, 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 \$682, a quantity we refer to as the *waiting equivalent* (the value of w that solves $\alpha = \beta^w$). The bottom panel of the table shows how closely the estimated model matches the monthly spending moments in the U.S. data (see Figure 1a).

Interaction between magnitude and duration. The estimated model also reproduces key features of the weekly spending data that are not directly targeted in the estimation. For groups that face shorter waiting times, the estimated model predicts that spending remains somewhat elevated as time passes, as Table A5 documents. This pattern arises in the mental-accounting model due to the interaction between magnitude and timing: 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 can offset the MPC reduction in the subsequent period, depending on the parameter values. Under our estimates, this effect is strong enough for households receiving stimulus payments in the earliest payment group to spend more than households receiving payments in later groups even when conditioning on calendar week (Figure 1b).

Informational assumptions. While Figure A1 and Kaplan and Violante (2014) support the perspective that the process of anticipation begins at the time of President Bush's state-

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

ment drawing widespread attention to the payment schedule, we explore the possibility that the IRS announcement serves as the relevant starting point. This leads to a higher estimate of α, reflecting a weaker decline in spending responses with anticipation time, and correspondingly a higher estimate of β, which keeps the estimated additional windfall amount necessary to reduce the MPC by the same amount as an extra period largely unaffected by the shift in anticipation durations. The difference in parameter estimates has several implications. The alternative informational assumption predicts a less steep decline in spending with time, providing a poorer fit for the monthly spending moments (see Table A6). Relatedly, since the alternative under-predicts the spending response for the earliest payment group and predicts a smaller decrease in MPCs with payment size, it does not reproduce the stronger pattern of excess anticipation-dependence (i.e., higher spending responses among households in the earliest payment group when holding calendar week fixed). Finally, it results in MPCs out of unexpected windfalls that vastly exceed estimates in the literature.[27] This leads to very different policy implications, as Section 4.5 discusses.

Sensitivity analysis. The choice of scaling factor depends on which broader category of spending is most relevant for contextualizing the NCP data. We consider a range between 1 and 9.4 to encompass our main specification as well as alternatives from the literature.[28] 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 1 show. The preferred specification provides the closest fit for the monthly spending moments in the U.S. data, as the bottom panel shows.

Heterogeneity. We also estimate the model separately for the subsamples in Figure 1a and document substantial heterogeneity in waiting equivalents. As Table A7 shows, for households that are liquidity constrained, do not make financial plans, or are self-classified spenders, an additional week of waiting time reduces the MPC by as much as an additional \$794 to \$906 in the size of the windfall. By contrast, the waiting equivalents for unconstrained households, financial planners, and self-classified savers range between \$429 and \$570. Predicted spending estimates, shown in the bottom panel, generally follow the data reported in Figure 1a. 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.

[27]For example, Fuster et al. (2021) estimate MPCs between 0.08 and 0.14 for unexpected one-time payments.

[28]Using the share of self-reported ESP spending on household goods (13.7 percent) implies a scaling factor of 7.3 (Broda and Parker, 2014). Using the fraction of spending on NCP-type goods relative to all spending categories in the Consumer Expenditure Survey implies a scale factor of 9.4 (Parker and Souleles, 2019). The scale factor of 5 roughly corresponds to using the share of total consumption spending (19 percent).

4.5 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 Supp. Appendix C.3.

Expenditure-maximizing payment size. With some caveats, the model provides guidance on the optimal size of stimulus payments from the perspective of the stated "goal of increasing consumer spending" (Figure S1). 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 \$879, \$834, and \$817, respectively, for windfalls that arrive one, two, and three weeks after the announcement. These values align much more closely with the actual payment amounts than the spending-maximizing amounts under the alternative informational assumption (\$2,706, \$2,450, and \$2,281, respectively). For a windfall that arrives completely by surprise, the spending-maximizing amount implied by the model is \$1,003. These calculations abstract from differences in household characteristics, income, and other financial circumstances which would likely alter these conclusions (e.g., the MPC may depend on the size of a windfall relative to income as in Kueng 2018). 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.

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 \$879 windfall that is anticipated for one week (i.e., the consumption-maximizing payment size). Our estimates imply that a completely unanticipated windfall of \$250 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 70 percent of the value of the payments themselves. Investing in solutions that synchronize payment timing provides a channel for policymakers to implement such unanticipated payments.

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 et al. (2012) describe arguments from academics and policymakers suggesting that a series of small payments may induce greater spending. 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 et al. (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. Three methods from the literature, summarized in recent work by Feldman and Heffetz (2022), offer different approaches for converting these data to MPC estimates. The Shapiro and Slemrod (2003), Parker and Souleles (2019), and Coibion et al. (2020) methods respectively imply MPCs of 0.29, 0.35, and 0.44 for the one-time payment, and 0.22, 0.32, and 0.41 for the reduction in withholding.[29] Calibrated based on the relatively large spending responses estimated in previous research, these methods make varying assumptions about the behavior of households that report mostly spending (specifically, that they spend a substantial portion of their additional income, with thresholds all set well above 50 percent).[30] 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 average MPC of 0.09 for the reduction in withholding compared to 0.12 for the one-time payment (Table A8).[31]

Targeting. Our results reinforce previous findings 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). While much policy discussion emphasizes identifying and directing payments toward "hand-to-mouth" households, our findings suggest that targeting larger payments to specific groups may not maximize overall spending. Because MPCs decline as windfall size increases, ensuring timely and widespread distribution may be a more effective strategy. Nevertheless, extending and applying our methodology to larger datasets could provide further guidance on optimizing both the size and timing of targeted 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 et al., 2024).[32] At an individual

[29] As we discuss in Section 3.3.3, studies accounting for the staggered timing of stimulus payments find smaller MPC estimates. Borusyak et al. (2024), using comparable methodology to ours, estimate the marginal propensity to consume nondurables three months after receiving tax rebate to be between 0.08 and 0.11.

[30] For example, Parker and Souleles (2019) assumes that the average spending propensity of households that report mostly spending is 80 percent.

[31] Estimates under the alternative informational assumption would imply higher MPCs for the 2009 reduction in withholding than what the data show for the 2008 stimulus payments, contrary to the results in Sahm et al. (2012).

[32] 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.

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 A.4.

5 Anticipation-dependence in other settings

Anticipation duration emerges in our data as a novel state variable for explaining variation in consumption responses to windfall payments. Given its importance in our data, we assess the broader applicability of our conclusions by investigating whether anticipation-dependence extends to other familiar settings. Revisiting well-established settings with new research questions allows us to build on a strong foundation of prior work while testing for previously overlooked factors that influence consumption behavior. Consistent findings would strengthen the external validity of our results and further suggest that anticipation-dependence has the potential to reconcile seemingly conflicting evidence on whether consumption responds to "anticipated" payments.

Our 2008 U.S. tax rebate setting has the advantage of being a large-scale natural experiment with variation in anticipation duration and high-frequency spending data in an important policy-relevant setting. While other settings lack some of these advantages, a thorough examination of the role of anticipation necessitates a systematic search of the literature for other spending/saving datasets with variation in anticipation duration.

We arrive at two additional settings that consist of exogenous variation in when households learn about a windfall payment relative to when they receive it.[33] Both stem from randomized controlled trials (RCTs) on unconditional cash transfers, one in Kenya (Haushofer and Shapiro, 2016) and the other in Malawi (Brune et al., 2017), with the Kenya setting having variation in both announcement and payment timing. 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. Unlike the U.S. stimulus payments, these programs operate in settings where households typically face greater liquidity constraints, more volatile income streams, and limited access to formal financial institutions. These factors undoubtedly shape spending responses to windfalls. Nevertheless, observing anticipation-dependence in these settings would reinforce its role as a general feature of consumption behavior rather than a byproduct of particular institutional or economic conditions.

Finally, we revisit a meta-analysis of the literature due to Havranek and Sokolova (2020)

[33]We document these systematic efforts by a pair of research assistants in Supp. Appendix A.

that covers four decades of research estimating MPCs to examine the explanatory power of anticipation duration in accounting for spending responses to windfall payments.

5.1 Cash transfers in Kenya

5.1.1 Setting

This section re-analyzes data from the Haushofer and Shapiro (2016) RCT, which evaluates the impacts of unconditional cash transfers by GiveDirectly in rural Kenya on a wide range of outcomes including assets and consumption. Out of 503 households randomized into receiving transfers, 193 households received one-time lump-sum payments.[34] The magnitude of these one-time payments equates to about six months of revenue for the average household.

We use random variation in both announcement timing and payment dates among households in the lump-sum treatment to estimate the impact of anticipation duration. Households learned of the transfers when a GiveDirectly representative visited between May 20, 2011 and December 12, 2011 to announce 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 (with some households waiting as little as three days for their payment, depending on the timing of the visit within the month). 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 number of days households waited for their transfer payments—to examine how anticipation affects decision-making.

The outcome measures come from an endline survey which takes place about 14 months after the baseline survey, and our sample consists of 172 households.[35] We consider four broad outcome measures: savings, assets (e.g., livestock), durables (e.g., furniture, agricultural tools, appliances), and investments (e.g., enterprise expenses, educational expenses).[36]

[34]Treated households received either KES 25,200 (USD 404 PPP) or KES 95,200 (USD 1,525 PPP). Among the 366 households receiving the smaller amount, 173 received monthly transfers over nine months instead of a lump sum. The 137 households receiving the larger amount also received the bulk of their payments on a monthly basis (see Supp. Appendix D.1). 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.

[35]The attrition and non-compliance rates in our sample are slightly lower than in the complete sample of 1,008 households. See Supp. Appendix D.1 for further information about the samples.

[36]While Haushofer and Shapiro (2016) analyze outcome variables in both levels and inverse-hyperbolic-sine form to demonstrate robustness, we exclude the latter due to the findings demonstrated in Thakral and Tô (2025). See Supp. Appendix D.1 for additional details on the outcomes.

5.1.2 Empirical strategy

We follow the econometric strategy in Haushofer and Shapiro (2016), using an analysis of covariance (ANCOVA) approach (Frison and Pocock, 1992) that conditions on baseline levels of the outcome variables to improve statistical power. Our main specification examines the effect of variation in anticipation duration on a time scale comparable to that of the setting in Section 3. Letting T_{vh} indicate a waiting time of four weeks or less since the announcement, we estimate

$$y_{vh}^{E} = \alpha_v + \beta T_{vh} + \gamma y_{vh}^{B} + \varepsilon_{vh}, \tag{6}$$

where y_{vh}^{t} 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, and ε_{vh} is an idiosyncratic error term. The parameter β represents the causal impact of a waiting time of four weeks or less relative to a longer waiting time. We test the null hypothesis of no anticipation-dependence from Section 2, which corresponds to $\beta = 0$.[37]

Treatment variable: Waiting time. While our primary specification uses a four-week threshold to define the treatment, the setting includes a waiting period of up to nine months, which allows us to explore a much broader range. A binned scatterplot of the outcomes across the entire nine months of possible waiting times shows that households facing the shortest waiting times exhibit especially strong reductions in endline savings, assets, durables, and investments (Figure A7).[38] The figure also lends support to the interpretation that the estimates reflect the impact of differences in waiting times rather than differences in the timing of the endline survey relative to the transfer. 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 have expected to see a linear or convex increasing relationship between anticipation duration and the various outcomes. Our data show the opposite pattern, with increases in waiting times leading to large changes in endline outcomes for households with the shortest waiting times and insignificant changes for households with longer waiting times, which points against an important role for elevated consumption time in explaining endline outcomes.[39]

[37] 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 model can explain excess smoothness and sensitivity but not excess anticipation-dependence. Thus, our focus here is on the latter.

[38] The binned scatterplots use the rule-of-thumb integrated-mean-square-error optimal estimator of the number of bins (Cattaneo et al., 2019); see Figure S2 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 (Figure S3), as plotting baseline outcomes shows evidence of balance (Figure S4).

[39] To elaborate, suppose that the difference in endline outcomes between households with varying waiting times solely reflects differences in time having elevated consumption due to the transfers. In this case, the

5.1.3 Results

The main results, comparing households who wait four weeks or less for their payment with those who wait longer, support the hypothesis that shorter waiting times lead to significant reductions in future-oriented decision-making (Figure 2). 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 receiving transfers in the first month after the announcement exhibit strong reductions in endline savings, assets, durables, and investments, on the order of about one to two months of average revenue.[40]

Estimating the impact of a shorter waiting time with different definitions of the treatment group (waiting time below a cutoff which varies between 2 weeks and 8 weeks) and the comparison group (waiting time up to a maximum which varies between 3 months and 9 months) gives a more comprehensive view of the data. 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 (Figure A8).[41] Varying waiting times in the comparison group between 3 and 9 months does not affect our results, consistent with the evidence in the raw data as shown in Figure A7.

Interpretation of results. We find no systematic differences in the impact of short waiting times across various subsamples (Figure 2), which helps rule out potential alternative explanations for our results. 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.[42] We investigate the importance of intrahousehold interactions by examining heterogeneity by the gender of the randomly assigned recipient of the transfer,

difference between households waiting 4 months and households waiting 1 month to receive their transfer represents three months of elevated consumption, and the difference between households waiting 8 months and households waiting 5 months also represents three months of elevated consumption. Since the endline survey takes place about 14 months after baseline, the former constitutes the difference between having 10 and 13 months of elevated consumption, while the latter constitutes the difference between having 6 and 9 months of elevated consumption. Since consumption responses decay over time (e.g., Auclert et al., 2024), the former would be smaller than the latter, producing a convex increasing relationship between waiting time and endline outcomes.

[40] We also document similar patterns for other outcomes variables: value of savings, durable investment, non-durable investment, and total assets including non-thatched roofs (Figure S5).

[41] We obtain similar results when using the difference between the endline and baseline measure as the outcome variable (Figure S6) and when extending Equation (6) to add quadratic controls for baseline outcomes (Figure S7) or remove village fixed effects (Figure S8).

[42] 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 A7 shows that the latter is much larger.

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. In each case, we obtain estimates of roughly the same magnitude as the estimates from the full sample.

5.2 Cash transfers in Malawi

5.2.1 Setting

This section re-analyzes data from the Brune et al. (2017) RCT, which examines 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 randomly vary whether households receive transfer payments of MK 25,000 (USD 176.50 PPP) via cash or direct deposit in March–April 2014.[43] 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). 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 5.1 show effects, as well as on a significantly shorter time horizon that the previous settings did not allow us to examine.[44]

[43] We convert to USD values at purchasing power parity using the factor 141.64 MK/USD (Brune et al., 2017).

[44] A distinction between our analysis and that of Brune et al. (2017) is that their 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. We discuss in Appendix A.3 how consumption responses to payment delays do not necessarily provide a test of time-inconsistent preferences or quasi-hyperbolic discounting.

5.2.2 Empirical strategy

To obtain the causal impact of anticipated payment delays, we estimate an analog of Equation (6) as in Brune et al. (2017):

$$y^E_{vwh} = \alpha_v + \beta_1 T^1_{vwh} + \beta_8 T^8_{vwh} + \gamma y^B_{vwh} + \delta_w + \varepsilon_{vwh}, \tag{7}$$

where y^t_{vwh} represents the baseline ($t = B$) or endline ($t = E$) outcome of interest (various forms of savings) 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, which corresponds to $\beta_1 = \beta_8 = 0$.

Outcome variable: Savings versus consumption. Forms of savings and consumption in Malawi merit discussion, as evidence on the difficulty of increasing formal savings suggests that households prefer to save using other means (Brune et al., 2016). Malawi's Third Integrated Household Survey (IHS-3), implemented by the Government of Malawi through the National Statistical Office (NSO) roughly every five years, includes questions on various forms of savings, including in-kind savings such as advance purchases of farm inputs, business inventory, and bags of maize (as shown in Figure S9). To distinguish between consumption and in-kind savings of maize, the main staple crop in Malawi, the original question in the IHS-3 questionnaire specifically asks how much maize households *consume* ("food both eaten communally in the household and that eaten separately by individual household members") over the past seven days (Figure S10).

The survey from Brune et al. (2017), which each household completes one week after their transfer payment date, consists of questions inspired by the IHS-3. Unlike the IHS-3, this survey contains a savings module and an expenditure module, rather than distinguishing between in-kind savings and consumption. In particular, the survey asks how much households *paid in total* for various consumption goods, including maize, over the past seven days (Figure S11). Since total expenditure can encompass in-kind savings in addition to consumption, we focus on various forms of savings in our analysis.

5.2.3 Results

The main results show that an anticipated eight-day payment delay significantly increases total savings (Table 2), corroborating the findings from Sections 3 and 5.1 that short delays on the order of a week can lead to greater savings. Consistent with households having difficulty with formal savings, the increase arises predominantly due to in-kind savings.[45]

[45]While (Brune et al., 2016) find that "[f]acilitating formal savings leads to higher deposits into formal savings accounts at the partner bank," they also note that "the majority of these funds were withdrawn almost

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. 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.

An eight-day delay leads to a USD 137.95 PPP increase in in-kind savings out of the USD 176.50 PPP cash transfer, a substantial amount relative to the USD 1.46 PPP daily consumption of an average rural household in central Malawi (Brune et al., 2016). The increase in total savings in response to the eight-day delay aligns with the finding by Brune et al. (2017) of a decrease in total expenditure; the reported decrease is relatively small in magnitude and not statistically significant because the measure includes maize expenditure, a form of in-kind savings, which increases in response to the eight-day payment delay.[46]

Interpretation of results. We find consistent effects across various subsamples, providing little support for alternative explanations, as in Section 5.1.3. 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 (Table S5), which suggests that the results are not driven by waiting times enabling households to find ways to save. Our results hold across married and unmarried households (Table S6) as well as large and small households (Table S7), 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.

5.3 Meta-analysis

To more systematically assess the role of the time horizon over which households anticipate changes in income, we draw on data from a recent meta-analysis compiled by Havranek and Sokolova (2020). We analyze the MPC estimates from the "strong studies," defined as those that "identify a causal parameter from plausibly exogenous variation."[47] As in our paper, "these studies exploit largely cross-sectional variation and employ data for actual observed income changes" to estimate the MPC "out of expected and temporary payments" (Havranek and Sokolova, 2020). Many studies report the number of months between the time when consumers learn about the income change and the time when the income change occurs, including those that focus on stimulus payments, tax refunds, and dividend payments.[48] Some studies involve "long-anticipated payments" such as changes in tax policy

immediately after being deposited."

[46]Given that one kilogram of maize in Malawi costs less than 1 USD (Caracciolo et al., 2014), the effect of payment delays on in-kind savings in Table 2 cannot arise due to short-run consumption of maize.

[47]We omit studies that aggregate different income changes or duplicate settings already in the data.

[48]The Johnson et al. (2006) analysis of the 2001 tax rebates involves staggered payment timing, but we use the reported aggregated estimates (see their Table 2). Separately, when we estimate the contemporaneous response of expenditures to the tax rebate for households receiving payments in different months, we find a decreasing relationship between nondurable spending and the month in which each household received their

announced a year in advance, paycheck income, and loan repayment terms.

We display the relationship between anticipation duration and estimated MPCs across a variety of specifications in Figure 3. While acknowledging that unobserved heterogeneity across settings can bias the results in either direction, the data show a decreasing relationship between anticipation duration and MPC estimates among studies with short anticipation durations. Additionally, studies with long-anticipated payments have lower estimated MPCs that are close to zero. We report results for subsamples that restrict to the most precise estimates (MPC standard error less than 0.3, 0.2, and 0.1); see the discussion of publication bias by Havranek and Sokolova (2020). The decreasing relationship continues to hold after adjusting for relevant covariates such as the payment amount (to account for the magnitude hypothesis), the standard error of the MPC estimate, whether the estimates reflect samples for which liquidity constraints bind, and whether the estimates reflect total consumption, food, or another specific category of consumption (to account for differences across settings). This relationship shows a smaller magnitude than in our primary analysis, reflecting several factors. First, measurement error in anticipation duration tends to bias the meta-analysis toward a smaller effect. Determining exactly when anticipation starts in different settings poses challenges because, for instance, payments announced two months in advance might not become salient to recipients until much closer to disbursement, as seen with the 2008 stimulus payments (Figure A1). Second, the relationship between anticipation and MPC is likely steepest at shorter durations (as our data from Section 5.1 corroborates), which our primary analysis focuses on, while the meta-analysis covers a broader range.

Three additional analyses extend and support these results (Supp. Appendix E). First, we analyze the effect of additional anticipation time on the estimated MPC, focusing on studies for which the number of months of anticipation can be determined. For each specification, we report results for the full sample of "strong studies" as well as subsamples that restrict to the most precise estimates. Across all six samples and the different specifications of controls as before, we find a consistent negative relationship between anticipation duration and the estimated MPC (Table S8). Second, a binary comparison between estimates from settings with long versus short anticipation durations confirms the patterns above (Table S9). Finally, we plot the relationship between estimated MPCs and payment magnitudes separately for long and short anticipation durations (Figure A9). The results reveal consistently higher MPC estimates for shorter anticipation durations across all payment amounts, while showing less conclusive evidence on the magnitude hypothesis once anticipation duration is accounted for.

payment; however, these estimates have large standard errors due to the small sample size.

6 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, 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.

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.

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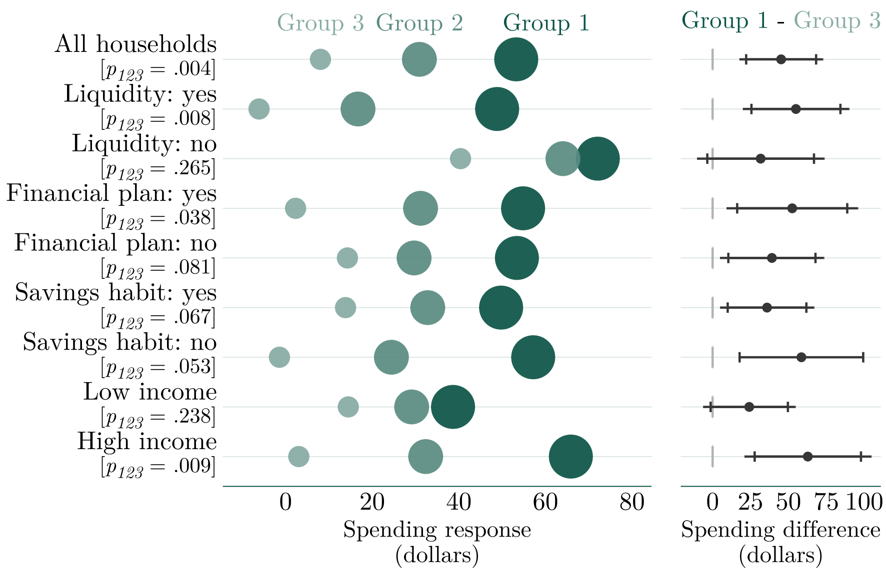
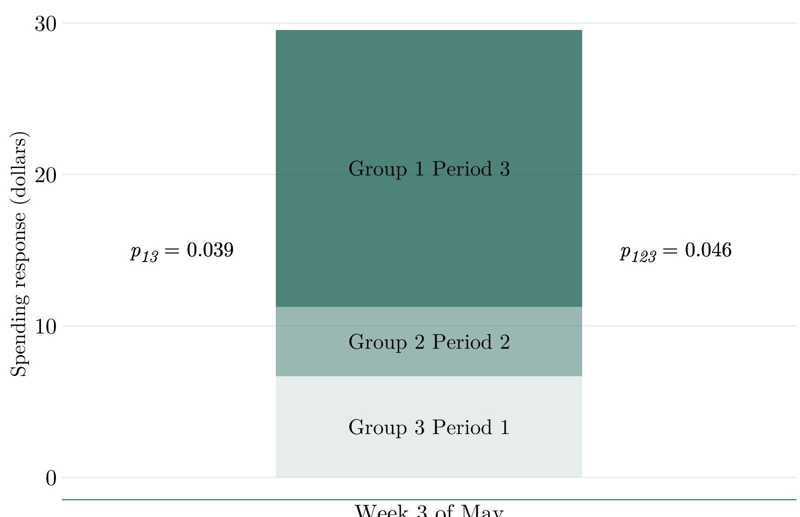
Figure 1: ESP Spending Responses by Timing of Payment

(a) *Four-week cumulative spending response*



(b) *Fixing calendar week*



Note: Figure 1a: The left panel shows four-week cumulative ESP spending response estimates (Γ_4^w from Equations (1) and (2)) 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. The p-value (p_{123}) corresponds to the null hypothesis of equality across groups. The right panel compares spending between Groups 1 and 3, with 95 percent (black line) and 90 percent (vertical endpoints) confidence intervals. Liquidity is an indicator for having at least two months of income in easily accessible funds. Financial plan is an indicator for a household having gathered together its financial information, reviewed it in detail, and formulated a financial plan for the long-term future. Savings habit is an indicator that household members would rather save more for the future than spend their money and enjoy it today. Low/high income refers to incomes below/above $50,000.

Figure 1b: The bar segments depict the spending response in the third week of May for households receiving EFTs in the first (γ_1^3), second (γ_2^2), and third (γ_3^1) payment groups, respectively, from Equations (1) and (2). 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.

All standard errors are adjusted for clustering at the household level and obtained from a block-bootstrap procedure with 100 replicates.

Table 1: Mental Accounting Model—Estimates and Fit

Scaling factor	3.33	1	5	7.3	9.4
Parameter estimates					
α (time)	0.4284	0.1893	0.4928	0.5490	0.5847
	(0.0398)	(0.0382)	(0.0418)	(0.0449)	(0.0477)
β (magnitude)	0.9988	0.9983	0.9989	0.9991	0.9992
	(0.0002)	(0.0003)	(0.0002)	(0.0002)	(0.0002)
Waiting equivalent	681.56	978.27	667.28	661.86	662.80
	(172.01)	(301.15)	(183.60)	(207.44)	(232.37)
Predicted monthly NCP spending					
Group 1 (actual: 53.26)	54.66	40.53	53.14	49.37	45.89
Group 2 (actual: 30.89)	23.79	7.67	26.94	28.32	28.38
Group 3 (actual: 8.05)	10.19	1.44	13.37	15.80	17.00

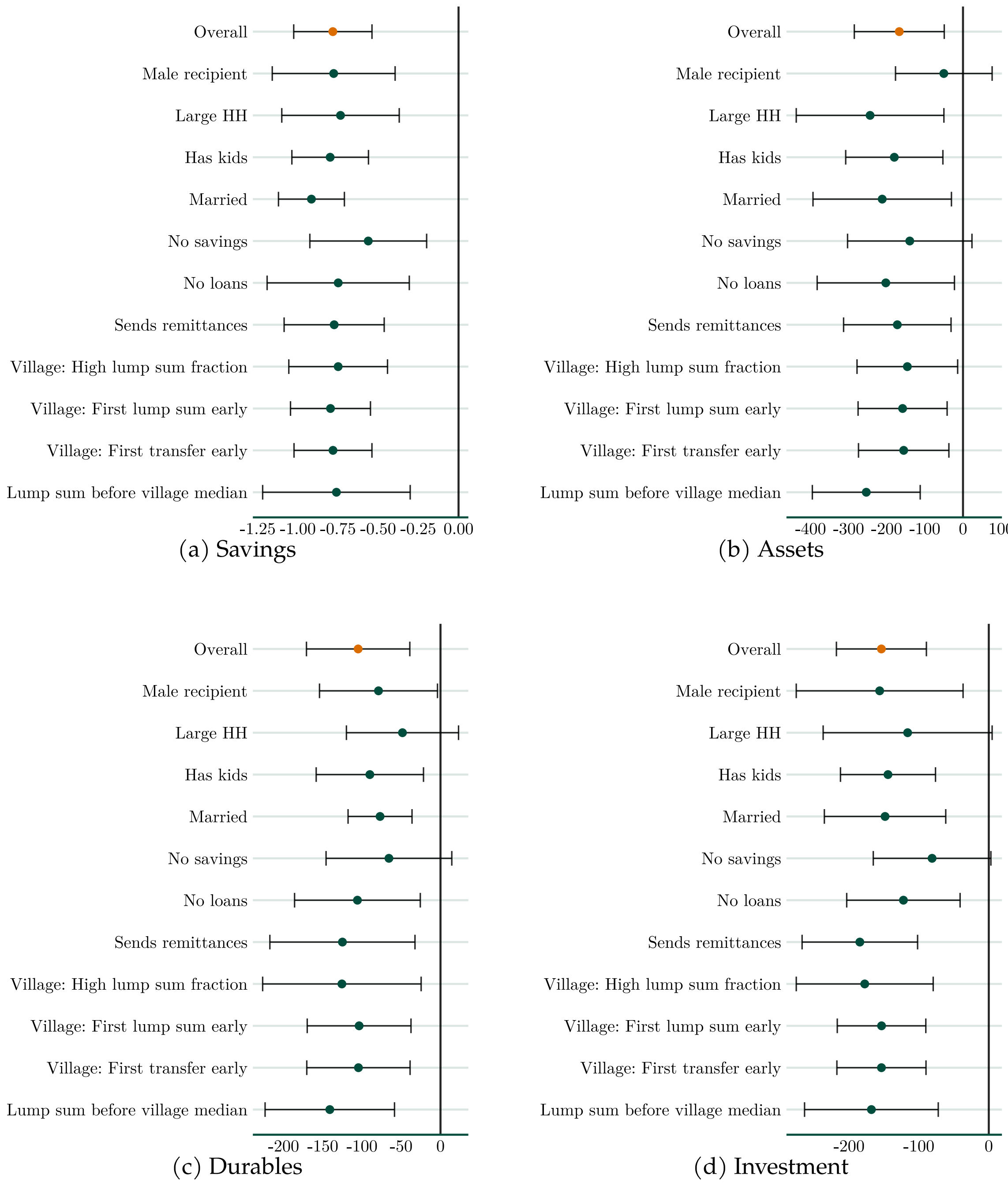
Note: Each column presents estimates of the model defined by Equations (3) and (4) for a different scaling factor. The top panel shows parameter estimates from Equation (5), and the waiting equivalent is computed as $\log(\alpha)/\log(\beta)$. The bottom panel displays the excess NCP spending implied by the model. Standard errors reported in parentheses are adjusted for clustering at the household level.

Table 2: 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 (7) with the outcome variable as a measure of savings (all values reported in USD PPP adjusted using the 2014 exchange rate 141.64 MK/USD). The sample includes 474 households receiving MK 25,000 (USD 176.50 PPP) windfalls: 156 receiving payments via cash or direct deposit without delay, 158 receiving payments after a one-day delay, and 160 receiving payments after an eight-day delay. Column 1 presents estimates of the causal impact of a one-day windfall delay vs. immediate receipt (β_1), and column 2 presents estimates the impact of an eight-day delay vs. immediate receipt (β_8). Column 3 reports the p-value for the null hypothesis of no difference among treatments ($\beta_1 = \beta_8 = 0$). Formal savings include balances in NBS bank accounts, other bank or microfinance institution accounts, and employee-based Savings and Credit Cooperatives (SACCOs). Informal savings consist of Rotating Credit and Savings Association (ROSCA) balances, village savings clubs, and extra cash stored for safe keeping. In-kind savings consist of advance purchases of farm inputs, business inventory, and bags of maize. Formal and informal savings constitute total financial assets, which forms total savings when combined with in-kind savings. Standard errors are reported in parentheses. The data come from the survey questions displayed in Figure S9.

Figure 2: Impact of Shorter Wait for Cash Transfers (Kenya): Four weeks wait



(a) Savings

(b) Assets

(c) Durables

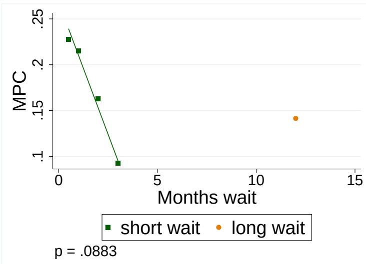
(d) Investment

Note: Each figure depicts estimates of the treatment effect, β_k, from Equation (6) 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 5.1.3 for details on the samples.

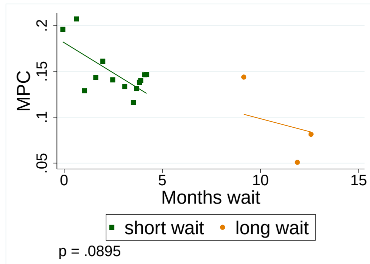
Figure 3: Relationship between Anticipation Duration and MPC Estimates (Meta-Analysis)

Panel A: MPC standard error less than 0.3

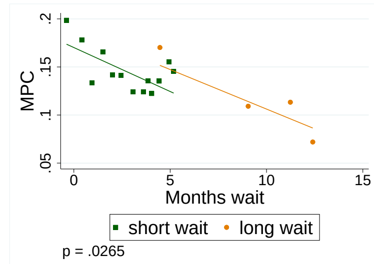
(a) Controls: None



(b) Add s.e., payment size

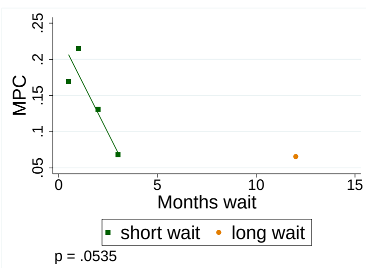


(c) Add liquidity, category

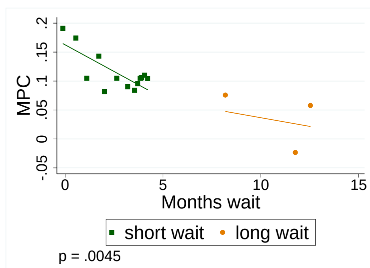


Panel B: MPC standard error less than 0.2

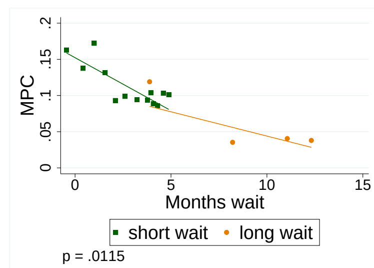
(d) Controls: None



(e) Add s.e., payment size

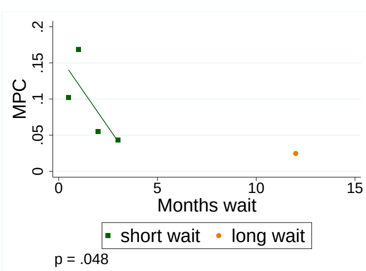


(f) Add liquidity, category

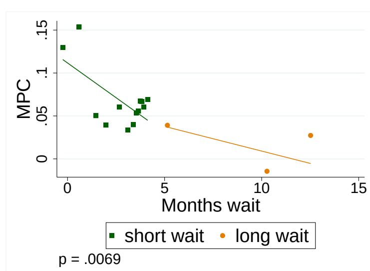


Panel C: MPC standard error less than 0.1

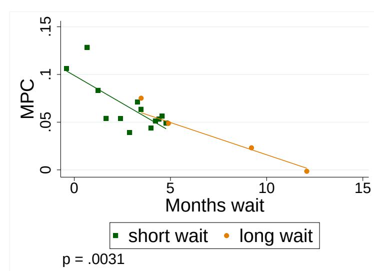
(g) Controls: None



(h) Add s.e., payment size



(i) Add liquidity, category



Note: Each figure depicts the relationship between anticipation duration and the estimated MPC. The first, second, and third rows consist of the subsamples of MPC estimates with standard errors less than 0.3, 0.2, and 0.1, respectively. The first column does not include any control variables. The second column includes controls for payment amount and MPC standard error. The third column adds controls for whether the estimates reflect samples for which liquidity constraints bind or not, and whether the estimates reflect total consumption, food, or another specific category of consumption. The *p*-value corresponds to a test of the null hypothesis of no relationship between anticipation duration and the estimated MPC among studies with short anticipation durations. See Supp. Appendix E and Havranek and Sokolova (2020) for further details.

A Online Appendix

A.1 Possible explanations for time dependence of MPCs

As Section 2 discusses, 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. This section describes several possible channels through which MPCs may exhibit such history dependence. We discuss how the passage of time may affect attitudes toward spending through multiple possible channels: adaptive reference points; preference for early self-control; internal commitments, plans, and goals; attention; anticipatory utility; and uncertainty about optimal actions. These channels provide possible interpretations for the reduced-form dependence of the MPC μ on the time dimension in the model in Section 4. We emphasize, however, that these channels alone do not suffice to explain our results, though they can operate in conjunction with mental accounting to explain the key patterns in the data. In particular, these models do not provide an explanation for why consumers with liquidity would not spend in response to news about a future windfall, which motivates the mental accounting framework.

Suppose the decision maker learns in period 0 about a windfall arriving in period s. We denote consumption t periods after learning about the period-s windfall by $c_{t,s}$, and we omit the s subscript at times for ease of notation. The utility from consumption t periods after learning about the period-s windfall is denoted $u(c_t)$. We impose the mental accounting assumption that $c_t = 0$ for $t < s$. The consumer chooses c_t each period $t \geq s$ to equate the marginal utility of consumption, $u'(c_t)$, with the marginal utility of lifetime income, denoted by λ.

We will describe how each of the models below can contribute to explaining the following results (where in each case the results apply on the domain $t \geq s$):

Result 1. More time between learning about a windfall and receiving it would lead to lower consumption responses ($c_{t,t}$ is decreasing in t), and

Result 2. The consumption response decreases with time since receiving the payment ($c_{t,s}$ is decreasing in t).

In addition, we note that the data show that consumption responses among households in the earliest payment group remain elevated in later weeks, compared to households receiving payments in those later weeks: Holding $t + s$ fixed, $c_{t,s}$ is decreasing in t. Figure 1b documents this pattern. As Section 4.4 discusses, this pattern arises from the interaction between the dependence of the MPC on magnitude and duration in Equations (3) to (5). The models in this section focus instead on describing channels through which the MPC depends on duration.

A.1.1 News utility

The passage of time may result in long-anticipated windfalls feeling more like wealth due to reference-point effects. Specifically, a model of news utility (Kőszegi and Rabin, 2009) can generate Results 1 and 2 above. Before proceeding, we note that the data show no anticipatory spending response, even for liquidity-unconstrained households, whereas the Kőszegi and Rabin (2009) model predicts immediate increases in consumption following news about future windfalls; thus, the mental-accounting assumption is crucial.

The utility function in models of reference dependence has the form

$$\sum_{t \geq s} (m(c_t) + n(m(c_t))), \tag{8}$$

where $m(\cdot)$ denotes consumption utility and $n(\cdot)$ denotes reference-dependent gain-loss utility. This results in the following first-order conditions:

$$m'(c_t)\left[1 + \frac{\partial n(m(c_t))}{\partial c_t}\right] = \lambda$$

To see that c_t is decreasing in t, it suffices to show that $\frac{\partial n(m(c_t))}{\partial c_t}$ is decreasing in t.

In the Kőszegi and Rabin (2009) model, the decision maker learns in period 0 about the windfall arriving in period s. Since the decision maker was not previously aware of this windfall, we set the initial reference point as 0. Upon receiving the news, the reference point adjusts instantaneously to $(c_t)_{t \geq s}$. The decision maker experiences prospective gain utility of

$$\sum_{t \geq s} \gamma^t \eta m(c_t),$$

where $\gamma < 1$ indicates that "prospective gain-loss utility does not loom as large as contemporaneous gain-loss utility" (Kőszegi and Rabin, 2009). Thus, the utility function in Equation (8) has the form $n(m(c_t)) = \gamma^t \eta m(c_t)$. Since $\gamma < 1$, we conclude that $\frac{\partial n(m(c_t))}{\partial c_t} = \gamma^t \eta m'(c_t)$ is decreasing in t as desired.

Finally, we note that this model implies $c(t_1, s_1) = c(t_1, s_2)$ for $t_1 \geq \max\{s_1, s_2\}$, which does not explain the distinction between time spent waiting for a windfall and time that elapses after receiving the windfall (Figure 1b). Explaining this additional result would require modifying the model to incorporate gradual updating of the reference point (Thakral and Tô, 2021) and the idea that the reference point for consumption updates more during waiting periods than during consumption periods (see Heffetz 2021 for evidence that reference-point updating occurs when decision makers internalize changes in expectations).

A.1.2 Preference for early self-control

A model of temptation disutility in which decision makers find it easier to exert self-control about consumption further in the future (Noor, 2007) can also generate the time dependence of the MPC. Fudenberg and Levine (2012) describe this intuition: "If the temptation will arrive fairly soon, there is not much point in trying to avoid it by making a commitment now, as the temptation is already important for the short-run self, so committing to reject it is about as costly as waiting and resisting it when it arrives. On the other hand, it is less costly to commit now to resist temptations that will arrive in the distant future."

The utility function in models with temptation costs has the form

$$\sum_{t \geq s} (m(c_t) - n(w, C_t)), \tag{9}$$

where $m(\cdot)$ denotes consumption utility, $n(\cdot)$ represents temptation disutility, w is the size of the windfall and C_t is cumulative consumption given by $C_t = \sum_{\tau < t} c_\tau$. This results in the following first-order conditions:

$$m'(c_t) - \sum_{\tau > t} \frac{\partial n(w, C_\tau)}{\partial c_t} = \lambda$$

To see that c_t is decreasing in t, it suffices to show that $\sum_{\tau > t} \frac{\partial n(w, C_\tau)}{\partial c_t}$ is increasing in t.

In the model of Noor (2007, 2011), agents experience temptation based on *future consumption*. The cost of temptation (Gul and Pesendorfer, 2004) is the maximized utility of the myopic short-run self. Since the myopic short-run self prefers to consume everything that has not yet been consumed, we take the temptation cost as $\delta^t m(w - C_t)$. Thus, the utility function in Equation (9) has the form $n(w, C_t) = \delta^t m(w - C_t)$. In this case, for any $\tau > t$, we have $\frac{\partial n(w, C_\tau)}{\partial c_t} = \frac{\partial \delta^t m(w - C_t)}{\partial c_t} = -\delta^t m'(w - C_t)$. We conclude that $\sum_{\tau > t} \frac{\partial n(w, C_\tau)}{\partial c_t}$ is increasing in t as desired.

A.1.3 Internal commitments, plans, and goals

A model in which more time makes it more likely for consumers to be able to formulate and stick to a forward-looking plan can also explain the dependence of the MPC on the time dimension. If consumers have some flexibility in deciding how to earmark windfall income, having more time may allow the decision maker to mentally commit not to overspend.

This can be modeled with a utility function of the form

$$\sum_{t \geq s} \Big(m(c_t) - L(t)(c_t - g) \mathbb{1}_{\{c_t > g\}} \Big), \tag{10}$$

where $m(\cdot)$ denotes consumption utility, $L(t)$ is the cost of deviating from a goal g which the decision maker has held for t periods, with $L'(t) > 0$ to capture the idea that it is more costly to deviate from a long-held plan or commitment. This results in the following first-order conditions:

$$m'(c_t) - L(t) = \lambda$$

The conclusion that c_t is decreasing in t follows from $L(t)$ being increasing in t.

A.1.4 Attention

A model in which the context distorts the weight that the decision maker puts on each dimension of utility (each period of consumption) as in Bordalo et al. (2020) can also account for the result that MPCs depend on timing.

The utility function in models of attention and salience has the form

$$\sum_{t \geq s} \sigma(t) m(c_t), \tag{11}$$

where $m(\cdot)$ denotes consumption utility and $\sigma(\cdot)$ denotes the decision weight, with $\sigma'(t) < 0$ to capture the idea that recent changes draw more attention from the decision maker. This results in the following first-order conditions:

$$\sigma(t) m'(c_t) = \lambda$$

The conclusion that c_t is decreasing in t follows from $\sigma(t)$ being decreasing in t.

A.1.5 Anticipatory utility

Anticipatory utility predicts a relationship between waiting times and future-oriented decision-making, as more time allows consumers to enjoy savoring future consumption, leading to more patient decision making. Thakral and Tô (2020); Thakral (2022) provide more details on this general result. For simplicity, we describe their result using an example with a windfall of size w that the decision maker learns about in period 0 and can be spent over two consumption periods: periods 1 and 2 in the first scenario (corresponding to $s = 1$), and periods 2 and 3 in the second scenario (corresponding to $s = 2$).

When the windfall becomes available to spend in period $s = 1$, flow utility takes the form

$$\begin{aligned} u_0 &= n\left(\alpha_1^0 \,\middle|\, 0\right) + n\left(\alpha_2^0 \,\middle|\, 0\right) \\ u_1 &= m_1(c_1) + n\left(m_1(c_1) \,\middle|\, \alpha_1^0\right) + n\left(\alpha_2^1 \,\middle|\, \alpha_2^0\right) \\ u_2 &= m_2(c_2) + n\left(m_2(c_2) \,\middle|\, \alpha_2^1\right), \end{aligned}$$

where $m(c_t)$ denotes period-t consumption utility and $n\left(\alpha_\tau^t \,\middle|\, \alpha_\tau^{t-1}\right)$ denotes the utility from adjusting anticipation of period-τ consumption to α_τ^t from α_τ^{t-1}, which has the shape of a prospect-theoretic value function. The initial anticipation level (α_1^0, α_2^0) is set at the level that maximizes the decision maker's flow utility from anticipation, and anticipation levels are otherwise chosen to maximize utility. In each period t, the agent maximizes $U_t = \sum_{t' \geq t} u_{t'}$. Since gain utility is concave, optimal anticipation levels are smooth. The initial anticipation levels are therefore given by $(\alpha_1^0, \alpha_2^0) = \left(\frac{w}{2}, 0\right)$, and the subsequent anticipation levels are $(\alpha_1^1, \alpha_2^1) = \left(c_1, \frac{w-c_1}{2}\right)$. The optimal choice of c_1 satisfies

$$c_1 - \frac{w}{2} = \frac{w - c_1}{2}$$

which implies $c_{1,1} = \frac{2}{3}w$.

When the windfall becomes available to spend in period $s = 2$, flow utility takes the form

$$\begin{aligned}
u_0 &= n\big(\alpha_2^0 \,\big|\, 0\big) + n\big(\alpha_3^0 \,\big|\, 0\big) \\
u_1 &= n\big(\alpha_2^1 \,\big|\, \alpha_2^0\big) + n\big(\alpha_3^1 \,\big|\, \alpha_3^0\big) \\
u_2 &= m_2(c_2) + n\big(m_2(c_2) \,\big|\, \alpha_2^1\big) + n\big(\alpha_3^2 \,\big|\, \alpha_3^1\big) \\
u_3 &= c_3 + n\big(c_3 \,\big|\, \alpha_3^2\big).
\end{aligned}$$

The initial anticipation levels in this case are given by $(\alpha_2^0, \alpha_3^0) = \left(\frac{w}{3}, 0\right)$, and the subsequent anticipation levels are $(\alpha_2^1, \alpha_3^1) = \left(\frac{c_2 + \frac{w}{3}}{2}, \frac{w - c_2}{3}\right)$ and $\alpha_3^2 = 2\frac{w-c_2}{3}$. The optimal choice of c_2 satisfies

$$\frac{c_2 - \frac{w}{3}}{2} = \frac{w - c_2}{3}$$

which implies $c_{2,2} = \frac{3}{5}w < c_{1,1}$.

This model also provides insight more generally about consumption responses to gains, losses, news about gains and losses (Fuster et al., 2021). Regarding gains, the model predicts consumption responses to gains (as shown above) but no response to news about future gains (due to mental accounting). Regarding losses, the model predicts the same response to losses as to news about future losses: since anticipatory utility is convex in losses, the decision maker optimally chooses to anticipate the entirety of a loss, whether future or current, at the same time. Thus, the model can explain why consumers react to losses (or to news about losses) by decreasing current consumption, and why the response to losses would be larger than the response to gains, as Fuster et al. (2021) document in their survey about hypothetical spending scenarios.

A.1.6 Uncertainty about optimal actions

If the decision maker is uncertain about the optimal consumption response, then the first-order condition for intertemporal optimization holds in expectation:

$$\mathbb{E}[m'(c_t + \epsilon_t)] = \lambda,$$

where $m(\cdot)$ denotes consumption utility, and ϵ_t is an error term with mean 0 and variance $\sigma(t)$ with $\sigma'(t) < 0$ capturing the idea that uncertainty is resolved over time. For example, the decision maker may internally reflect or reason about the optimal consumption response by producing noisy and unbiased signals about the optimal action and updating their beliefs over time (Ilut and Valchev, 2023).

Under the assumption of prudence (Kimball, 1990), m' is convex. By Jensen's inequality, the expected marginal utility associated with the level of consumption that the decision maker would choose under certainty is too large compared to λ, which pushes the optimal consumption level under uncertainty to be higher. The conclusion that c_t is decreasing in t follows from $\sigma(t)$ being decreasing in t.

A.2 Alternative explanations for time dependence

This section discusses other models that incorporate backward-looking elements but do not predict the patterns in our data.

A.2.1 External commitments

One attempt to explain the result that a longer anticipation duration leads to lower spending would be that more time enables households to seek external commitments to save. Four facts in the data cast doubt on the plausibility of this explanation.

First, we observe similar effects for households that do not face binding liquidity constraints (Figure 1a). In particular, these households do not spend in advance of receiving the windfall, even though they have the means to do so. This suggests that they must already have access to ways of committing not to spend.

Second, we find similar effects for households that have different levels of access to formal savings at baseline (Figure 2). To the extent that households with access to formal savings are more likely to have access to external commitments to save, the evidence suggests that the channel of having more time to access savings is not responsible for generating our results.

Third, since the US stimulus payment timeline is announced in March, if households value commitment, then regardless of whether they are scheduled to receive payments in the first, second, or third week of May, they would all have ample time to seek such external commitments. In that case, since the probability of securing an external commitment is likely a concave function of time, we would expect to see little difference between the three payment groups.

Finally, and perhaps most importantly, having more time to seek external commitments would not explain why households in the earliest payment group continue to spend more in later weeks compared to households that receive their payments in those later weeks. By that time, the households in the earliest payment group would have had as much time to seek external commitments, yet they spend more.

A.2.2 Intertemporal consumption complementarities

Habit formation (Ryder and Heal, 1973) or other forms of intertemporal complementarities in consumption could explain why groups with higher initial spending responses continue to spend at a higher rate (Figure 1b) but would not explain why higher initial spending responses emerge for earlier payment groups (i.e., Result 1 in Appendix A.1).

Models of consumption commitments also predict that the timing of income conditional on the announcement date should not affect consumption; the Chetty and Szeidl (2016) result on excess sensitivity is based on the idea that households eventually adjust their commitments, with delayed updating arising due to adjustment costs, but not tied to the timing of income.

A.2.3 Myopia

Gabaix and Laibson (2017) propose a theory of myopia and discounting in which decision makers to estimate the value of future rewards using mental simulations. If waiting enables decision makers to make more precise forecasts, this potentially explains why longer waiting times lead decision makers to prefer larger, later rewards over smaller, sooner rewards. However, as DeJarnette (2020) points out, this prediction only holds definitively for the case of a single good; the Gabaix and Laibson (2017) model can accommodate effects in the opposite direction in the case of two different goods. In addition, several features distinguish the consumption-savings problem in our setting from work suggesting that waiting times increase patience by applying the Gabaix and Laibson (2017) model: decisions involve a large number of goods, decisions occur each period rather than at a single point in time, and decision-makers can choose to spend in advance of receiving a windfall.

See the section on "Uncertainty about optimal actions" in Appendix A.1 for a related potential explanation for the time dependence of the MPC.

A.2.4 Rational inattention

A model of rational inattention (Reis, 2006) provides another potential explanation for how anticipation duration interacts with intertemporal consumption plans. Since consumers update their plans infrequently, a greater time distance between the announcement and the payment increases the probability that the plan they hold upon receiving the ESP accounts for the news about the windfall.[49] Those who have not yet updated their consumption plans by the time they receive payment—which disproportionately consists of consumers in the earliest payment group—would not account for the windfall and therefore would save whatever remains after consuming their previously planned amount. Thus, this model predicts the opposite of the pattern we observe in the data.[50] Alternatively, if we view the financial crisis and stimulus payments as what Reis (2006) refers to as an "extraordinary event," then consumers in all payment groups would revise their plans, in which case the waiting time would not affect spending. Moreover, the result that consumption responses decline over time contrasts with the predictions of rational-inattention models.

A.2.5 Rational illiquidity

Gelman et al. (2022) provide an explanation for high MPCs out of annual income tax refunds: Taxpayers do not fully anticipate the extent of variability in non-paycheck income, which leads to overwithholding of income taxes in the case of negative income shocks. Their model does not make predictions about how the MPC varies with the timing of tax rebates. While a notion of anticipation plays an important role in their model to explain spending responses to tax refunds, the degree of anticipation in our setting pertains to the time duration rather than uncertain payment amounts.

A.3 Forward-looking behavioral models

This section elaborates on the inability of prominent classes of forward-looking behavioral models to explain the patterns in our data.

A.3.1 Discounted utility

If consumers maximize a discounted sum of future utilities, and no excess consumption takes place in advance of receiving a windfall, then the timing of news about the windfall does not affect consumption. Even if consumers receive windfalls at different times, they face identical intertemporal tradeoffs in discounted-utility models once the consumption opportunity arises. This holds even if decision makers have time-inconsistent preferences (e.g., quasi-hyperbolic or hyperbolic discounting): They may be myopic in the sense that they put additional weight on the present relative to future periods, but the history (how much time has elapsed since learning of the windfall) does not matter. Discounted-utility models (including models with quasi-hyperbolic or hyperbolic discounting) thus fail to predict a

[49]Longer waiting times would not matter for consumers with high planning costs since they rationally choose not to form consumption plans and therefore live "hand-to-mouth" by absorbing all income shocks through consumption.

[50]Another possibility might be that consumers receiving payments later update their consumption plans downward, e.g., because of new information on the severity of the crisis, which might lead to larger absolute spending responses for households in the earliest payment group. However, this does not apply to our difference-in-differences results in Section 3 because the households that we use to construct the counterfactual spending trend face the same macroeconomic conditions and would be just as likely to have adjusted their spending plan.

complementarity between waiting times and the value of saving. As a result, consumption responses to payment delays do not provide a test of time-inconsistent preferences or quasi-hyperbolic discounting.[51] Additionally, these models would have difficulty explaining why spending does not take place in advance of receiving payment for unconstrained households.

A.3.2 Temptation disutility

Our result that consumption responses do not occur until the receipt of payment contrasts with the predictions of the Gul and Pesendorfer (2004) model, in which temptation would induce consumers who are able to do so to start spending once they learn about the upcoming windfall.

A.3.3 Expectations-based reference dependence

As above, our result that consumption responses do not occur until the receipt of payment contrasts with the predictions of the Kőszegi and Rabin (2009) model, in which decision makers "increase immediate consumption following surprise wealth increases" to "generate pleasant surprises."

A.4 Sufficient statistics for welfare

We adopt a reduced-form modeling approach (Mullainathan et al., 2012) in which the intertemporal marginal rate of substitution captures how consumption deviates from the neoclassical benchmark. This object corresponds to the "behavioral wedge" that Farhi and Gabaix (2020) argue serves as a sufficient statistic for welfare analysis. We use a nonlinear least squares approach to provide estimates of this wedge using the NCP data and describe how the size of the wedge varies over time for different groups of households.

In the benchmark model (Supp. Appendix B), the intertemporal Euler equation is

$$u'(c_{i0}) = u'(c_{it}),$$

which implies consumption smoothing. Deviations from consumption smoothing can be captured by

$$\frac{u'(c_{i0})}{u'(c_{it})} = 1 + \eta,$$

where the parameter η corresponds to the "behavioral wedge" defined by Farhi and Gabaix (2020): the difference between the price and marginal utility vectors (assuming that the price of consumption is constant across periods).

[51] If the decision maker commits to an action at time 0 upon learning about the windfall, then waiting time treatments can test for hyperbolic discounting as in the lab experiment by Dai and Fishbach (2013). We address commitment-related explanations in Sections 3.6, 5.1.3 and 5.2.3.

We assume an exponential utility function $u(c) = \frac{1-e^{-ac}}{a}$, which gives

$$\begin{aligned} e^{-a(c_{i0}-c_{it})} &= 1+\eta \\ -a(c_{i0}-c_{it}) &= \log(1+\eta) \\ c_{it} &= c_{i0} + \frac{\log(1+\eta)}{a} \end{aligned}$$

We assume that idiosyncratic shocks may result in deviations between predicted and actual spending, so what we observe in the data is $\widetilde{c_{it}} = c_{it} + \epsilon_{it}$, where $\epsilon_t \overset{\text{i.i.d.}}{\sim} \mathcal{N}(0, \sigma^2)$. We then obtain a specification of the following form which can be estimated using nonlinear least squares:

$$\widetilde{c_{it}} = c_{i0} + \frac{\log(1+\eta)}{a} + \epsilon_{it}.$$

Motivated by our empirical findings in Section 3, we propose a simple parameterization to allow η to vary by the timing of payment

$$\eta = \eta_1 \mathbb{1}_{\{w=1\}} + \eta_2 \mathbb{1}_{\{w=2\}} + \eta_3 \mathbb{1}_{\{w=3\}}$$

where $w \in \{1,2,3\}$ denotes the payment group (first week of May, second week of May, or third week of May). The nonlinear least squares estimates are $\eta_1 = 1.63$, $\eta_2 = 0.96$, $\eta_3 = 0.32$, and $a = 0.06$. As Farhi and Gabaix (2020) highlight, the estimated misoptimization wedge provides a sufficient statistic for the welfare effects of marginal changes in consumption. Farhi and Gabaix (2020) also discuss several ways to use these sufficient statistic estimates, for example, in testing the optimality of an observed tax and transfer system and identifying the direction of welfare-improving marginal tax reforms.

Table A1: Literature on mental accounting and consumption

Reference	Excerpt
Kueng (2018)	"mental accounting suggests that...richer households might feel less guilty squandering [a windfall] than the less affluent"
McDowall (2019)	"Excess sensitivity pervades the liquid wealth and income distributions" and "consumption responses are highly front-loaded," consistent with "a model of mental accounts in which households partition their consumption choice set between a current income and a current asset account."
Fuster et al. (2021)	lack of a "correlation between liquid wealth and MPCs out of gains" may arise due to "some behavioral phenomenon, such as mental accounting" which can "lead households to act as if they are hand-to-mouth"
Boutros (2022)	"highly liquid households have large consumption responses out of income shocks that cannot be driven by borrowing constraints" and "households distinguish between typical and windfall income shocks and opt to expend windfall income shocks over relatively short horizons"
Baugh et al. (2021)	"when faced with anticipated income, households, across the liquidity spectrum, increase consumption on the date of cash flow arrival [but] when faced with anticipated payments, the same households keep their consumption level intact and use liquid reserves or credit to fund payments...best explained by a model of mental accounting"
Fagereng et al. (2019)	"households...perceive capital gains as a distinct form of income in the budgeting process, e.g. due to 'mental accounting."'
Gathergood and Olafsson (2024)	"co-holding points to a prominent role for mental accounting" because "co-holders are willing to pay excess interest costs in order to assign categories of consumption to credit accounts and debit accounts in a financial sub-optimal way"

Note: This table presents excerpts from recent papers that discuss mental accounting in the context of consumption responses to income shocks, referenced in Section 2.3.

Table A2: Balance Tests for Direct Deposit Households

	EFT date			
	Group 1	Group 2	Group 3	p-value
Rebate amount ($)	975.15	985.67	990.98	0.6587
Amount known since Feb	0.52	0.49	0.49	0.2643
Amount known since Mar	0.18	0.19	0.19	0.8573
Amount known since Apr	0.11	0.13	0.13	0.3535
Less than expected	0.12	0.12	0.12	0.7000
Baseline average spending ($/week)	153.96	155.54	154.43	0.8270
Baseline maximum spending ($/week)	450.71	450.23	455.48	0.7888
Baseline spending frequency (weeks)	0.84	0.84	0.83	0.7552
Liquidity	0.59	0.58	0.59	0.9112
Savings habit	0.58	0.60	0.60	0.4096
Regrets purchases	0.41	0.40	0.40	0.7949
Financial plan	0.47	0.49	0.48	0.2779
Plans vacations	0.57	0.59	0.60	0.1133
No vacations	0.17	0.16	0.15	0.3421
Household size	2.52	2.60	2.61	0.1580
Married	0.56	0.59	0.59	0.1874
Lives alone	0.26	0.24	0.25	0.3805
No kids	0.66	0.65	0.64	0.4216
Has kids under 6	0.12	0.14	0.14	0.2576
Hispanic	0.07	0.08	0.08	0.2613
Nonwhite	0.18	0.19	0.17	0.3410
No female head	0.14	0.12	0.12	0.1695
No male head	0.25	0.26	0.24	0.2670
Female head HS grad	0.84	0.85	0.85	0.6354
Male head HS grad	0.71	0.70	0.71	0.3612
Female head college grad	0.28	0.29	0.30	0.2562
Male head college grad	0.25	0.25	0.26	0.3636
Income <$15k	0.06	0.05	0.06	0.1798
Income $15k–$30k	0.17	0.18	0.16	0.0744
Income $30k–$50k	0.26	0.25	0.25	0.6835
Income $50k–$70k	0.20	0.20	0.20	0.8885
Income $70k–$100k	0.23	0.25	0.25	0.1823
Income ≥$100k	0.08	0.08	0.09	0.2689

Note: This table presents summary statistics for households receiving direct-deposit payments in the first (Group 1), second (Group 2), and third (Group 3) week of May, respectively. See Supp. Appendix C.1 for details on the variable definitions. The p-values in the final column correspond to the null hypothesis of equality across groups.

Table A3: Balance Tests for Paper Check Households

	Check date			
	Group 1	Group 2	Group 3	p-value
Rebate amount ($)	858.30	818.80	842.37	0.0162
Amount known since Feb	0.44	0.42	0.43	0.7304
Amount known since Mar	0.19	0.14	0.14	0.0001
Amount known since Apr	0.16	0.17	0.19	0.1055
Less than expected	0.11	0.11	0.08	0.0041
Baseline average spending ($/week)	143.67	134.81	137.25	0.0105
Baseline maximum spending ($/week)	409.97	393.25	394.55	0.2167
Baseline spending frequency (weeks)	0.85	0.85	0.86	0.4324
Liquidity	0.69	0.70	0.70	0.7701
Savings habit	0.63	0.67	0.68	0.0068
Regrets purchases	0.40	0.38	0.40	0.2919
Financial plan	0.54	0.54	0.55	0.7218
Plans vacations	0.55	0.51	0.54	0.0489
No vacations	0.19	0.21	0.19	0.1939
Household size	2.28	2.17	2.18	0.0272
Married	0.56	0.56	0.55	0.6236
Lives alone	0.29	0.31	0.33	0.0426
No kids	0.78	0.82	0.81	0.0086
Has kids under 6	0.07	0.05	0.06	0.0437
Hispanic	0.06	0.06	0.06	0.9249
Nonwhite	0.16	0.18	0.16	0.2446
No female head	0.13	0.12	0.14	0.2138
No male head	0.26	0.28	0.28	0.3410
Female head HS grad	0.83	0.83	0.82	0.7577
Male head HS grad	0.66	0.66	0.66	0.9305
Female head college grad	0.24	0.21	0.23	0.0862
Male head college grad	0.20	0.20	0.22	0.1732
Income <$15k	0.08	0.10	0.10	0.0150
Income $15k–$30k	0.22	0.25	0.22	0.1128
Income $30k–$50k	0.25	0.25	0.26	0.7110
Income $50k–$70k	0.19	0.16	0.17	0.0712
Income $70k–$100k	0.20	0.17	0.19	0.1537
Income ≥$100k	0.06	0.07	0.06	0.7595

Note: This table presents summary statistics for households receiving paper-check payments in the first three weeks (Group 1), weeks 4–6 (Group 2), and weeks 7–9 (Group 3) of the disbursement period, respectively. See Supp. Appendix C.1 for details on the variable definitions. The p-values in the final column correspond to the null hypothesis of equality across groups.

Table A4: ESP Spending Responses by Timing of Payment—Pre-Rebate Differences

	Group 1	Group 2	Group 3
Panel A: Relative to all households			
Period -3 (γ^w_{-3})	-3.09	2.71	-1.83
	(3.87)	(2.33)	(2.34)
Period -2 (γ^w_{-2})	-0.26	-0.81	1.02
	(4.26)	(1.99)	(2.92)
Period -1 (γ^w_{-1})	-6.15	-2.36	-0.59
	(3.86)	(1.97)	(2.04)
Panel B: Relative to households receiving paper checks			
Period -3 (γ^w_{-3})	-3.14	2.49	-1.90
	(4.18)	(2.18)	(2.56)
Period -2 (γ^w_{-2})	-0.47	-0.86	1.03
	(3.98)	(1.94)	(2.45)
Period -1 (γ^w_{-1})	-6.24	-2.42	-0.99
	(4.06)	(1.92)	(1.99)
Panel C: Relative to households receiving paper checks in July			
Period -3 (γ^w_{-3})	-1.82	2.16	-0.90
	(3.69)	(1.94)	(2.13)
Period -2 (γ^w_{-2})	-0.66	0.21	2.37
	(3.71)	(1.66)	(2.31)
Period -1 (γ^w_{-1})	-5.40	-1.25	-1.67
	(3.71)	(1.73)	(2.04)

Note: This table presents estimates of γ^w_k from alternative specifications, described in Supp. Appendix C.2, for households receiving EFTs in the first (Group 1), second (Group 2), and third (Group 3) week of May, respectively. Standard errors reported in parentheses are adjusted for clustering at the household level and obtained from a block-bootstrap procedure with 100 replicates.

Table A5: Mental Accounting Model—Predicted Weekly Spending

	Week 1	Week 2	Week 3	Week 4
Group 1	123.53	54.09	23.26	9.97
Group 2	52.92	22.93	9.87	4.23
Group 3	22.67	9.77	4.19	1.80

Note: The table presents predicted weekly total spending out of a $1,000 windfall implied by the model estimated in Table 1. The index of the group denotes the number periods they wait before receiving the windfall.

Table A6: Mental Accounting Model—Estimates and Fit: Alternative Specification for Timing

	Timing of Group 1 windfall	
	$t = 0$	$t = 5$
Parameter estimates		
α (time)	0.4284	0.6923
	(0.0398)	(0.0253)
β (magnitude)	0.9988	0.9995
	(0.0002)	(0.0002)
Waiting equivalent	681.56	713.49
	(172.01)	(385.35)
Predicted monthly NCP spending		
Group 1 (actual: 53.26)	54.66	41.48
Group 2 (actual: 30.89)	23.79	29.19
Group 3 (actual: 8.05)	10.19	20.40

Note: Each column presents estimates of the model defined by Equations (3) and (4). The first column shows the main specification, in which Group 1 receives the windfall at time $t = 0$ as in Table 1. The second column shows an alternative specification, in which Group 1 receives the windfall at time $t = 5$. All specifications use a scaling factor of 3.33. The top panel shows estimates of the parameters from Equation (5), 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 1a for comparison). Standard errors reported in parentheses are adjusted for clustering at the household level.

Table A7: Mental Accounting Model—Estimates and Fit: Heterogeneity

	Liquidity		Planning		Savings habit		Income	
	Yes (1)	No (2)	Yes (3)	No (4)	Yes (5)	No (6)	Low (7)	High (8)
Parameter estimates								
α (time)	0.4351	0.4356	0.5042	0.3929	0.4414	0.4336	0.4289	0.4192
	(0.0593)	(0.0523)	(0.0610)	(0.0510)	(0.0568)	(0.0558)	(0.0509)	(0.0664)
β (magnitude)	0.9985	0.9990	0.9984	0.9990	0.9985	0.9990	0.9988	0.9988
	(0.0003)	(0.0002)	(0.0003)	(0.0002)	(0.0003)	(0.0002)	(0.0003)	(0.0003)
Waiting equivalent	569.90	793.86	428.54	906.45	551.65	808.36	699.09	705.63
	(201.78)	(280.34)	(153.58)	(310.29)	(191.11)	(296.83)	(266.42)	(267.73)
Predicted NCP spending								
Group 1	47.58	64.75	54.00	56.66	47.39	66.38	55.70	53.68
Group 2	20.81	29.10	27.20	22.83	21.06	29.48	24.57	22.59
Group 3	9.05	12.71	13.70	9.01	9.28	12.80	10.55	9.47

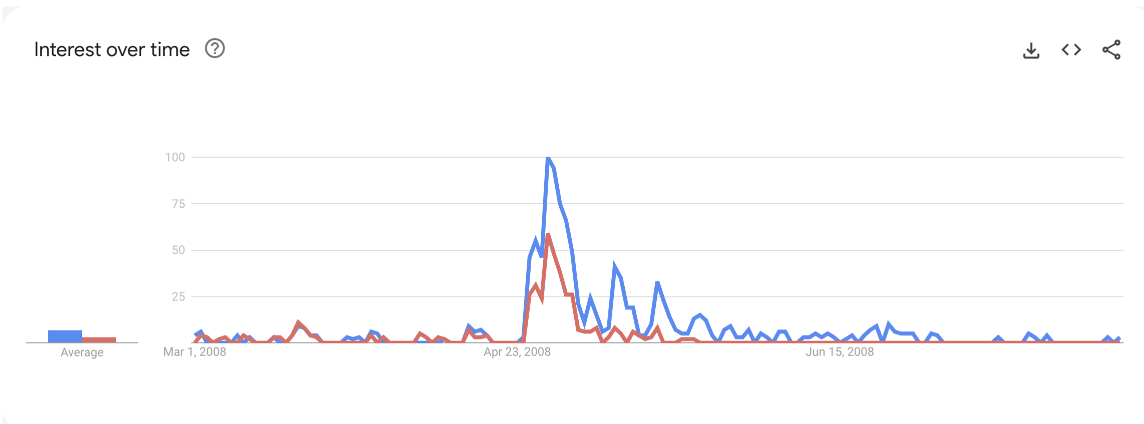
Note: Each column presents estimates of the model defined by Equations (3) and (4) for a different subsample of households. The subsamples follow those presented in Figure 1a: liquidity unconstrained (Column 1), liquidity constrained (Column 2), those that make financial plans (Column 3), those that do not make financial plans (Column 4), those that classify themselves as savers (Column 5), those that classify themselves as spenders (Column 6), below-median income (Column 7), and above-median income (Column 8). The top panel shows estimates of the parameters from Equation (5), 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 monthly NCP spending implied by the model (see the data in Figure 1a for comparison). Standard errors reported in parentheses are adjusted for clustering at the household level.

Table A8: Mental Accounting Model—One-Time Payment vs. Reduced Withholding

Panel A: Survey data from Sahm et al. (2012)		
	One-time Payment	Reduced Withholding
Percent mostly spend	19	13
Percent mostly save	27	33
Percent mostly pay debt	53	54
Panel B: Model prediction		
	One-time Payment	Reduced Withholding
MPC	0.12	0.09

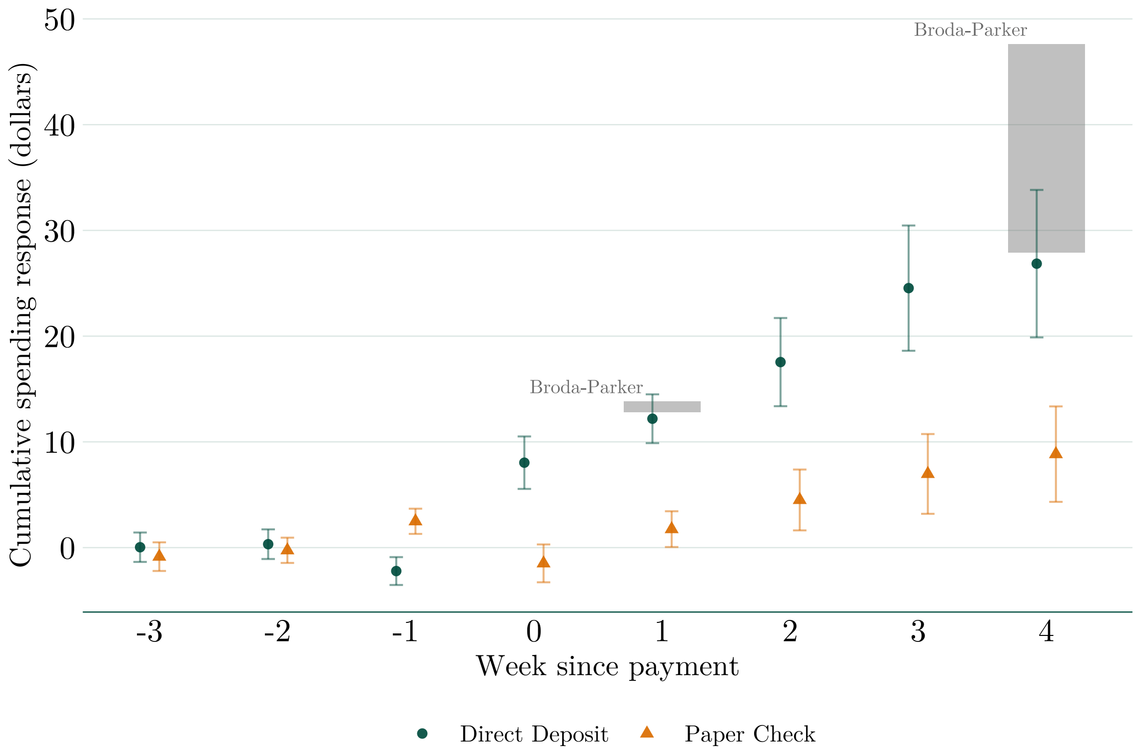
Note: The top panel reports data from the Thomson Reuters/University of Michigan Surveys of Consumers documented by Sahm et al. (2012, Table 1). The 2008 survey (Column 1) asks respondents how the tax rebates were affecting their spending; the 2009 survey (Column 2) asks respondents how the 2009 reduction in withholding is affecting their spending. The bottom panel states the prediction of our model; see Section 4 for additional details.

Figure A1: Google Trends Search Data for Stimulus



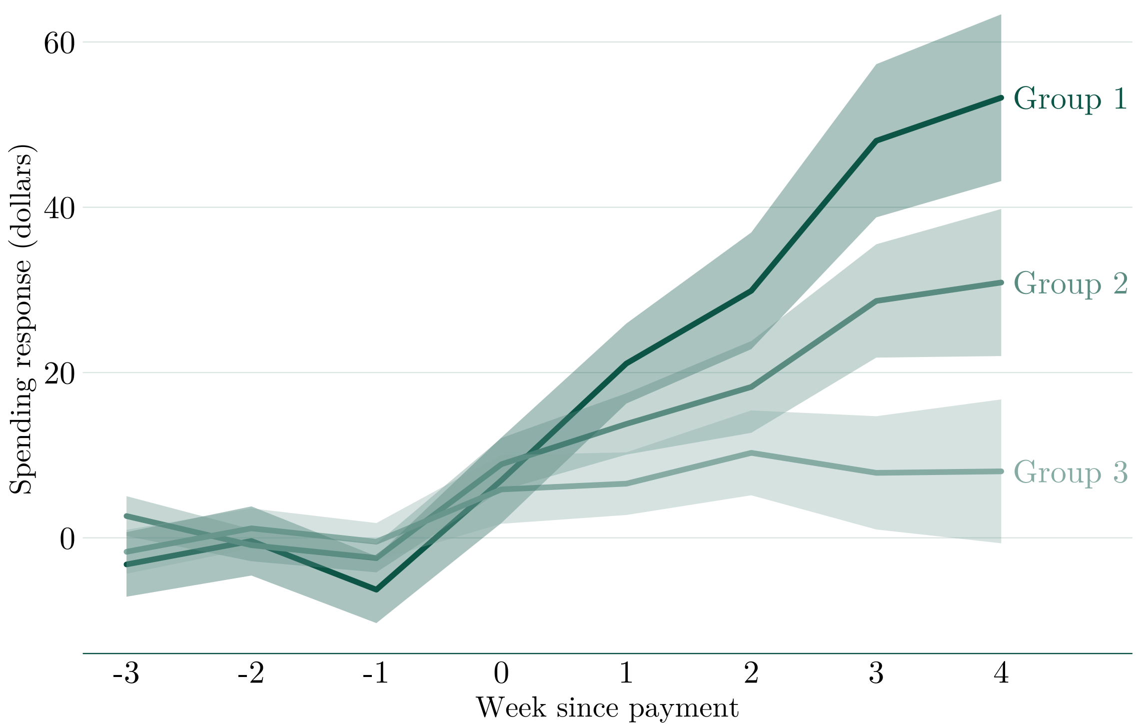
Note: This figure shows the relative number of news searches for the terms "stimulus" (in blue) and "tax rebate" (in red) between March 1, 2008 and July 31, 2008. The peak coincides with President Bush's announcement on April 28, 2008 that the Treasury would start distributing stimulus payments several days earlier than expected. Elevated search activity, to a lesser extent, also occurs on May 2 (when households with SSNs ending in 00–20 receive payments), May 9 (when households with SSNs ending in 21–75 receive payments), and May 16 (when households with SSNs ending in 76–99 receive payments). Even though the share of households receiving payments on these dates is lowest on May 2 and highest on May 9, the graph shows that the peaks on the three payment dates decrease over time.

Figure A2: ESP Spending Responses—Average Impacts



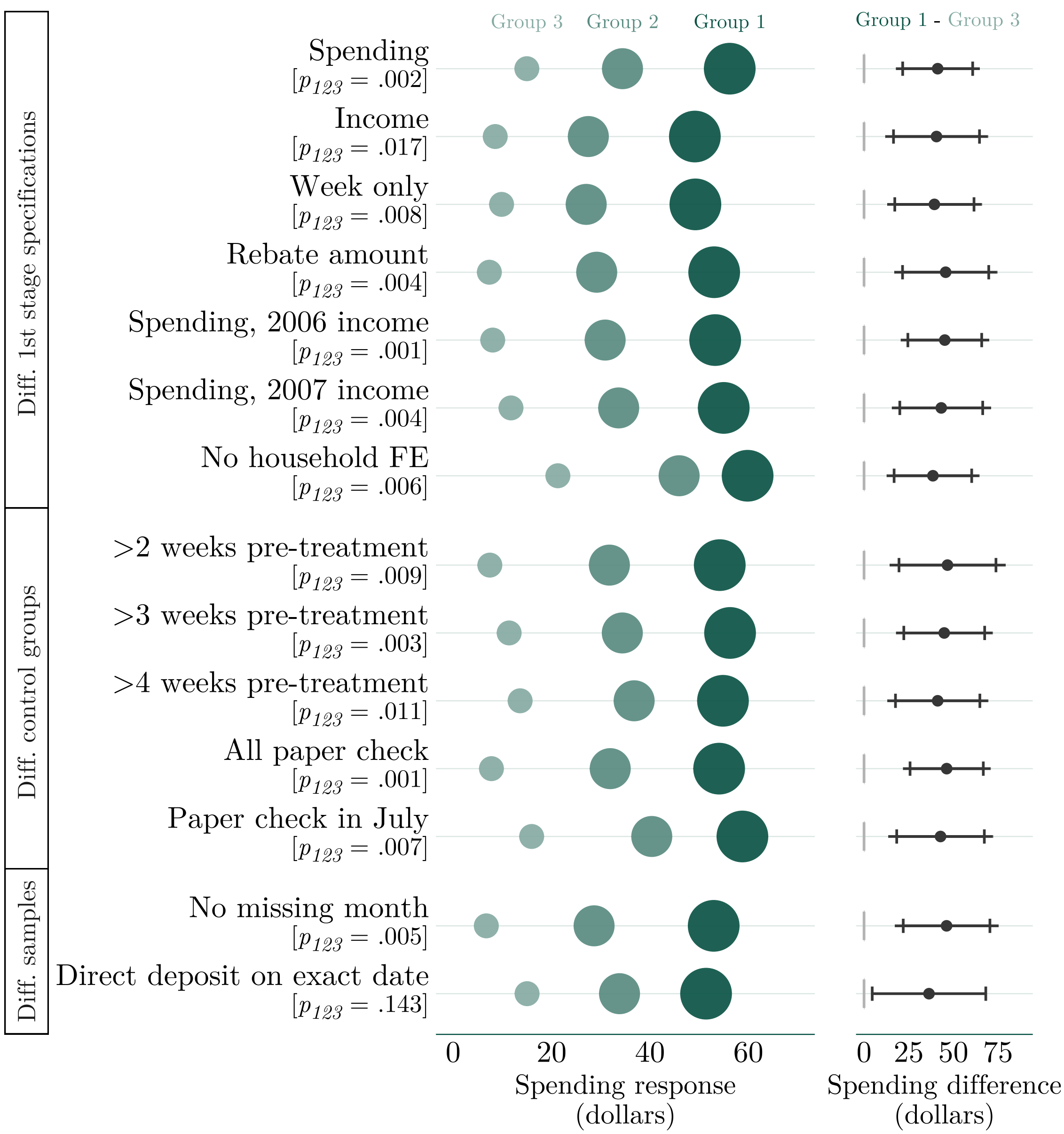
Note: This figure presents estimates of the weekly spending response γ_k (weeks -3 to 0) and the cumulative spending response Γ_k ($k = 1, \ldots, 4$) from Equations (1) and (2) (using two-stage differences in differences) for various samples. Weekly spending refers to expenditure on NielsenIQ goods between Sunday and Saturday of the corresponding week. Week 0 corresponds to the week during which the stimulus payment is received. Week 1 corresponds to the first week in which all households are fully treated. 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 "Direct deposit" and "Paper check" samples consist of households receiving stimulus payments during the weeks (Monday to Sunday) surrounding the scheduled payment dates specified in Table S1. Standard errors reported in parentheses are adjusted for clustering at the household level and obtained from a block-bootstrap procedure with 100 replicates.

Figure A3: ESP Spending Responses over Time by Timing of Payment



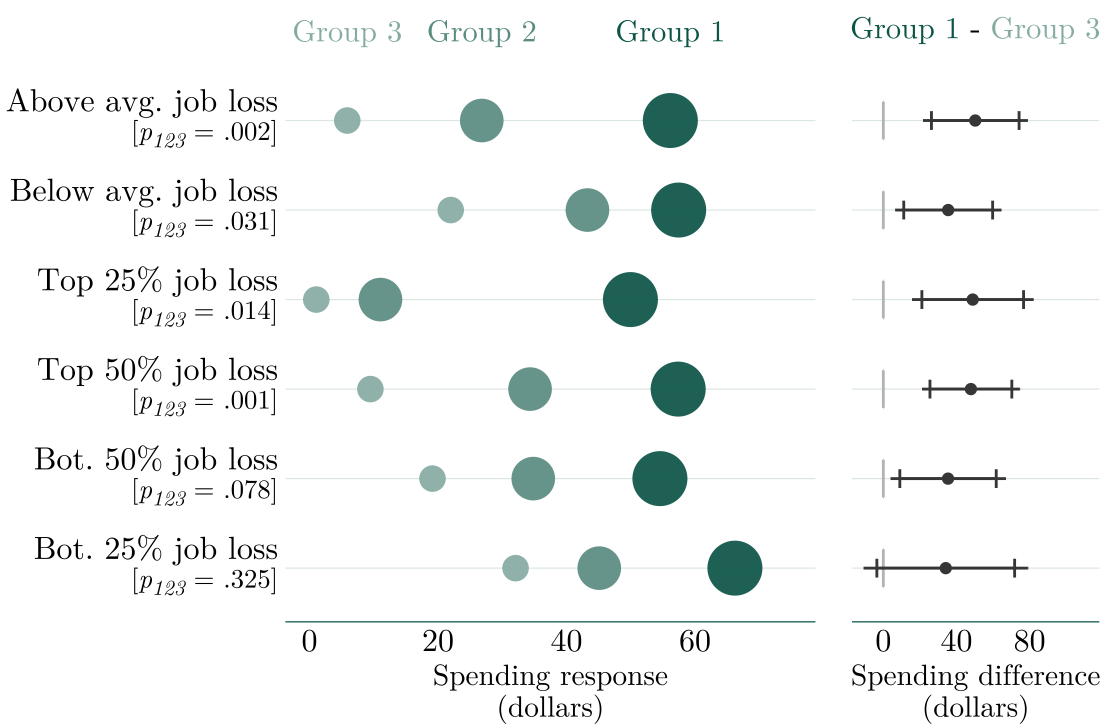
Note: This figure presents estimates from Equations (1) and (2) of the ESP spending response for households receiving EFTs in the first (Group 1), second (Group 2), and third (Group 3) week of May, respectively. The horizontal axis denotes the number of weeks relative to the event of payment receipt. Week 0 corresponds to the week during which the stimulus payment is received. Week 1 corresponds to the first week in which all households are fully treated. For $t > 0$, the figure depicts the cumulative t-week spending impact Γ_t^w, measured in dollars, among households in Group w. For $t \leq 0$, the figure displays estimates of γ_t^w, the impact of ESP receipt on spending in periods prior to the event. Standard errors reported in parentheses are adjusted for clustering at the household level and obtained from a block-bootstrap procedure with 100 replicates.

Figure A4: ESP Spending Responses by Timing of Payment—Alternative Specifications



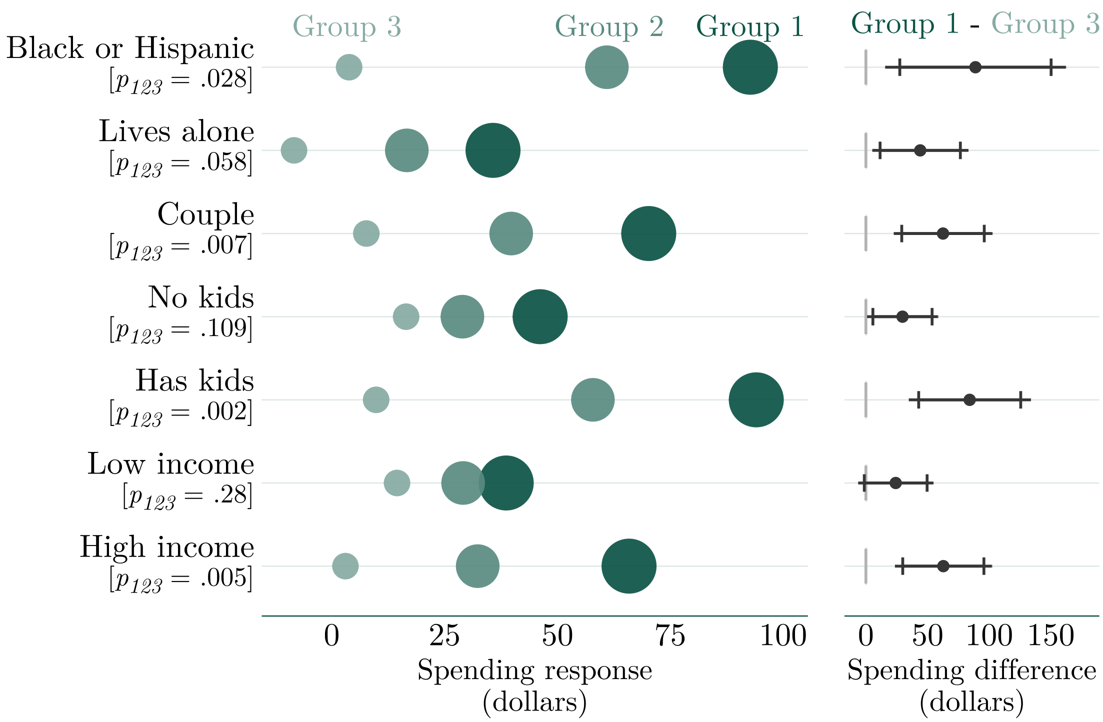
Note: The panel on the left presents estimates from alternative specifications of Equations (1) and (2) of the four-week cumulative ESP spending response Γ_4^w for households receiving EFTs in the first (Group 1, large-size dot, $w = 1$), second (Group 2, medium-size dot, $w = 2$), and third (Group 3, 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.

Figure A5: Spending Response by Timing of Payment—Heterogeneity by States



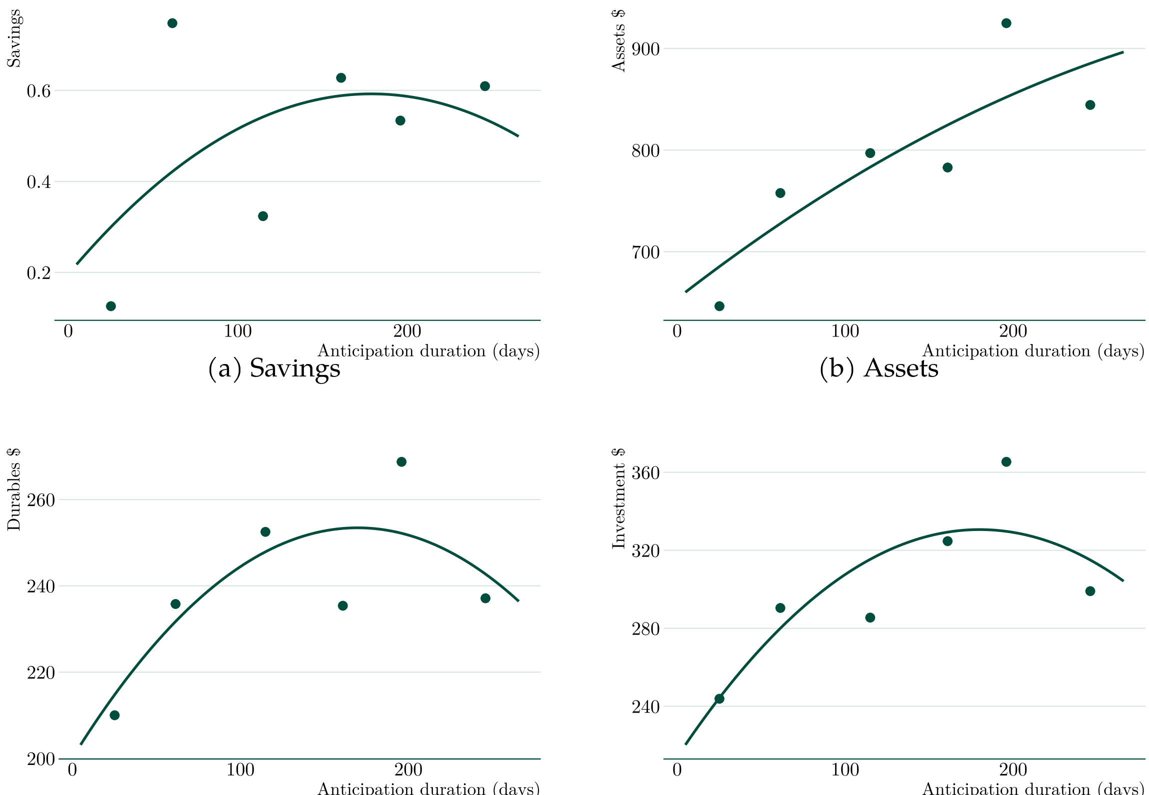
Note: Each row presents estimates from Equations (1) and (2) of the four-week cumulative ESP spending response Γ_4^w for households receiving EFTs in the first (Group 1), second (Group 2), and third (Group 3) week of May, respectively, for a different subsample of households. The *p*-value labeled p_{123} 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). Connaughton and Madsen (2012) provide the ranking of states by job loss during the Great Recession. Standard errors reported in parentheses are adjusted for clustering at the household level and obtained from a block-bootstrap procedure with 100 replicates.

Figure A6: Spending Response by Timing of Payment—Heterogeneity by Demographic Characteristics



Note: Each row presents estimates from Equations (1) and (2) of the four-week cumulative ESP spending response Γ_4^w for households receiving EFTs in the first (Group 1), second (Group 2), and third (Group 3) week of May, respectively, for a different subsample of households. 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.

Figure A7: Relationship between Anticipation Durations and Outcomes (Kenya)



(a) Savings

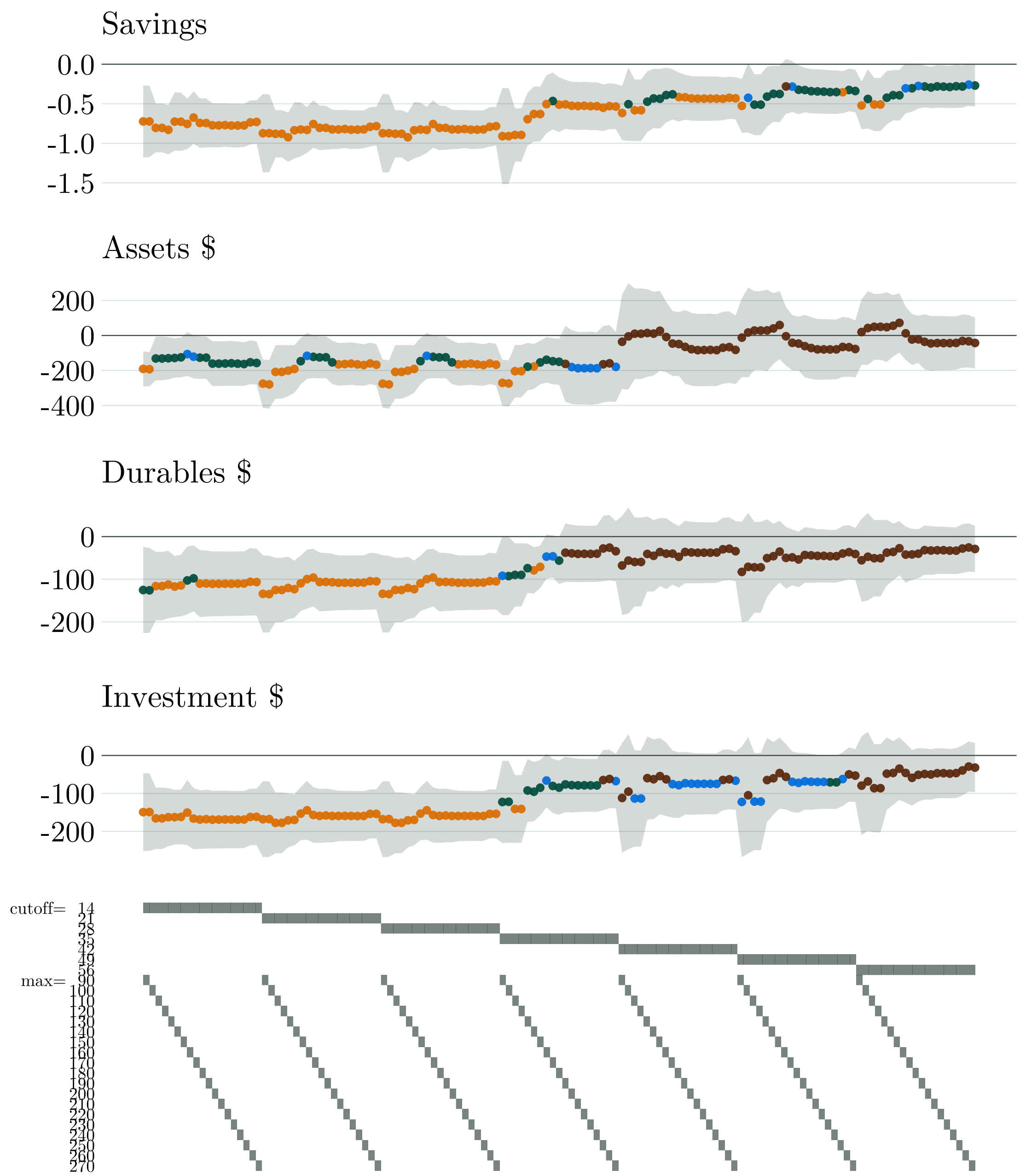
(b) Assets

(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 5.1.1 for details on the outcomes.

Figure A8: Impact of Shorter Wait for Cash Transfers (Kenya): Specification curve varying waiting times

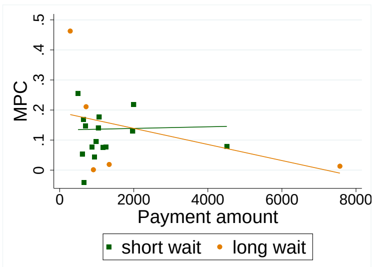


Note: The top four panels correspond to different outcome variables, and the bottom panel contains details on the set of specifications that we estimate. In the top four panels, each dot corresponds to an estimate of the treatment effect, β_k, from Equation (6), with the associated 95 percent confidence interval shaded vertically. Each estimate corresponds to a different specification of the treatment group (short waiting times) and the comparison group (long waiting times). The bottom panel indicates the specification that corresponds to each estimate in the top panels, aligning vertically. "Cutoff" denotes the threshold for defining a short waiting time (treatment group receives payment before the cutoff time), and "max" denotes the maximum number of days of waiting time in the comparison group (comparison group receives payment after the treatment group but before the max days). For the four outcomes, 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.

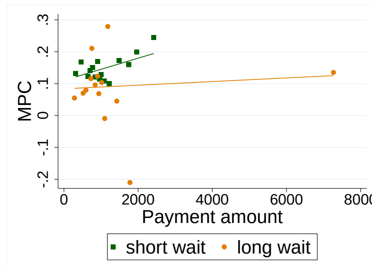
Figure A9: Relationship between Anticipation Duration, Payment Magnitude, and MPC Estimates (Meta-Analysis)

Panel A: MPC standard error less than 0.3

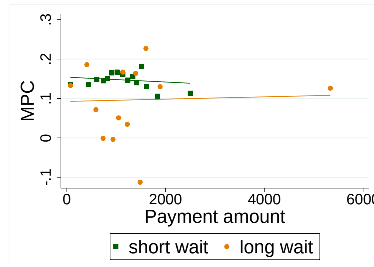
(a) Controls: None



(b) Add s.e., payment size

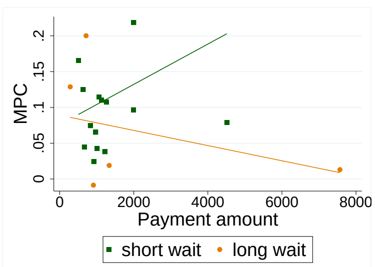


(c) Add liquidity, category

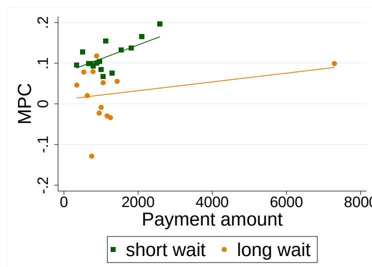


Panel B: MPC standard error less than 0.2

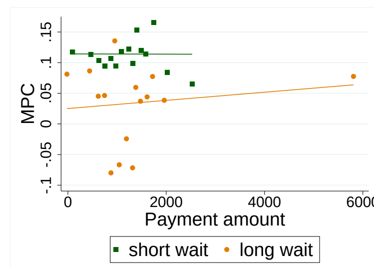
(a) Controls: None



(b) Add s.e., payment size

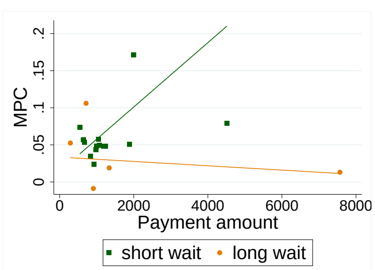


(c) Add liquidity, category



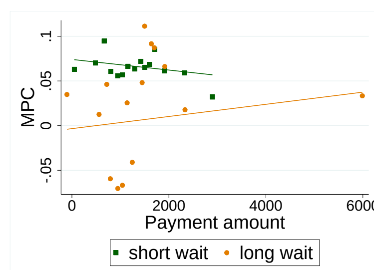
Panel C: MPC standard error less than 0.1

(a) Controls: None



(b) Add s.e., payment size

(c) Add liquidity, category



Note: Each figure depicts the relationship between payment magnitude and the estimated MPC, fit separately for long and short anticipation durations. The first row consists of the subsamples of MPC estimates with standard errors less than 0.3. The second row consists of the subsamples of MPC estimates with standard errors less than 0.2. The third row consists of the subsamples of MPC estimates with standard errors less than 0.1. The first column does not include any control variables. The second column includes controls for MPC standard error. The third column adds controls for whether the estimates reflect samples for which liquidity constraints bind or not, and whether the estimates reflect total consumption, food, or another specific category of consumption. See Supp. Appendix E, the note accompanying Table S8, and Havranek and Sokolova (2020) for further details.