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Anticipation and Consumption

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Anticipation and Consumption

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Abstract

Cash transfer payments are an increasingly widespread policy tool in developed and developing countries, used for both short-term and long-term objectives. We study the design of these policies by examining how the time horizon over which households anticipate receiving transfer payments affects consumption and savings. Using Nielsen Consumer Panel data, we estimate higher marginal propensities to spend for US households scheduled to receive the 2008 Economic Stimulus Payments sooner. Analyzing data from randomized experiments in Kenya and Malawi, we document higher savings among households scheduled to wait longer before receiving lump-sum transfers. We discuss implications of our results through a model of mental accounting.

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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, Gentilini and Yemtsov, 2015). Direct cash payments also play an important role in developed economies to restore growth during economic downturns; the United States, for example, spent almost 1 percent of GDP in 2008 to put \$120 billion in the hands of households at the onset of the Great Recession.

These policies use the same tool for contrasting goals: long-term objectives such as poverty alleviation, and short-term objectives such as boosting consumer spending. 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 life-cycle and permanent income models (Modigliani and Brumberg, 1954; Friedman, 1957) prescribe that households may respond to “unanticipated” changes in income but should respond little, if at all, to “anticipated” changes. Yet effective policymaking often requires households to behave in ways that deviate from this prediction. Fiscal stimulus policies that announce direct payments to households best exemplify the tension that can arise between policy objectives and the theoretical benchmark. More generally, the use of cash payments to achieve different policy goals further highlights the need to understand the design of transfer systems both theoretically and empirically.

The basic economic intuition that spending responses depend on the timing of information about changes in income 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 (Auclert, Rognlie and Straub, 2018). The literature instead considers “two distinct questions,” namely how consumption responds to anticipated income changes and how consumption responds to unexpected shocks (Jappelli and Pistaferri, 2010). This 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. We systematically survey the literature and arrive at three settings that consist of exogenous variation in when households

learn about a windfall payment relative to when they receive it.¹ The first consists of a natural experiment provided by the randomized disbursement dates of a U.S. fiscal stimulus payment (Parker et al., 2013). The second and third involve variation induced by randomized controlled trials (RCTs) on unconditional cash transfers in Kenya (Haushofer and Shapiro, 2016) and Malawi (Brune et al., 2017), respectively. Although these settings have been explored in previous work, our empirical findings in each case—greater consumption responses among households that receive payments sooner after announcement—are new.

In our first empirical setting, we use Nielsen Consumer Panel data to study consumption expenditure responses to the tax rebates sent to low- and middle-income American households as part of the Economic Stimulus Act of 2008 (Broda and Parker, 2014; Parker, 2017). Our identification strategy relies on the fact that the last two digits of the recipient’s Social Security number (SSN) determined the timing of payment over a three-month period. 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, we find no evidence that households increase spending in advance of receiving their stimulus payment. Second, we find that households in the earliest payment group increase spending in the month after receiving their payment by twice as much as the average household does. Third, faster disbursement of stimulus payments leads to a continuing shift in spending behavior: In a given calendar week, spending among households in earlier payment groups exceeds that of households in later payment groups that have received their payment more recently. These patterns emerge for households with different levels of liquidity, financial planning tendencies, and savings habits. We rule out alternative explanations relying on a direct relationship between waiting time and spending needs or saving ability as well as those relying on intrahousehold or social interactions.

In the next pair of settings, we present new analyses of raw data from published RCTs. The first is an impact evaluation of unconditional cash transfers by a non-

¹Appendix A documents that, to the best of our knowledge, other settings with consumption data lack exogenous variation in waiting times.

governmental organization (GiveDirectly) using a sample of households in Rarieda, Kenya (Haushofer and Shapiro, 2016). The second is a windfall experiment in partnership with a commercial bank (NBS Bank) using a sample of households in villages near Mulanje, Malawi to understand how households manage cash without formal financial products (Brune et al., 2017).

The Kenya study contains a set of treatments to compare lump-sum payments with a series of nine monthly installments. To facilitate that comparison, the lump-sum transfers take place at randomly selected but pre-announced times within nine months of enrollment in the program. This previously unexploited random variation in the timing of lump-sum transfers thus provides an ideal experiment for evaluating the role of waiting times. Among households that wait longer to receive their transfer payments, we find increases in savings and investments. We find a concave relationship between waiting time and these outcomes, showing substantial consequences of waiting times less than about five weeks.

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

We introduce a mental-accounting model to interpret the results and examine the policy implications of our findings. To describe how consumers categorize windfalls based on the duration of anticipation, we extend the behavioral life-cycle model of Shefrin and Thaler (1988). Specifically, in a way that our model will make precise, a long-anticipated windfall feels more like wealth and less like income to consumers. We propose a simple specification and estimate the model using nonlinear least squares. Our estimates show that the marginal propensity to consume (MPC) decreases by the same amount from an additional one week of waiting as it would from increasing the size of the windfall by about \$300. The estimated model matches the weekly and monthly spending moments in our data and also matches MPCs reported in related work showing that one-time stimulus payments boost spending by more than equivalent reductions in income tax withholding (Sahm, Shapiro and Slemrod, 2012). We discuss the implications of our model for the design of fiscal stimulus policies, focusing on

payment frequency, amounts, and targeting.

Our empirical results in the domain of tax rebates make several contributions to the extensive literature in household finance, public economics, and macroeconomics on tests of intertemporal consumption models.² First, our work goes beyond the anticipated-unanticipated distinction by positing the importance of the duration over which an income shock is anticipated. Second, we build on existing work methodologically by using a two-step estimation approach.³ Finally, our findings point toward a novel role for the timing of information in designing effective stabilization policies.

This paper also relates to an expansive body of research in development economics on cash transfers as a tool for alleviating poverty (Hanlon, Barrientos and Hulme, 2012). A systematic review of experiments on cash transfers yields a long list of design features (Bastagli et al., 2016): complementary interventions, conditionality, duration, frequency, main recipient, predictability and reliability, size, and timing of transfer payments. We propose a new design feature—waiting times—and evaluate its impact in multiple settings.⁴ A unique aspect of our study is the use of existing data from published experiments that aim to answer a different set of research questions from our paper.

Our work also has significant implications for models of mental accounting. First, we contribute real-world evidence of flexibility in how decision makers classify additional income and explore its consequences in policy-relevant domains.⁵ This points toward important considerations not captured by theories of mental accounting that are based on the idea that setting rigid budgets help consumers overcome self-control problems (Shefrin and Thaler, 1988). Second, we shed light on the dynamics of the mental-accounting process by which consumers treat additional income differently based on its

²The most closely related papers in this literature to ours are those that use household-level data to estimate the consumption impacts of stimulus payments, e.g., Johnson, Parker and Souleles (2006) and Parker et al. (2013).

³Gardner (2021) also proposes this methodology in independent and contemporaneous work.

⁴Waiting time relates to, but is distinct from, the issues of timing and predictability. Timing refers to making funds available to households at specific instances when needs arise, such as the time to pay school fees or to acquire agricultural inputs (Duflo, Kremer and Robinson, 2011). Our results instead pertain to the timing of payments relative to when households learn about them. Predictability refers to reducing uncertainty associated with failing to deliver expected transfers on time (Bazzi, Sumarto and Suryahadi, 2015). Our evidence complements this by focusing on how anticipated delays, or waiting periods, affect household decision-making.

⁵This complements lab experiments demonstrating that decision makers exercise some discretion in assigning expenses to different mental accounts—in other words, that mental accounts can be flexible (Soman and Cheema, 2001; Soman and Gourville, 2001; Cheema and Soman, 2006). Also see Thakral and Tô (2021) for related evidence that the passage of time affects how decision makers react to additional income.

source. Theories of mental accounting invoking self-control (Galperti, 2019), attention (Kőszegi and Matějka, 2020), and multiple selves (Lian, forthcoming) 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. 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. Our results suggest that models elucidating how decision makers move money between different mental accounts or further considering the time dimension may provide additional insights.

Our results also relate to a broader range of phenomena pertaining to waiting times and patience. Dai and Fishbach (2013) document in lab experiments that waiting times can increase patience both when choosing among monetary amounts and among consumption goods (e.g., different models of electronics, or different sizes of chocolate truffles). More recent lab and field experiments find similar patterns for intertemporal effort allocations and consumption goods in developing countries (Imas, Kuhn and Mironova, forthcoming) as well as for healthy food choices (Brownback, Imas and Kuhn, 2019; DeJarnette, 2020). Our paper adds evidence from a canonical consumption-savings problem in multiple policy-relevant settings. Studying consumption-savings decisions also requires a distinct conceptual framework relative to the previous literature, which considers one-time decisions among small, well-defined choice sets involving one or two goods at specific time periods.

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

2 Conceptual framework

We start by describing a standard benchmark in which an agent chooses consumption to maximize discounted expected utility subject to an intertemporal budget constraint

(Hall, 1978), focusing our discussion on how consumption responds to predictable income changes. We then define the empirical quantities of interest, state our hypotheses, and characterize the empirical strategies.

Let $c_{i,t}$ denote consumption for individual i at time t . With a time-separable utility function $u(c)$, assuming that consumers can borrow and lend at interest rate r_t and have intertemporal discount rate δ , consumption satisfies the following Euler equation: $u'(c_{i,t-1}) = \frac{1}{1+\delta} \mathbb{E}_{t-1}[(1+r_t)u'(c_{i,t})]$. With a constant interest rate equal to the intertemporal discount rate, marginal utility follows a martingale process: $u'(c_{i,t-1}) = \mathbb{E}_{t-1}[u'(c_{i,t})]$. According to this benchmark, consumers incorporate expectations of income changes in their optimal consumption plans as soon as they learn about such changes. The marginal utility of consumption therefore does not change when anticipated changes in income occur. In other words, consumption does not change when predictable income changes occur. This prediction holds regardless of whether we assume quadratic utility (certainty equivalence) or a precautionary savings motive, and under a range of different theories of consumption, including the life-cycle and permanent income hypothesis, the buffer-stock savings model, and models with complete insurance markets (Jappelli and Pistaferri, 2010).⁶ Further extensions of the model may result in violations of this prediction: this includes models that relax the assumption of perfect credit markets or introduce illiquid assets (Kaplan and Violante, 2014). When the prediction fails, consumption exhibits *excess sensitivity* to anticipated income changes.

The general equilibrium effects and policy implications of excess sensitivity depend on a set of empirical quantities that Auclert, Rognlie and Straub (2018) refer to as the intertemporal marginal propensities to consume. Letting C_t and Z_t denote period- t aggregate consumption and real after-tax income, respectively, define the intertemporal marginal propensities to consume (iMPCs) as $M_{t,s} := \frac{\partial C_t}{\partial Z_s}$. The first column $(M_{t,0})_t$ of the resulting iMPC matrix captures the impulse response of consumption to an unexpected increase in income. The s^{th} column, $(M_{t,s})_t$, describes how consumption changes in response to an additional unit of income that is known as of period 0 to arrive in period s . In other words, $s = 0$ corresponds to an unanticipated windfall and $s > 0$ corresponds to an anticipated windfall. Varying s corresponds to varying the duration of anticipation of an income shock. The quantity $A_s := \sum_{t < s} M_{t,s}$ represents the total anticipatory spending response, with $M_{0,s}$ being the immediate change in consumption that occurs upon learning about the additional period- s income. The

⁶The framework also makes predictions about how consumption responds to unpredictable income changes. See Jappelli and Pistaferri (2010) for a more detailed exposition.

quantity $\Gamma_t^s := \sum_{t'=s}^{s+t-1} M_{t',s}$ captures the cumulative t -period spending response upon receiving additional income in period s , and $\Gamma_1^s = M_{s,s}$ represents the spending response on impact.

The benchmark models described above predict changes in consumption in advance of an expected income change but no changes in consumption when expected changes occur. Stated in the iMPC framework, this corresponds to the following: (i) a change in spending upon learning about a windfall ($M_{0,s} \neq 0$), and (ii) no significant change in spending after receiving an anticipated windfall ($M_{s,s} = M_{s-1,s}$). We also note the following implication of consumption smoothing: (iii) the duration of anticipation does not affect spending responses (Γ_t^s is independent of s). Hand-to-mouth models (Campbell and Mankiw, 1989) and models of inattention (Reis, 2006) predict violations of (i) and (ii). However, (iii) still holds: With no anticipatory spending (i.e., (i) is violated), households react to anticipated income changes as if they were unanticipated (i.e., (iii) holds). Incorporating liquidity constraints into consumption theories also leads to violations of (i) and (ii) but, for the same reason as above, also does not predict violations of (iii). Prediction (iii) is thus quite robust to a broad class of models in which (i) fails.⁷ Theories based on mental accounting, e.g., the behavioral life-cycle hypothesis due to Shefrin and Thaler (1988), predict violations of both (i) and (ii). We introduce in Section 6 a natural extension of the mental accounting framework that accommodates violations of (iii).

Testing these predictions requires exogenous variation in s , i.e., the duration between when a consumer learns about an income shock and when the shock occurs. Our systematic survey of the literature reveals three settings that have this feature (see Appendix A), which we analyze in Section 3, Section 4, and Section 5.

3 Tax rebates in the US

This section analyzes our first empirical setting: the natural experiment provided by the randomized disbursement dates of the 2008 Economic Stimulus Payments (Parker et al., 2013; Broda and Parker, 2014; Parker, 2017).

⁷See Appendix E for a discussion of models based on time preferences, reference dependence, rational inattention, and rational illiquidity.

3.1 Setting

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

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

Payment dates followed a pre-announced timeline. The Internal Revenue Service (IRS) announced a disbursement schedule on March 17, with the earliest payments scheduled for the first week of May and further batches of payments scheduled in the following weeks. [Appendix Table 1](#) shows the ESP disbursement schedule for on-time filers.⁸ Although the payment schedule and amounts were known in advance, households received notification letters from the IRS several days prior to their payment date. Payment dates were staggered because of the infeasibility of mailing all notification letters at the same time. The last two digits of a taxpayer’s Social Security Number (SSN), which are effectively randomly assigned, determined their scheduled payment date.⁹ On April 25, President Bush stated that the Treasury would start distributing stimulus payments several days earlier than expected, consistent with households in the earliest payment group treating the payments as a surprise ([Kaplan and Violante, 2014](#)).

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

⁸See [Martinez, Meier and Sprenger \(2017\)](#) for evidence that the 2008 ESPs induce earlier filing in a sample of low-income tax filers.

⁹SSNs 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.

2007, and tax filers who had provided the IRS with a personal bank account number for their income tax refunds received ESPs through direct deposit into their bank accounts. For tax returns that either provided no bank information or a tax preparer’s bank information (e.g., due to a refund anticipation loan, or due to using the refund amount to pay tax preparation fees), the IRS sent paper checks in the mail.

3.2 Data

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

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

The survey asks households whether they received a tax rebate via direct deposit or check, the dollar amount, the month and day they received their payment, and several questions related to general household financial planning. About 48,000 households provided responses to the survey, of which about 39,000 report receiving a stimulus payment. Among these, [Broda and Parker \(2014\)](#) note that some households do not report a payment date, report a payment date outside the randomized disbursement period, or provide inconsistent responses across multiple waves of the survey. Removing such observations, the remaining sample consists of about 29,000 households. We obtain the same analysis sample thanks to the replication files provided by [Parker \(2017\)](#). We further restrict the sample to households that report receiving a stimulus payment of at least \$300. We interpret our results as internally valid estimates for the subsample of NCP panelists or the population that they represent ([Bronnenberg et al., 2015](#)).

To examine the consistency of payment dates in our sample with the randomization, we test whether households receiving ESPs at different times have similar characteristics in [Appendix B.1](#). The sample of households receiving ESPs by direct deposit appears to be randomly distributed across the scheduled payment dates in the first three weeks of May ([Appendix Table 2](#)). However, among the sample of households receiving ESPs

by paper check, our balance tests ([Appendix Table 3](#)) reveal systematic differences by payment date across a wide range of characteristics (see [Appendix B.1](#) for further discussion). Our analysis therefore focuses on the sample of households receiving payments by direct deposit.

3.3 Estimation

3.3.1 Methodology

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

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

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

Formally, denote by E_i the time period of the event that i becomes treated, let $D_{it} = \mathbf{1}_{\{t \geq E_i\}}$ be an indicator for being treated, and define $K_{it} = t - E_i$ to be time relative to treatment. Let Θ be a set of time-invariant household characteristics, and let Y_{it} denote an outcome at time t for household i with time-invariant characteristics $\Theta_i \subset \Theta$.

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

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

where α_i are household fixed effects and $\beta_{\theta t}$ are characteristic-specific time trends. Note

that we also exclude data within \underline{k} periods from the treatment date to avoid estimating possible changes in outcomes resulting from the upcoming treatment.

In the second step, we model

$$Y_{it} = \widehat{\alpha}_i + \sum_{\theta \in \Theta_i} \widehat{\beta}_{\theta t} + \sum_{k=-\tilde{k}}^{\bar{k}} \gamma_k \mathbf{1}_{\{K_{it}=k\}} + \varepsilon_{it}, \quad (2)$$

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

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

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

$$Y_{it} = \widehat{\alpha}_i + \sum_{\theta \in \Theta_i} \widehat{\beta}_{\theta t} + \sum_{k=0}^{\bar{k}} \gamma_k^{E_i} \mathbf{1}_{\{K_{it}=k\}} + \varepsilon_{it}. \quad (4)$$

The parameter γ_k^τ represents the causal impact of receiving a rebate k periods ago among households treated in period τ . Analogous to before, we define $\Gamma_t^\tau := \sum_{k=0}^{t-1} \gamma_k^\tau$. To understand whether households receiving rebate payments sooner after the announcement exhibit higher spending responses ($\Gamma_k^\tau > \Gamma_k^{\tau'}$ for $\tau < \tau'$), we test the null hypothesis of implication (iii).

3.3.2 Assumptions

Operationalizing the two-step econometric procedure from [Section 3.3.1](#) requires making assumptions such as how spending would have evolved over time for treated households in the absence of the stimulus payment. For our main results, the treatment group consists of households that report receiving a stimulus payment by direct deposit within two days of the scheduled payment date, and the comparison group consists of all households that report receiving a stimulus payment within the disbursement period associated with their reported payment method (direct deposit or paper check) as in [Broda and Parker \(2014\)](#) and [Parker \(2017\)](#). We make the following assumptions in estimating [Equation \(1\)](#). First, to determine the counterfactual time trend for spending, the set of characteristics Θ consists of income groups (less than \$15,000; \$15,000–\$30,000; \$30,000–\$50,000; \$50,000–\$70,000; \$70,000–\$100,000; over \$100,000) and deciles of average expenditure by household size in the first quarter of 2008. Second, receiving a rebate check does not affect household spending two weeks in advance ($k = 1$). [Section 3.4.3](#) shows that our results are not sensitive to any of the above assumptions.

3.4 Impact of stimulus payments on spending

3.4.1 Average spending impacts

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

We find broadly similar magnitudes to those in [Broda and Parker \(2014\)](#) when estimating [Equation \(2\)](#) for three subsamples of EFT households: our main estimation sample consisting of households receiving EFTs near the scheduled payment date, the subset of households receiving EFTs exactly on the scheduled payment date, and all other households that report receiving EFTs. Across these subsamples, our point estimates for Γ_1 range from \$6.67 to \$11.24, and our point estimates for Γ_4 range from \$24.98 to \$44.04, as shown in [Figure 1](#) and [Appendix Table 4](#); we also find insignificant

spending responses after the month of payment receipt, with point estimates for $\Gamma_8 - \Gamma_4$ ranging from $-\$12.06$ to $\$11.59$.¹⁰ Consistent with their results, we find no spending response in weeks prior to receiving payment.

3.4.2 Impact of timing of stimulus payments

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

The data show a clear pattern of lower spending impacts for households that wait longer to receive their payments. Figure 2 summarizes our main results for various samples of households.¹¹ The left panel displays estimates of Γ_4^w for households receiving payments in different weeks, as well as p -values from testing the null hypotheses that $\Gamma_4^1 = \Gamma_4^2 = \Gamma_4^3$, while the right panel displays the confidence interval for the difference in spending between the first and last groups.

We begin by discussing the full sample of households receiving EFTs near the scheduled payment date. Among households randomly assigned to receive payments in the first week, we estimate a $\$65.25$ increase in spending during the four weeks after receiving the ESP, about twice as large as the increase in spending for the average household. The monthly spending impact for a household receiving payment in the first week is similar in magnitude to combining the impact on a household receiving payment one week later ($\$45.24$) with the impact on a household receiving payment two weeks later ($\$18.73$). This suggests an important role for the timing of payments in

¹⁰In estimating the impact of ESPs on spending in the week of receiving payment (Γ_1), Broda and Parker (2014) report point estimates ranging from $\$12.8$ to $\$13.8$. They obtain point estimates of the four-week or one-month cumulative increase in spending (Γ_4) ranging from $\$27.9$ to $\$47.6$. See Tables 3 and 4 in Broda and Parker (2014) and the discussion therein regarding the differences in magnitudes between their weekly and monthly analyses. They also report an insignificant average increase in spending of $\$9.3$ one month later ($\Gamma_8 - \Gamma_4$) in their preferred specification.

¹¹Appendix Figure 1 displays cumulative spending effects during the four weeks following ESP receipt. Also see Appendix Table 5 for the main results in the form of a table.

designing effective fiscal stimulus. The remaining rows of [Figure 2](#) examine subsamples based on survey responses to questions pertaining to liquid assets and behaviors related to financial planning and spending as explored by [Parker \(2017\)](#).

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

We next examine heterogeneity in ESP spending responses by financial planning tendencies. We divide households into two groups based on whether they report reviewing their household’s financial information in the last few years and formulating a financial plan for their long-term future. Intuitively, we might expect households that formulate consumption plans to exhibit lower propensities to spend out of windfalls ([Reis, 2006](#)). Indeed [Parker \(2017\)](#) finds a negative relationship between financial planning and ESP spending responses, and we find a similar relationship on average. In a possible exception to this general pattern, households that make financial plans and receive ESPs in the first week exhibit the largest spending responses (\$74.58 for planners compared to \$58.06 for non-planners). The finding that the largest spending responses come from households that engage in financial planning does not seem consistent with

the view that planning generically induces higher savings.

The last pair of rows in [Figure 2](#) separately consider households that characterize themselves as spending types and saving types, a measure of impatience.¹² We find, consistent with the results in [Parker \(2017\)](#), that more patient households spend less in response to the ESPs. Moreover, both self-reported spending types and saving types exhibit stronger responses to payments that arrive earlier. The consistency across these groups corroborates the notion that more time to anticipate future consumption impacts intertemporal decision-making through channels distinct from impatience.

In addition to analyzing spending responses across households with different self-reported financial circumstances, we estimate heterogeneity in spending impacts by objective household characteristics. The relationship between waiting times and spending responses persists for households receiving different rebate amounts ([Appendix Figure 2](#)).¹³ The same pattern also emerges for high- and low-expenditure households as well as high- and low-income households ([Appendix Figure 3](#)).

3.4.3 Robustness

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

We begin by considering alternative sets of characteristics in the first step of the estimation (Panel A of [Figure 3](#) and [Appendix Table 8](#)). In our baseline specification, these characteristics include deciles of pre-rebate average expenditure and six income categories. Removing the income categories from the set Θ does not change the magnitudes of the estimated ESP spending impacts. Instead removing the expenditure deciles leads to slightly smaller estimates, though the differences across households receiving ESPs in different weeks remains equally substantial. The same holds if we remove both sets of characteristics and include only household fixed effects and period fixed effects. Allowing for differential spending trends based on the rebate amount leads to similar magnitudes as our main specification, as does replacing contemporaneous

¹²The 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 rather than their response to the stimulus payments.

¹³Since the rebate amounts differ, we report marginal propensities to consume, extending [Equations \(3\) and \(4\)](#) by interacting the treatment indicator with the rebate amount. Also see [Appendix B.2](#) and [Appendix Table 6](#).

income with lagged values of income (for which the data contain much fewer missing values). Omitting household fixed effects leads to somewhat larger estimates.

We next consider alternative sets of comparison households (Panel B of [Figure 3](#) and [Appendix Table 8](#)). The baseline specification uses all households that receive ESPs within the disbursement period associated with their reported payment method to estimate counterfactual spending, using only data from at least two weeks before their reported payment weeks. Excluding one, two, or three additional weeks of data preceding ESP receipt slightly increases our estimates of the spending impacts. We also examine the sensitivity of our estimates to alternative specifications of the set of comparison households. Restricting the set of households to only those receiving paper checks, or further restricting to those that receive paper checks near the scheduled payment dates, leads to similar estimates of the ESP spending impacts. We obtain slightly larger point estimates if we use households receiving paper checks in July to ensure that the composition of households used to estimate each of the week fixed effects in [Equation \(1\)](#) remains stable. In our main specification as well as each of these alternative specifications, we find no significant spending responses in the weeks prior to receiving the ESP, providing evidence to support the validity of the estimated counterfactual spending trend ([Appendix Table 9](#)).

Lastly, we examine how our estimates change under different sample restrictions (Panel C of [Figure 3](#) and [Appendix Table 8](#)). Excluding households that report no spending for a consecutive four-week period does not change the magnitudes of our estimates. Restricting the sample of direct deposit households to those that report receiving their ESP on the exact day specified by the disbursement schedule also leads to similar point estimates.

3.5 Alternative explanations

To interpret the results, [Section 6](#) posits a mental-accounting framework that captures the intuition that households spend more in response to more surprising windfalls. The fact that liquidity constrained and unconstrained households exhibit similar patterns suggests an important role for this channel.

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 longer waiting times lead to lower ESP spending, and other reasons why households that face longer waiting times would spend less.

3.5.1 Anticipatory spending

Smaller spending responses among households that wait longer before receiving payments may arise if more time allows households to spend more of their ESPs in advance. However, our data show no significant differences in spending prior to ESP receipt, with the total spending response in the month before receiving an ESP ranging from -\$8.10 to \$2.43 across the various specifications in [Appendix Table 9](#).¹⁴ 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 about \$100 in monthly spending.¹⁵ This is over five times as large as the anticipatory spending response implied by the [Kaplan and Violante \(2014\)](#) model.¹⁶

3.5.2 Borrowing, debt, and non-Nielsen spending

Since our consumption data only consist of spending on household items ([Broda and Weinstein, 2010](#)), changes in other forms of spending could potentially occur. Although our data show no evidence of additional spending in advance, households might either increase debt payments or increase non-NCP consumption (e.g., by borrowing, assuming that households have access to credit or are more likely to have access to credit if they have more time). The former possibility appears inconsistent with previous work on the 2001 and 2008 tax rebates ([Agarwal, Liu and Souleles, 2007](#); [Bertrand and Morse, 2009](#)) documenting increases in debt payments upon *receiving* ESPs as opposed to in advance, while evidence on responses to state tax rebates from the Consumer Expenditure Survey ([Heim, 2007](#)) rejects the latter.

Alternatively, we might also observe a relationship between waiting times and spending responses if longer wait 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

¹⁴Using daily-level data on 17.2 million households from a large U.S. financial institution, [McDowall \(2019\)](#) also finds insignificant anticipatory spending responses.

¹⁵This corresponds to a difference of \$3.32 per day (i.e., \$46.52 over 14 days).

¹⁶Their Table IV reports a 6 percent marginal propensity to consume one quarter in advance of receiving a \$500 tax rebate.

responses and households' awareness of their spending responses. With this caveat in mind, we find that households in later payment groups do not report higher ESP spending on average than households in earlier payment groups.¹⁷

3.5.3 Time effects

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

If the marginal propensity to consume varies over the course of a month with fluctuations in cash on hand, we might expect to find larger spending responses in weeks when households must make rent payments or pay other bills, which tends to occur at the beginning of the month. On the other hand, we might expect to find smaller spending responses in weeks when households receive paychecks, which tends to push in the opposite direction. For a household making rent payments at the beginning of the month and receiving weekly paychecks, this would plausibly lead to larger spending responses to payments received in the first week of May and similar (smaller) responses to payments received in later weeks.¹⁸ We do not find any evidence of larger consumption responses to payments received at the beginning of the month for households receiving ESPs in June and July.¹⁹ The finding that households with different levels of income and liquidity exhibit similar patterns further limits the plausibility of explanations relying on week-of-month effects such as interactions with the paycheck cycle.

Next we address the possibility of variation in MPCs arising due to calendar-time effects specific to the EFT disbursement period. In particular, new information over time about the severity of the financial crisis could lead to smaller absolute spending responses for households in later payment groups. We would expect this channel to

¹⁷This holds for all five spending categories. 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.

¹⁸Similarly, for households receiving biweekly paychecks, we would expect a non-monotonic pattern, with the largest response to receiving payments in the second week, and the smallest response to receiving payments in the third week. For households receiving monthly paychecks, we would expect to find larger responses to ESPs received in later weeks of the month.

¹⁹As a caveat, note that this test does not use the ideal source of random variation in payment dates; see [Appendix Table 3](#) and the discussion in [Section 3.2](#).

be particularly relevant for states that experience higher levels of job loss during the recession. However, when analyzing differences in spending responses across states, we find that those in the top and bottom quartiles of the distribution exhibit similarly strong MPC reductions in response to longer waiting times ([Appendix Figure 4](#)). For a more direct test of the relevance of such calendar-time effects, we hold fixed calendar time and analyze differences in spending behavior across groups (to compare households when they would have access to the same information). Households in the last group to receive EFT payments spend *less* in the week after receiving payment compared to households in the first payment group *during the same week* ([Appendix Figure 5](#)). This occurs despite the fact that households in earlier payment groups would have less of their ESPs to spend.

3.5.4 Other mechanisms for waiting time to affect spending

We also consider individual- and group-level channels through which longer waiting times could potentially affect spending. Waiting 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 waiting times matter.

The evidence that liquidity unconstrained households exhibit the same effect ([Figure 2](#)) suggests that explanations based on waiting times enabling consumers to find ways to save or other external commitments cannot fully explain the patterns in the data. If the effect arises because the passage of time allows households to accumulate or remember expenses that would dampen their spending response (e.g., having more time for long-term needs to arise, having more time to remember high-value investments), then we would expect to find that households in earlier payment groups spend no more than households in later payment groups when holding the calendar week fixed, contrary to the evidence in [Appendix Figure 5](#). The results replicate for single individuals, couples, households with and without children ([Appendix Figure 6](#)), suggesting that the effects do not reflect specific forms of intrahousehold decision-making. Finally, if the patterns result 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 [Appendix Figure 5](#).

4 Cash transfers in Kenya

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

4.1 Setting and data

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

The researchers randomized 503 households into treatment arms that vary by whether households receive KES 24,000 (USD 384 PPP) or KES 94,000 (USD 1,505 PPP).²⁰ Among the 366 households receiving the smaller transfer amount, 193 households received one-time lump-sum transfers.²¹ The magnitude of these one-time payments equates to about six months of revenue for the average household.

Households learned of the transfers during a visit from a GiveDirectly representative. During these visits, the representative announced the amount and timing of the payments. Households receiving one-time lump-sum transfers would receive their payment on the first day of a randomly selected month among the nine months following the date of the visit.²² The outcome measures come from an endline survey which takes place about 14 months after the baseline survey. Eliminating 7 households for which transfer dates do not appear in the data, 8 attriting households for which the data do not contain endline survey outcomes, and 6 households that receive transfers after the endline survey

²⁰As in [Haushofer and Shapiro \(2016\)](#), we report all USD values at purchasing power parity using the World Bank PPP conversion factor of 62.44 KES/USD for private consumption in 2012. The transfer amounts roughly correspond to USD 300 nominal and USD 1,000 nominal.

²¹The remaining 173 households received monthly transfers over a nine month period. The 137 treated households receiving the larger transfer amount received the bulk of their payments at a monthly frequency as well, as [Appendix C.1](#) explains.

²²Households also received an initial transfer of KES 1,200 immediately following the announcement visit.

(primarily due to registration issues with M-Pesa), our remaining sample consists of 172 households.²³

We use random variation in payment dates among households in the lump-sum treatment to estimate the impact of longer waiting times. Since previous research using the GiveDirectly data does not utilize this source of variation in waiting times, we conduct balance tests before proceeding. Consistent with random assignment, household characteristics and baseline measures do not significantly differ across households experiencing different waiting times ([Appendix Table 10](#)). We define a longer waiting time as more than $k \in \{2, \dots, 8\}$ weeks from the announcement visit. While the [Haushofer and Shapiro \(2016\)](#) experimental design involves randomizing the timing of the lump-sum transfers to facilitate comparability with their monthly-transfer treatment, our paper uses a distinct, previously unexploited source of variation—experimentally induced random variation in the extent to which households anticipated their transfer payments—to examine how waiting periods affect decision-making.

4.2 Estimation and results

To estimate the impact of longer waiting times, we follow the econometric strategy in [Haushofer and Shapiro \(2016\)](#) by conditioning on baseline levels of the outcome variables to improve statistical power. Letting T_{vh}^k indicate a waiting time of more than $k \in \{2, \dots, 8\}$ weeks since the announcement, we estimate

$$y_{vh}^E = \alpha_v + \beta_k T_{vh}^k + \gamma y_{vh}^B + \varepsilon_{vh}^B, \quad (5)$$

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, T_{vh}^k indicates treatment with a longer waiting time, and ε_{vh}^B is an idiosyncratic error term.²⁴ The parameter β_k represents the causal impact of a longer waiting time relative to a shorter waiting time. We test the null hypothesis of implication (iii) from [Section 2](#), which corresponds to $\beta_k = 0$.²⁵

²³The attrition and non-compliance rates in our sample are similar to but slightly lower than in the complete sample of 1,008 households. See [Appendix C.1](#) for additional details on the samples.

²⁴For the small set of outcomes with a few missing baseline measures, we encode missing values and control for an indicator δM_{vh}^B for missing values: $y_{vh}^E = \alpha_v + \beta T_{vh} + \gamma y_{vh}^B + \delta M_{vh}^B + \varepsilon_{vh}^B$.

²⁵Liquidity constraints play an important role in this setting ([Haushofer and Shapiro, 2016](#)). As we outline in [Section 2](#), incorporating liquidity constraints into the benchmark models leads to violations of predictions (i) and (ii) but not (iii). In addition, these data do not contain high-frequency measures

We consider four broad outcome measures: savings, assets, durables, and investments. The measure of savings consists of the total value of savings in all savings accounts, including M-Pesa. Assets consist of various types of livestock (cattle; small livestock such as pigs, sheep, and goats; birds such as chicken, turkeys, doves, and quails) and durables. Durables include furniture, agricultural tools, appliances, and other movable assets such as bicycles and cell phones. Investments consist of durable investment (durable assets and non-agricultural business investment in durables) and non-durable investment (agricultural inputs, enterprise expenses, educational expenses, and savings). We present all values in 2012 USD PPP. These measures from [Haushofer and Shapiro \(2016\)](#) capture outcomes at the time of the endline survey, unlike the results in [Section 3.4](#) which constitute an impulse response of spending to windfalls.

We present results under a variety of specifications, varying the definitions of the treatment group (shorter waiting times) and comparison group (longer waiting times). [Figure 4](#) displays the main results, which support the hypothesis that shorter waiting times lead to significant reductions in future-oriented decision-making. Each dot in the figure corresponds to an estimate of the treatment effect from [Equation \(5\)](#) and the associated 95 percent confidence interval for a given definition of shorter and longer waiting times. We vary the definition of a shorter waiting time between 2 weeks and 8 weeks, and we vary the regression sample to include waiting times between 90 days and 270 days. For example, the first specification compares households receiving transfers within 14 days of the announcement date with households receiving transfers up to 90 days after the announcement date. We find substantial decreases in the probability of having nonzero savings among households randomly assigned to receive cash transfers sooner after the announcement visit. The decrease in savings does not arise due to substitution into other stores of value such as durables or other assets and investments. Households facing the shortest waiting times—those receiving transfers in the first month after the announcement—exhibit the strongest reductions in endline savings, assets, durables, and investments.

Varying the range of waiting times in the comparison group does not affect our results, suggesting that the estimates reflect the impact of differences in waiting times rather than differences in endline survey timing. [Figure 5](#) corroborates this by plotting outcomes across the distribution of waiting times. If shorter waiting times lead to lower savings solely because households can experience a longer period of elevated

of consumption as [Section 3](#) did. Our analysis in this section thus focuses on (iii).

consumption before the endline survey takes place, we would expect to see a linear relationship between waiting times and the various outcomes. The binned scatterplots instead confirm that households facing the shortest waiting times exhibit especially strong reductions in endline savings, assets, durables, and investments, consistent with a substantive shift in decision-making.²⁶

We obtain similar results under various alternative estimation approaches. Equation (5) uses an analysis of covariance (ANCOVA) approach (Frison and Pocock, 1992; McKenzie, 2012). As an alternative, we analyze differences-in-differences, and we find similar differences between the treatment and comparison groups when defining the outcome variable as the difference between the endline and baseline measure (Appendix Figure 10). We also obtain similar estimates when altering the ANCOVA approach by adding quadratic controls for baseline outcomes (Appendix Figure 11) or removing village fixed effects (Appendix Figure 12). We also document similar patterns for other outcomes variables: value of savings, durable investment, non-durable investment, and total assets including non-thatched roofs (Appendix Figure 13).

4.3 Alternative explanations

This section considers alternative individual- and group-level factors that may result in waiting times influencing savings and investment decisions.²⁷ First, consumers may find ways to save or to find other ways to spend the money with longer waiting times. Second, intrahousehold or social interactions may result in a role for waiting times.

To address the possibility that finding ways to save or spend as time passes may explain our results, we estimate the impact of short waiting time separately for households that report having no savings at baseline and those that report having no loans at baseline.²⁸ We investigate the importance of intrahousehold interactions by examining heterogeneity by the gender of the randomly assigned recipient of the transfer, household

²⁶All specifications contain controls for baseline outcomes and village fixed effects. Plotting the difference between endline and baseline outcomes gives the same pattern (Appendix Figure 7). Plotting only baseline outcomes provides evidence of balance (Appendix Figure 8). Appendix Figure 9 presents a formal test which rejects the null hypothesis of a linear relationship between waiting times and outcomes.

²⁷Section 3.5.4 discusses these channels in the context of our results on the 2008 stimulus payments in the US. The other possible explanations in Section 3.5 pertain to specific features of the tax-rebate setting.

²⁸Moreover, 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 5 shows that the latter is much larger.

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 lump-sum transfer, villages in the bottom half of the distribution of the waiting time for the first transfer, and households that receive their lump-sum transfer before the median household in their village.

Estimates of the impact of a short waiting time (less than four weeks) on savings, assets, durables, and investment for the subsamples described above appear in [Figure 6](#). In each case, we obtain estimates of roughly the same magnitude as the estimates from the full sample, with none of the subsamples showing systematic differences relative to the full sample.

5 Cash transfers in Malawi

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

5.1 Setting and data

[Brune et al. \(2017\)](#) conduct an experiment to examine how formal financial products influence consumption decisions by making windfall payments to a sample of 474 randomly selected households living in villages within six kilometers of the NBS bank branch in Mulanje, Malawi. The researchers randomly vary whether households receive transfer payments of MK 25,000 (USD 176.50 PPP) via cash or direct deposit in March–April 2014.²⁹ The magnitude of the transfers equates to about four times the existing 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.

²⁹We report USD values at purchasing power parity using the conversion factor 141.64 MK/USD as in [Brune et al. \(2017\)](#). The transfer amounts correspond to about USD 60 nominal.

Participants either receive payments immediately or with a delay, randomized independently of the main treatment arm (i.e., whether the household receives the transfer via cash or direct deposit). The stated goal of the payment delay was to “test the presence of time inconsistency” to shed light on the mechanisms through which formal bank accounts affect spending, though we discuss in [Appendix E.1](#) how payment delays do not provide a test of time-inconsistent preferences or quasi-hyperbolic discounting.

A total of 318 households receive non-immediate payments, with 158 receiving payments after a one-day delay and 160 receiving payments after an eight-day delay. The remaining 156 households in our sample receive payments immediately. In our main specifications, we pool together households treated with payment delays because [Brune et al. \(2017\)](#) note that specifications that separately estimate the impacts of different payment delays tend not to have enough power to detect small effect sizes.³⁰ Consistent with random assignment, baseline characteristics do not significantly differ among households receiving payments immediately or with a delay ([Appendix Table 12](#)); [Brune et al. \(2017, Table 3\)](#) also show that baseline characteristics across the treatment arms appear balanced.

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

5.2 Estimation and results

To obtain the causal impact of non-immediate payments, we estimate an analog of [Equation \(5\)](#) as in [Brune et al. \(2017\)](#):

$$y_{vwh}^E = \alpha_v + \beta T_{vwh} + \gamma y_{vwh}^B + \delta_w + \varepsilon_{vwh}^B, \quad (6)$$

³⁰[Appendix Table 11](#) presents results that disaggregate the delayed-windfall treatment groups.

where y_{vwh}^t represents the baseline ($t = B$) or endline ($t = E$) outcome of interest for household h in village v surveyed in week w , α_v and δ_w capture village and week-of-first-survey fixed effects, T_{vwh} indicates treatment with a payment delay, and ε_{vwh}^B is an idiosyncratic error term. The parameter β represents the causal impact of a delayed relative to an immediate windfall. We test the null hypothesis of implication (iii) from [Section 2](#), which corresponds to $\beta = 0$.³¹

The outcomes consists of various forms of savings. Total savings, as [Table 1](#) shows, increases significantly as a consequence of anticipated payment delays. While the estimates tend to have low precision, the large magnitudes appear to arise due to increases in *in-kind savings*. In-kind savings consist of advance purchases of farm inputs, business inventory, and bags of maize (see the questionnaire in [Appendix Figure 14](#)). The analysis in [Brune et al. \(2017\)](#) focuses on expenditure rather than savings and finds little influence of payment delays. As a possible explanation for the discrepancy between the large impact on savings that we observe and the previous results on spending, note that the expenditure survey asks how much households *paid in total* for various consumption goods over the past seven days ([Appendix Figure 15](#)); these consumption goods include maize, which households also purchase as a form of in-kind savings.³²

We also find a large positive point estimate for financial assets, which consist of both formal savings (accounts at NBS or other banks) and informal savings (village savings groups, ROSCAs, cash not for daily living expenses kept at home or in a secret hiding place). Disaggregating these components of financial assets, we find slightly higher increases in informal savings ([Appendix Table 11](#)). Furthermore, increases in savings stem primarily from the behavior of households in the eight-day-delay treatment rather than in the one-day-delay treatment (also see [Appendix Table 11](#)). Overall the results support the hypothesis that waiting periods cause substantial shifts in household decision-making.

³¹As in [Section 4](#), we focus on (iii) since liquidity constraints predict violations of (i) and (ii) but not (iii), and the data do not contain high-frequency measures of consumption.

³²See [Browning, Crossley and Winter \(2014\)](#) for a discussion of the well-known challenges of measuring household consumption using survey data. The Malawi IHS-3 questionnaire, which serves as a basis for the expenditure survey in this field experiment, asks specifically about how much households *consume* (“food both eaten communally in the household and that eaten separately by individual household members”) over the past seven days ([Appendix Figure 16](#)).

5.3 Alternative explanations

This section follows [Section 4.3](#) by considering alternative mechanisms that could potentially explain the relationship between waiting times and savings. We find similar point estimates for the impact of a delayed windfall for households receiving direct deposit payments into an account with the NBS Bank rather than cash ([Table 1](#)); this suggests that the results are not driven by waiting times enabling households to find ways to save. Our results hold across households of different sizes and marital status [Appendix Table 13](#), suggesting that the mechanism does not rely on intrahousehold interactions. Finally, the relatively small share of treated households limits the scope for social interactions to provide a plausible explanation in this setting.

6 Mental accounting of windfalls

This section builds on the framework in [Section 2](#) by introducing a simple descriptive model of mental accounting. Mental accounting provides a central explanation for violations of consumption smoothing ([Thaler, 1990](#)) and, as we show in this section, can shed light on how spending responses vary with time to anticipate receiving a windfall. A discussion of how other classes of models do not account for the evidence appears in [Appendix E](#).³³ We estimate our model using the weekly spending NCP data from [Section 3](#) and discuss implications for policy design.

6.1 Model specification and estimation

Our model focuses on describing spending decisions out of windfalls. The seminal work by [Shefrin and Thaler \(1988\)](#) argues that spending decisions depend on the magnitude and source of income changes. They describe wealth as separated into three mental accounts, each with a different MPC: current income (highest MPC), current assets, and future income (lowest MPC). In their framework, households classify additional income based on the magnitude of the change: “People tend to consume from income and leave perceived ‘wealth’ alone. The larger is a windfall, the more wealth-like it becomes.”

³³This includes models based on time preferences, reference dependence, rational inattention, and rational illiquidity. Also see [Section 2](#) for discussion of the life-cycle and permanent income hypothesis, the buffer-stock savings model, models with complete insurance markets, and hand-to-mouth models.

To describe how consumers mentally categorize windfalls, we posit that the MPC depends both on the magnitude of the windfall and on how long the decision maker anticipates the windfall. Our results suggest that the time dimension matters beyond the classification of income as “future” vs. “current” (or “anticipated” vs. “unanticipated”). In other words, the duration of anticipation plays an important role in determining how “wealth-like” a windfall feels to consumers. We take a reduced-form approach (Mullainathan, Schwartzstein and Congdon, 2012) to model the dependence of the MPC on the time dimension (see Section 6.3.2 for a discussion of possible explanations for this relationship).

We model the decision making of a consumer who learns of a windfall and processes it through three mental accounts: a current income account, an intermediate account, and a future income account. For simplicity, assume that the consumer has a positive MPC only for current income and narrowly brackets the windfall separately from other sources of income (Read, Loewenstein and Rabin, 1999). Information about a windfall of magnitude m arrives at time $t = 0$. Before the windfall arrives, the consumer thinks of it as future income. Once the windfall arrives, it enters a separate intermediate or windfall account. In each period t , consumers transfer a fraction $\mu(m, t)$ of the amount that remains in their windfall account to their current income or spending account.³⁴ If $\mu_m < 0$, then consumers treat smaller windfall amounts as current income to a greater extent than as wealth. If $\mu_t < 0$, then consumers treat windfalls that they learned about more recently as current income to a greater extent than as wealth.

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

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

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

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

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

³⁴This heuristic could arise from a model in which 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.

and Group 3 for 2 periods.³⁵

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

$$\mu(m, t) = \beta^m \alpha^t. \quad (9)$$

While we do not constrain the values of α or β in the estimation, note that if $\alpha, \beta \in (0, 1)$, then the consumer treats smaller windfalls and more recent windfalls as more spendable ($\mu_m < 0$ and $\mu_t < 0$). In addition, under this condition, actual windfall spending (y) will not exceed the amount that remains in the windfall account.

Equations (7) to (9) recursively define windfall spending in each period as a nonlinear function of parameters (α and β) and data (m and t).³⁶ 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 $\epsilon_{it} \stackrel{\text{i.i.d.}}{\sim} \mathcal{N}(0, \sigma^2)$. We use nonlinear least squares to estimate the resulting specification.

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

6.2 Results

6.2.1 Parameter estimates

Table 2 presents estimates of the model. In our preferred specification (Column 1, which uses a scaling factor of 3.33) we obtain $\beta = 0.9984$ and $\alpha = 0.57894$. 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 15 percent. We also calculate that the MPC decreases by the same amount from an additional one week of waiting as it would

³⁵This is consistent with the modeling choice by Kaplan and Violante (2014), which they refer to as the “intermediate informational assumption:” All households learn about the rebate payments upon disbursement of the first set of payments.

³⁶The resulting expression has the form $y_\tau = \beta^m \alpha^t m \mathbf{1}_{\{\tau=t\}} + (\beta^{(1-\beta^m)\alpha^t} m \alpha^{t+1} (1 - \beta^m \alpha^t) m) \mathbf{1}_{\{\tau=t+1\}} + \dots$

from increasing the size of the windfall by \$340, a quantity we refer to as the *waiting equivalent*.³⁷

To assess the sensitivity of our results, we vary the scaling factor and the informational assumptions. Varying the scaling factor corresponds to making different assumptions about the fraction of spending that the NCP data account for. We consider a range between 1 and 10 to encompass our preferred specification and an alternative from [Broda and Parker \(2014\)](#); they propose to use the share of self-reported ESP spending on household goods (13.7 percent), which would imply a scaling factor of 7.3. Although larger scaling factors result in larger estimates of α , the value of the waiting equivalent remains relatively stable across specifications as the remaining columns of [Table 2](#) show. The bottom panel of the table shows how well the estimated model matches the monthly spending moments in the data (see [Figure 2](#)), with the preferred specification providing the closest fit. Varying the informational assumptions corresponds to shifting the number of periods that households anticipate receiving the windfall. In particular, we consider the possibility that households in the earliest payment group anticipate receiving the payment starting at the time of the original IRS announcement, which ignores President Bush’s announcement soon before the payment dates that the Treasury would start distributing payments sooner than expected (see [Section 3.4.2](#) for a reminder of the timeline). We find that the waiting equivalents remain stable under the alternative specification, though the baseline informational assumption provides a better fit for the monthly spending moments (see [Appendix Table 14](#)).

The estimated model also reproduces key features of the weekly spending data. As [Appendix Table 15](#) documents, for groups that face shorter waiting times, the estimated model predicts that spending remains somewhat elevated as time passes. Since a shorter waiting time leads to a higher initial spending response, a smaller amount remains in the consumer’s windfall account, which partly mitigates the MPC reduction in the subsequent period. This provides an explanation for the finding that spending among households receiving stimulus payments in the earliest payment group exceeds that of households receiving payments in later groups even when conditioning on calendar week ([Appendix Figure 5](#)).

We also estimate the model separately for the subsamples in [Figure 2](#) and document substantial heterogeneity in waiting equivalents. As [Table 3](#) shows, for households that are liquidity constrained, those that do not make financial plans, and those that classify

³⁷We calculate the waiting equivalent by setting $\alpha = \beta^w$ and solving for w .

themselves as spenders, an additional week of waiting time reduces the MPC by as much as an additional \$450 to \$750 in the size of the windfall. By contrast, the waiting equivalents for unconstrained households, those that make financial plans, and those that classify themselves as savers range between \$150 and \$250.³⁸ Predicted spending estimates, shown in the bottom panel, generally follow the data reported in [Figure 2](#) (and [Appendix Table 5](#)). According to the estimates, households that do and households that do not make financial plans exhibit similar spending responses after two weeks of waiting, as do households that classify themselves as savers or spenders, consistent with the data. Perhaps not surprisingly, our model tends to underpredict the spending response of liquidity-constrained households.

6.2.2 Policy implications

We discuss the implications of our estimated model for three design features of fiscal stimulus policies: payment duration/frequency, payment amounts, and targeting.

Our model provides a possible explanation for the greater effectiveness of one-time payments (e.g., stimulus check) over flows of payments (e.g., reductions in withholding). [Sahm, Shapiro and Slemrod \(2012\)](#) describe arguments from academics and policymakers suggesting that a series of small payments may induce greater spending.³⁹ Their survey evidence on the 2008 stimulus payments and the 2009 reduction in withholding in the US shows the opposite result, contrary to the prediction of a mental-accounting framework based on the idea of smaller MPCs from larger payment amounts. Our work helps to resolve this tension by pointing out the crucial role of anticipation and timing, which suggests that a lower spending response to a series of smaller payments may result from having more time to anticipate receiving those payments. Quantitatively, the estimated model matches the difference in MPCs between the one-time payment and the reduction in withholding implied by the data from [Sahm, Shapiro and Slemrod \(2012\)](#). Their survey contains data on the fraction of households that use the additional income to mostly spend, mostly save, or mostly pay off debt, and we apply three methods from the literature, following recent work by [Feldman and Heffetz \(2021\)](#), to convert these data to MPC estimates. These methods imply an MPC ranging from

³⁸These characteristics (liquidity constraints, financial planning, and spender/saver tendency) are not highly correlated; their pairwise correlations are less than one-third.

³⁹[Feldman \(2010\)](#), for example, documents an increase in consumption in response to the 1992 decrease in US federal income tax withholding, which shifts lump-sum tax-refund income to additional monthly income.

0.22 to 0.41 for the reduction in withholding and ranging from 0.29 to 0.44 for the one-time payment. Despite the wide range across methods, all three approaches imply a difference in MPCs of only 0.03 to 0.07. Consistent with these data, when we model the reduction in withholding as a series of small windfalls of varying levels of anticipation, our estimates imply an MPC of 0.27 for the reduction in withholding compared to an MPC of 0.32 for the one-time payment (Table 4).

With some caveats, the model can also provide guidance on the optimal size of stimulus payments. On net, lower MPCs resulting from larger windfalls may decrease the total spending response. The model implies that the payment amounts that maximize spending are \$757, \$696, and \$664, respectively, for windfalls that arrive one, two, and three weeks after the announcement. An unanticipated windfall of \$346 would increase aggregate consumption by the same amount as a \$757 windfall that is anticipated for one week. For a windfall that arrives completely by surprise, the spending-maximizing amount implied by the model is \$872. These calculations abstract from differences in household characteristics, income, and other financial circumstances which would likely alter these conclusions.⁴⁰ In addition, whether the functional form we assume for μ constrains substantively important interactions between the time and magnitude of payment remains an open question that future work, given sufficiently detailed data, can investigate using our approach. Extensions of our methodology applied to larger datasets may provide further guidance on targeted payment amounts.

Finally, our results echo previous work supporting the common practice of providing broad-based stimulus payments over more narrowly targeted payments to increase aggregate consumption (e.g., see McDowall 2019 and Andreolli and Surico 2021). The addition of the time dimension does not alter this conclusion since MPCs in our model decrease with windfall size for any given waiting time.

6.3 Discussion

This section discusses broader implications of our framework for consumption decisions, theories of mental accounting, and macroeconomic policy.

⁴⁰The MPC may depend on the size of a windfall relative to income. See Kueng (2018) for evidence of higher MPCs among higher-income households in the context of the Alaska Permanent Fund Dividend payments.

6.3.1 Consumption smoothing

Despite the considerable empirical evidence related to consumption smoothing, the literature does not provide a consensus on when deviations from the standard model occur (Jappelli and Pistaferri, 2010). Our model reconciles seemingly conflicting results that consumption responds to anticipated payments in some settings but not others by emphasizing the timing of information and the time horizon over which households anticipate changes in income. For example, Spanish workers who receive extra paychecks as fully predictable non-performance-related bonus payments appear to smooth consumption (Browning and Collado, 2001), but consumption increases in response to receiving large predetermined payments from the Alaska Permanent Fund Dividend (PFD), even for high-income consumers (Kueng, 2018). Previous research investigates a “magnitude effect” whereby consumers smooth only when facing large income changes but finds mixed evidence (Kreinin, 1961; Souleles, 1999; Stephens and Unayama, 2011; Scholnick, 2013). In the case of the PFD, payments average \$1,650 to each Alaskan citizen or about \$4,600 per household (Kueng, 2018), which is comparable in scale to the bonus payments in Spain that provide households with one-fourteenth of their annual income in the form of an extra paycheck in June and December (Browning and Collado, 2001), yet the data show excess sensitivity in the former but not the latter setting. Viewing both of these as “anticipated” income changes would overlook a significant difference in timing: Spanish workers face virtually no uncertainty regarding the bonus payments due to the highly institutionalized system; Alaskan households, by contrast, learn about the size of their PFD payments through an official announcement from the governor in September, and they receive payments in October.⁴¹ Analyzing two different types of “anticipated” income changes in a consistent setting, Hori and Shimizutani (2009) find much higher marginal propensities to consume from end-of-year tax refunds than from extra paychecks using Japanese household-level data. Our model clarifies that the anticipated-unanticipated dichotomy may be misleading if consumption responses depend on the duration of anticipation.

⁴¹Despite the high predictability of the PFD payments at the end of the fiscal year in June, Alaskan households may rationally face uncertainty about the payments until the official announcement in September; for example, a gubernatorial veto in 2016 cut the dividend payments in half (a reduction of about \$2,300 per household) relative to their predicted value.

6.3.2 Models of mental accounting

Our model relates to a central idea in mental accounting that the process by which households set and manage budgets can meaningfully affect consumption patterns (Thaler, 1990). Ample evidence supports the prediction that consumers readily spend unexpected small windfalls (Arkes et al., 1994; Milkman and Beshears, 2009). However, existing theories leave open 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). We complement the existing literature by incorporating dynamics and thus enriching the description of the mental-accounting process.

Despite the pervasiveness of thinking about money, economic models generally offer little guidance as to how time spent anticipating future consumption affects decision-making. We view the reduced-form modeling approach as fruitful given the lack of a well-specified general model that explains how consumers categorize funds into different mental accounts. The passage of time may affect attitudes toward spending through multiple possible channels. First, having more time may enable consumers to exert self-control.⁴² Second, consumers may be able to formulate a forward-looking plan with additional time, especially if consumers have some flexibility in deciding how to earmark windfall income.⁴³ Third, time may encourage broad bracketing over narrow bracketing (Read, Loewenstein and Rabin, 1999). Fourth, the passage of time may result in long-anticipated windfalls feeling more like wealth due to reference-point effects (Thakral and Tô, 2021). Finally, decision makers may experience anticipatory utility, which results in more weight on future consumption with longer waiting times (Thakral and Tô, 2020). These channels provide possible interpretations for the reduced-form

⁴²Shefrin and Thaler (1988) argue that setting systems of personal rules such as mental accounts can help consumers overcome the conflict that arises between a planner and a doer through willpower effort. In addition to the planner-doer model (Thaler and Shefrin, 1981), other theories proposing two-system approaches include the hot-mode/cold-mode model of Bernheim and Rangel (2004), the affective-deliberative model of Loewenstein and O’Donoghue (2004), the automatic-process/control-process model of Benhabib and Bisin (2005), the dual-self model of Fudenberg and Levine (2006, 2012), and the dual-system model of Brocas and Carrillo (2008).

⁴³As Arkes et al. (1994) from the psychology literature explain, “unanticipated money may be in no account. Planning for its expenditure takes time. Until some reasonable target is decided upon, the money remains uncommitted and therefore available for extravagant, frivolous, or speculative use. When funds are anticipated, the budgeting process occurs before receipt of the funds. When the funds eventually arrive, they are not available to be spent on some whim.”

dependence of the MPC μ on the time dimension in our model.

6.3.3 Macroeconomic implications

Understanding how household spending responds to transitory variation in income at different time horizons provides a crucial input for evaluating the macroeconomic impact of tax and labor-market policies and for designing effective stabilization policies. A recent contribution due to [Auclert, Rognlie and Straub \(2018\)](#) highlights that while partial equilibrium analysis relies on estimates of the MPC, the matrix of iMPCs— $(M_{t,s})$, the period- t consumption response to additional income in period s —constitutes a sufficient statistic for general equilibrium responses to fiscal shocks and policies. They offer empirical estimates of the first column $(M_{t,0})$ of this matrix (the impulse response of spending to an unanticipated increase in income) and use a heterogeneous-agent model with illiquid assets to extrapolate the rest, resulting in very similar magnitudes for the spending response to income shocks that agents expect over different time horizons. Our finding that waiting times can dampen spending suggests that responses to anticipated increases in income may look meaningfully different.

7 Conclusion

We document a consistent set of new results across multiple settings using existing observational and experimental data. In the context of both developed and developing countries, additional time spent anticipating a windfall payment leads to lower consumption responses. This robust pattern holds across consumers differing by levels of income, liquidity, access to formal financial products, demographic characteristics, and the magnitude of windfall payments. 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 disbursement of payments. To encourage longer-term investments, as policymakers may desire when delivering cash transfers to impoverished households, announcing payments in advance may lead to more future-oriented decision-making.

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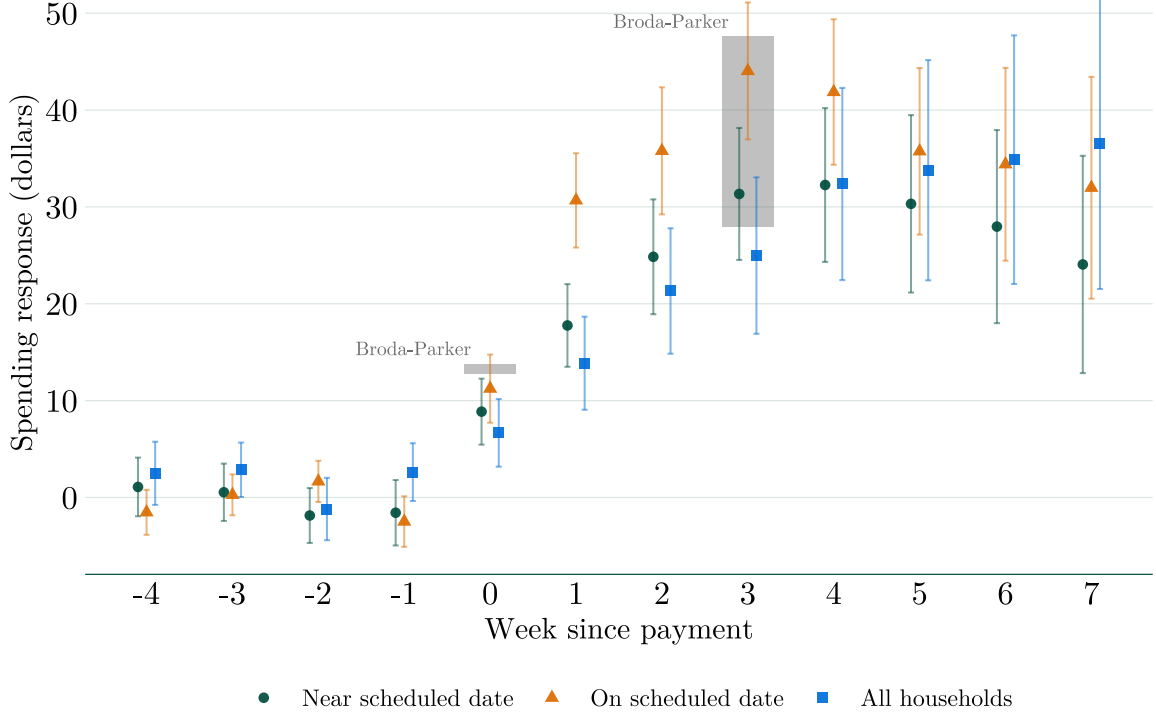
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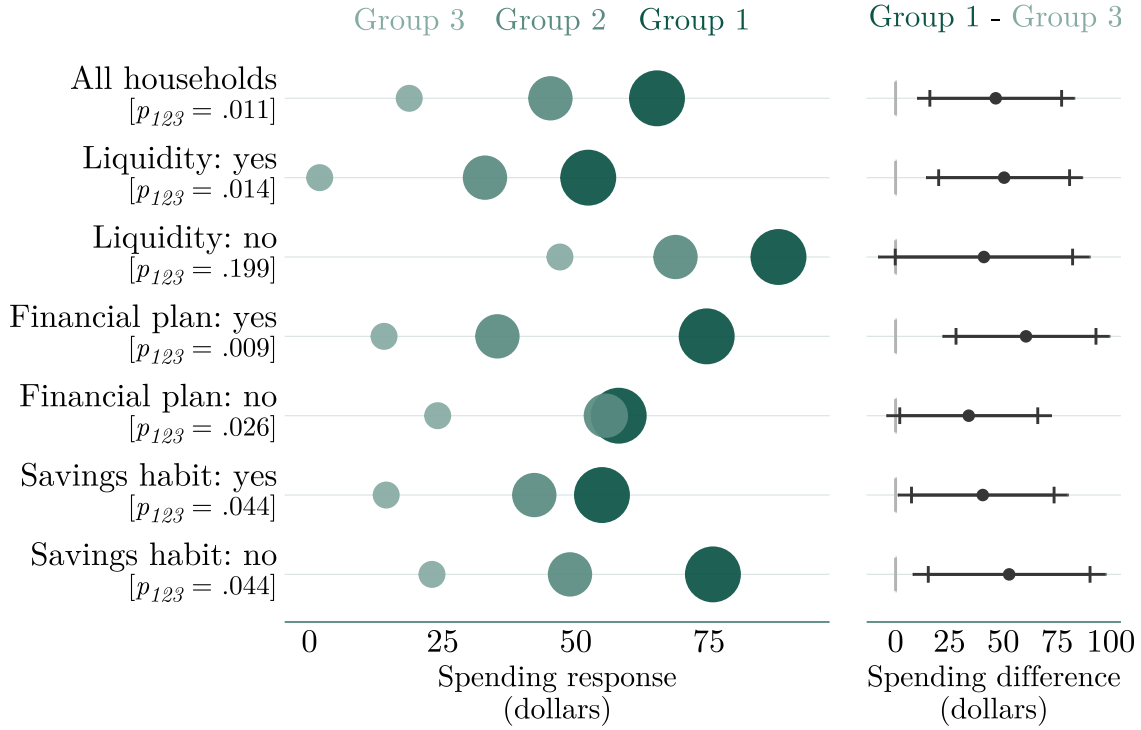
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Figure 1: ESP Spending Responses—Average Impacts



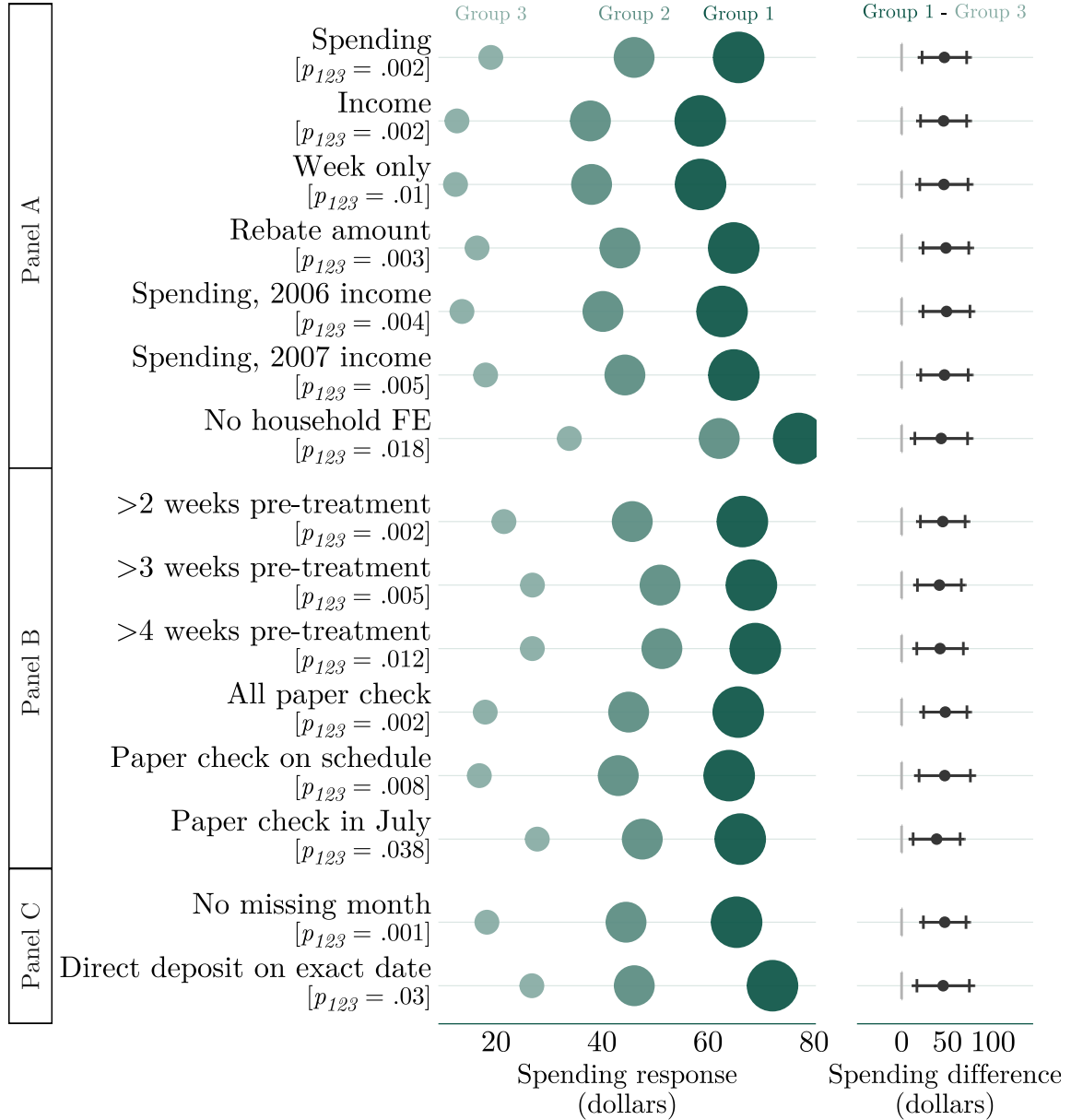
Note: This figure presents estimates of the weekly spending response γ_k (weeks -4 to -1) and the cumulative spending response Γ_k (weeks 0 to 7) from Equation (2) for various samples. For comparison, the shaded box denotes the range of point estimates reported by Broda and Parker (2014). The “Near scheduled date” sample consists of households receiving direct deposits three days leading up to the scheduled payment date or the weekend after. The “On scheduled date” sample consists of households receiving direct deposits on the date specified in Appendix Table 1. The “All households” sample consists of all households receiving direct deposits. Standard errors reported in parentheses are adjusted for clustering at the household level and obtained from a block-bootstrap procedure with 100 replicates. Calculated based on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business.

Figure 2: ESP Spending Responses by Timing of Payment



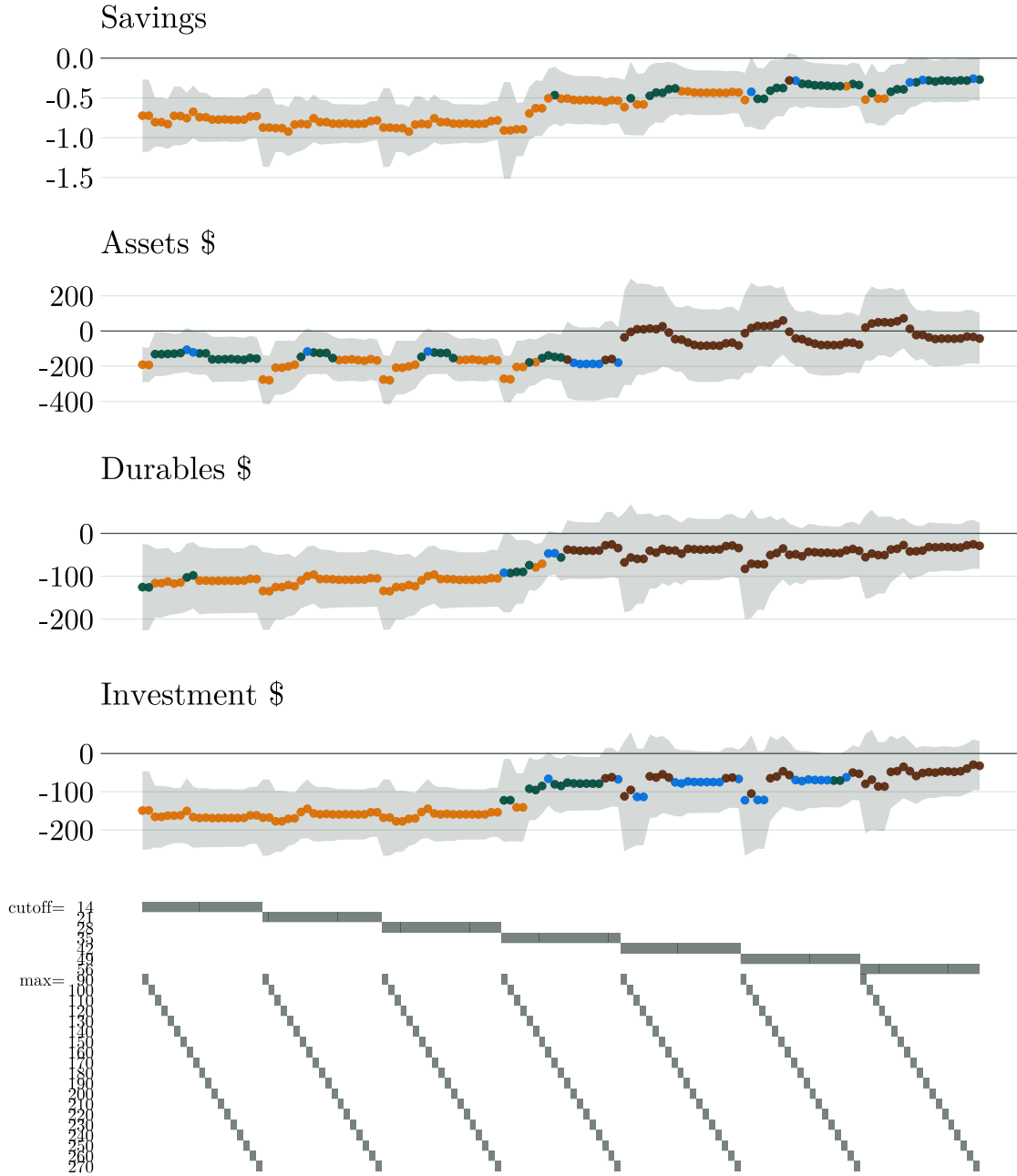
Note: The panel on the left presents estimates from Equation (4) of the four-week cumulative ESP spending response Γ_4^w for households receiving EFTs in the first (Group 1), second (Group 2), and third (Group 3) week of May, respectively, and the p -value labeled p_{123} corresponds to the null hypothesis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

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



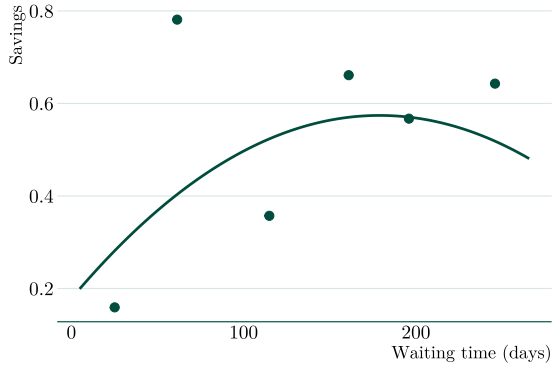
Note: The panel on the left presents estimates from alternative specifications of Equation (4) of the four-week cumulative ESP spending response Γ_4^w for households receiving EFTs in the first (Group 1), second (Group 2), and third (Group 3) week of May, respectively. Panel A considers alternative sets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

Figure 4: Impact of Shorter Wait for Cash Transfers (Kenya)

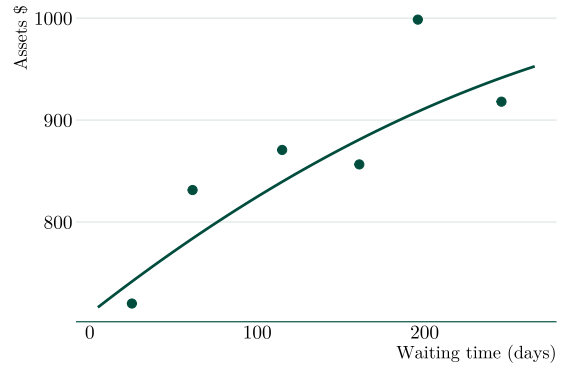


Note: Each dot corresponds to an estimate of the treatment effect, β_k , from Equation (5) and the associated 95 percent confidence interval. Each specification corresponds to a different definition of the treatment group (short waiting times) and the comparison group (long waiting times), with “cutoff” denoting the threshold for defining a short waiting time and “max” denoting the maximum number of days of waiting time in the comparison group. Savings is an indicator for reporting nonzero savings, and the remaining magnitudes are reported in 2012 USD PPP. Colors denote statistical significance at the 1 percent (orange), 5 percent (green), and 10 percent (blue) levels.

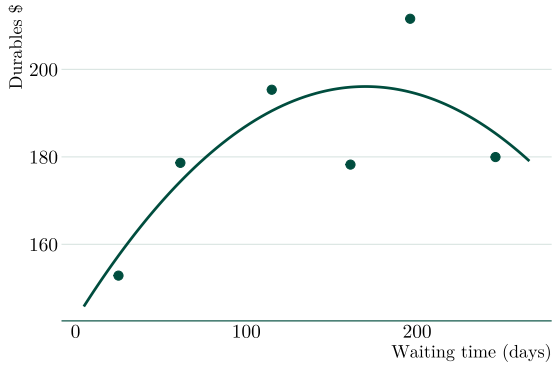
Figure 5: Relationship between Waiting Times and Outcomes (Kenya)



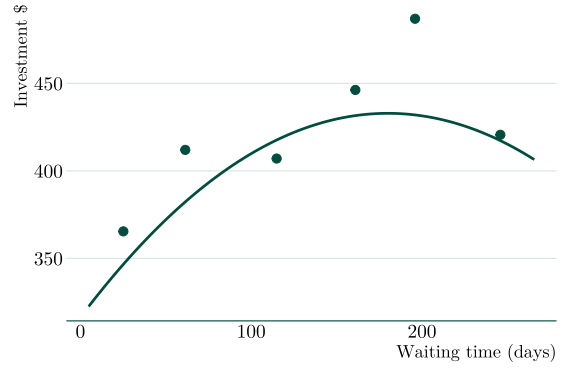
(a) Savings



(b) Assets



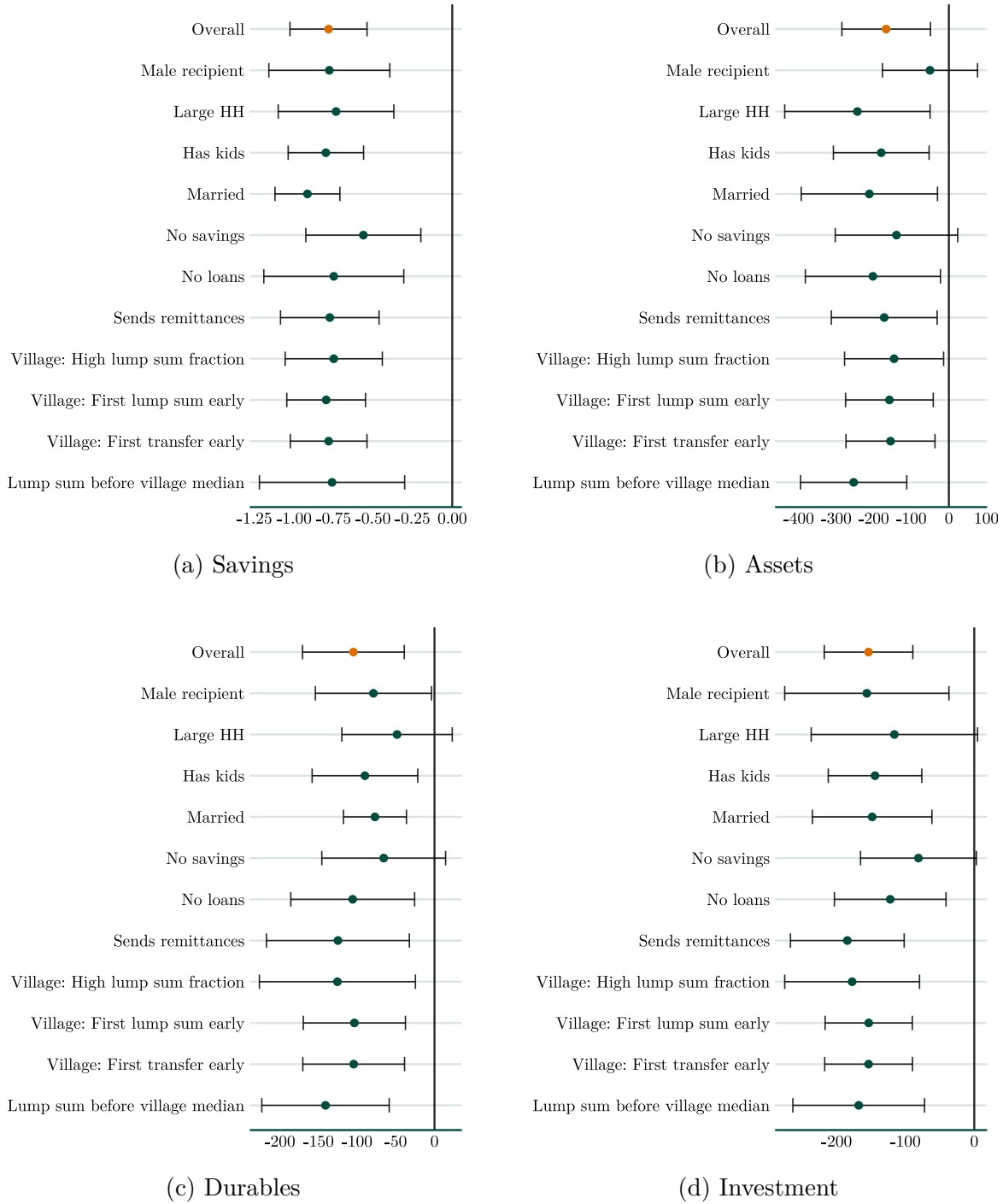
(c) Durables



(d) Investment

Note: Each figure depicts the relationship between waiting times and outcomes in the form of a binned scatterplot. The line shows the fit of a global second-order polynomial. See [Section 4.2](#) for details on the outcomes.

Figure 6: Impact of Shorter Wait for Cash Transfers (Kenya)—Heterogeneity



Note: Each figure depicts estimates of the treatment effect, β_k , from Equation (5) and the associated 95 percent confidence interval for various samples of households. See Section 4.3 for details on the samples.

Table 1: Impact of Non-Immediate Windfall on Savings (Malawi)

	(1) All	(2) Cash	(3) Direct Deposit
Total	77.95 (34.89)	82.35 (51.37)	57.38 (49.73)
In-kind	68.49 (25.59)	77.42 (37.04)	57.34 (34.04)
Financial	20.66 (17.05)	13.42 (24.78)	16.47 (24.67)

Note: Each cell presents estimates of β , the casual impact of a delayed relative to an immediate windfall, from Equation (6) for the outcome specified in the row and the sample specified in the column. The full sample (Column 1) consists of 474 households receiving MK 25,000 (USD 176.50 PPP) windfalls from the field experiment by Brune et al. (2017). The sample in Column (2) consists of 234 households randomly assigned to receive cash windfall payments, and the sample in Column (3) consists of 230 households randomly assigned to receive windfall payments deposited into an account with the bank. The outcome in the first row, total savings, combines in-kind savings and total financial assets. In-kind savings consist of advance purchases of farm inputs, business inventory, and bags of maize. Total financial assets consist of formal savings (e.g., balances at bank, microfinance institution, and employee savings accounts) and informal savings (e.g., savings clubs, safely kept cash). All values are reported in USD PPP adjusted using the 2014 exchange rate 141.64 MK/USD. Standard errors are reported in parentheses.

Table 2: Mental Accounting Model—Estimates and Fit

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

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

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

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

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

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

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

Note: The top panel reports data from the Thomson Reuters/University of Michigan Surveys of Consumers documented by [Sahm, Shapiro and Slemrod \(2012, Table 1\)](#). The 2008 survey (Column 1) asks respondents how the tax rebates were affecting their spending; the 2009 survey (Column 2) asks respondents how the 2009 reduction in withholding is affecting their spending. The middle panel applies three methods from the literature to convert the survey responses into a measure of the MPC. The bottom panel states the prediction of our model; see [Section 6.2](#) for additional details. Derived based on data from Nielsen Consumer LLC and marketing databases provided through the NielsenIQ Datasets at the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business.