Supplementary Appendix for Excess Anticipation-Dependence in Consumption

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Abstract

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Contents

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A Settings with variation in anticipation duration

Our paper uses three different datasets which have the feature that there is variation in how much time elapses between when people are informed about receiving a cash windfall and when they actually receive it. This was the result of our best attempts to search for papers that have such variation in waiting times and have data on spending/saving. Tables S3 and S4 document these systematic efforts by a pair of research assistants.

Table S3 documents the Google Scholar search terms used in searching through the literature for papers that have this feature. The search terms consist of combinations of the phrases (in quotes) "cash transfer," "windfall," "time delay," "waiting time," "payment delay," "delayed," "consumption," and "savings." For each combination of terms they used, the research assistants examined the first 50 search results and made note of any papers that appear relevant. In addition, they were instructed to search for papers that cite and are cited by Haushofer and Shapiro (2016), Parker (2017), and Brune et al. (2017). The papers identified as potentially relevant from these searches are collected in Table S4. In addition, we have communicated with the authors of the papers that we analyze data from, and we have also solicited suggestions for references at presentations of our work in seminars and conferences in behavioral economics and public economics, and checked that any papers that were brought up are included in our list.

The notes in the final column of Table S4 explain the relevance of each paper to our research question. Many of the papers are eliminated due to not having variation in waiting times. Some papers focus on the predictability of payments and are eliminated because the variation in payment timing is entirely unanticipated. Other papers are eliminated because the timing of payment is not the only difference between treatment groups: settings involving weekly vs. lump-sum transfers lead to a difference in liquidity constraints across treatment groups; several settings involve changes in the payment method which change not only the payment timing but also the ease of accessing funds (e.g., mobile or electronically); one of the papers combines delayed transfer payments with conditions on saving and educational investment; and one of the papers involves payment delays due to beneficiaries filing paperwork late. Two papers are eliminated because they consider one-time decisions or do not collect consumption data: one paper focuses on charitable giving rather than consumption, and the other studies effort decisions and a one-time decision to redeem a coupon rather than a consumption-savings problem. We analyze the datasets from the remaining papers in our work.

B Conceptual framework

We first describe how consumption responds to predictable income changes through a standard benchmark model in which agents maximize discounted expected utility subject to an intertemporal budget constraint (Deaton, 1992). We enumerate the model's core predictions, including a new anticipation-dependence notion that we define, and describe conditions under which testing for

excess anticipation-dependence can distinguish among various theories of consumption. In particular, we establish that when no anticipatory spending occurs in response to a future income shock, excess anticipation-dependence rejects forward-looking theories of consumption. We then use the framework of *intertemporal marginal propensities to consume* (Auclert, Rognlie and Straub, 2024) to define the empirical quantities of interest, state our hypotheses, and characterize the empirical strategies.

B.1 Predictions of benchmark model

With a time-separable utility function $u(\cdot)$, assuming that consumers can borrow and lend at interest rate r and have intertemporal discount rate δ, consumption $c_{i,t}$ for individual i at time t satisfies the standard Euler equation: $u'(c_{i,t}) = \frac{1}{1+\delta}\mathbb{E}_t\big[(1+r)u'(c_{i,t+1})\big]$. If the interest rate equals the intertemporal discount rate, then marginal utility follows a martingale process: $u'(c_{i,t}) = \mathbb{E}_t\big[u'(c_{i,t+1})\big]$. In other words, given the information available in period t, the individual chooses $c_{i,t}$ to equate the marginal utility of present consumption with the expected marginal utility of future consumption. Any change in the marginal utility of consumption from period t to period $t+1$ must therefore result from new information in period $t+1$. In particular, past information cannot predict changes in marginal utility: Consumers incorporate expectations of income changes in their optimal consumption plans as soon as they learn about such changes, so the marginal utility of consumption does not change when predictable changes in income occur.

This benchmark model thus makes a clear prediction about the consumption response to a transitory income shock: Upon learning about a transitory income shock, consumption changes immediately and remains constant thereafter, with individuals consuming only the annuity value of the income shock (Jappelli and Pistaferri, 2010, 2017). This has straightforward implications for how consumption responds before the shock, at the time of the shock, and after the shock:

> *Implication 1*: Upon learning about the shock (period 0), consumption immediately changes, with no further changes before the shock occurs.
>
> *Implication 2*: Consumption does not change from the period preceding the shock to the period when the shock occurs (period $t-1$ to period t).
>
> *Implication 3*: The marginal propensity to consume out of the shock in the period of the shock and the periods after the shock does not depend on when the shock occurs.

In this case, as in "the Barro (1974) definition of Ricardian equivalence, the time path of (lump-sum) taxes used to finance any given fiscal expenditure is irrelevant for consumption" (Wolf, 2021).

Canonical tests of theories of consumption focus on the first two implications, using the term *excess smoothness* to describe a failure of Implication 1 (Deaton, 1987; West, 1988; Campbell and Deaton, 1989) and the term *excess sensitivity* to describe a failure of Implication 2 (Hall, 1978; Flavin, 1981).

The third implication is the main subject of this paper. We use the term *excess anticipation-dependence* to describe a failure of Implication 3. Several factors help explain why Implication 3, despite its simplicity, does not receive attention in previous work.[1] First, this prediction holds under a large class of alternative models, including models that accommodate failures of Implications 1 and 2 (see Supp. Appendix B.2). Second, testing Implication 3 imposes more demanding data requirements (see Supp. Appendix B.3).

B.2 Predictions of alternative models

The benchmark model above captures the basic intuition of the life-cycle and permanent income hypothesis (Modigliani and Brumberg, 1954; Friedman, 1957; Hall, 1978) that consumption responds little, if at all, to anticipated income changes. Likewise, in models that incorporate precautionary savings (Leland, 1968; Sandmo, 1970; Dreze and Modigliani, 1972), e.g., due to incomplete markets and income risk, consumers use savings to smooth income fluctuations. In these models, Implications 1–3 continue to hold.

Implications 1 and 2 may fail to hold in models that incorporate liquidity constraints (Hayashi, 1985; Zeldes, 1989), e.g., due to imperfect credit markets, and therefore also in models that combine precautionary saving motives and liquidity constraints (Aiyagari, 1994) such as buffer-stock models (Deaton, 1991; Besley, 1995; Carroll, 1997). Even if Implications 1 and 2 fail, Implication 3 continues to hold when no anticipatory consumption takes place because these models predict that consumers respond to changes anticipated over any duration the same as they would to an unexpected income change.

Of course, models based on liquidity constraints can only explain violations of Implications 1 and 2 for a subset of consumers: They explain neither the lack of an aggregate response to information about future income changes (McDowall, 2019; Fuster, Kaplan and Zafar, 2021) nor the evidence that households with high levels of liquidity also respond to predictable income changes (Kueng, 2018; Olafsson and Pagel, 2018; Ganong and Noel, 2019; McDowall, 2019; Baugh et al., 2021).[2] The same holds for a large class of models that explain excess sensitivity by adding heterogeneity, for example in discount factors (Krusell and Smith, 1998) or by adding a fraction of "rule of thumb" consumers (Campbell and Mankiw, 1989). This also applies to models with wealthy hand-to-mouth agents who hold illiquid assets that have transaction costs (Kaplan and Violante, 2014) and models with consumption adjustment costs more generally (Grossman and Laroque, 1990; Flavin and Nakagawa, 2008). These extensions of the benchmark model cannot explain violations of Implication 1 in the aggregate or violations of Implication 2 for households with liquidity.

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

[2] These papers present evidence from personal finance apps or household financial accounts. Also see earlier evidence using survey data, e.g., Poterba (1988); Heim (2007); Mertens and Ravn (2012); Stephens and Unayama (2011); Broda and Parker (2014).

A key feature of all the models discussed above is that consumption decisions are forward-looking (Browning and Lusardi, 1996). This also applies to many popular extensions incorporating persistent household behavioral characteristics. For example, models of time-inconsistent preferences (Laibson, 1997; Angeletos et al., 2001), temptation (Gul and Pesendorfer, 2004; Bucciol, 2012), and reference dependence (Kőszegi and Rabin, 2009; Pagel, 2017) maintain forward-looking decision making (see Appendix A.3). Under the wide range of theories in which consumers make forward-looking decisions, when households do not respond to information about future income changes, consumers react to predictable income changes as if those changes were unexpected. In other words, Implication 3 holds in a broad class of models in which Implications 1 and 2 fail.

Implication 3 therefore distinguishes forward-looking behavior from alternatives. A violation of Implication 3 requires a theory that incorporates backward-looking elements; Section 4 discusses this in more detail.

B.3 Intertemporal MPCs

The general equilibrium effects and policy implications of failures of Implications 1–3 depend on a set of empirical quantities that Auclert, Rognlie and Straub (2024) refer to as the intertemporal marginal propensities to consume (iMPCs). Letting C_t and Z_t denote period-t aggregate consumption and real after-tax income, respectively, define the intertemporal marginal propensities to consume 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 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.

Stated in the iMPC framework, failures of Implications 1–3 correspond to the following:

1. Excess smoothness—No change in spending upon learning about a windfall: $M_{0,s} = 0$.
2. Excess sensitivity—Increase in spending after receiving an anticipated windfall: $M_{s,s} > M_{s-1,s}$.
3. Excess anticipation-dependence—Spending responses upon receiving additional income depend on the duration of anticipation: Γ_t^s is not independent of s.

Testing these implications requires observing how consumption changes in response to news about future income (Implication 1) and the onset of previously expected changes in income (Implications 2 and 3). Moreover, testing Implication 3 requires exogenous variation in s, i.e., the duration between when a consumer learns about an income shock and when the shock occurs.

As Supp. Appendix B.2 highlights, when no anticipatory spending occurs, testing Implication 3 provides a way to differentiate between classes of consumption theories based on whether they impose forward-looking behavior.

We cannot rely on existing empirical estimates of the iMPC matrix to test for excess anticipation-dependence (Implication 3). Existing estimates use exogenous variation to estimate the first column of this matrix ($M_{t,0}$, the impulse response of spending to an unanticipated increase in income) and calibrate a heterogeneous-agent model with illiquid assets to extrapolate the response to future anticipated income shocks (Auclert, Rognlie and Straub, 2024).

Our systematic survey of the literature reveals three settings with exogenous variation in the timing of payment (see Supp. Appendix A), which we analyze in Sections 3, 5.1 and 5.2. Our empirical findings suggest that the elements in the main diagonal of the iMPC matrix appear in decreasing order (i.e., $M_{k,k} > M_{k+1,k+1}$), which can provide some guidance for selection across models, though we note that the stimulus payment setting does not allow us to pin down anticipation effects for several quarters in the future. We also present results from a meta-analysis of the literature estimating MPCs in response to anticipated payments that include longer time horizons, examining how estimated consumption responses vary with the time horizon over which households anticipate receiving a payment, in Supp. Appendix E.

C Economic stimulus payments

C.1 Data

C.1.1 Balance

The disbursement schedule announced by the IRS in February 2008, reported in Table S1, provides a natural experiment for analyzing how the timing of payments relative to when they are announced influences consumption. Tables A2 and A3 test whether the characteristics of households that report receiving stimulus payments in different weeks via direct deposit and paper check, respectively, appears consistent with randomization. The sample of households receiving ESPs by direct deposit appears to be randomly distributed across the scheduled payment dates (Table A2). However, among the sample of households receiving ESPs by paper check, our balance tests reveal systematic differences by payment date across a wide range of characteristics (Table A3). These patterns could arise due to a systematic relationship between household characteristics and reporting payment dates inaccurately among respondents receiving paper checks. Another possibility, even if households report payment dates accurately, is that the group of households receiving payments in the later weeks consists not only of households that were randomized into receiving payments in those weeks but also late filers (whose rebate payments were not able to be paid on time) which represent a non-random selection of the population. Since households that did not file their tax returns on time could receive stimulus payments later than dictated by the disbursement schedule, households who receive paper checks late (but still within the randomized disbursement period) would be

misclassified as being randomly assigned to a late payment date.[3] Our analysis therefore focuses primarily on the former group.

Among households receiving electronic funds transfers via direct deposit in different weeks (Table A2), out of 33 tests of equality, none are significant at the 5 percent level, and one is significant at the 10 percent level. At the 15 percent significance level, we reject the null hypothesis of equality across the three groups of households in a total of 2 out of 33 cases (6.1 percent). At the 20 percent significance level, we reject the null hypothesis of equality across the three groups of households in a total of 7 out of 33 cases (21.2 percent). At the 25 percent significance level, we reject the null hypothesis of equality across the three groups of households in a total of 7 out of 33 cases (21.2 percent). The pattern of results appears consistent with reported payment dates among the households in our sample receiving electronic funds transfers reflecting the randomized disbursement schedule.

Households in our sample receiving payments via paper check in different weeks (Table A3) exhibit significant differences along a wide range of characteristics. We find significant differences at the 1 percent and 5 percent levels, respectively, in a total of 4 and 11 characteristics, respectively, out of 33. Rebate amounts systematically vary across households receiving paper checks in different weeks: $858.30 for households in the first three weeks (Group 1), $818.80 in the weeks 4–6 (Group 2), and $842.37 in weeks 7–9 (Group 3). Households that received payments in the first six weeks are more likely to report receiving less than they expected. Households that received payments in the earlier weeks are also larger in size, more likely to have children, less likely to describe themselves as having saving habits, and less likely to be in the lowest income group (<$15k).

C.1.2 Variable definitions

Some of the variables that appear in the balance tables come from a survey due to Broda and Parker (2014). The following provides additional information about those variables. Baseline spending refers to weekly spending during the first quarter of 2008. Liquidity is an indicator for reporting that the household has at least two months of income available in easily accessible funds. Spending habit is an indicator for reporting that household members would rather spend their money and enjoy it today than save more for the future. Regrets purchases is an indicator for reporting that household members often or occasionally, rather than rarely or never, make purchases that they later regret. 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.

C.2 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

[3]The confounding presence of late filers would be more likely to pose concerns for households receiving paper check due to the longer and later disbursement period (9 weeks starting May 16) compared to households receiving direct deposits (3 weeks starting May 2).

alternative sample restrictions.

We begin by considering alternative sets of characteristics in the first step of the estimation (Panel A of Figure A4). In our baseline specification, these characteristics include deciles of pre-rebate average expenditure and six income categories. Removing the income categories from the set Θ does not change the magnitudes of the estimated ESP spending impacts. Instead removing the expenditure deciles leads to slightly smaller estimates, though the differences across households receiving ESPs in different weeks remains equally substantial. The same holds if we remove both sets of characteristics and include only household fixed effects and period fixed effects. Allowing for differential spending trends based on the rebate amount leads to similar magnitudes as our main specification, as does replacing contemporaneous income with lagged values of income (for which the data contain much fewer missing values). Omitting household fixed effects leads to somewhat larger estimates.

We next consider alternative sets of comparison households (Panel B of Figure A4). 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 comparison households to only those receiving paper checks, or further restricting to those that receive paper checks near the scheduled payment dates, leads to similar estimates of the ESP spending impacts. We obtain slightly larger point estimates if we use comparison households receiving paper checks in July to ensure that the composition of households used to estimate each of the week fixed effects in Equation (1) remains stable. In our main specification as well as each of these alternative specifications, we find no significant spending responses in the weeks prior to receiving the ESP, providing evidence to support the validity of the estimated counterfactual spending trend (Table A4).

Lastly, we examine how our estimates change under different sample restrictions (Panel C of Figure A4). 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.

C.3 Implications for 2020–2021 stimulus

Some crises may necessitate larger payments or a series of payments. Our model suggests that (i) anticipated payment delays can undermine the effectiveness of efforts to stimulate spending, and (ii) larger payment amounts can also decrease spending. However, instead of making a single large payments or a series of pre-announced payments, a series of relatively unanticipated payments may lead to more effective fiscal stimulus. The 2020–2021 U.S. Economic Impact Payments in response to the COVID-19 pandemic provides a recent example.

The three rounds of Economic Impact Payments were approved in March 2020 (CARES Act), December 2020 (CRRSA Act), and March 2021 (American Rescue Plan Act), with payments largely sent in April 2020, January 2021, and March 2021, respectively. Armantier et al. (2021) find relatively similar MPCs across all three rounds of stimulus payments. In particular, they estimate approximately the same MPC of 0.25 in the month when the second and third rounds of payments were made.[4]

Discussions regarding the second round of stimulus payments started soon after households received the first round of payments in April 2020, and discussions regarding the third round of stimulus payments began soon after households received the second round of payments. Despite the lengthy deliberation periods for the second and third rounds of payments, the finding of similar MPCs is consistent with these stimulus payments having relatively similar durations between when households learn that they will be receiving payments and when they actually receive payments. Compared to the 2008 tax rebate, all three rounds of payments were made relatively quickly, with a large fraction of payments being sent out within two weeks. The MPCs reported by Armantier et al. (2021) for all three rounds of stimulus payments also align with the meta-analysis in Figure 3 for payments with short anticipation durations.

Overall, these results suggest that making a series of relatively unanticipated payments may provide a more effective approach to providing larger payments or payments over a longer time horizon.

D Unconditional cash transfers in Kenya

D.1 Data

This section provides additional details about the various analysis samples. The *complete sample* in the data from Haushofer and Shapiro (2016) consists of 1,008 households. Among the complete sample of 1,008 households, 503 receive cash transfers, and 505 are in the control group. The 503 treated households are cross-randomized into three treatment arms. One of the treatment arms randomizes the transfer frequency: 258 treated households receive transfer payments for nine months, paid on the first day of each month after they register with M-Pesa; 245 treated households receive a one-time lump-sum payment on the first day of a randomly selected month among the nine months following the date of the announcement. Another treatment arm randomizes the transfer amount: 366 treated households receive the smaller transfer amount of KES 24,000 (of which 193 received lump-sum transfers and 173 received monthly transfers), while 137 households receive an additional KES 70,000 (paid in seven monthly installments of KES 10,000) for a total of KES 94,000. A third treatment arm randomizes the transfer recipient within the household, namely whether the transfer targeted the wife or husband, whenever possible.

As Section 5.1 explains, our analysis focuses on households receiving lump-sum transfers on

[4]Their estimate for the first round of payments is slightly higher at 0.29, but this captures spending over a longer period of about three months.

randomly assigned payment dates. The larger transfer amounts, by contrast, all involve monthly payments on a common schedule: the KES 70,000 top-up payments are all paid in monthly installments starting in February 2012. Among the 193 treated households receiving one-time lump-sum payments of KES 24,000, the data contain a payment date for 186 households. We refer to this as the *lump-sum-transfer sample*. Our lump-sum-transfer sample has a somewhat lower rate of attrition than the complete sample. Among the 186 households in our lump-sum-transfer sample, 8 households (4.3 percent) do not have endline outcomes available compared to 68 out of 1,008 households (6.7 percent) in the complete sample. Compliance issues arise because some households had not received transfers before the endline survey took place, largely due to registration issues with M-Pesa. The research team administered the endline survey to measure outcomes between August and December 2012, while some treatment households received transfers in 2013. In our lump-sum-transfer sample, 6 households (3.2 percent) received transfers after the endline survey, a slightly lower rate of non-compliance than in the complete sample (18 out of 503 or 3.6 percent of treated households). Overall, 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 (primarily due to registration issues with M-Pesa), our sample consists of 172 households.

We use the random variation in payment dates among the households in the lump-sum treatment to estimate the impact of longer wait times. This contrasts with the analysis in the original impact evaluation by Haushofer and Shapiro (2016), which aimed to compare the monthly and lump-sum treatments (as well as a control group). In Haushofer and Shapiro (2016), the reason for randomizing the timing of the lump-sum transfers was to facilitate comparability with the monthly-transfer treatment.

We consider four broad outcome measures: savings, assets, durables, and investments. Our primary measure of savings consists of an indicator for having any savings, but we analyze the value of savings as well. 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. The measure of total assets also includes the value of any non-thatched roofs. Durables include furniture, agricultural tools, appliances, and other movable assets such as bicycles and cell phones. Durable investment consists of durable assets and non-agricultural business investment in durables. Non-durable investment consists of agricultural inputs, enterprise expenses, educational expenses, and savings. Investments consist of durable investment and non-durable investment. 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.

Haushofer and Shapiro (2016) analyze outcome variables in both levels (main results) and inverse-hyperbolic-sine form (robustness); however, our analysis does not consider any outcomes in inverse-hyperbolic-sine form. As Thakral and Tô (2025*b*) show, the inverse-hyperbolic-sine transformation (or any transformation other than power and log transformations) leads to results that depend on the measurement units of the data. They provide a demonstration using the

Haushofer and Shapiro (2016) data that the sign and significance of treatment effect estimates can reverse depending on whether outcomes are measured in U.S. Dollars or Kenyan Shillings.

D.2 Estimation: Standard errors

The unit of randomization for our analysis is at the household level. Since all of the outcomes we consider are measured at the household level, the standard errors do not need to be adjusted for clustering. The analysis of balance and treatment effects in Haushofer and Shapiro (2016) consists of some outcomes that are measured at the individual level, and in those cases their standard errors are adjusted for clustering at the household level. Haushofer and Shapiro (2016) also analyze spillover effects within villages, and for that analysis the standard errors are clustered at the village level; however, we do not conduct any village-level analyses.

E Variation in anticipation duration across settings

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).[5] Our approach 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). 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.

To understand why consumption responds to anticipated payments in some settings but not others, previous research investigates a "magnitude hypothesis" (that is, 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; for Alaskan households, by contrast, uncertainty remains about the size of their PFD payments until the official announcement from the governor in September, and they receive payments in October. Despite the

[5] Also see earlier surveys by Deaton (1992) and Browning and Crossley (2001).

high predictability of the PFD payments at the end of the fiscal year in June, Alaskan households 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.

Meta-analysis. We use data from a recent meta-analysis by Havranek and Sokolova (2020), which covers four decades of research estimating MPCs, to evaluate how the time horizon over which households anticipate changes in income affects spending. Havranek and Sokolova (2020) focus on published studies that satisfy two criteria: (1) "the study must present an empirical estimate of the consumption response to changes in income that are unlikely to have a contemporaneous effect on expected lifetime income, i.e. to anticipated or unanticipated transitory shocks;" and (2) "the study must report standard errors for its estimates or other statistics from which standard errors can be computed." Their dataset consists of 3,127 estimates from 144 published studies that satisfy these criteria. Much of this literature consists of Euler-equation-based studies that rely on time-series variation, which "strictly speaking...is designed only to test the hypothesis of zero excess sensitivity" (Havranek and Sokolova, 2020). Section 5 of their article separately analyzes the 654 MPC estimates from "strong studies," which refers to the "17 studies that identify a causal parameter from plausibly exogenous variation."

We analyze the MPC estimates from these "strong studies." A pair of research assistants independently read each of the "strong studies" and recorded the associated anticipation duration. We distinguish "long-anticipated payments," in which consumers may know about income changes at least one year in advance (e.g., changes in tax policy announced a year in advance, paycheck income, and loan repayment terms), from settings in which the number of months between the time when consumers learn about the income change and the time when the income change occurs can be determined (e.g., those that focus on stimulus payments, tax refunds, and dividend payments). With these data, we conduct four sets of analyses of the relationship between MPCs and anticipation duration.

First, we plot the relationship between anticipation duration and estimated MPCs. For this analysis, we classify long-anticipated payments as having an anticipation duration of 12 months. Figure 3 presents a series of binned scatterplots that provide visual confirmation of a decreasing relationship between anticipation duration and MPCs. We estimate three different regression specifications: one that consists of no control variables; one that adds controls for the payment amount and the standard error of the MPC estimate; and one that also adds controls for whether the estimates reflect samples for which liquidity constraints bind or not, and whether the estimates reflect total consumption, food, or another specific category of consumption. Controlling for the payment amount accounts for the magnitude hypothesis discussed above. Controlling for the standard error of the MPC estimate accounts for publication bias. Controlling for study characteristics accounts for differences across settings. For each specification, we report results for subsamples that restrict to the most precise estimates (MPC standard error less than 0.3, 0.2, and 0.1); see the discussion of

publication bias by Havranek and Sokolova (2020). Each subfigure shows a negative relationship between anticipation duration and MPC estimates among studies with short anticipation durations. Additionally, as we also see in Table S9 below, the subfigures show that studies with long-anticipated payments have lower estimated MPCs that are close to zero. These figures also show that MPC estimates from studies with the shortest waiting times (about 0.1 to 0.2) align with the magnitudes we estimate: about 0.18 for Group 1 and 0.10 for Group 2.[6]

Second, we plot the relationship between estimated MPCs and payment magnitude, fit separately for long and short anticipation durations. The series of binned scatterplots in Figure A9 allow us to examine both the magnitude effect (how the estimated MPC varies with payment amount) and the duration effect (how the estimated MPC varies with anticipation duration). As before, the first, second, and third rows consist of the subsamples of MPC estimates with standard errors less than 0.3, 0.2, and 0.1, respectively. The three columns correspond to the same three specifications of controls as before. The results consistently show higher estimated MPCs in settings with short anticipation durations throughout the distribution of payment amounts. The figures provide particularly strong visual evidence after adjusting for differences in MPC standard errors. In contrast, the figures show less clear evidence on the magnitude hypothesis.

Third, we analyze the effect of an additional month of anticipation on the estimated MPC. This analysis focuses on studies for which the number of months of anticipation can be determined. For each specification, we report results for the full sample of "strong studies" as well as subsamples that restrict to the most precise estimates (MPC standard error less than 0.5, 0.4, 0.3, 0.2, and 0.1). The three columns correspond to the same three specifications of controls as before. All 18 of the resulting estimates in Table S8 show a consistent negative relationship between anticipation duration and the estimated MPC.

Fourth, we analyze how the estimated MPC differs between studies with long-anticipated payments and studies with short anticipation durations (three months or less). We consider the same three specifications of controls as before, and the same six samples as before. Across all five subsamples, Table S9 shows a negative relationship between long-anticipated payments and MPC estimates under the full set of controls. Regardless of which set of controls we use, a negative relationship between long-anticipated payments and MPC estimates arises when restricting to precisely estimated effects.

[6]We compute these MPCs by dividing the cumulative four-week spending responses in Table S2 by the average rebate amount (about $1,000), and then scaling by a factor of 3.33 since the NCP data account for about 30 percent of household spending (Coibion, Gorodnichenko and Koustas, 2021).

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F Supplementary Appendix Figures and Tables

Figure S1: Democratic Policy Committee Fact Sheet: Economic Stimulus Act of 2008

FACT SHEET | February 12, 2008

Congress Passes Stimulus Package to Help the Economy and America's Families

On February 7, 2008, both Houses of Congress passed **H.R. 5140**, the *Economic Stimulus Act of 2008*, by overwhelmingly bipartisan votes. In passing this legislation so quickly, the Democratic-led Congress has kept its promise to take action to respond to the economic downturn. While we recognize that there are structural problems in the economy that the short-term stimulus measure will not solve, **H.R. 5140**, will provide direct financial relief to those who need it most and are most likely to use it to jumpstart the slowing economy.

The legislation that the Democratic-led Congress has sent to the President will boost the economy with timely, targeted, and temporary measures to:

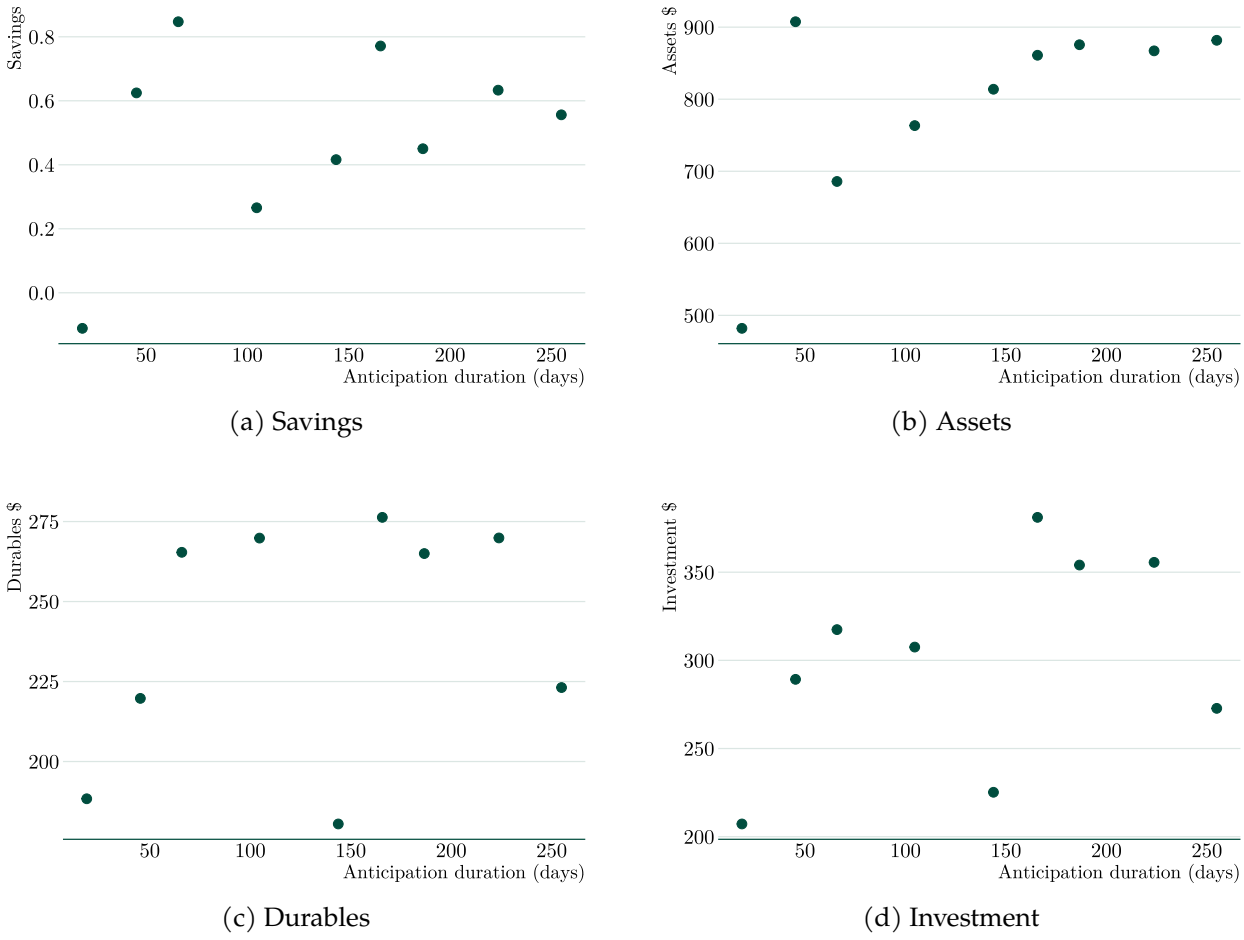
1)Put hundreds of dollars into the hands of millions of American families who will spend the money immediately to help reinvigorate the economy. The rebate has two components:

·**$300–$600 rebate (or $600–$1200 for married couples).**The rebate is based on a combination of qualifying income, gross income and tax liability. There is, for all eligible taxpayers, a minimum rebate of $300 ($600 for joint filers). The maximum rebate, which is based on tax liability, is $600 ($1200 for joint filers). Eligible taxpayers are those reporting: a) a non-zero tax liability and gross income greater than the sum of the basic standard deduction plus exemptions or; b) $3,000 of qualifying income, defined as the sum of net self employment income, veterans' disability payments (including payments to survivors of veterans), and Social Security retirement and disability benefits.

The compromise legislation sent to the White House to be signed into law will provide stimulus checks to most Americans including seniors living only on Social Security and disabled veterans, as Senate Democrats proposed. Stimulus checks for the survivors of disabled veterans have also been added. These Americans are most likely to spend stimulus funds quickly – fulfilling the bill's goal of increasing consumer spending and providing a short-term boost to the American economy.

Note: This figure displays an excerpt of the Democratic Policy Committee Fact Sheet regarding the goals of H.R. 5140, the Economic Stimulus Act of 2008: "The legislation…will boost the economy with timely, targeted, and temporary measures to: Put hundreds of dollars into the hands of millions of American families who will spend the money immediately to help reinvigorate the economy.…These Americans are most likely to spend stimulus funds quickly – fulfilling the bill's goal of increasing consumer spending and providing a short-term boost to the American economy." Source: https://www.dpc.senate.gov/dpcdoc.cfm?doc_name=fs-110-2-20

Figure S2: Relationship between Anticipation Durations and Outcomes (Kenya)



(a) Savings

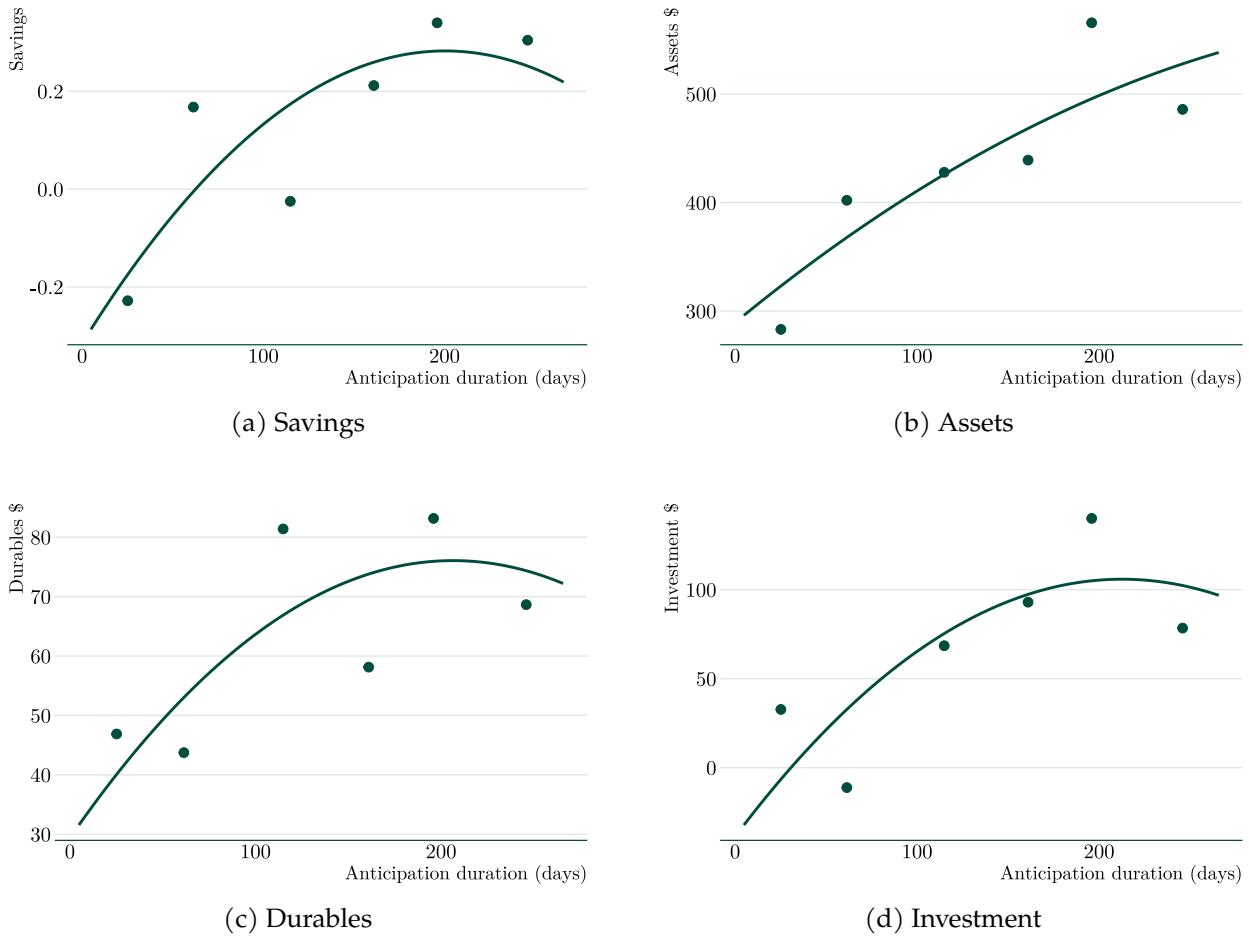
(b) Assets

(c) Durables

(d) Investment

Note: Each figure depicts the relationship between anticipation duration in days and the specified outcome (savings, assets, durables, and investments) in the form of a binned scatterplot. This figure is analogous to Figure A7, with the number of bins corresponding to the maximum number of months of waiting time. See Section 5.1.2 for additional details.

Figure S3: Relationship between Anticipation Durations and Difference between Endline and Baseline Outcomes (Kenya)



(a) Savings

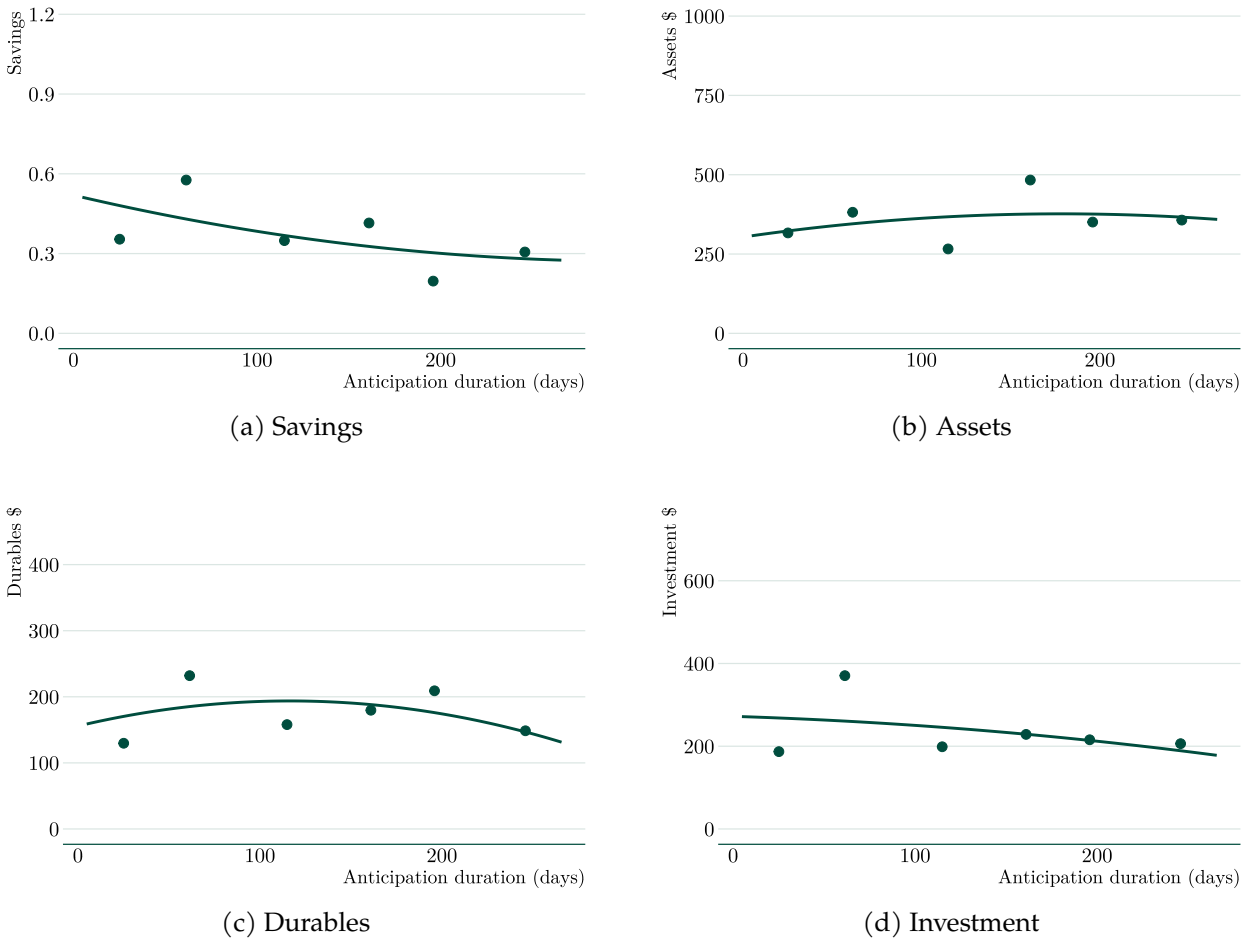
(b) Assets

(c) Durables

(d) Investment

Note: Each figure depicts the relationship between anticipation durations and the difference between endline and baseline outcomes in the form of a binned scatterplot. We use the rule-of-thumb integrated-mean-square-error optimal estimator of the number of bins (Cattaneo et al., 2019). The line shows the fit of a global second-order polynomial. See Section 5.1.2 for details on the outcomes.

Figure S4: Relationship between Anticipation Durations and Baseline Outcomes (Kenya)



(a) Savings

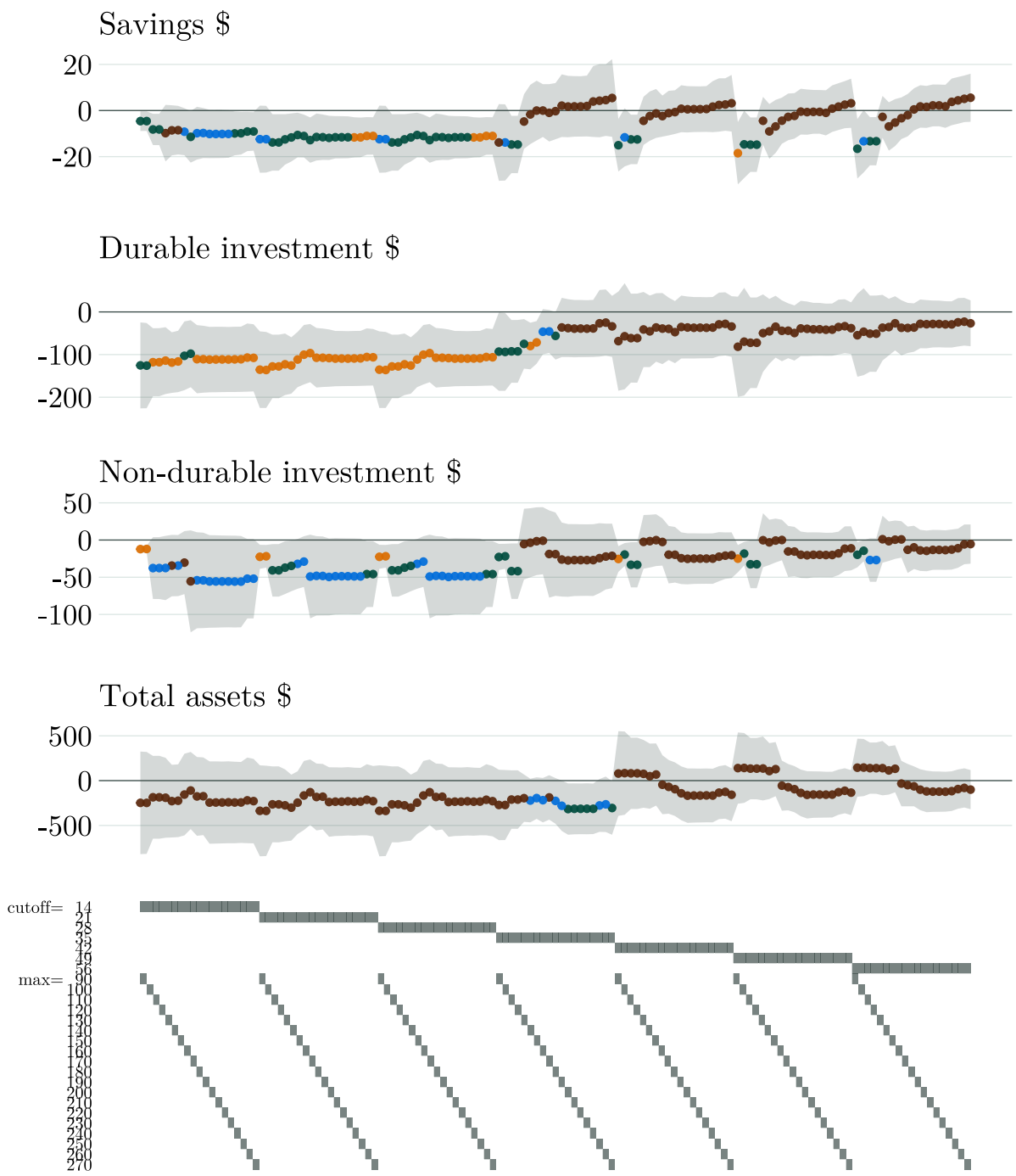
(b) Assets

(c) Durables

(d) Investment

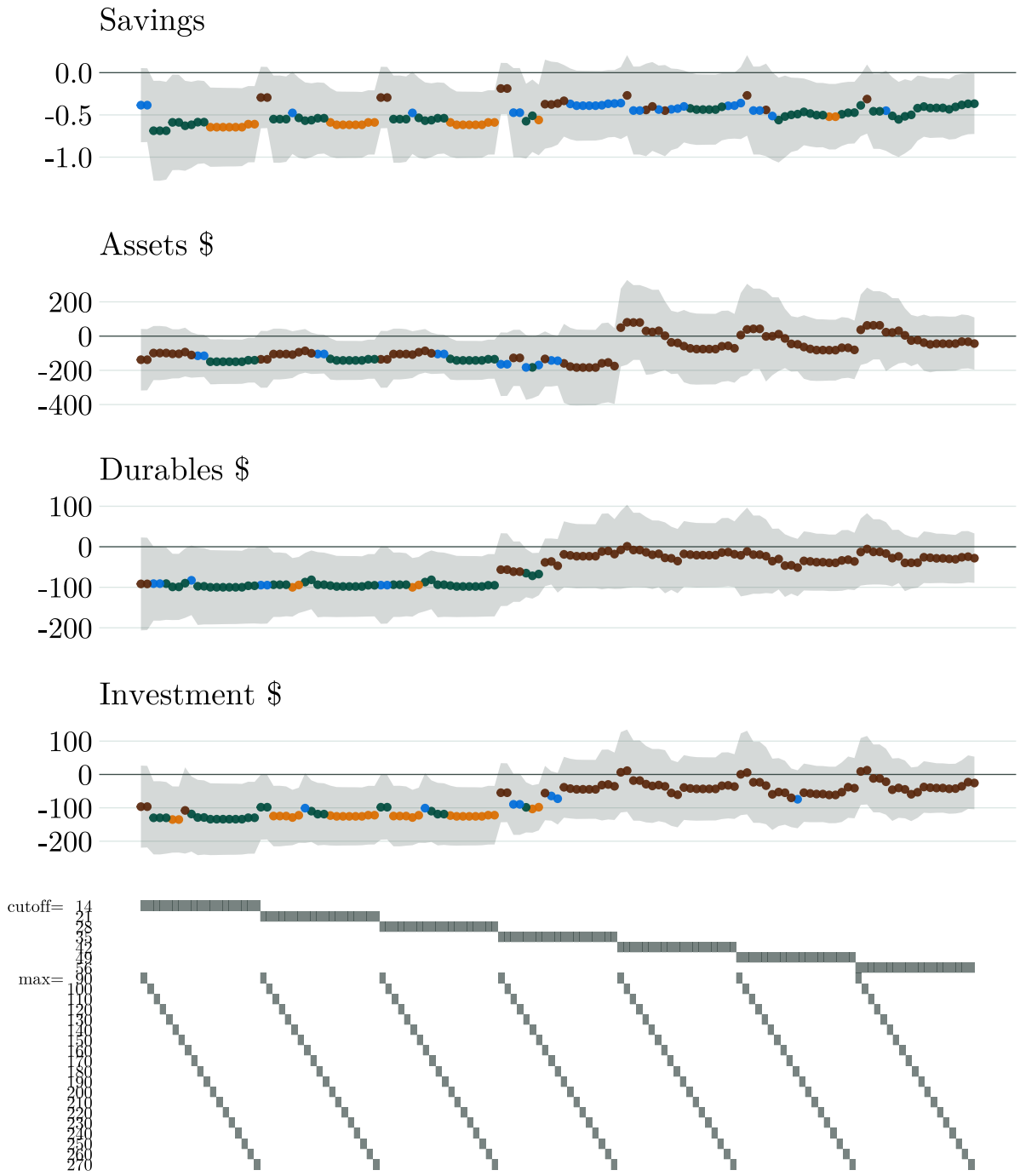
Note: Each figure depicts the relationship between anticipation durations and baseline outcomes in the form of a binned scatterplot. We use the rule-of-thumb integrated-mean-square-error optimal estimator of the number of bins (Cattaneo et al., 2019). The line shows the fit of a global second-order polynomial. See Section 5.1.2 for details on the outcomes.

Figure S5: Impact of Shorter Wait for Cash Transfers (Kenya)—Additional Outcomes



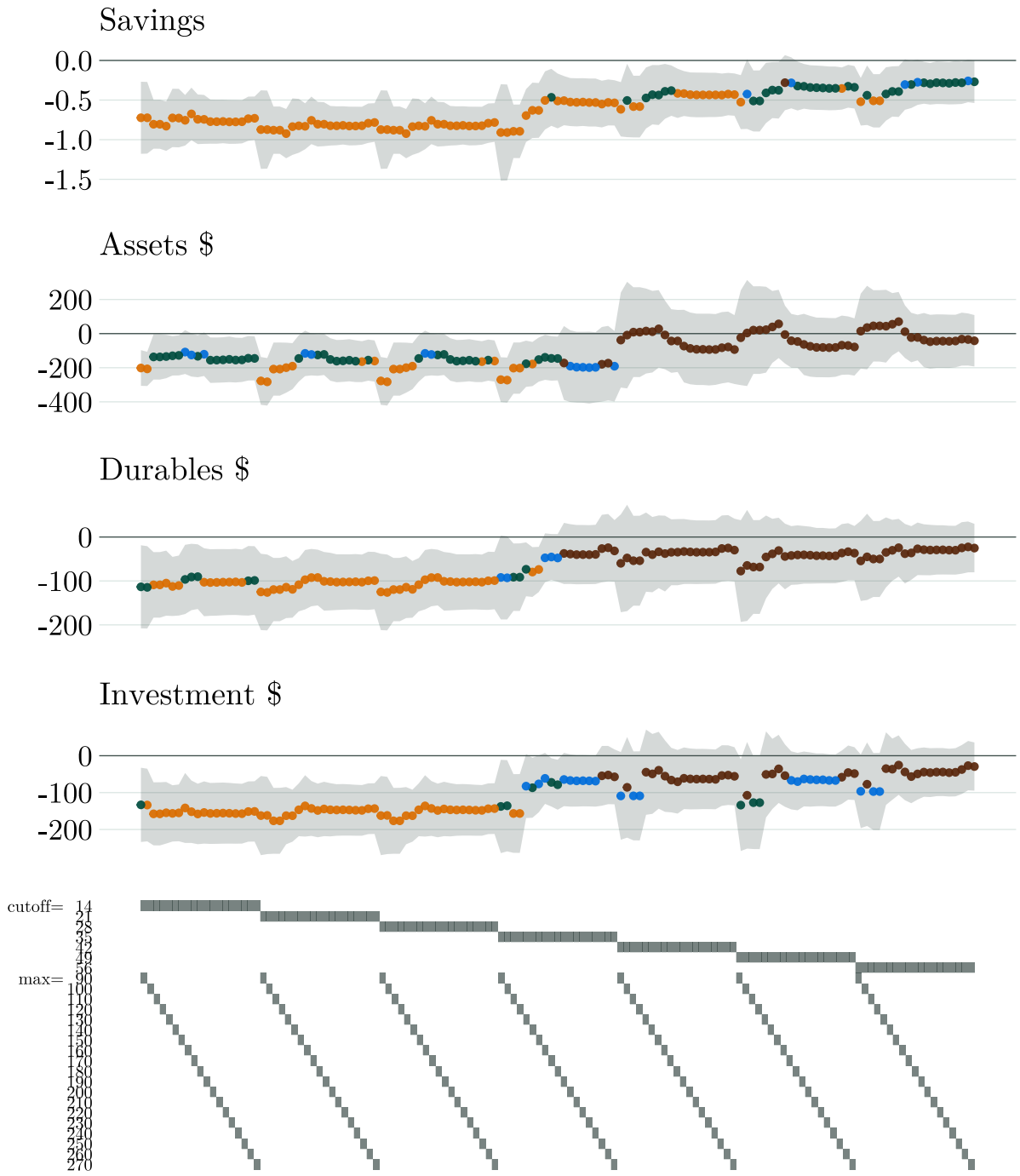
Note: Each specification corresponds to a different definition of the treatment group (short waiting times) and the comparison group (long waiting times), with "cutoff" denoting the threshold for defining a short waiting time and "max" denoting the maximum number of days of waiting time in the comparison group. See Figure A8 for additional information. Details about outcome variables appear in Supp. Appendix D.1. Colors denote statistical significance at the 1 percent (orange), 5 percent (green), and 10 percent (blue) levels.

Figure S6: Impact of Shorter Wait for Cash Transfers (Kenya)—Difference in Differences



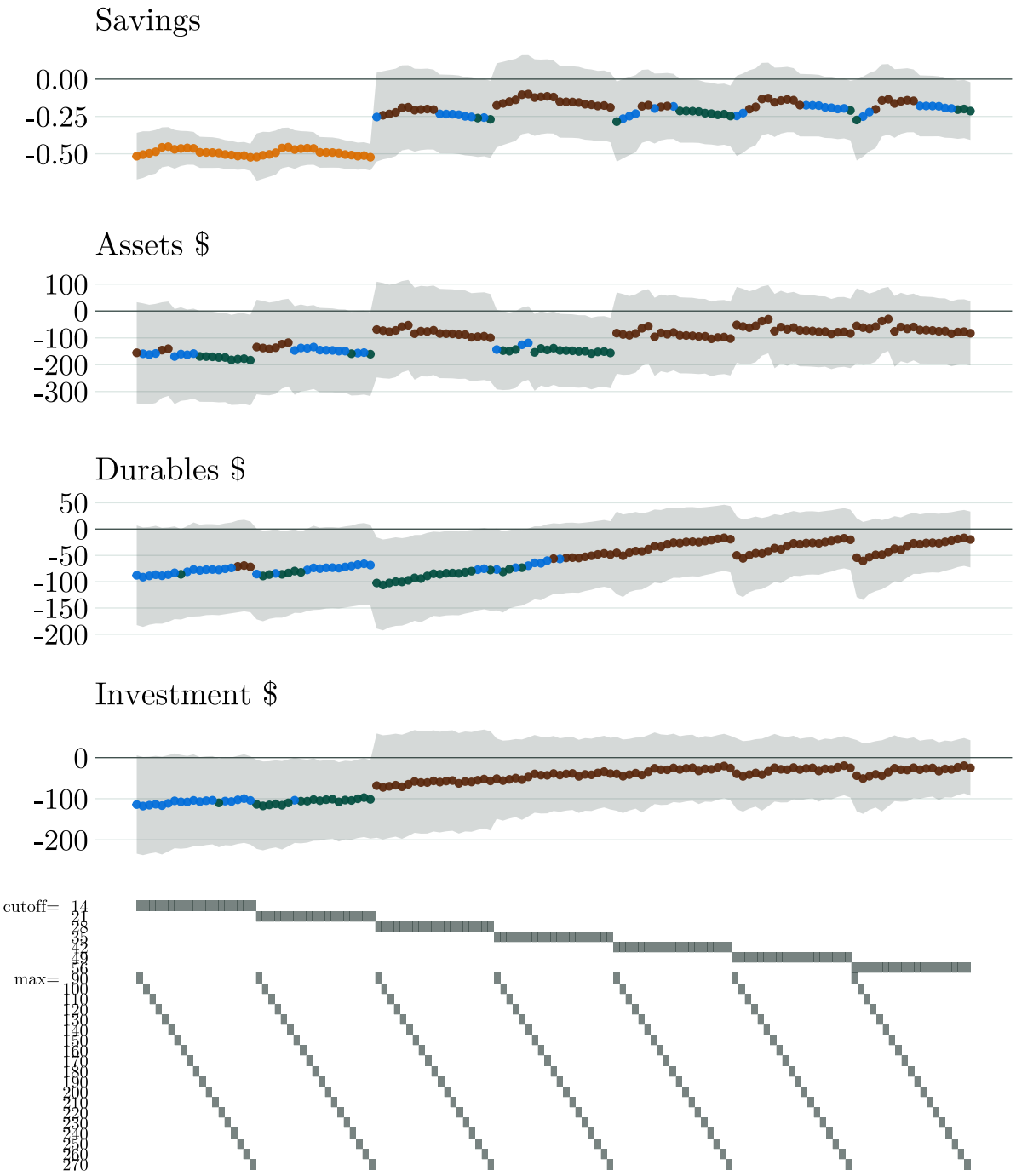
Note: Each specification corresponds to a different definition of the treatment group (short waiting times) and the comparison group (long waiting times), with "cutoff" denoting the threshold for defining a short waiting time and "max" denoting the maximum number of days of waiting time in the comparison group. See Figure A8 for additional information. Details about the estimation approach appear in Section 5.1.2. Colors denote statistical significance at the 1 percent (orange), 5 percent (green), and 10 percent (blue) levels.

Figure S7: Impact of Shorter Wait for Cash Transfers (Kenya)—Quadratic Baseline Controls



Note: 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. See Figure A8 for additional information. Details about the estimation approach appear in Section 5.1.2. Colors denote statistical significance at the 1 percent (orange), 5 percent (green), and 10 percent (blue) levels.

Figure S8: Impact of Shorter Wait for Cash Transfers (Kenya)—Without Village Fixed Effects



Note: 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. See Figure A8 for additional information. Details about the estimation approach appear in Section 5.1.2. Colors denote statistical significance at the 1 percent (orange), 5 percent (green), and 10 percent (blue) levels.

Figure S9: Expenditure Survey—Savings Method

SAVINGS					
Read: I would like to ask you about different ways in which people keep savings. First, we will ask bank accounts and then informal ways.					
[PERIOD] = the 7 day period starting from [Start Day] and ending on including [End Day]					
	Savings Method	**IS.1.** Does your household ever save money with [SAVING METHOD]?	**IS.2.** During [Period], how much money has your household added to [SAVING METHOD] / what is the value that you added to [SAVINGS METHOD]?	**IS.3.** During [Period], how much money has your household taken from [SAVING METHOD] / by how much have you reduced the value of [SAVINGS METHOD]?	**IS.4.** What is the total value your household, including all its members, holds in [SAVING METHOD]?
		2 - No ▸ **next row**	MWK	MWK	MWK
a)	NBS Bank Account	1 -Y 2 - N	|___|___|___|,|___|___|___|	|___|___|___|,|___|___|___|	|___|___|___|,|___|___|___|
aa)	Other bank or microfinance institution account	1 -Y 2 - N	|___|___|___|,|___|___|___|	|___|___|___|,|___|___|___|	|___|___|___|,|___|___|___|
b)	SACCO	1 -Y 2 - N	|___|___|___|,|___|___|___|	|___|___|___|,|___|___|___|	|___|___|___|,|___|___|___|
e)	Informal savings and credit group, ROSCA	1 -Y 2 - N	|___|___|___|,|___|___|___|	|___|___|___|,|___|___|___|	|___|___|___|,|___|___|___|
f)	Village Saving (kalabu yosunga ndalama)	1 -Y 2 - N	|___|___|___|,|___|___|___|	|___|___|___|,|___|___|___|	|___|___|___|,|___|___|___|
h)	Cash at home or in a secret hiding place that is not for your living expenses	1 -Y 2 - N	|___|___|___|,|___|___|___|	|___|___|___|,|___|___|___|	|___|___|___|,|___|___|___|
i)	Someone else for safe keeping	1 -Y 2 - N	|___|___|___|,|___|___|___|	|___|___|___|,|___|___|___|	|___|___|___|,|___|___|___|
j)	Advance buying of farm inputs	1 - YES	|___|___|___|,|___|___|___|	|___|___|___|,|___|___|___|	|___|___|___|,|___|___|___|
n)	Business Inventory	1 - YES	|___|___|___|,|___|___|___|	|___|___|___|,|___|___|___|	|___|___|___|,|___|___|___|
l)	Bags of Maize	1 - YES	|___|___|___|,|___|___|___|	|___|___|___|,|___|___|___|	|___|___|___|,|___|___|___|
k)	Other, specify:_____	1 -Y 2 - N	|___|___|___|,|___|___|___|	|___|___|___|,|___|___|___|	|___|___|___|,|___|___|___|

Cash on Hand		MWK
m)	**IS1.** How much cash do you and other members of your household carry with you today, in total?	|___|___|___|,|___|___|___|

Note: This figure displays an excerpt of the survey questions pertaining to savings from Brune et al. (2017). In-kind savings consist of advance buying of farm inputs, business inventory, and bags of maize.

Figure S10: Malawi's Third Integrated Household Survey—Module on Food Consumption

MODULE G: FOOD CONSUMPTION OVER PAST ONE WEEK

DATA ENTRY LINE NUMBER	Over the past one week (7 days), did you or others in your household consume any [. . .]? INCLUDE FOOD BOTH EATEN COMMUNALLY IN THE HOUSEHOLD AND THAT EATEN SEPARATELY BY INDIVIDUAL HOUSEHOLD MEMBERS.	G01 YES..1 NO...2>> **NEXT ITEM**	G02 ITEM CODE	G03 How much in total did your household consume in the past week?		G04 How much came from purchases?		G05 How much did you spend?	G06 How much came from own-production?		G07 How much came from gifts and other sources?	
				QUANTITY	UNIT	QUANTITY	UNIT	MK	QUANTITY	UNIT	QUANTITY	UNIT
1	**Cereals, Grains and Cereal Products**											
2	Maize *ufa mgaiwa* (normal flour)		101									
3	Maize *ufa* refined (fine flour)		102									
4	Maize *ufa madeya* (bran flour)		103									
5	Maize grain (not as *ufa*)		104									
6	Green maize		105									
7	Rice		106									
8	Finger millet (*mawere*)		107									
9	Sorghum (*mapira*)		108									
10	Pearl millet (*mchewere*)		109									
11	Wheat flour		110									
12	Bread		111									
13	Buns, scones		112									
14	Biscuits		113									
15	Spaghetti, macaroni, pasta		114									
16	Breakfast cereal		115									
17	Infant feeding cereals		116									
18	Other (specify)		117									

Note: This figure displays an excerpt of the survey questions pertaining to consumption from Malawi's IHS-3. The consumption survey asks 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.

Figure S11: Expenditure Survey—Weekly Household Expenditure

EXPENDITURE: WEEK

[PERIOD] = the 7 day period starting from [Start Day] and ending on including [End Day]

READ: I am now going to ask you questions about your household's expenditures over [PERIOD]. So we're asking you about the period starting from last [START DAY] up until and including yesterday; do not include today's purchases.
Please note that your answers will NOT affect any subsidies you receive, any aid you receive, your interest rates on loans, etc. Your answers will also not affect whether any projects will be implemented in this area. The information is soley used for research purposes and learning about the expenditure patterns of Malawians in Mulanje district.

	EX_WK1. Considering purchases of the last [PERIOD], did your household buy any [ITEM] with cash or credit?		**EX_WK2.** Did you plan to buy [ITEM] or did you decide to buy it only when you saw it?	**EX_WK3.** Did you pay in cash, credit or both?	**If EX_WK3= "1-Cash" or "3-B" ask: EX_WK4.** How much cash did you pay in total? MWK	**IF EX_WK3= "2-Cred" or "3-B" ask: EX_WK5.** How much was on loan / credit? MWK
	IF 2-N ---> Skip Columns EX_WK2 - 5					
	Cereals, Grains and Cereal Products					
1	Maize *ufa mgaiwa* (normal flour)	1-Y 2-N	1-Plann'd 2-Not Plann'd	1-Cash 2-Cred 3-B	|___|___|,|___|___|___|	|___|___|,|___|___|___|
2	Maize *ufa* refined (fine flour)	1-Y 2-N	1-Plann'd 2-Not Plann'd	1-Cash 2-Cred 3-B	|___|___|,|___|___|___|	|___|___|,|___|___|___|
3	Maize *ufa madeya* (bran flour)	1-Y 2-N	1-Plann'd 2-Not Plann'd	1-Cash 2-Cred 3-B	|___|___|,|___|___|___|	|___|___|,|___|___|___|
4	Maize grain (not as *ufa*)	1-Y 2-N	1-Plann'd 2-Not Plann'd	1-Cash 2-Cred 3-B	|___|___|,|___|___|___|	|___|___|,|___|___|___|
5	Green maize	1-Y 2-N	1-Plann'd 2-Not Plann'd	1-Cash 2-Cred 3-B	|___|___|,|___|___|___|	|___|___|,|___|___|___|
6	Rice	1-Y 2-N	1-Plann'd 2-Not Plann'd	1-Cash 2-Cred 3-B	|___|___|,|___|___|___|	|___|___|,|___|___|___|
7	Finger millet (*mawere*)	1-Y 2-N	1-Plann'd 2-Not Plann'd	1-Cash 2-Cred 3-B	|___|___|,|___|___|___|	|___|___|,|___|___|___|
8	Sorghum (*mapira*)	1-Y 2-N	1-Plann'd 2-Not Plann'd	1-Cash 2-Cred 3-B	|___|___|,|___|___|___|	|___|___|,|___|___|___|
9	Pearl millet (*mchewere*)	1-Y 2-N	1-Plann'd 2-Not Plann'd	1-Cash 2-Cred 3-B	|___|___|,|___|___|___|	|___|___|,|___|___|___|
10	Wheat flour	1-Y 2-N	1-Plann'd 2-Not Plann'd	1-Cash 2-Cred 3-B	|___|___|,|___|___|___|	|___|___|,|___|___|___|
11	Bread	1-Y 2-N	1-Plann'd 2-Not Plann'd	1-Cash 2-Cred 3-B	|___|___|,|___|___|___|	|___|___|,|___|___|___|
12	Buns, scones	1-Y 2-N	1-Plann'd 2-Not Plann'd	1-Cash 2-Cred 3-B	|___|___|,|___|___|___|	|___|___|,|___|___|___|
13	Biscuits	1-Y 2-N	1-Plann'd 2-Not Plann'd	1-Cash 2-Cred 3-B	|___|___|,|___|___|___|	|___|___|,|___|___|___|
14	Spaghetti, macaroni, pasta	1-Y 2-N	1-Plann'd 2-Not Plann'd	1-Cash 2-Cred 3-B	|___|___|,|___|___|___|	|___|___|,|___|___|___|
15	Breakfast cereal	1-Y 2-N	1-Plann'd 2-Not Plann'd	1-Cash 2-Cred 3-B	|___|___|,|___|___|___|	|___|___|,|___|___|___|
16	Infant feeding cereals	1-Y 2-N	1-Plann'd 2-Not Plann'd	1-Cash 2-Cred 3-B	|___|___|,|___|___|___|	|___|___|,|___|___|___|
17	Other (specify)	1-Y 2-N	1-Plann'd 2-Not Plann'd	1-Cash 2-Cred 3-B	|___|___|,|___|___|___|	|___|___|,|___|___|___|

Note: This figure displays an excerpt of the survey questions pertaining to expenditures from Brune et al. (2017). The expenditure survey asks households how much households paid in total for various consumption goods over the past seven days.

Table S1: Timing of 2008 US Tax Rebates

Payments by electronic funds transfer		Payments by paper check	
Last two digits of taxpayer SSN	Date by which payment funds are deposited	Last two digits of taxpayer SSN	Date by which payment check is in mail
00–20	May 2	00–09	May 16
21–75	May 9	10–18	May 23
76–99	May 16	19–25	May 30
		26–38	June 6
		39–51	June 13
		52–63	June 20
		64–75	June 27
		76–87	July 4
		88–99	July 11

Note: Reproduced from Parker et al. (2013).

Table S2: ESP Spending Responses by Timing of Payment

	Group 1	Group 2	Group 3	p-value	
All households	53.26	30.89	8.05	0.0045	0.0013
	(10.09)	(8.90)	(8.70)		
Liquidity: yes	48.84	16.72	-6.13	0.0079	0.0021
	(15.54)	(11.55)	(12.68)		
Liquidity: no	72.09	64.04	40.40	0.2652	0.1386
	(17.79)	(13.83)	(15.84)		
Financial plan: yes	54.84	31.16	2.30	0.0377	0.0173
	(17.93)	(10.85)	(12.52)		
Financial plan: no	53.41	29.67	14.28	0.0813	0.0251
	(16.67)	(11.87)	(13.97)		
Savings habit: yes	49.77	32.82	13.84	0.0666	0.0228
	(14.49)	(9.66)	(11.04)		
Savings habit: no	57.17	24.43	-1.42	0.0534	0.0181
	(22.51)	(17.05)	(18.87)		
Low income	38.66	29.11	14.47	0.2380	0.1188
	(13.48)	(9.89)	(10.08)		
High income	65.87	32.34	3.04	0.0086	0.0032
	(18.13)	(13.95)	(14.63)		

Note: This table presents estimates from Equation (2) of the four-week cumulative ESP spending response Γ_4^w for households receiving EFTs in the first (Group 1), second (Group 2), and third (Group 3) week of May, respectively. 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. Low income refers to incomes below \$50,000, while high income refers to those above. Standard errors reported in parentheses are adjusted for clustering at the household level and obtained from a block-bootstrap procedure with 100 replicates. The first p-value corresponds to the null hypothesis of equality across groups, and the second p-value corresponds to the null hypothesis of equality between Group 1 and Group 3.

Table S3: Search Terms for Literature Review

Search term	Number of results
"time delay" "cash transfer"	161
"time delay" "windfall"	756
"waiting time" "cash transfer"	912
"waiting time" "windfall"	948
"delayed" "cash transfer"	8,580
"delayed" "windfall"	29,700
"payment delays" "cash transfer"	238
"payment delays" "windfall"	194
"waiting time" "savings" "cash transfer"	416
"waiting time" "consumption" "cash transfer"	539
"waiting time" "savings" "windfall"	482
"waiting time" "consumption" "windfall"	496
"delayed" "savings" "cash transfer"	3,740
"delayed" "consumption" "cash transfer"	5,140
"delayed" "savings" "windfall"	12,200
"delayed" "consumption" "windfall"	13,100
"payment delays" "saving" "cash transfer"	93
"payment delays" "consumption" "cash transfer"	194
"payment delays" "saving" "windfall"	78
"payment delays" "consumption" "windfall"	113

Note: This table contains a list of search terms for the literature review described in Supp. Appendix A. The "number of results" records the number of Google scholar search results for the associated combination of keywords (with quotation marks) as of April–May 2021.

Table S4: Survey of the Literature

Paper	Source	Notes
Haushofer and Shapiro (2016)	"cash transfer" "time delay"	Used in Section 5.1 of this paper
Haushofer and Shapiro (2013)	"cash transfer" "time delay"	Earlier version of the paper above
Carvalho, Meier and Wang (2016)	"cash transfer" "time delay"	No variation in anticipation duration
Aker et al. (2016)	"waiting time" "cash transfer"	Changes both the time to payment and payment method
Masino and Niño-Zarazúa (2020)	"waiting time" "cash transfer"	Change both the time to payment and payment method
Barrera-Osorio, Linden and Saavedra (2019)	"delayed" "savings" "cash transfer"	Payment conditional on savings decisions or educational decisions
Brune et al. (2017)	"delayed" "savings" "cash transfer"	Used in Section 5.2 of this paper
Bazzi, Sumarto and Suryahadi (2015)	"delayed" "consumption" "cash transfer"	About predictability of payments
Kerwin, Li and Brune (2022)	"payment delays" "saving" "cash transfer"	Differences in liquidity constraints across treatments (fixed lump-sum versus weekly payments)
Brune and Kerwin (2019)	Cites Brune et al. (2017)	Differences in liquidity constraints across treatments (fixed lump-sum versus weekly payments)
Brune, Chyn and Kerwin (2021)	Cites Brune et al. (2017)	Differences in liquidity constraints across treatments (fixed lump-sum versus weekly payments)
Muralidharan, Niehaus and Sukhtankar (2016)	"payment delays" "saving" "cash transfer"	About predictability of payments
Dreber et al. (2016)	"payment delays" "saving" "windfall"	About charitable giving and not consumption
Imas, Kuhn and Mironova (2022)	"waiting time" "consumption" "windfall"	One-time choice of redeeming a coupon rather than consumption-savings decisions out of a windfall
Salehi-Isfahani and Mostafavi-Dehzooei (2018)	Cites Haushofer and Shapiro (2016)	Endogenous timing of program participation
Gelman et al. (2020)	Cites Parker (2017)	No variation in anticipation duration
Ganong et al. (2020)	Cites Parker (2017)	No variation in anticipation duration
Baugh et al. (2021)	Cites Parker (2017)	No variation in anticipation duration
Jones and Michelmore (2019)	Cites Parker (2017)	No variation in anticipation duration

Note: This table contains a list of papers considered in the literature survey described in Supp. Appendix A.

Table S5: Impact of Delayed Windfalls on Savings—by Payment Method (Malawi)

	1-Day Delay	8-Day Delay	p-value
Panel A: Cash			
NBS account	-9.63	-3.92	0.6287
	(10.32)	(10.20)	
Formal savings	-5.16	4.54	0.9105
	(22.95)	(23.62)	
Informal savings	-17.71	11.47	0.2092
	(14.45)	(15.82)	
Total financial assets	-6.13	31.75	0.5135
	(28.93)	(31.76)	
In-kind savings	-27.92	179.90	0.0031
	(30.89)	(59.13)	
Total savings	-39.56	199.31	0.0016
	(42.17)	(74.58)	
Panel B: Direct deposit			
NBS account	-15.68	2.66	0.1619
	(10.67)	(10.15)	
Formal savings	-11.66	19.42	0.0867
	(17.00)	(14.14)	
Informal savings	20.38	23.11	0.1987
	(15.79)	(13.39)	
Total financial assets	-2.98	33.86	0.1937
	(27.82)	(26.61)	
In-kind savings	16.07	95.31	0.0183
	(38.85)	(37.59)	
Total savings	-13.03	121.58	0.0228
	(55.18)	(57.15)	

Note: Each row presents estimates of Equation (7) with the outcome variable as a measure of savings. The sample in Panel A consists of 234 households randomly assigned to receive cash windfall payments, and the sample in Panel B consists of 230 households randomly assigned to receive windfall payments deposited into an account with the bank. See Table 2 for more details on the sample, treatments, and variable definitions.

Table S6: Impact of Delayed Windfalls on Savings—by Marital Status (Malawi)

	1-Day Delay	8-Day Delay	*p*-value
Panel A: Married			
NBS account	-12.29	1.40	0.3010
	(10.23)	(9.97)	
Formal savings	5.82	28.90	0.3443
	(18.29)	(20.33)	
Informal savings	7.06	15.88	0.5087
	(14.41)	(13.69)	
Total financial assets	20.76	44.91	0.3124
	(27.14)	(29.38)	
In-kind savings	-20.23	132.38	0.0039
	(35.67)	(46.59)	
Total savings	-17.85	158.72	0.0131
	(46.80)	(67.14)	
Panel B: Unmarried			
NBS account	-9.83	-6.68	0.6687
	(11.02)	(10.40)	
Formal savings	-10.90	-13.76	0.6428
	(15.20)	(15.20)	
Informal savings	3.19	19.20	0.5355
	(14.97)	(17.64)	
Total financial assets	-5.62	13.28	0.7787
	(20.69)	(26.49)	
In-kind savings	28.78	138.60	0.0201
	(30.25)	(49.10)	
Total savings	20.18	168.52	0.0015
	(39.39)	(47.32)	

Note: Each row presents estimates of Equation (7) with the outcome variable as a measure of savings. The sample in Panel A consists of 297 married households, and the sample in Panel B consists of 177 unmarried households. See Table 2 for more details on the sample, treatments, and variable definitions.

Table S7: Impact of Delayed Windfalls on Savings—by Household Size (Malawi)

	1-Day Delay	8-Day Delay	p-value
Panel A: Large households			
NBS account	-11.68	-0.97	0.3625
	(9.69)	(9.96)	
Formal savings	-6.71	-8.29	0.8892
	(21.08)	(17.15)	
Informal savings	-3.96	17.65	0.3654
	(16.57)	(15.78)	
Total financial assets	-6.71	13.68	0.7688
	(34.47)	(29.23)	
In-kind savings	-20.14	145.51	0.0115
	(30.38)	(50.35)	
Total savings	-38.87	156.45	0.0026
	(40.18)	(60.71)	
Panel B: Small households			
NBS account	-12.45	-0.72	0.4328
	(11.26)	(10.77)	
Formal savings	-3.43	35.76	0.2303
	(14.69)	(22.39)	
Informal savings	16.55	20.91	0.2529
	(13.99)	(13.27)	
Total financial assets	16.58	56.91	0.0995
	(19.67)	(26.60)	
In-kind savings	-0.19	124.42	0.0094
	(40.21)	(46.81)	
Total savings	2.40	175.60	0.0254
	(53.84)	(68.59)	

Note: Each row presents estimates of Equation (7) with the outcome variable as a measure of savings. The sample in Panel A consists of 243 large households (five or more members), and the sample in Panel B consists of 231 small households (less than five members). See Table 2 for more details on the sample, treatments, and variable definitions.

Table S8: Effect of an Additional Month of Anticipation Duration on MPC (Meta-Analysis)

	(1)	(2)	(3)
All studies	-0.0305	-0.0134	-0.0123
	(0.0566)	(0.0256)	(0.0119)
MPC Std. Err. < 0.5	-0.0406	-0.0134	-0.0157
	(0.0497)	(0.0203)	(0.0116)
MPC Std. Err. < 0.4	-0.0472	-0.0158	-0.0162
	(0.0422)	(0.0178)	(0.0104)
MPC Std. Err. < 0.3	-0.0576	-0.0208	-0.0200
	(0.0291)	(0.0105)	(0.0071)
MPC Std. Err. < 0.2	-0.0542	-0.0245	-0.0239
	(0.0226)	(0.0056)	(0.0066)
MPC Std. Err. < 0.1	-0.0395	-0.0240	-0.0258
	(0.0159)	(0.0059)	(0.0054)

Note: Each cell displays an estimate of the relationship between the estimated MPC and anticipation duration among the subsample of studies for which the number of months of anticipation can be determined (i.e., those with short anticipation durations); see Supp. Appendix E for definitions. The first column shows results from a regression with a single independent variable for anticipation duration in months, with no additional control variables. The second column adds controls for the standard error of the estimated MPC and the payment amount. The third column additionally controls for whether the estimates reflect samples for which liquidity constraints bind or not, and whether the estimates reflect total consumption, food, or another specific category of consumption. Each successive row restricts the sample to more precise estimates: the second row reports results based on 428 estimates from 8 studies; the third row reports results based on 414 estimates from 8 studies; the fourth row reports results based on 385 estimates from 8 studies; the fifth row reports results based on 347 estimates from 7 studies; and the final row reports results based on 278 estimates from 7 studies. Standard errors are adjusted for clustering at the study level. See Havranek and Sokolova (2020) for the full details of studies included in the meta-analysis.

Table S9: Effect of Long Anticipation Duration on MPC (Meta-Analysis)

	(1)	(2)	(3)
All studies	0.1817	0.0259	0.0239
	(0.1726)	(0.1638)	(0.1538)
MPC Std. Err. < 0.5	0.0312	-0.0356	-0.0267
	(0.1405)	(0.0779)	(0.0882)
MPC Std. Err. < 0.4	0.0449	-0.0293	-0.0199
	(0.1367)	(0.0718)	(0.0791)
MPC Std. Err. < 0.3	0.0051	-0.0645	-0.0574
	(0.0927)	(0.0578)	(0.0639)
MPC Std. Err. < 0.2	-0.0422	-0.0995	-0.0933
	(0.0613)	(0.0488)	(0.0505)
MPC Std. Err. < 0.1	-0.0400	-0.0786	-0.0707
	(0.0300)	(0.0384)	(0.0440)

Note: Each cell displays an estimate of the difference in MPCs between studies involving long vs. short anticipation durations; see Supp. Appendix E for definitions. The first column shows results from a regression with a single binary independent variable for long-anticipated payments, with no additional control variables. The second column adds controls for the standard error of the estimated MPC and the payment amount. The third column additionally controls for whether the estimates reflect samples for which liquidity constraints bind or not, and whether the estimates reflect total consumption, food, or another specific category of consumption. Each successive row restricts the sample to more precise estimates: the second row reports results based on 537 estimates from 14 studies; the third row reports results based on 519 estimates from 14 studies; the fourth row reports results based on 479 estimates from 14 studies; the fifth row reports results based on 425 estimates from 13 studies; and the final row reports results based on 328 estimates from 13 studies. Standard errors are adjusted for clustering at the study level. See Havranek and Sokolova (2020) for the full details of studies included in the meta-analysis.