What Would You Do with $500? Spending Responses to Gains, Losses, News and Loans*

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Abstract

We use survey questions about spending in hypothetical scenarios to investigate features of propensities to consume that are useful for distinguishing between consumption theories. We find that (i) responses to unanticipated gains are vastly heterogeneous (either zero or substantially positive); (ii) responses increase in the size of the gain, driven by the extensive margin of spending adjustments; (iii) responses to losses are much larger and more widespread than responses to gains; and (iv) even those with large responses to gains do not respond to news about future gains. These four findings suggest that limited access to disposable resources, and frictions in adjusting consumption, are important determinants of consumption behavior. We also find that (v) households do not respond to the offer of a one-year interest-free loan, suggesting that this is not a consequence of short-term credit constraints; and (vi) people do cut spending in response to news about future losses, suggesting that neither is this a consequence of myopia. A calibrated precautionary savings model with utility costs of changing consumption, and a sufficient fraction of low-wealth households, can account for these features of propensities to consume on both the extensive and intensive margins.

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1 Introduction

A large amount of research has been devoted to measuring marginal propensities to consume (MPCs). The majority of this work has focused on searching for observable characteristics that correlate with the heterogeneity in MPCs out of income shocks. However, this search has been largely fruitless: the only observable characteristic that has been robustly shown to correlate with MPCs is holdings of liquid wealth, and even then the explanatory power of wealth for MPC heterogeneity is weak.[1] In addition, most of the empirical work has focused on the consumption response to small, unanticipated one-time gains. Other than the cross-sectional correlation with liquid wealth, the limited variation in income changes has provided little in the way of evidence that is useful for evaluating theoretical models of consumption.

In this paper, we use survey evidence on reported spending in various scenarios to generate new evidence that is useful for testing and refining existing models of consumption. Rather than focusing on correlates with observed heterogeneity as in the existing literature, we use variation in consumption responses to different hypothetical treatments. In addition to MPCs out of unexpected gains of different amounts, we elicit MPCs out of unexpected losses, news about future gains, news about future losses, and an interest-free loan.

A key advantage of our approach is that we generate variation in shocks (in terms of size, timing, and sign) that is otherwise very difficult to generate cleanly in natural settings. Moreover, using randomized treatments and within-person variation generates results that are free from other confounds. For example, although it is possible to examine consumption responses to positive and negative income shocks in observational data, individuals who receive positive shocks are likely to differ along observable and unobservable dimensions from those who receive negative shocks, which limits the inferences that one can draw.

Our survey instrument also allows us to distinguish between the extensive and intensive margins of propensities to consume—a dimension of consumption that has largely been abstracted from in existing work and which we show to be important. Comparing spending responses across these treatments on both the extensive and intensive margins yields several insights about consumption behavior. We demonstrate these by implementing the survey treatments inside simple theoretical models,

[1]Early work in this literature failed to find strong evidence for a correlation between liquid wealth and MPCs (see e.g. Johnson et al., 2006; Parker et al., 2013), but more recent work that uses larger samples and richer data routinely finds a significant correlation (see e.g. Fagereng et al., 2016; Baker, 2018; Aydin, 2018). However, the R-squared measures remain very low.

inside benchmark calibrated precautionary savings models, and inside a modified precautionary savings model that can speak to the extensive margin of MPCs.

Our first three findings describe a pattern of MPC behavior that suggests that many individuals act as if access to disposable resources is limited. First, as in the existing literature, we find a large amount of heterogeneity in consumption responses to small unexpected gains. Most people do not change their spending when given a $500 gain, but there is a set of people who spend a substantial fraction of the $500. Second, we find evidence of sign asymmetry. Spending responses to losses are larger and more widespread than spending responses to equal-sized gains. Third, we find that very few respondents say that they would increase spending in response to news about a future gain, even those respondents who indicate that they would increase spending in response to an actual gain. These three findings are all consistent with a subset of the population acting as if they do not have access to disposable resources.

Our next two findings provide insights into the possible reasons why this group might act in this way. Fourth, we find that respondents do not increase their spending when offered a one-year interest-free loan, suggesting that short-term credit constraints are not a key factor in explaining high MPCs. Fifth, whereas very few respondents react to news about a future gain, the majority of respondents do react to news about a future loss, including those who react strongly to an immediate loss. This finding suggests that even low-wealth individuals are at least somewhat forward-looking and is evidence against extreme forms of myopia.

For each of these findings, we show that the extensive margin plays an important role. But the extensive margin is particularly important for a sixth finding. As we increase the size of the windfall from $500 to $2,500 to $5,000, a larger fraction of respondents say that they would increase their spending. We refer to this as a positive extensive-margin size effect. At the same time, the average MPC conditional on responding decreases, which we refer to as a negative intensive-margin effect. We find that the extensive margin effect is stronger than the intensive margin effect so that, on net, we observe a positive size effect.

We then show that a calibrated precautionary savings model is consistent with these six findings from our hypothetical treatments. The model also does well in terms of matching the heterogeneity on the extensive margin across treatments. Two features of the model are important for its success. First, it is important that the model delivers meaningful extensive-margin MPCs. Our model achieves this by allowing for non-pecuniary costs of deviating from past consumption, which makes past consumption a state variable in the household decision problem.[2] These costs are intended to capture, in a reduced-form way, the effects of cognitive or real costs of altering consumption. We calibrate the size of the consumption adjustment cost to match the fraction of respondents who say they would increase their spending in response to a $500 windfall. We evaluate the model based on its predictions for intensive- and extensive-margin MPCs in the remaining treatments. Second, it is important that the model features a sufficiently large fraction of households with limited access to disposable liquid resources. Our model achieves this by allowing for discount factor heterogeneity in the spirit of Carroll et al. (2017) and Krueger et al. (2016), which we calibrate to match the fraction of households with less than $1,000 in financial wealth.[3]

One obvious explanation for why many households act as if their access to disposable resources is limited, is that they in fact possess very little liquid wealth. Indeed, this is the sole explanation in our calibrated models and almost all existing models of consumption behavior.[4] However, although we find a strong correlation in our data between liquid wealth and MPCs out of losses, we do not find a significant correlation between liquid wealth and MPCs out of gains. One possible reason is that in reality, liquid wealth is an imperfect proxy for disposable resources. For example, different households have different pre-committed expenditures, different expense risk and different access to informal credit, and hence consider themselves hand-to-mouth at different levels of liquid wealth. Another possible reason is that some behavioral phenomenon, such as mental accounting or salience (Kueng, 2018), lead households to act *as if* they are hand-to-mouth. In fact, Parker (2017) finds that spending responses to the 2008 stimulus payments are related with certain behavioral characteristics, such as impatience (but not with measures of self-control or procrastination).

There is a large literature on estimating MPCs, that has followed one of two

[2]Our model shares this feature with the large literature on (internal) habit models that have been used to rationalize asset-pricing facts (e.g. Campbell and Cochrane, 1999), and to generate hump-shaped impulse responses in DSGE models (e.g. Christiano et al. 2005; Smets and Wouters 2007). However, the form of the dependence of current consumption on past consumption in our model of consumption adjustment costs is different from that in the habit models used in these literatures, which would not deliver a meaningful extensive-margin MPC.

[3]An alternative approach would be to allow for liquid and illiquid assets as in Kaplan and Violante (2014). An advantage of the illiquid asset approach is that it is also consistent with the fraction of households with low levels of total wealth, which the model with heterogeneous discount factors overstates. Unfortunately, it is computationally infeasible to include both consumption adjustment costs and illiquid assets in the model since this would require three endogenous state variables.

[4]An important exception is Campbell and Hercowitz (2019) who propose a model in which some households have liquid wealth that has been earmarked for a foreseen large future expenditure.

approaches. One strand of the literature uses what Parker and Souleles (2019) label the "revealed preference" approach, in which consumption is measured using data on actual expenditures. These data come either from household surveys or financial datasets—e.g. Consumer Expenditure Survey (Johnson et al., 2006), Kilts-Nielsen Consumer Panel (Parker, 2017), or banks and other financial service providers (Gelman et al., 2014; Baker, 2018; Ganong and Noel, 2017; Aydin, 2018)—or by backing out expenditures from administrative data on income and wealth (Fagereng et al., 2016). The revealed preference approach uses these data to estimate MPCs either by cleverly exploiting natural experiments that mimic unexpected changes in household budgets—e.g. fiscal stimulus payments (Parker et al., 2013), lottery winnings (Fagereng et al., 2016), minimum wage hikes (Aaronson et al., 2012), or mortgage modifications (Ganong and Noel, 2017)—or expected changes in household budgets, for instance due to social security withholding (Parker, 1999) or payments from the Alaska Permanent Fund (Hsieh, 2003; Kueng, 2018). Within the revealed preference literature, another approach has been to study consumption responses to changes in household budgets by extracting the transitory component of stochastic income fluctuations (Blundell et al., 2008).

A second strand of the literature uses a "reported preference" approach, in which individuals are asked how their spending would respond in hypothetical or actual scenarios. A large part of the reported preference literature elicits qualitative spending responses using survey questions that follow Shapiro and Slemrod (2003). More recently, there has been a growing body of work, including ours, that elicit quantitative spending responses (Jappelli and Pistaferri, 2014, 2019; Graziani et al., 2016; Christelis et al., 2019). Using strategically-designed survey questions in conjunction with structural models has also been fruitfully applied to other questions related to household financial decisions (Ameriks et al., 2018, 2019).

This paper sits firmly in the "reported preference" approach. Our data come from a survey of 2,586 household heads from the NY Fed's Survey of Consumer Expectations, an online rotating panel of US household heads. We ask respondents to report how they would adjust their spending over the next quarter in response to receiving or losing dollar amounts ranging from \$500 to \$5,000, with the gain/loss occurring either now or in the future, or coming from a loan. Each respondent participates in two or more such treatments, allowing us to study the robustness of our findings by exploiting only within-person variation in responses. In addition, some treatments were fielded in multiple survey waves, which allows us to investigate the stability of responses over time, both at the aggregate and the individual level. Overall, we find a high level of persistence and stability, and little evidence of systematic measurement error. All these pieces of evidence should increase our confidence in the quality of the data and our conclusions. Ideally, we would compare actual spending data under these alternative scenarios rather than hypothetical spending data. The trade-off is that by using reported rather than revealed preferences, we have flexibility in designing treatments. We are not aware of any natural experiments that would allow us to compare actual spending data across scenarios in a controlled way.

Within the reported preference approach, our paper makes three main contributions. First, the variation that we generate across our scenarios is more extensive than has been implemented to date. This is important since this allows us to generate a richer set of empirical results against which we can evaluate existing theoretical models of consumption behavior. Whereas some previous studies have considered the size and sign effect, we are not aware of any study that has investigated responses to news (about gains or losses) or loans. The closest paper to ours, fielded contemporaneously, is Christelis et al. (2019) who also use hypothetical scenarios (in a Dutch household survey) to study sign and size asymmetry. Our findings on these points are qualitatively similar to theirs, which is reassuring given differences in the survey population, the design of the questions, and the size of the income shocks.

Second, our survey instrument has advantages over those used in the existing literature. These advantages, which we discuss in Section 2, include wording the question in a way that does not prime respondents towards a non-zero response; a two-stage set-up which allows respondents to first think about whether they would change their spending at all and then by how much; explicitly stating the spending horizon; and allowing respondents to report an MPC outside the [0,1] range. In particular, the fact that we elicit extensive margin responses turns out to be a crucial ingredient in understanding differences in MPCs across treatments. Depending on the treatment, between 42% and 86% of respondents state they would not adjust their spending in the presented scenario.[5]

Third, we implement the hypothetical survey questions inside calibrated consumption models, a step which the existing literature has largely avoided. Comparing the predictions of consumption theory with the elicited consumption responses turns out to provide new insights into the relative importance of different model ingredients for matching consumption responses. Our findings indicate that two of the most important ingredients are a meaningful extensive margin and a mechanism

[5]The importance of the extensive margin is also hinted at by Misra and Surico (2014), who use quantile regressions to study the distribution of actual (revealed preference) consumption responses to fiscal stimulus payments. They are unable to reject a zero consumption response for around half of the population, consistent with our findings.

that generates a sufficient fraction of households with limited access to disposable liquid resources.

The remainder of the paper is structured as follows. Section 2 describes the survey instrument and the various treatments. Section 3 presents the results from the baseline gains treatment, and Section 4 analyzes the additional treatments (news, losses, and loans) and presents robustness checks. Implications for theory are discussed in Section 5, and the last section concludes.

2 Data

2.1 NY Fed Survey of Consumer Expectations

Our data come from four modules added to the Federal Reserve Bank of New York's Survey of Consumer Expectations (SCE). The SCE is a monthly internet-based survey of a rotating panel of approximately 1,300 heads of household from across the US. The goal of the survey is to elicit expectations about a variety of economic variables, such as inflation and labor market conditions. Respondents participate in the panel for up to twelve months, with a roughly equal number rotating in and out of the panel each month. Respondents are invited to participate in at least one survey each month.

The survey is administered by the Demand Institute, a non-profit organization jointly operated by The Conference Board and Nielsen. The sampling frame for the SCE is based on that used for The Conference Board's Consumer Confidence Survey (CCS). Respondents to the CCS, itself based on a representative national sample drawn from mailing addresses, are invited to join the SCE internet panel. Each survey typically takes 15-20 minutes to complete, and respondents receive $15 for completing a survey. The response rate for first-time invitees hovers around 55 percent, and for repeat respondents is around 80 percent.[6]

The four modules were added to the end of the monthly surveys in March 2016, May 2016, January 2017 and March 2017. Repeat and active panelists (i.e., those who were not participating in the SCE for the first time) were invited to participate in the modules. Because of the panel nature of the SCE some respondents answered multiple modules – those that were less than 12 months apart. In total we collected 9,061 responses to hypothetical spending questions from 2,586 panelists.[7]

[6]See Armantier et al. (2017) for technical background information on the SCE, and `www.newyorkfed.org/microeconomics/sce.html` for additional information.

[7]There were a total of 9,086 scenarios submitted to these panelists, with 25 non-responses (corresponding to less than 0.3% of observations).

Table 1: Sample Characteristics

	Overall	Mar-16	May-16	Jan-17	Mar-17	U.S. Pop.
Sample Size	2586	1086	1087	1190	1180	
Demographics						
White/Non-Hispanic	0.75	0.75	0.75	0.75	0.76	0.69
Age	50.43	50.74	50.62	51.09	50.73	51.06
Education BA+	0.56	0.57	0.57	0.56	0.55	0.31
Married	0.64	0.65	0.64	0.63	0.65	0.50
Homeowner	0.73	0.73	0.73	0.73	0.73	0.59
Midwest	0.25	0.25	0.26	0.25	0.25	0.21
Northeast	0.20	0.19	0.18	0.19	0.21	0.18
South	0.33	0.34	0.34	0.33	0.32	0.38
West	0.22	0.22	0.22	0.23	0.22	0.24
Financial Characteristics						
Income <= 50k	0.36	0.36	0.37	0.36	0.36	0.37
Income 50k-100k	0.36	0.36	0.35	0.36	0.35	0.30
Income 100k+	0.28	0.28	0.28	0.28	0.29	0.31
Liquid Fin. Assets >= 20k	0.50	0.48	0.51	0.51	0.51	0.35
Non-housing Debt > 20k	0.35	0.35	0.35	0.33	0.34	0.23
Net Worth > 200k	0.42	0.48	0.48	0.38	0.37	0.34

For demographics, comparison is with the ACS 2015; for Financial Characteristics, comparison is with the SCF 2013.

Demographic and financial characteristics of respondents in the sample align well with corresponding characteristics of the US population. We report several of these characteristics in Table 1, along with their population counterparts from the 2015 American Community Survey (ACS) or the 2013 Survey of Consumer Finances. For example, the average age of respondents in our sample is 50.4 years, and 36% of respondents report annual household income of less than $50,000. The corresponding numbers in the US population are 51.1 years and 37%. 75% of our respondents are white and non-Hispanic, compared to 69% of household heads in the ACS. 73% of respondents are homeowners, compared to a homeownership rate of 59% in the ACS. Households in our sample are also on average more highly educated than the overall US population—56% of our respondents have at least a Bachelor's degree, compared with 31% of household heads in the ACS. We conjecture that this is partly due to differential internet access and computer literacy across education groups.

To account for these differences in observables, all the statistics that we report in the paper are weighted, unless otherwise specified. The weights that we use are provided by the survey vendor, and are designed to make the sample consistent with the ACS along the dimensions of age, income, education, and region.[8] Another reason for using weights is that both the response and retention rates in the SCE are correlated with some demographics. For example, older respondents are more likely to participate and to stay in the panel (Binder, 2019). This is also the case for our specific survey modules. However, importantly, we find no evidence that retention is related to respondents' MPC in the previous survey, or to the scenarios they were assigned to.[9]

2.2 Survey Instrument

Our baseline survey instrument asked respondents to report how they would change their spending behavior in response to an unexpected gain in resources. Respondents are first asked in what direction each of their spending, debt payments, and savings would change in response to the windfall. Next, respondents who say that they would change their (spending; debt payment; savings) are asked for the magnitude of the change. For example, the survey instrument for the $500 gain is as follows.

Respondents are first asked:

> Now consider a hypothetical situation where you unexpectedly receive a one-time payment of **$500** today.
>
> We would like to know whether this extra income would cause you to change your spending behavior in any way over the **next 3 months**.
>
> *Please select only one*
> • Over the next 3 months, I would **spend/donate more** than if I hadn't received the $500
> • Over the next 3 months, I would **spend/donate the same** as if I hadn't received the $500
> • Over the next 3 months, I would **spend/donate less** than if I hadn't received the $500
>
> *Please select only one*
> • Over the next 3 months, I would **pay off more debt (or borrow less)** than if I hadn't received the $500
> • Over the next 3 months, I would **pay off the same amount of debt** as if I hadn't received the $500
> • Over the next 3 months, I would **pay off less debt (or borrow more)** than if I hadn't received the $500
>
> *Please select only one*

[8] On some dimensions (such as gender), the sample is also already well-aligned with national statistics, and no weighting is needed. Other potentially important dimensions, such as race, are not targeted, because of small sample sizes.

[9] See Appendix B.1 for an analysis of retention and response rates.

- Over the next 3 months, I would **save more** than if I hadn't received the $500
- Over the next 3 months, I would **save the same** as if I hadn't received the $500
- Over the next 3 months, I would **save less** than if I hadn't received the $500

Respondents are then asked by how much they would change their behavior for each category for which they do not select the middle option (spend/donate the same; pay off the same amount of debt; save the same). For example, a respondent who indicates that they would spend/donate more is asked the following question:

> You indicated that you would increase your spending/donations over the next 3 months following the receipt of the $500 payment.
> How much more would you spend/donate than if you hadn't received the $500?

The quantitative response to the increase or decrease in spending/donating forms the basis of our estimates of the marginal propensity to consume (MPC).[10] We refer to this baseline treatment for eliciting MPCs as the GAIN treatment:

GAIN: MPC over 1 quarter out of a one-time unexpected receipt of $Y, with Y={500; 2,500; 5,000}

Our survey instrument differs from those used in the existing literature on hypothetical consumption responses in several ways. The majority of this literature has based their survey instrument on the categorical response wording of Shapiro and Slemrod (2003), who focus on tax rebates. They ask respondents to choose between three uses of their tax rebate: (i) mostly spend; (ii) mostly save; or (iii) mostly pay off debt.

More recently, the literature has started to employ survey questions that elicit direct quantitative responses for spending changes. For example, Jappelli and Pistaferri (2014), use the following question in the Survey of Household Income and Wealth (SHIW): "*Imagine you unexpectedly receive a reimbursement equal to the amount your household earns in a month. How much of it would you save and how much would you spend? Please give the percentage you would save and the percentage you would spend.*"[11] Whereas the Shapiro-Slemrod instrument asks a qualitative question and hence requires additional assumptions to be informative about the level of

[10]Note that the survey question distinguishes between paying down debt and saving. While paying down debt is a form of saving (and enters the same way in simple budget constraints), consumers may think of paying down debt as distinct from saving. Therefore, consistent with the approach used in the prior literature, we also make this distinction.

[11]The SHIW is administered to a sample of Italian households. The translation of the survey question from Italian to English is reproduced from Jappelli and Pistaferri (2014).

MPCs, the Japelli-Pistaferri instrument directly elicits a quantitative MPC. Similarly, Graziani et al. (2016) use a quantitative instrument to elicit consumption responses to the 2011 payroll tax cuts: "*Please indicate what share of the extra income [from the payroll tax cut] you are using or plan to use to save or invest, spend or donate, and pay down debts.*"

Christelis et al. (2019) use the following question to measure quantitative responses to hypothetical gains in an online survey of Dutch households: "*Imagine you unexpectedly receive a one-time bonus from the government equal to the amount of net income your household earns in (one-month / three months). In the next 12 months, how would you use this unexpected income transfer?*", with the respondent asked to allocate 100 points to saving; repaying debt; durable spending, and non-durable spending. They employ a similar wording for hypothetical losses, which are framed as one-time taxes. Finally, in a survey of British households, Bunn et al. (2018) ask respondents about the retrospective quantitative change in spending in response to unanticipated shocks to income over the past year. More specifically, they first ask households whether their income differed from what they expected a year ago, and if so, by how much. Next, they ask them how they adjusted their spending over the previous year in response to this unexpected change in income.

An advantage of eliciting a quantitative response is that it gives a direct measure of the individual MPC; this can then be aggregated up to yield the average MPC, which is often the parameter of interest to policymakers. Although this elicitation approach may be more challenging for respondents to answer (as opposed to qualitative questions), it provides a much richer set of evidence to compare with theory.

We believe that our survey instrument is more precise than those in the existing literature. First, we explicitly state the size of the windfall, which we then vary, allowing us to measure potential size effects. Second, we start by asking respondents if they would change their spending at all, before asking the amount by which they would change their spending. This allows a more precise estimate of zero MPCs and does not prime respondents towards a non-zero response. We then ask only those respondents who say that they would actually change their spending behavior about how much they would spend. Third, our survey instrument is more explicit than most in the existing literature about the time horizon over which we are asking about spending responses (one quarter, in our case).[12] This is important because almost all economic models predict that any windfalls will ultimately be entirely

[12] Bunn et al. (2018) and Christelis et al. (2019) specify time horizons of one year.

spent over the respondents' remaining lifetime. So without explicitly stating a time horizon, it is difficult to make any comparisons with theory. Fourth, our elicitation strategy does not impose a household's MPC to be between 0 and 1. We leave open the possibility that an unexpected cash windfall may lead some respondents to increase their consumption by a larger amount than the windfall. This could occur if, for example, the respondent had been saving toward an expense and the windfall leads them to alter the timing of the expense.[13]

An important underlying assumption when using reported preferences is that the responses contain information about what households would actually do in response to a current or future cash windfall. Parker and Souleles (2019) compare reported responses to tax rebates with actual spending responses, and conclude that the two approaches yield similar estimates. Shapiro and Slemrod (2003) and Sahm et al. (2010) find a close correspondence between the *ex-ante* MPC (the MPC based on how respondents say they will change their spending) and the *ex-post* MPC (the MPC based on what respondents say about how their spending changed). Bunn et al. (2018) compare responses to retrospective survey questions that ask how spending adjusted in response to income being higher/lower than had been expected, with responses to a survey featuring hypothetical scenarios similar to ours. They find that sign asymmetry, which we also find, is present in both, although average MPCs are slightly smaller in the hypothetical scenarios. Similarly, for a payroll tax cut, Graziani et al. (2016) find that the *ex-post* MPC tends to be larger than the *ex-ante* MPC. In the context of labor markets, other recent papers have shown that the reported approach yields preference estimates that are similar to those from revealed choice (Mas and Pallais, 2017), and are predictive of real-world choices (Wiswall and Zafar, 2018). There is thus a growing consensus that the reported approach yields meaningful responses when the hypothetical scenarios presented to respondents are realistic and relevant for them, as is the case for the scenarios that we consider.

2.3 Treatments

Differences in the survey instrument aside, our study advances the literature by also exposing respondents to a series of additional treatments beyond MPCs for income windfalls. These treatments are designed to elicit aspects of consumption behavior that are particularly useful for evaluating the predictions of theoretical models of consumption. In addition to the GAIN treatment, we conducted the following four treatments:

[13] Campbell and Hercowitz (2019) consider the effect that saving in anticipation of major expenditures has on MPCs.

LOSS: MPC over 1 quarter out of a one-time unexpected loss of \$500.

NEWS-GAIN: MPC over 1 quarter out of unexpected news about a one-time gain of \$X, with X={500; 5,000}, 1 quarter from now.

NEWS-LOSS: MPC over 1 quarter out of unexpected news about a one-time loss of \$500 Z quarters, with Z={1, 8}, from now.

LOAN: MPC over 1 quarter out of an unexpected interest-free loan of \$5,000 to be repaid 1 year from now.

In each module, we exposed respondents to two possible treatments. The months in which the treatments were fielded are displayed in Table 2. For example, in the May 2016 module, all respondents were exposed to the \$5,000 GAIN treatment and, in addition, were randomly assigned to either the \$2,500 GAIN treatment or the \$5,000 LOAN treatment. The order of the treatments within each survey was randomized. This design allows us to compare how the same respondent's spending behavior differs across alternative scenarios, thus providing a way to control for fixed unobserved individual characteristics. Finally, for some treatments we asked follow-up questions regarding the timing of spending adjustments (within the one-quarter horizon) and the composition of spending adjustments across different categories; these follow-up questions are discussed further below. The full texts of the survey instruments for each treatment are reproduced in Appendix A.

In addition, some treatments were fielded in multiple months. For example, as shown in Table 2, the \$5,000 GAIN treatment was fielded in March 2016, May 2016 and January 2017. This allows us to study whether the response distributions are consistent over time.[14] Moreover, the panel structure of the survey ensures that some people appear in multiple surveys and, in some instances, in the same treatment in different months. This allows us to investigate whether individual respondents report stable spending responses.

Another important advantage of exogenously varying the treatments is that we do not have to worry about observable and unobservable individual characteristics confounding the effects across the different treatments. In observational data, positive and negative shocks are not randomly distributed and are usually systematically

[14] As shown in Table 2, the \$500 NEWS-LOSS scenario was fielded in both January 2017 and March 2017. However, the wording of the question for this scenario was modified in March 2017. The change was prompted because of a concern that the January 2017 instructions may not have been clear that we were asking about spending changes today even though the loss was going to happen in 3 months. Accordingly, we exclude the January-2017 NEWS-LOSS scenario from the analysis. Inclusion of this scenario does not change any of our qualitative conclusions.

Table 2: Treatments and Survey Months

	Mar-16	May-16	Jan-17	Mar-17
Gain				
\$500 Gain	n =1085		n =594	
\$2500 Gain		n =540		
\$5000 Gain	n =361	n =1084	n =595	
\$500 Gain in 3 months	n =362			
\$5000 Gain in 3 months			n =594	
Loss				
\$500 Loss	n =362			n =1174
\$500 Loss in 3 months			n =594	n =586
\$500 Loss in 2 years				n =589
Loan				
\$5000 Loan		n =541		

Note: Table shows number of respondents in each treatment and survey month. In Jan-17, half the sample got the \$500 Gain and \$500 News-Loss blocks, and the other half got the \$5,000 Gain and News-Gain blocks.

related with individual characteristics. For example, Bunn et al. (2018) find that households in their sample who experience positive shocks tend to be younger and hold more liquid assets than those who experience negative shocks. This makes it difficult to interpret any differences in the observed response to positive and negative shocks. Similarly, the size of tax rebates usually tends to be a function of household income or size, which makes it hard to disentangle the size effect from underlying heterogeneity in characteristics and preferences of the different subsamples. Our approach bypasses these issues.

2.4 Summary Findings

Table 3 reports a summary of the MPCs implied by the responses to each treatment. We include this summary here without discussion in order to provide the reader with a concise overview of the findings. We will refer back to this table in the following sections as we discuss each treatment in turn. For each treatment, the table reports the total number of respondents (aggregated across multiple survey rounds for treatments that were conducted in more than one survey), the average MPC, the share of respondents with negative, zero and positive MPCs, and the average and median MPC conditional on being positive. If a respondent answered the same

Table 3: Summary Statistics on MPCs Across Treatments

	Count	MPC Mean (SE)	Share of Respondents with MPC < 0	=0	> 0	MPC | MPC > 0 Mean (SE)	Median
Gain							
$500	1638	0.07 (0.007)	0.08	0.74	0.18	0.53 (0.007)	0.50
$2500	540	0.09 (0.010)	0.09	0.69	0.22	0.43 (0.010)	0.40
$5000	1629	0.12 (0.006)	0.08	0.56	0.36	0.36 (0.006)	0.30
Loss							
$500	1536	0.32 (0.010)	0.04	0.42	0.53	0.61 (0.010)	0.60
News-Gain							
$500 in 3 months	362	-0.02 (0.007)	0.11	0.86	0.04	0.43 (0.007)	0.50
$5000 in 3 months	594	0.04 (0.005)	0.05	0.82	0.13	0.30 (0.005)	0.30
News-Loss							
$500 in 3 months	586	0.29 (0.016)	0.02	0.52	0.46	0.63 (0.016)	0.60
$500 in 2 years	589	0.15 (0.013)	0.04	0.65	0.31	0.51 (0.013)	0.40
Loan							
$5000	541	0.01 (0.005)	0.17	0.75	0.08	0.34 (0.005)	0.40

Note: Weighted statistics reported. Positive MPC corresponds to a negative change in spending for the loss treatments.

treatment multiple times, we first average the MPC for this respondent-treatment combination. When reporting average MPCs, we winsorize at the 2.5th and 97.5th percentiles.

3 Baseline MPC Responses

In this section we consider responses to the GAIN treatment, in which we elicit the MPC out of a one time unexpected windfall of $500, $2500 or $5000. This treatment has been examined in the existing literature, both through surveys and choice data. In Section 4 we then compare the responses to the GAIN treatment with the four additional treatments that have been less well-studied, and report a series of robustness checks.

3.1 Responses to Gains

The average reported quarterly MPC out of a $500 windfall is 7% (see Table 3). This small average MPC masks a large degree of heterogeneity across respondents. Three quarters of respondents say that they would not change their spending behavior at all, and hence have an MPC of zero, and an additional 8% report that they would reduce spending in response to the windfall. Only 18% of respondent say that they would increase their spending, but these households plan to spend a substantial fraction of the $500—the mean and median MPC conditional on a positive response are 53% and 50%, respectively. A more detailed breakdown of the distribution of MPCs is shown by the black bars in Figure 1. For those respondents with a positive MPC the distribution is fairly evenly dispersed, although there is some evidence of bi-modality. Around 4.5% of households report that they would spend all of the $500 over the following quarter, while very few report spending more than 75% but less than 100% of the payment.[15]

Our average MPC is towards the lower end of the estimates found in the literature, for both hypothetical and actual gains of around this size. However, existing studies, like us, have found that a majority of households respond little or not at all in response to an income windfall, but that a small sub-group of households (in our case, around one-fifth) spend a substantial fraction of the income windfall (see, for example, Bunn et al., 2018, and Christelis et al., 2019).

The existing literature has not focused on the distinction between the intensive and extensive margins of MPCs. Most existing *reported* preference studies do not separately examine the intensive and extensive margins, while almost all existing *revealed* preference studies only estimate average responses conditional on characteristics. One notable exception is Misra and Surico (2014), who use quantile regressions to estimate the full distribution of consumption responses to the 2001 and 2008 fiscal stimulus programs. Their estimates imply that they cannot reject a zero response for around half of the population. These findings are consistent with ours regarding the importance of the extensive margin for understanding heterogeneity in MPCs.

3.2 Effect of Windfall Size

As we increase the size of the windfall, a larger fraction of respondents say that they would increase their spending, but on average say they would spend a smaller fraction of the payment. For the $5,000 gain, 36% of respondents report a positive MPC, compared with 22% for the $2,500 gain and 18% for the $500 gain. Conditional on increasing spending, the median MPC is 30%, 40%, and 50% for the $5,000, $2,500 and $500 gains respectively. Overall the effect of the greater number of respondents with positive MPC dominates so that the average MPC increases slightly, from 7% to 9% to 12%, as the payment size increases (the MPCs are statistically different across the three gain treatments; we return to this point in Section 4.5).

[15] In only 0.6% of responses does the MPC exceed 1 in the $500 GAIN treatment. Across all treatments, the share of responses with MPC > 1 is 0.5%.

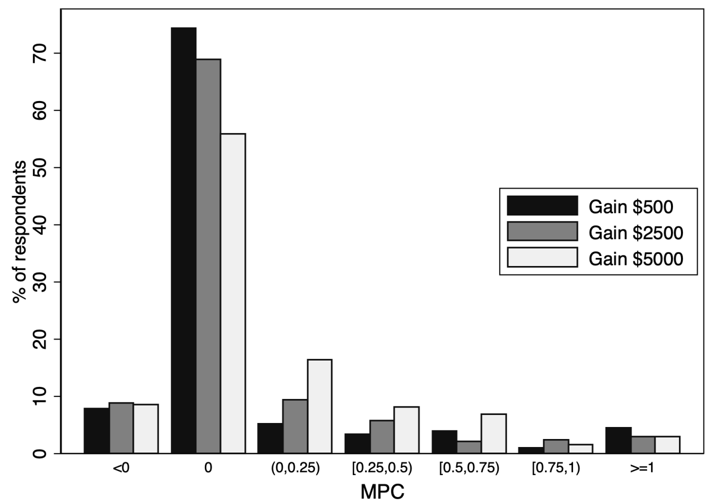


Figure 1: Histogram of MPCs for different Gain scenarios

This size effect in reported MPCs can be seen in Figure 1 by comparing the black histogram ($500 windfall) with the gray histogram ($2,500 windfall) and white histogram ($5,000 windfall). As the size of the windfall increases, the smaller mass of respondents with an MPC of zero is clearly evident, as is the larger mass of people with small, positive MPCs. As we discuss in Section 4.5 below, these patterns also hold when we exploit the fact that some respondents were presented with multiple gain scenarios to study within-respondent variation in MPCs across treatments.

We also asked respondents who indicated that they would adjust their spending about how much of that additional spending would come from different spending categories. We find that for the three GAIN treatments, most of the adjustment comes from non–durable spending. However, as the size of the gain increases, the share that comes from durables increases (Christelis et al., 2019, find a similar result).[16] This suggests that adjustment costs or other non-convexities may be important for understanding the positive size effect, which is otherwise difficult to reconcile with standard models of optimal consumption behavior. We return to this possibility in Section 5, where we explicitly model consumption adjustment costs.

The size effect has not been studied much empirically, largely due to the fact that such variations are usually not observed in natural settings. We are aware of three other studies that investigate size effects, with little agreement. Bunn et al. (2018) find that for positive actual income shocks, MPCs increase in the size of the shock, in line with our results. Christelis et al. (2019) find similar overall MPC

[16]The details of our analysis are provided in Appendix B.2. There, we also discuss data on the timing of spending that we collected for some of the treatments.

distributions for hypothetical positive shocks corresponding to one month or three months of income, though in line with our results, they find a smaller fraction of respondents that say they would not change their spending when the shock is larger. Fagereng et al. (2016), on the other hand, find that MPCs out of lottery winnings in Norway decline in the size of the amount won, which is consistent with our findings on the intensive margin (MPCs conditional on changing spending behavior), but not on the extensive margin (fraction of respondents who indicate they would change behavior).

3.3 Individual Characteristics

We also examined how the average MPC and the share of respondents with MPC>0 in each treatment vary across subgroups of respondents, defined by demographic characteristics (such as age or education), financial characteristics (such as income or liquid wealth), or preference parameters (discount rates). For the gain treatments, we found little systematic heterogeneity in spending responses.[17] In particular, we did not find strong evidence of a relationship between MPCs out of gains and either income or liquid wealth. The lack of strong evidence for an income or wealth gradient is in line with Bunn et al. (2018), Christelis et al. (2019) and Parker and Souleles (2019) who similarly find that reported MPCs out of gains are not significantly related with financial resources. In contrast, in their Italian survey data, Jappelli and Pistaferri (2014, 2019) find a strong relationship.[18] Furthermore, recent work that uses larger samples and richer data routinely finds a significant correlation (see e.g. Fagereng et al., 2016; Baker, 2018; Aydin, 2018).

We did find some evidence that respondents with (i) lower discount factors, as measured from an incentivized choice experiment; or (ii) inconsistent time preferences, as measured from choices that involve trade-offs today versus trade-offs in the future, report larger spending responses out of the $5,000 windfall. But we did not find significant differences for smaller-size gains.[19] We also found that respondents who indicated in a qualitative question that they tend to spend rather than save do

[17] We provide detailed results in Tables A-6 and A-7 in Appendix B (these two tables are not weighted, since our weights are not necessarily valid within subsamples).

[18] See Jappelli and Pistaferri (2019) for a discussion of what might explain these differences (e.g. differences in question wording and shock sizes, or less developed household credit markets in Italy leading to stronger concavity in the consumption function).

[19] Time-inconsistent preferences could lead respondents to understate consumption responses to gains and overstate consumption responses to losses, relative to what they actually would end up doing, if they are "naive" (i.e., unaware of their inconsistency) rather than "sophisticated". The fact that we see some larger stated consumption responses for those respondents with inconsistent time preferences suggests at least a degree of sophistication.

indeed have higher MPCs out of gains.[20]

4 Additional Treatments: News, Losses, Loans

The distribution of spending responses to a small unanticipated income windfall has been extensively studied in the existing literature. The role of the findings from the GAIN treatment is to act as a point of comparison for the more novel treatments that we discuss in this section. Unlike the GAIN treatment, there are few, if any, examples of behavioral studies that explore the NEWS, LOSS, NEWS-LOSS and LOAN treatments that we discuss in this section, necessitating a survey approach for these alternative treatments. In Section 5, we show that the responses to these additional treatments, and their comparison with the GAIN treatment, provide a richer set of findings for alternative theoretical models to confront.

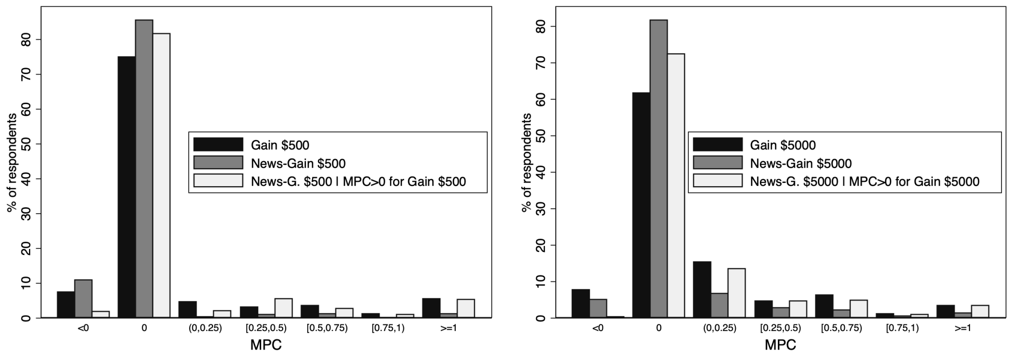
4.1 News About Gains

In the NEWS treatment, we ask respondents how they would change their spending behavior over the next three months if they were to learn about a one-time windfall of either \$500 or \$5,000 that will be received in three months' time. These questions were asked only of respondents who also were exposed to the GAIN treatment of the same amount. A summary of our findings is that respondents do not react to news about a future windfall—even those respondents who say that they would react to the windfall if it were received immediately.

For the \$500 news treatment, the average MPC is -2% and for the \$5,000 treatment, the average MPC is only 4% (Table 3).[21] Moreover, 86% (82%) of respondents in the \$500 (\$5,000) treatment explicitly state that they would not change their spending over the quarter leading up to the payment in any way at all. Only 4% (13%) of respondents say they would increase spending in response to the news, compared with 18% (36%) for the immediate payment. The differences in these MPC distributions between the GAIN treatment (black histograms) and NEWS treatment (gray histograms) is displayed in Figures 2a and 2b. In both figures the additional mass of respondents with a MPC of zero, and the much smaller fraction with a positive MPC, is clearly evident in the gray histograms compared with the black

[20] This question had been asked in an earlier SCE wave (in February 2016); the fact that responses correlate with our measured quantitative MPCs provides comfort in our measurement. The question is very similar to one in Parker (2017), who also finds that those who indicate that they are the "type of people who spend and enjoy today" have higher MPCs out of lump-sum payments.

[21] The negative average MPC for the \$500 gain arises because 11% of respondents report small negative MPCs, compared to only 4% of respondents reporting small positive MPCs. See Table 3.



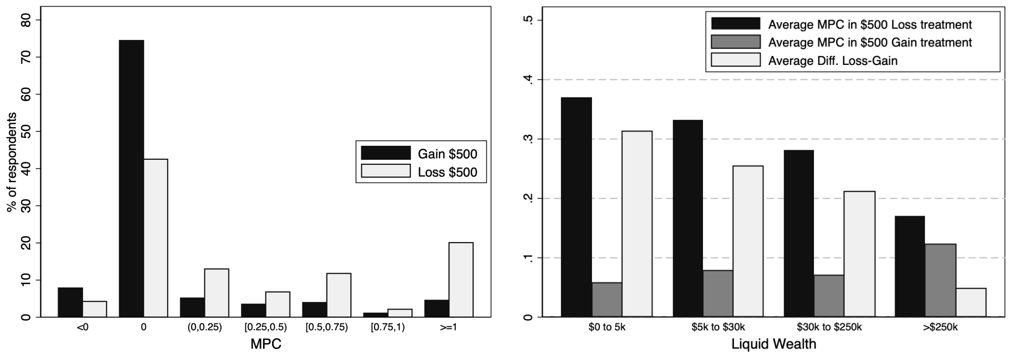
(a) Gain vs news of gain: $500 (b) Gain vs news of gain: $5,000

Figure 2: Spending response to news about future gains vs. response to gains today

histograms.

We find even stronger evidence for the absence of a spending response to the news of a future windfall when we examine MPCs in the NEWS treatment for the subset of households who say that they would indeed increase their spending in the GAIN treatment (the white bars in figures 2a and 2b). Focusing on the $5,000 windfall where this subset is larger (195 out of 595 respondents), Figure 2b shows that more than 70% of the respondents who would react to an instantaneous windfall, would not react to a windfall in three months' time.

These findings are consistent with existing studies that examine the consumption response to tax rebates using expenditure data, such as Johnson, Parker and Souleles (2006). The identification strategy in Johnson et al. exploits randomness in the timing of *when* households received their tax rebates, among a set of households who receive the rebate at some point during the observation period. As explained in Kaplan and Violante (2014), under reasonable assumptions about when households learned about their tax rebates, the estimated coefficients in the regression of consumption growth on the rebate received should be interpreted as measuring the difference between the MPC out of a surprise tax rebate and an MPC out of an anticipated rebate, similarly to the difference between our GAIN and NEWS-GAIN treatments. The average coefficients of 20%-30% reported by Johnson et al. are thus indicative of a large difference between these two treatments. The analysis of consumption responses to different mortgage modification programs by Ganong and Noel (2017) is also consistent with the lack of a news effect on spending, although they study much larger amounts over a much longer time period than in our treatment.



(a) Losses vs. gains

(b) Losses vs. gains, by liquid wealth

Figure 3: Spending response to losses

4.2 Losses

We investigate the importance of sign asymmetry through a LOSS treatment, in which respondents were asked how they would change their spending in the event of an immediate unexpected loss of $500. We find that respondents are significantly more likely to react to a $500 loss than to a $500 gain, with an average MPC of 32% compared with with an average MPC of 7% for a $500 gain.

This sign asymmetry in the MPC is present along both the intensive and extensive margins. Whereas only 18% of respondents said they would increase spending under the GAIN treatment, 53% of respondents say they would decrease spending under the LOSS treatment. Conditional on being positive, the median MPC is 60% for the loss compared with 50% for the gain. Figure 3a shows how the distribution of MPCs under the $500 LOSS treatment compares with the distribution under the GAIN treatment. The MPC distribution for the LOSS treatment is strongly suggestive of bi-modality, with nearly 20% of respondents saying that they would fully absorb the loss of $500 through a reduction in current spending.[22]

Because of the high average MPC and the bi-modality in responses, the LOSS treatment is a useful setting to compare the observable characteristics of individuals with high MPCs and low MPCs. As noted in Section 3.3, we examined how average MPCs for each treatment differ by various individual characteristics that one might *a priori* expect to be correlated with MPCs. We found that variables that proxy for being financially constrained, such as income, liquid assets and credit scores, showed

[22]This asymmetric response to gains and losses is consistent with evidence from the the expiration of the 2013 payroll tax cuts (see Zafar et al., 2013, Bracha and Cooper, 2014, and Sahm et al., 2015). Gelman et al. (2020) also find sizable decreases in spending of federal government workers due to a temporary drop in liquidity because of the 2013 government shutdown.

a strong correlation with the MPC in the LOSS treatment, with respondents who were more likely to be constrained having higher MPCs out of losses.[23]

Figure 3b shows how the average MPCs out of the \$500 GAIN and the \$500 LOSS treatments differ across liquid wealth categories for respondents that answered both treatments.[24] The average MPC for the LOSS treatments declines in wealth, whereas the average MPC for the GAIN treatment is not strongly related to wealth. Hence the average LOSS-GAIN MPC difference decreases in wealth (white bars). The differences are particularly stark for the lowest and highest liquid wealth groups: among respondents with less than \$5,000 (more than \$250,000) in liquid wealth, the average MPC in the LOSS treatment is 0.37 (0.17) and in the GAIN treatment is 0.06 (0.12).[25]

The sign asymmetry in average MPCs masks important heterogeneity in the extent and direction of sign asymmetry at the individual level. In Figure 4 we report the distribution of the *difference* in MPCs between the LOSS treatment and the GAIN treatment, separately for two groups of individuals – those who report a zero (or negative) MPC in the GAIN treatment (black histogram), and those who report a positive MPC in the GAIN treatment (white histogram). Of those respondents who do not react to the \$500 windfall, Figure 4 shows that more than 40% also do not react to the \$500 loss. The remainder primarily say that they would cut spending if faced with a \$500 loss, resulting in a larger average MPC for losses than gains. On the other hand, for those respondents who do react to the \$500 windfall, Figure 4 shows that just over half of them react *less* to the loss than the gain; in fact, 36% would not cut their spending at all in response to the loss (not shown in the figure). However, since the latter group is much smaller than the former group (21% versus 79% of the sample that responds to these two treatments), the average MPC in the LOSS treatment is significantly larger than the MPC in the GAIN treatment.

4.3 News About Losses

The NEWS-LOSS treatment asks respondents how they would alter their spending behavior over the following three months if they were to immediately learn that they will suffer a \$500 loss at a specified future date. Respondents are randomly

[23] See Tables A-6 and A-7 in the Appendix for detailed results.

[24] This figure is not weighted, since our weights are not necessarily valid within subsamples.

[25] Table A-8 in the appendix shows the statistical significance of these differences in a regression with fixed effects for respondents and order-by-date combinations. For the highest-wealth group, MPCs out of gains and losses are not significantly different, while for the other groups the differences are strongly significant. Bunn et al. (2018) and Christelis et al. (2019) also find higher MPCs out of losses for households with lower liquid wealth.

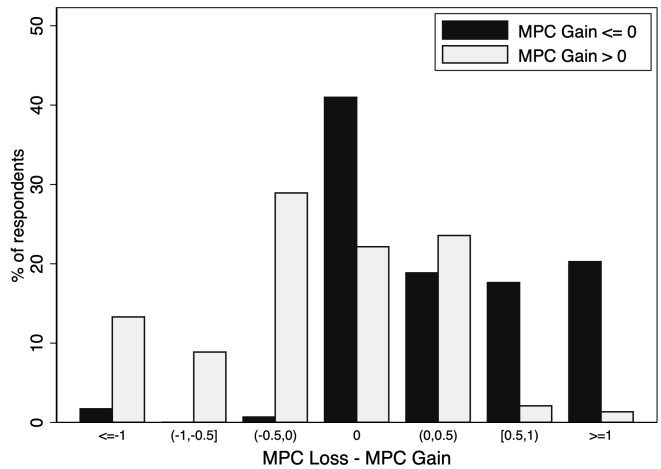


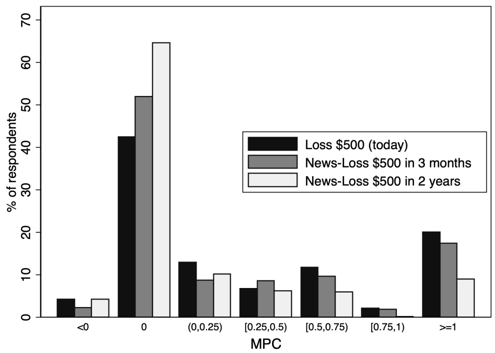
Figure 4: Distribution of difference between MPCs out of losses and gains

assigned to two groups, one for which the loss is to occur in three months' time, the other for which the loss is to occur in two years' time. All respondents exposed to the NEWS-LOSS treatment in March 2017 are also exposed to the LOSS treatment.

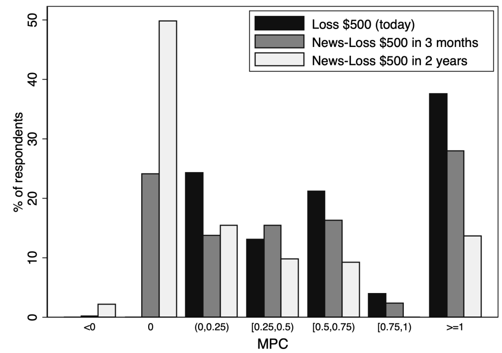
Table 3 shows that along both the extensive and intensive margins, the responses in the 3-month NEWS-LOSS treatment are very similar to the responses in the LOSS treatment. In the 3-month NEWS-LOSS treatment, 46% of the respondents say that they would reduce their spending, compared with 53% of the respondents the LOSS treatment. Conditional on cutting spending, the average MPC is 0.63 for the 3-month NEWS-LOSS treatment, compared with 0.61 for the LOSS treatment. The black and gray bars in Figure 5a further illustrate the similarity of the distribution of responses for these two treatments.

Moreover, Figure 5b shows the spending response to the NEWS-LOSS treatment for the subset of respondents who say that they would cut spending in the LOSS treatment. More than three-quarters of these respondents say that they would also cut spending in the 3-month NEWS-LOSS treatment.

The similarity of the MPC distributions for the $500 LOSS and NEWS-LOSS treatment lies in stark contrast to the comparison of the GAIN and NEWS-GAIN treatments in Section 4.1, where we found much smaller responses to news about a future windfall than to an immediate windfall. It is also evidence against the idea that high MPCs are driven by myopia, or even that high MPCs are due to low liquid wealth which in turn is driven by myopia. Instead, these findings suggest an element of rational, forward-looking behavior among individuals with high propensities to consume. Despite having a high MPC out of an immediate loss, they are willing



(a) News about a $500 loss at different horizons



(b) News-loss effect for those respondents who have MPC>0 out of loss today

Figure 5: News-Loss

to cut contemporaneous consumption in order to smooth out the effects of future anticipated losses.

Figure 5a also shows the distribution of MPCs out of an anticipated loss 2 years in the future (the white histogram). The MPC for a loss that far out is smaller than the MPC for a loss in three months' time, but even in this treatment almost one-third of people respond. Moreover, Figure 5b shows that around half of the households who say that they would cut consumption when faced with an immediate loss, also cut consumption in response to a loss in 2 years' time, albeit by a smaller amount. That so many high-MPC households react to an anticipated loss 2 years in advance also implies that people are forward looking and that myopia alone cannot explain patterns of spending responses. In fact, 61% (28%) of respondents who cut spending in the LOSS treatment report that they would cut spending by the same amount or more in the 3 months (2 year) NEWS-LOSS treatment.

4.4 Loans

Binding borrowing constraints are often cited as a possible explanation for why some individuals have high MPCs out of small transitory gains and losses. To examine whether a loosening of borrowing constraints might have a large impact on MPCs, we included a LOAN treatment in the survey. In this treatment, respondents were asked how they would change their spending if offered an interest-free $5,000 loan to be repaid in one year's time. The distribution of MPCs for this treatment is displayed in Figure 6. The black histogram shows that respondents react very little to the offer of a loan. 75% of respondents say that they would not change their spending at all, 17% say that they would *reduce* spending (suggesting that they did

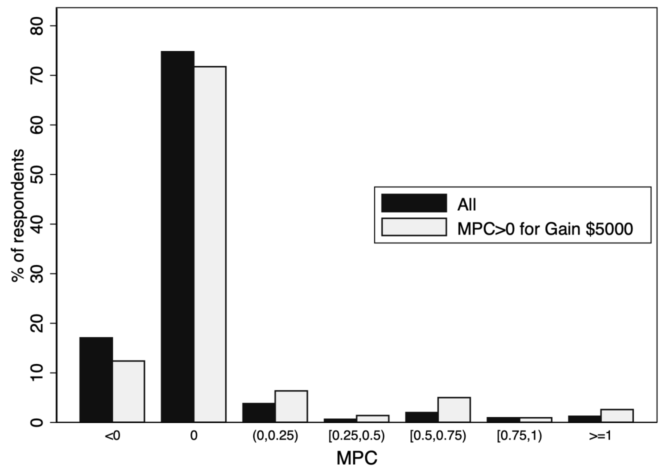


Figure 6: Response to Loan: all respondents and the subset who have MPC>0 for $5000 Gain

not fully understand the concept of an interest free loan, or are worried about their ability to refrain from spending part of the loan in the time until repayment is due) and only 8% of respondents say that they would increase spending.

The white bars in Figure 6 show the MPC distribution for the LOAN treatment for the 34% of respondents who had a positive MPC in the $5,000 GAIN treatment (which was the other treatment these respondents were asked about). Among these respondents, the average MPC for the $5,000 windfall was 0.43, yet when offered an interest-free loan for the same amount, more than 70% of this group said that they would not change their spending at all, and the average MPC was only 0.04.[26]

That individuals who are known to have a large MPC out of an unanticipated windfall also have a zero MPC out of an interest free loan for the same amount is strong evidence that short-term borrowing constraints (shorter than the duration of the loan) are not a key reason for their high MPC. This does not rule out that longer-term borrowing constraints are important, but it is suggestive that high MPCs are associated with persistent low levels of disposable resources (i.e. longer than one year) rather than temporarily low levels.[27]

[26]These numbers are slightly different from those in Table 3, because here we only use respondents who were in both the $5,000 GAIN and LOAN treatments in the May 2016 survey.

[27]In a trial at a large European retail bank where credit lines were randomly expanded, Aydin (2018) finds a MPC out of credit of about 0.20 after three months. One way to reconcile this with our findings is that the credit line expansions in his data had indefinite duration.

4.5 Statistical Significance and Within-Individual Variation

In this section, we use regressions to test for the statistical significance of the differences across treatments that we have emphasized. We also show that the size and significance of these differences are robust to using only within-respondent variation.

Columns (1) and (3) of Table 4 show the difference in average MPCs (and the likelihood of reporting a positive MPC) across the different gain treatments (panel A); the $500 GAIN vs. $500 LOSS treatments (panel B); the $5,000 GAIN vs. $5,000 LOAN treatments (panel C); the GAIN vs. NEWS-GAIN treatments[28] (panel D); and the LOSS vs. NEWS-LOSS treatments (panel E). The regressions in these columns use all the available responses for the respective treatments and do not control for respondent, date or order fixed effects.

Panel A shows that the positive size effects for the average MPC and the extensive margin are statistically significant at conventional levels. Panel B shows that the sign asymmetry is also statistically significant. Panel C show that the response to a gain is significantly stronger than the response to an equal-sized loan and panel D shows that the response to a gain is significantly stronger than the response to news about an equal-sized gain in the future. Panel E shows that the response to a loss occurring in 2 years is weaker than the response to a current loss, while the response to a loss occurring in 3 months' time is indistinguishable from the response to a current loss in terms of average MPCs, though at the extensive margin fewer people respond.

Columns (2) and (4) report analogous results controlling for respondent fixed effects. These specifications use only respondents that answered at least two of the treatments in a panel and include date-by-order fixed effects. Thus, these specifications use only within-individual variation, and therefore absorb, for instance, idiosyncratic differences in respondents' survey-taking behavior (e.g., heterogeneity in rounding). Adding these fixed effects makes little difference for the estimates. but in some cases reduces precision. The conclusions discussed above still hold. Given that individuals were randomly assigned to treatments and that order effects are minimal (as discussed below), this is not entirely surprising.

4.6 Robustness Checks

Before we summarize the broad qualitative takeaways from our survey treatments, we first discuss a series of robustness checks. One concern is whether these stated responses accurately reflect how individuals would actually respond in real situations. We cannot address this directly, but we report a series of checks which should increase

[28] The $500 and $5,000 gain sizes are pooled, but we control for size fixed effects (not shown).

Table 4: Comparison Across Treatments: Within-Respondent Regressions

	(1) MPC	(2) MPC	(3) I(MPC > 0)	(4) I(MPC > 0)
Respondent FEs?	No	Yes	No	Yes
Date X Order FEs?	No	Yes	No	Yes
Panel A. Size effect for gains (omitted category: Gain = $500)				
Gain = $2500	0.024*	0.025	0.047**	0.052
	(0.015)	(0.019)	(0.023)	(0.034)
Gain = $5000	0.061***	0.056***	0.149***	0.155***
	(0.010)	(0.017)	(0.016)	(0.029)
p-value $\beta_{2500} = \beta_{5000}$	0.003	0.005	0.000	0.000
Obs.	4259	3094	4259	3094
Panel B. Gain-loss asymmetry (omitted category: Gain = $500)				
Loss	0.255***	0.257***	0.358***	0.358***
	(0.015)	(0.030)	(0.020)	(0.045)
Obs.	3215	1653	3215	1653
Panel C. Gain vs. loan (omitted category: Gain = $5000)				
Loan	-0.115***	-0.129***	-0.244***	-0.259***
	(0.011)	(0.016)	(0.022)	(0.027)
Obs.	1625	1082	1625	1082
Panel D. Gain now vs. news ($500 and $5000; omitted category: gain now)				
News	-0.084***	-0.066***	-0.158***	-0.139***
	(0.009)	(0.010)	(0.016)	(0.018)
Size FE	Yes	Yes	Yes	Yes
Obs.	2636	1937	2636	1937
Panel E. Loss timing (omitted category: loss now)				
Loss in 3 months	-0.023	-0.008	-0.064**	-0.034
	(0.020)	(0.019)	(0.025)	(0.024)
Loss in 2 years	-0.159***	-0.175***	-0.211***	-0.240***
	(0.018)	(0.021)	(0.028)	(0.030)
p-value $\beta_{3m} = \beta_{2y}$	0.000	0.000	0.000	0.000
Obs.	2349	2344	2349	2344

Table reports regressions of a respondent's MPC (columns 1-2) or indicator for having a positive MPC (columns 3-4) on different treatment indicators. In columns (2) and (4), sample sizes are smaller because only respondents that participated in more than one of the relevant scenarios are included. Observations weighted by population weights. Robust standard errors clustered by respondent in parentheses. Significance: * < 0.1, ** < 0.05, *** < 0.01.

readers' confidence in the quality of the data. In addition, as discussed earlier, there is growing consensus that stated choices are meaningful when the hypothetical scenarios are relevant and realistic, which is the case in our setup.

4.6.1 Response Stability

For the treatments that were fielded in multiple survey waves, we compared the aggregate distribution of responses across waves to examine the stability of responses over time. For the \$500 GAIN, \$5,000 GAIN, and the \$500 LOSS treatments, we found the distributions to be very similar across the different survey waves.[29]

For the respondents who answered the same GAIN treatment in two survey waves, we can also examine the *within-individual* stability of responses. There are several reasons why responses to the same question for a given respondent may change over time. These include changes in household financial situation or composition, and particular spending needs or preferences. Nonetheless, stability over time within a respondent in the same scenario may help give confidence in our methodology. We examine this in Figure 7. The figure shows a bin-scatter that combines all the repeated blocks. The first time a respondent answers a scenario is on the x-axis, and the average MPC from the second survey is reported on the y-axis (if a respondent answers more than two repeat blocks, they appear more than once). The size of each circle reflects the total weight of the respondents in that bin: the total number of repeat responses is 452. We see a strong positive relationship between the two MPCs, even though the slope of the relationship is well below 1 (which would have been the case if responses were perfectly stable over time; shown by the 45-degree line in the figure). For MPC values above 0.2 in the first survey, the subsequent MPCs are on average lower. This could be a result of mean reversion, measurement error, or shocks/changes at the individual level.[30]

4.6.2 Order Effects

A potential concern with our survey design is that it may bias respondents toward stating that they would not adjust their spending so they can avoid the follow-up question of how much they would adjust their spending. If this were the case, we

[29] Figure A-2 in the Appendix provides histograms for all three comparisons. For the two \$500 gain MPC distributions, a Kolmogorov-Smirnov (KS) test of equality of distributions gives a p-value of 0.95. Pairwise KS tests for the \$5000 gain MPC distributions yield p-values of 0.71, 0.43, and 0.15 (where the smallest p-value is for the comparison between March 2016 and January 2017). For the two \$500 loss MPC distributions, the p-value of a KS test of equality of distributions is 0.90.

[30] In Appendix B.3, we show that within-respondent MPC changes are significantly correlated with income changes. We do not find evidence that absolute MPC changes are correlated with respondent cognitive abilities, as one might have expected if the changes were just measurement error.

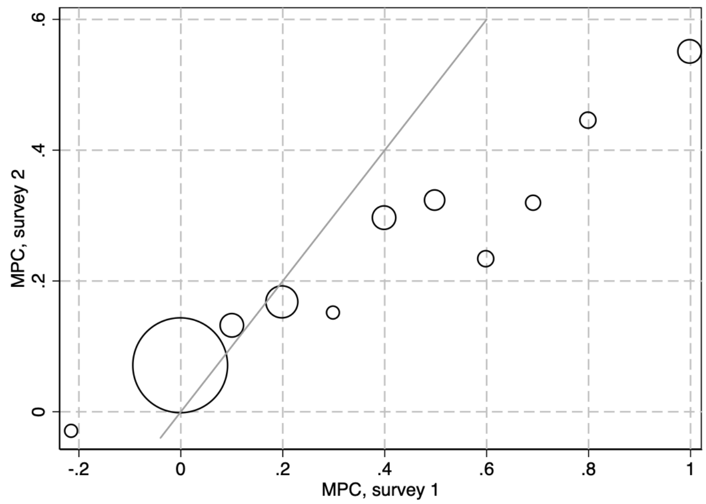


Figure 7: Within-respondent response stability

would expect that for a given treatment, we should see more non-zero spending responses if the treatment is shown to a respondent first rather than second. We tested for order effects by testing for the equality of distributions of MPCs depending on whether respondents saw a treatment first or second, for each treatment shown in Table 2. Out of the 14 treatments, we found only 1 for which the null of equal distributions is rejected at $p < 0.05$, which is what one would expect based on random chance.[31] We further conducted a regression analysis that controls for treatment-date fixed effects, and found at most small and weakly significant effects of the order in which a treatment was seen.[32] Thus, order effects seem to be minimal in this context.

4.6.3 Measurement Error

Our data, like any survey data, is likely to suffer from measurement error. We view our findings regarding the size effect, the gain-loss asymmetry, and muted response to news-gain (relative to gains), all as primarily qualitative in nature. As long as measurement error affects all treatments in similar ways, our qualitative conclusions should remain unchanged. We note that if we restrict our sample to responses where MPCs lie within the [0,1] range (since responses outside this range are more likely to be errors) and to respondents who have high numeracy (a proxy for both

[31]The treatment for which the distributions of MPCs between those respondents who see this treatment first and those who see it second are significantly different is the $5,000 GAIN treatment in March 2016. The other treatment seen by these respondents was the $500 GAIN treatment. The fact that in May 2016 we do not see similar order effects for the $5,000 GAIN treatment when fielded together with the $2,500 GAIN treatment arguably makes it more likely that the difference in March happened by chance.

[32]See Appendix B.4 for the details of this analysis.

cognitive ability and the seriousness with which someone takes the survey[33]), the patterns shown in Table 3 are qualitatively unaffected.[34]

Finally, the fact that our within-individual estimates are very similar to the pooled sample estimates (Table 4) further suggests that measurement error is not a serious issue in our context.[35]

4.7 Takeaways

Before turning to theory, it is useful to summarize what we see as the broad qualitative takeaways from our survey treatments, with which we will confront existing theories. We will focus on the following six general findings. For all six of these findings, the extensive margin of responses plays a larger role than the intensive margin.

1. Heterogeneity: Most people do not respond to gains, but there is a set of people who respond substantially.

2. Size effect: For bigger amounts, more people respond, i.e., a bigger average response driven by the extensive margin.

3. Sign asymmetry: More people respond, and by bigger amounts, to losses than to gains. Response to losses are correlated with liquid assets.

4. Small response to news: Few people respond to news about future gains, even those with large responses to actual gains.

5. Response to news about losses: People do respond to news about future losses. Of those that respond to a loss, about half of them respond even when the loss is 2 years in the future.

[33] The survey included a battery of five questions taken from Lipkus et al. (2001) and Lusardi (2008) to measure respondents' numeracy. We refer to respondents as having high numeracy if they answer at least three of the five questions correctly (89% of the sample).

[34] This version of the table is provided in the Appendix (Table A-9).

[35] Recall that our survey elicited changes in spending, saving, and paying off debt. Our focus in this paper is only on the spending changes. The survey did not force respondents to give answers that satisfied the current period budget constraint (for example, for the $500 gain treatment, the responses for spending, saving, and paying off debt should sum to $500 for a given respondent). In almost all of our treatments, we find that the responses to the three categories do not sum to the current period budget constraint for the majority of the respondents. This may introduce additional measurement error in our estimates. We discuss this concern in Appendix Section B.5, which also presents analysis of additional data that we collected for this purpose. The additional data indicates that in many cases, the reason for the total response not summing up to the available budget increase is that respondents do not recognize that "doing nothing" corresponds to saving the money.

6. Small response to loans: Few people respond to loans, even among those who do respond to gains.

5 Implications for Theory

What does economic theory predict for these treatments? In this section we view the MPCs for each treatment through the lens of alternative models. We start by formally describing each treatment in a way that is amenable to theoretical and quantitative analysis. We then use the broad findings from Section 4.7 that summarize the survey results to evaluate a series of models. We start with the two simplest models of consumption behavior — the polar extremes of rule-of-thumb and permanent income behavior — before considering richer models that incorporate precautionary savings and borrowing constraints. Since none of these existing models can speak to the extensive margin of consumption responses, we then introduce a meaningful extensive margin into an otherwise-standard precautionary savings model.

5.1 Definition of the Treatments

To organize ideas, it is useful to write an individual's budget constraint as

$$\begin{aligned} c_{it} + s_{it} &= x_{it} \\ x_{i,t+1} &= y_{i,t+1} + R\left(s_{it}\right) s_{it}, \end{aligned}$$

where x_{it} is cash on hand at the beginning of period t, c_{it} is the amount spent during period t and s_{it} is the amount saved in period t. We assume that interest is paid at the end of the period and that income $y_{i,t+1}$ is received at the beginning of the following period. Period $t+1$ cash-on-hand is thus given by period $t+1$ income plus savings from period t with accumulated interest. We allow the gross interest rate R to depend on the amount saved s_{it} to reflect the possibility that individuals face different interest rates on savings and borrowing. Consistent with the time horizon in our survey questions, we think of each time period as representing one quarter.

With this budget constraint, we can formally describe the five treatments. In the GAIN and LOSS treatments, the budget constraint unexpectedly becomes

$$\begin{aligned} c_{it} + s_{it} &= x_{it} + \Delta \\ x_{i,t+1} &= y_{i,t+1} + R\left(s_{it}\right) s_{it}, \end{aligned}$$

with $\Delta > 0$ in GAIN and $\Delta < 0$ in LOSS. In the NEWS-GAIN and NEWS-LOSS treatments, the budget constraint unexpectedly becomes

$$\begin{aligned} c_{it} + s_{it} &= x_{it} \\ x_{i,t+1} &= y_{i,t+1} + R\left(s_{it}\right) s_{it} + \Delta, \end{aligned}$$

with $\Delta > 0$ in NEWS-GAIN and $\Delta < 0$ in NEWS-LOSS. In the LOAN treatment, the budget constraint at time t unexpectedly becomes

$$c_{it} + s_{it} = x_{it} + \Delta$$

and the budget constraint at time $t + 4$ unexpectedly becomes

$$x_{i,t+4} = y_{i,t+4} + R\left(s_{i,t+3}\right) s_{it+3} - \Delta.$$

To ease notation, we label the treatments as follows: GAIN (G) , LOSS (L), NEWS-GAIN (NG), NEWS-LOSS (NL), and LOAN (LN). For each treatment $T \in \{G, L, NG, NL, LN\}$, we then define the MPC for an amount Δ as

$$MPC_{it}^{T} = \frac{c_{it}^{\Delta} - c_{it}}{\Delta},$$

where c_{it}^{Δ} is consumption under the treatment and c_{it} is consumption in the absence of the treatment.

5.2 Simple Benchmark Models

Before advancing to quantitatively plausible consumption-savings models, it is useful to clarify the predictions of four simple benchmark models of consumption behavior.

Rule-of-thumb consumers Rule-of-thumb consumers consume all of their disposable income in every period. Hence they set $c_{it} = x_{it}$ and $s_{it} = 0$. This yields the following MPCs:

$$\begin{aligned} MPC^{G} = MPC^{L} &= MPC^{LN} = 1 \\ MPC^{NG} &= MPC^{NL} = 0. \end{aligned}$$

Rule-of-thumb behavior is thus not consistent with the substantial fraction of respondents who report not changing their consumption behavior in the GAIN and LOAN treatments, nor does it generate a size effect or sign asymmetry. Moreover, rule-of-thumb behavior is not consistent with the NEWS-LOSS responses, which suggests that people are at least somewhat forward looking, in contrast with the extreme myopia of rule–of-thumb consumers.

PIH consumers Strict permanent income consumers have quadratic utility, face a fixed gross interest rate $R = 1 + r$ that is equal to the inverse of the discount rate, and face no constraints on borrowing other than a No-Ponzi condition that imposes that they cannot die in debt. For such a household the optimal consumption policy is

$$c_{it} = \frac{R-1}{R}\left[x_{it} + \sum_{j=1}^{\infty} R^{-j} E_t y_{t+j}\right].$$

This gives the following MPCs:

$$\begin{aligned} MPC^L = MPC^G &= \frac{r}{1+r} \approx 0 \\ MPC^{NL} = MPC^{NG} &= \frac{r}{(1+r)^2} \approx 0 \\ MPC^{LN} &= \frac{r}{1+r} - \frac{r}{(1+r)^5} \approx 0, \end{aligned}$$

where the approximations hold for low interest rates $r \approx 0$, which is true for the types of assets typically held for short-term consumption smoothing (e.g. cash, checking accounts). The strict PIH thus implies that households will have small responses in both the LOSS and GAIN treatments, and will not generate sign asymmetry nor a size effect. One of the starkest predictions of the PIH model is that the MPC out of gains should be essentially identical to the MPC out of news about future gains (with the only difference being the negligible effect of discounting). This prediction is not consistent with the finding from Section 4.1 that even among those respondents who reported substantial MPCs in the GAIN treatment, most reported low or zero MPCs in the NEWS-GAIN treatment.

Spender-saver model The spender-saver model is one in which the population is comprised of two groups of individuals – one group of rule-of-thumb consumers (the spenders), and another group of permanent income consumers (the savers) (Campbell and Mankiw, 1989). Assuming that a fraction α of the population are spenders and the remaining $1 - \alpha$ are savers, and that the interest rate $r \approx 0$ then the MPCs for each of the five treatments are

$$\begin{aligned} MPC^G = MPC^L = MPC^{LN} &= \alpha \\ MPC^{NG} = MPC^{NL} &= 0. \end{aligned}$$

The spender-saver model is thus able to generate large average MPCs, that are heterogeneous across individuals, as well as an average MPC out of news about future gains that is smaller than the MPC out of the actual gain. However, the model inherits from the rule-of-thumb and permanent income models the inability to generate meaningful sign asymmetry or size effects. Moreover the spender-saver model predicts no response to the NEWS-LOSS treatment, and predicts the same size response to the LOAN treatment as to the GAIN and LOSS treatments. Both of these latter features are inconsistent with the survey responses.

Precautionary Savings Models Modern workhorse models for understanding consumption behavior feature precautionary motives, due to either an occasionally binding borrowing constraint or the convexity of marginal utility. Consider the following infinite-horizon precautionary-savings model, expressed in recursive form

$$V(x,y) = \max_{c,s} u(c) + \beta E\left[V(x',y')\,|y\right]$$

$$\text{subject to}$$

$$\begin{aligned} c + s &= x \\ x' &= Rs + y' \\ s &\geq 0 \end{aligned}$$

The budget constraints are the same as previously described, except for the addition of the borrowing constraint $s \geq 0$. The source of uncertainty is labor income y, which is assumed to follow a Markov process. We also assume that the utility function $u(c)$ is convex with positive third derivative. The solution to this problem implies a value function $V(x,y)$ and an associated consumption policy function $c(x,y)$.

In the case with IID income risk, it is possible to develop several sharp theoretical predictions for the MPCs in each of the five treatments. These predictions typically also hold in versions of the model with a realistic income process, for example as in the quantitative model described below. First, both the value and policy functions are well-known to be strictly concave (see e.g. Carroll and Kimball, 1996; Carroll, 1997). Strict concavity of the consumption function implies that $MPC^L > MPC^G$, meaning that the consumption response to a windfall of a given size is bigger for a loss than a gain, which is qualitatively consistent with the sign asymmetry reported in Section 4.2. But concavity also implies a negative size effect: the MPC in the GAIN treatment is smaller for larger windfalls. This is consistent with negative intensive margin size effect reported in Section 3.2, but baseline precautionary savings models cannot generate an overall positive size effect because of the absence of an extensive margin MPC.

As the level of an individual's wealth increases, both the sign and size asymmetry in MPCs get weaker. In fact, one can show (see e.g. Benhabib et al., 2011) that for Constant Relative Risk Aversion (CRRA) utility functions, as $x \to \infty$, the consumption function approaches the linear function,

$$c(x) = \left[R(\beta R)^{-\frac{1}{\gamma}} - 1\right] x,$$

where γ is the coefficient of relative risk aversion. Thus for individuals with sufficiently high levels of wealth, there is neither any sign asymmetry nor a size effect. Moreover, when either βR is close to 1 or γ is close to 1, the MPC is approximately equal to $\beta^{-1} - 1$, as in the PIH. Thus both MPC^L and MPC^G are negligibly small.

In practice, this high wealth approximation of the consumption function tends to hold well except for individuals who are on, or very close to, the borrowing constraint $x = 0$.[36] For individuals who are borrowing constrained, the consumption function takes the simple form $c(y) = y$. It follows that $c(y - \Delta) = y - \Delta$ because if the borrowing constraint is binding at $x = y$ then it will also bind at $x = y - \Delta$. Hence borrowing constrained individuals respond to the LOSS treatment by cutting consumption by the amount of the loss, i.e. $MPC^L = 1$. Whether MPC^G is also equal to 1 depends on whether the borrowing constraint is also binding at the slightly higher level of wealth $x = y + \Delta$, which is less likely the larger is the size of Δ. This means that a borrowing constrained agent has $MPC^G = 1$ for small windfalls and $MPC^G < 1$ for larger windfalls.

Thus, in terms of the simple GAIN and LOSS treatment, the simple precautionary savings model delivers MPCs out of small windfalls that are qualitatively similar to the even simpler spender-saver model. There is one group of individuals who have $MPC^G = MPC^L = 1$ and another for whom have $MPC^G = MPC^L \approx 0$. In the precautionary savings model, the identity of the individuals in each group is endogenous and time-varying, whereas in the spender-saver model it is fixed exogenously. However, for larger windfalls, the precautionary savings model differs from the spender-saver model in that it can generate the correct pattern of sign asymmetry and intensive margin size asymmetry.

We can also analyze the NEWS treatment separately for constrained and unconstrained agents. For unconstrained agents, the first-order condition for consumption

[36] With sufficiently large transitory income risk, it is possible to generate consumption functions with substantial concavity (and hence high MPCs) even at moderate levels of liquid wealth. However, typically there are very few households at this part of the wealth domain in the ergodic distribution, since optimal savings decisions imply that households desire to save themselves away from the region where the consumption function is very concave.

is

$$u'(c) = \beta R E\left[V'\left(R(x-c)+\Delta+y'\right)\right],$$

where the Δ on the right-hand side reflects the future windfall at time $t+1$ that is learned about at time t. For small Δ, it is straightforward to show that the MPC out of news and the MPC out of actual gains or losses are related by

$$MPC^{NG} = R^{-1} MPC^{G}$$
$$MPC^{NL} = R^{-1} MPC^{L}.$$

The MPC in the NEWS-GAIN treatment is less than the MPC in the GAIN treatment by a factor R and thus, for high and medium wealth individuals the MPCs in these two treatments are similar.[37] For constrained individuals, whose consumption function is $c(x) = x$, the MPC in the NEWS-GAIN treatment is 0, and thus for low wealth individuals the MPCs in the GAIN and NEWS-GAIN treatments can be very different.

The gap $MPC^{G} - MPC^{NG}$ is thus informative about whether an individual is hand-to-mouth (i.e., has the consumption function $c(x) = x$), since in this framework only hand-to-mouth agents exhibit a large difference between these two MPCs. The survey responses in Section 4.1 suggest that there are a substantial number of individuals for whom $MPC^{G} - MPC^{NG}$ is far from zero and hence may be hand-to-mouth. On the other hand, the informativeness of the corresponding gap for losses, $MPC^{L} - MPC^{NL}$ is more ambiguous. Constrained individuals have a large MPC out of the immediate loss but may or may not have a large MPC out of the news about a future loss, depending on the size of the multiplier on their borrowing constraint. In the calibrated precautionary savings model below, we find that the MPC in the NEWS-LOSS treatment is less than the MPC in the LOSS treatment, but is substantially higher than the MPC in the NEWS-GAIN treatment, i.e., $MPC^{NL} > MPC^{NG}$, as in the data. This latter property of the precautionary savings model is another dimension in which it improves on the spender-saver model.

In the context of the precautionary savings model with borrowing constraints, the LOAN treatment is informative about whether individuals are currently constrained and, if so, for how long they expect to be constrained. For unconstrained individuals with sufficient wealth that there is a low probability of still being constrained a year

[37] Differentiating with respect to Δ and evaluating at $\Delta = 0$ defines the MPC out of news implicitly as $u''(c)\frac{\partial c}{\partial \Delta} = \beta R E\left[V''\left(R(x-c)+y'\right)\right]$. Differentiating with respect to x and evaluating at $\Delta = 0$ defines the MPC out of an immediate gain implicitly as $u''(c)\frac{\partial c}{\partial \Delta} = \beta R^2 E\left[V''\left(R(x-c)+y'\right)\right]$. Taking the ratio yields the result.

later, the loan has a negligible effect on their inter-temporal budget constraint, and hence $MPC^{LN} \approx 0$.[38] For individuals who are currently borrowing constrained but expect to be unconstrained in the near future, the MPC in the LOAN treatment is similar to the MPC from the GAIN treatment, i.e., $MPC^{LN} \approx MPC^{G}$. However, for individuals who are constrained and expect to remain constrained for longer than the duration of the loan, the MPC in the LOAN treatment is approximately zero. In Section 4.4 we reported that almost no respondents indicated that they would increase spending when offered a one-year interest free loan, even those respondents who had large MPCs in the GAIN treatment and small MPCs in the NEWS-GAIN treatment. Viewed through the lens of the precautionary savings model, these responses are consistent with the presence of hand-to-mouth individuals who expect to remain in a low wealth state for a substantial period of time.

5.3 Quantitative Models of Consumption Adjustment Costs

Model description and calibration One feature of all of the models discussed in the previous section is that they do not generate a meaningful extensive margin of consumption responses, whereas our survey results indicated a large number of reported zero responses. Moreover, variation in the number of zero responses is the key driver of the size effect, as well as several of the other findings. In order to model the extensive margin of adjustment, we appeal to small costs involved with changing consumption plans. We remain agnostic on the underlying micro-foundations of these costs and simply model them as a utility cost of changing consumption. The costs may reflect cognitive costs of re-optimizing as in models of inattention (Sims, 2003; Tutino, 2013), observation costs as in models of inattentiveness (Reis, 2006), real costs of changing consumption bundles as in models of consumption commitments (Chetty and Szeidl, 2007), or salience (Kueng, 2018). In this section we present a quantitative precautionary savings model with adjustment costs and compare its predictions with our empirical findings.

[38] The inter-temporal budget constraint is affected only to the extent that $\beta < 1$.

Expressed in recursive form, the model is

$$
\begin{aligned}
V(x,c,y) &= \max\left\{V^A(x,y)-\psi, V^N(x,c,y)\right\}\\
V^A(x,y) &= \max_{c',s} u(c') + \beta E\left[V(x',c',y')\,|y\right]\\
&\quad\text{subject to}\\
c'+s &= x\\
x' &= Rs + y'\\
s &\geq 0\\
V^N(x,c,y) &= u(c) + \beta E\left[V(R(x-c)+y',c,y')\,|y\right]
\end{aligned}
$$

In each period, a household with cash-on-hand x and earnings y can choose whether to adjust their consumption, in which case their value function is denoted by $V^A(x,y)$, or to not adjust their consumption, in which case their value function is denoted by $V^N(x,c,y)$. If the household adjusts their consumption, they incur a utility cost ψ and then choose how much to consume and save, subject to standard budget and borrowing constraints. If the household does not adjust their consumption, then they consume c, which is the same amount that they consumed in the previous period. Note that it is always feasible for a household to adjust their consumption, so households who are consuming more than their income will eventually be forced to adjust their consumption downwards in order to satisfy the budget constraint.

This model introduces one additional parameter, ψ, relative to the standard consumption-savings model. For values of $\psi > 0$, the model generates an inaction region for consumption, in which small changes in liquid resources do not induce a consumption response. Hence the model can, in principle, generate an extensive margin response. Larger windfalls are more likely to push a household out of its inaction region. Hence the model can, in principle, also generate a positive extensive-margin size effect.

Our calibration of the model is relatively standard. We assume log utility and solve the model at a quarterly period so that we can produce quarterly MPCs as in the survey. We set the annual return on savings to 2%. The remaining parameters are the discount factor β, adjustment cost parameter ψ, and the stochastic process for idiosyncratic income.

We estimate a quarterly process of idiosyncratic earnings using annual data on total household labor income from the Panel Study of Income Dynamics from 1967 to 2008. The process is a discretized version of the following persistent-transitory process.

$$\begin{aligned}\log y_{it} &= z_{it} + \varepsilon_{it} \\ z_{it} &= \rho z_{it-1} + \eta_{it},\end{aligned}$$

where ε_{it} and η_{it} are persistent and transitory shocks that are drawn from the following distributions independently across households and over time

$$\eta_{it} \sim \begin{cases} 0 & \text{with p} = 1-\lambda_\eta \\ N\left(0, \sigma_\eta^2\right) & \text{with p} = \lambda_\eta \end{cases} \qquad \varepsilon_{it} \sim \begin{cases} 0 & \text{with p} = 1-\lambda_\varepsilon \\ N\left(0, \sigma_\varepsilon^2\right) & \text{with p} = \lambda_\varepsilon \end{cases}$$

Relative to typical annual models of earnings dynamics, this model features two additional parameters $(\lambda_\eta, \lambda_\varepsilon)$ that describe the arrival rate of persistent and transitory shocks. We set the arrival rates at 0.25 so that households receive income shocks on average once per year, in order for our quarterly earnings model to be as similar as possible to typical models of annual earnings dynamics.[39] We estimate the remaining three parameters $\left(\rho, \sigma_\eta^2, \sigma_\varepsilon^2\right)$ using the variance of log annual income income and log annual income changes at different lags. The parameter estimates are $\left(\rho, \sigma_\eta^2, \sigma_\varepsilon^2\right) = (0.9878, 0.0439, 0.6373)$. A full description of the estimation is contained in Appendix C.1. We also include a lump-sum transfer of \$1,000 per quarter to capture the effects of welfare programs.

In our preferred calibration, we also allow for (fixed) heterogeneity in discount factors β, as in Carroll et al. (2017) and Krueger et al. (2016). In a precautionary savings model with a single savings instrument such as ours, discount factor heterogeneity is necessary to simultaneously match the mean level of wealth in the data and the fraction of households with a low level of wealth. We choose the mean discount factor so that the model generates a ratio of mean assets to mean annual earnings of 3.2. This number is based on data from the 2016 Survey of Consumer Finances, in which mean household labor income was \$62,000 and mean financial wealth among the bottom 99% of households was \$201,000. We calibrate to the SCF, rather than the sample of SCE respondents, because the SCE only provides income and wealth data as a categorical variable. Nonetheless both mean financial wealth and mean

[39] An alternative approach followed, for example, by Krueger et al. (2016) is to set $\lambda_\eta = 1$ and to estimate the remaining parameters by transforming corresponding estimates from an annual model. By implicitly assuming that households realize income shocks every single quarter, this approach generates unrealistically large transitory risk, and is inconsistent with data on higher-order moments of annual income changes. Yet another approach is to estimate $(\lambda_\eta, \lambda_\varepsilon)$ by explicitly targeting these higher-order moments in estimation. See Appendix C.1 for details.

annual earnings are similar in the two surveys.[40] We allow for a three-point distribution of discount factors, equally spaced around the mean, with a spread chosen to match a fraction of households with financial wealth less than \$1000 of 23% (2016 SCF).[41] For comparison, we also report results from the two corresponding versions of the model without discount factor heterogeneity, one in which we choose β to match the mean wealth target, and one in which we choose β to match the fraction of households with financial wealth less than \$1000.

The remaining parameter is the adjustment cost ψ. Since the adjustment cost governs the size of the inaction region for consumption, we choose it so that the model generates the correct fraction of households with a positive MPC in the \$500 GAIN treatment. We pick 20% as the target fraction.[42] By targeting this one feature of the MPC distribution with our one free parameter, we have thus tied our hands in terms of whether the model can match the remaining pattern of MPCs across the various treatments along both the extensive and intensive margin. Appendix C.2 contains additional statistics on the wealth, income and consumption distributions in each version of the model discussed below.

MPCs in the calibrated model The MPCs for each of the five treatments are displayed in panel A of Table 5. These should be compared with the corresponding MPCs from the survey, which are reported in Table 3. The model generates 20% of households with a positive MPC in the \$500 GAIN treatment by construction. The remaining MPC statistics were not targeted in calibration. Nonetheless, the model with consumption adjustment costs and discount factor heterogeneity is consistent with several of the main findings from the survey.

First, the model generates the observed heterogeneity in the GAIN treatments, with most households not responding to gains, but a minority of households responding by a large amount. The median and mean intensive margin responses in

[40]Mean financial wealth is \$330,000 in the SCF and \$326,000 in the SCE. Mean annual earnings is \$62,000 in the SCF and \$67,000 in the SCE.

[41]An alternative approach is to introduce a second illiquid asset as in Kaplan and Violante (2014). One advantage of the illiquid asset approach over the discount factor heterogeneity approach is that the discount factor approach implicitly assumes that all households with low levels of liquid wealth (which is what matters for MPCs) also have low levels of total wealth. For example, even though our calibrated model matches the fraction of households with less than \$1,000 in financial wealth (23%), it overstates the fraction with less than \$1,000 in total wealth (23% vs 15%) and the fraction with less than \$10,000 in total wealth (55% vs 25%). Introducing illiquid assets into the model with consumption adjustment costs is computationally infeasible because it would mean that the model would contain three endogenous state variables.

[42]This target fraction is slightly larger than the 18% reported in Table 3, since in the data we have some respondents report negative MPCs, which the model will not generate. As shown in Table A-9, when we exclude respondents with MPCs outside the [0,1] range, the fraction is 20%.

the GAIN treatments are similar in the data and the model. Second, the model is able to generate a positive extensive-margin and negative intensive-margin size effect in the GAIN treatments, netting out to a positive overall size effect, as in the data. Third, the model generates sign asymmetry as in the data: the average MPC is larger in the \$500 LOSS treatment than in the \$500 GAIN treatment, driven by both the extensive and intensive margins. However, the magnitude of this sign asymmetry is smaller than in the data, as further discussed below.

Fourth, the MPCs in the NEWS-GAIN treatments are smaller than the corresponding MPCs in the GAIN treatment, as in the data, with fewer households responding to the \$500 NEWS-GAIN treatment—even some of the households with large responses in the \$500 GAIN treatment. Fifth, a larger fraction of households respond to the NEWS-LOSS treatment than the NEWS-GAIN treatment. As in the data, the average MPC in the NEWS-LOSS treatment is larger than the average MPC in the NEWS-GAIN treatment, driven mostly by differences on the extensive margin. Finally, the fraction of households responding to the \$5000 LOAN treatment, and the MPC conditional on responding, are both smaller than in the same size GAIN and NEWS-GAIN treatments.

Table 5: MPCs from Treatments in Model with Consumption Adjustment Costs

		Mean MPC	Share of Resp. with MPC < 0	= 0	> 0	MPC | MPC > 0 Mean	Median
Panel A: Baseline with β Heterogeneity							
Gain	\$500	0.13	0.00	0.80	0.20	0.65	0.56
	\$2500	0.20	0.00	0.44	0.56	0.36	0.33
	\$5000	0.20	0.00	0.35	0.65	0.30	0.28
Loss	\$500	0.18	0.00	0.75	0.25	0.72	0.71
News-Gain	\$500 in 3 months	0.05	0.00	0.89	0.11	0.50	0.46
	\$5000 in 3 months	0.11	0.00	0.42	0.58	0.19	0.20
News-Loss	\$500 in 3 months	0.10	0.00	0.82	0.18	0.52	0.46
	\$500 in 2 years	0.01	0.00	0.99	0.01	0.75	0.50
Loan	\$5000	0.08	0.00	0.58	0.42	0.19	0.14
Panel B: No β Heterogeneity, targeting mean wealth							
Gain	\$500	0.04	0.00	0.80	0.20	0.19	0.16
	\$2500	0.04	0.00	0.55	0.45	0.10	0.07
	\$5000	0.04	0.00	0.40	0.60	0.07	0.05
Loss	\$500	0.05	0.00	0.78	0.22	0.20	0.16
News-Gain	\$500 in 3 months	0.03	0.00	0.81	0.19	0.18	0.16
	\$5000 in 3 months	0.04	0.00	0.33	0.67	0.06	0.04
News-Loss	\$500 in 3 months	0.04	0.00	0.76	0.24	0.18	0.15
	\$500 in 2 years	0.01	0.00	0.95	0.05	0.11	0.07
Loan	\$5000	0.01	0.00	0.86	0.14	0.04	0.03
Panel C: No β Heterogeneity, targeting households with < \$1000							
Gain	\$500	0.14	0.00	0.80	0.20	0.69	0.61
	\$2500	0.24	0.00	0.31	0.69	0.34	0.31
	\$5000	0.24	0.00	0.15	0.85	0.28	0.27
Loss	\$500	0.21	0.00	0.74	0.26	0.80	0.80
News-Gain	\$500 in 3 months	0.08	0.00	0.87	0.13	0.62	0.52
	\$5000 in 3 months	0.16	0.00	0.23	0.77	0.21	0.21
News-Loss	\$500 in 3 months	0.13	0.00	0.81	0.19	0.72	0.57
	\$500 in 2 years	0.01	0.00	0.99	0.01	1.26	1.11
Loan	\$5000	0.08	0.00	0.52	0.48	0.17	0.12

Note: Positive MPC corresponds to a negative change in spending for the loss treatments.

Admittedly, along some of these dimensions the model does not generate as large differences across treatments as in the data. First, the model does not match the magnitude of the sign asymmetry: while in the data the mean MPC is more than four times larger in the LOSS treatment than in the GAIN treatment, in the model the MPC is only 40% larger in the LOSS treatment. Second, the response to the NEWS-LOSS treatment in the data is almost as large as the response in the corresponding

LOSS treatment. Although the model is not able to deliver such a large NEWS-LOSS response, qualitatively the results go in the right direction. Moreover, the average MPC in the NEWS-LOSS treatment is twice as large as the average MPC in the NEWS-GAIN treatment. Third, the model is also unable to generate much of a response to the 2-year NEWS-LOSS treament. We conjecture that one reason for the failure of the model along this dimension may be due to the use of discount factor heterogeneity, rather than illiquid assets, as a mechanism for generating low liquid wealth household. The relative impatience of households with low wealth (who are the households with large responses in the LOSS treatment) limits the forward looking behavior that is necessary to generate large responses to the NEWS-LOSS treatment.[43] Fourth, for similar reasons, the model generates larger responses to the LOAN treatment along both the extensive and intensive margin than in the data.

Table 6: Extensive-margin MPC in Data and Model for Different Treatments

Scenarios		Data (%)				Model (%)			
(A)	(B)	(A) Only	(B) Only	Both	Neither	(A) Only	(B) Only	Both	Neither
GAIN \$500	GAIN \$5000	6	25	14	56	0	45	20	35
GAIN \$500	LOSS \$500	7	45	11	37	5	11	15	69
GAIN \$500	NEWS-GAIN \$500	10	2	2	86	10	1	10	79
LOSS \$500	NEWS-LOSS \$500	12	9	37	42	8	1	17	74

Notes: Table shows percentage of respondents with positive MPC in each combination. Number of observations in the data across pairs of scenarios: 980, 792, 361, 584.

The model also matches some of the heterogeneity on the extensive margin across treatments. For the four pairs of treatments where we have sufficiently large samples, Table 6 shows the proportion of individuals who respond to both treatments, to neither treatment, or to only one of them, in the data and in the model. In most cases the model matches the qualitative patterns of cross-correlation across treatments in the data. The main exception is the second row, in which we compare the \$500 GAIN and LOSS treatments. For the reasons described above, the model under-predicts the fraction of respondents who respond to the LOSS but not the GAIN.

Nonetheless, for a highly stylized model that introduces only one additional parameter relative to off-the-shelf consumption models, this is an encouraging collec-

[43] In addition, behavioral factors may be at play, especially when it comes to the surprisingly large number of respondents in the 2-year NEWS-LOSS treatment that state they would cut their spending now rather than in the future. For instance, this finding may be related to other evidence showing that, in contrast to standard models of discounting, people tend to prefer incurring losses now rather than later (see e.g. Loewenstein and Prelec, 1991).

tion of results. There are two important features that combine to yield this set of consumption responses. First, it is important that the model generates a wealth distribution that matches the fraction of households with low wealth. Our model achieves this through discount factor heterogeneity. To illustrate the role of this heterogeneity, panels B and C of Table 5 display analogous MPCs in the version of the model without heterogeneity. Panel B shows the MPCs when we choose the common discount factor so that mean wealth is the same as in the baseline model and we re-calibrate the consumption adjustment cost so that the fraction of households that respond to the $500 GAIN treatment is 20%, also as in the model with heterogeneous discount factors. In this model, only 2% of households have wealth less than $1,000. Consequently, the mean MPCs in the GAIN and LOSS treatments are much lower than in the data, without meaningful size or sign asymmetry. There is also no meaningful difference between the average MPC or fraction of respondents with MPC>0 in the NEWS-GAIN treatment versus the GAIN treatment. This is because almost all households have sufficient wealth to act like the permanent-income consumers described in Section 5.2.

In panel C of Table 5, we report MPCs when we choose the common discount factor so that 23% of households have wealth less than $1000, as in the baseline model. Again, we re-calibrate the consumption adjustment cost so that the fraction of households that respond to the $500 GAIN treatment is 20%. In this version of the model, both the fraction of households responding to each of the treatments, as well as the distribution of MPCs conditional on responding, are very similar to the baseline model with heterogeneous discount factors. However, the mean level of wealth in this version of the model is only 17% of average annual earnings (about $10,000), which is approximately twenty-times less than in the SCF data. This makes it clear that the role of discount factor heterogeneity is to generate a sufficiently large fraction of households with low wealth, while remaining consistent with the observed average level of wealth.

The second important feature of our model is the inclusion of consumption adjustment costs. Clearly, this feature is necessary to speak to the extensive margin of responses. But it also helps in matching some patterns of the average MPCs across treatments, ignoring the extensive margin. To see the importance of the consumption adjustment costs, Table 7 shows the mean MPCs in three versions of the model without consumption adjustment costs—with heterogeneous discount factors, with a common discount factor calibrated to mean wealth, and with a common discount factor calibrated to the fraction of households with less than $1,000. In all three models, the absence of an extensive margin means that the model generates a size effect in the wrong direction. Moreover the models without consumption adjustment costs generate less sign asymmetry and a smaller gap between the MPCs in the GAIN and NEWS-GAIN treatments, than in the model with consumption adjustment costs. The reason is that in both the survey responses and the baseline model, the extensive margin contributes a substantial component to these differences in average MPCs, which is missed in the models without adjustment costs.

Table 7: Mean MPCs from Treatments in Models without Consumption Adjustment Costs

	Heterogeneous β	Common β Target Mean Wealth	Common β Target Frac $<$ \$1000
Gain			
\$500	0.23	0.05	0.29
\$2500	0.21	0.05	0.27
\$5000	0.20	0.04	0.25
Loss			
\$500	0.27	0.06	0.32
News-Gain			
\$500 in 3 months	0.17	0.05	0.24
\$5000 in 3 months	0.12	0.04	0.19
News-Loss			
\$500 in 3 months	0.21	0.05	0.26
\$500 in 2 years	0.06	0.03	0.03
Loan			
\$5000	0.08	0.01	0.10

Overall, these quantitative findings suggest that two features are important in generating the observed pattern of MPCs across the survey treatments: (i) a meaningful extensive margin of consumption response, and (ii) a sufficient fraction of households with limited access to disposable liquid resources. Our model generates the former through a utility cost of changing consumption and the latter through discount factor heterogeneity.

6 Conclusions

We have shown how carefully constructed survey questions about hypothetical treatments can be useful in distinguishing models of consumption behavior.

Asking survey respondents how their spending would change in multiple different scenarios yielded six broad findings. First, there is a large amount of heterogeneity in consumption responses to small unexpected gains: most people do not react but there is a set of people who spend a substantial fraction of the windfall. Second, there is a positive extensive margin size effect: for bigger gains, more people respond. Third, we find evidence of sign asymmetry: spending responses to losses are larger and more widespread than spending responses to the same size gains. These responses to losses are correlated with holdings of liquid assets. Fourth, very few respondents increase their spending in response to news about future gains, even those respondents who indicate that they would increase spending in response to actual gains. Fifth, people generally do react to news about future losses. Sixth, almost no respondents indicated that they would increase spending when offered a one-year interest-free loan.

For all of these findings, we found that the extensive margin plays a crucial role, in particular the positive size effect, the sign effect and the differences in responses to immediate gains and news about future gains. Moreover, several of the findings are suggestive of limited access to disposable resources being an important determinant of MPCs: higher MPCs out of losses than gains, the fact that MPCs out of losses are related to liquid wealth, and the very low MPCs out of news about future gains. The survey findings are also informative about the underlying reasons why many individuals act as if they have limited access to disposable resources: the substantial MPC out of news about future losses is evidence against excessive impatience, myopia or extreme forms of present-bias; the lack of a spending response to the loan suggests that it is unlikely that short-term credit constraints play an important role.

We then showed that a standard precautionary savings model with two key ingredients can match this pattern of MPCs: (i) a meaningful extensive margin of consumption responses, which we generate through a utility cost of changing consumption, and (ii) a realistically large fraction of households with limited access to disposable liquid resources, which we generate through discount factor heterogeneity.

The models that we considered all equate limited access to disposable resources with having little or no liquid wealth (i.e. being hand-to-mouth). A strict interpretation of these models thus suggests that liquid wealth, which is in principle easy to measure, should have strong explanatory power for cross-sectional heterogeneity in MPCs. However, with the exception of the LOSS treatment, we failed to find a significant correlation between MPCs and measures of hand-to-mouth behavior in terms of liquid wealth. This is true more generally in the literature on estimating MPCs: even in the more recent studies which have sufficient power to uncover a correlation between liquid wealth and MPCs, measured liquid wealth explains very little of the overall cross-sectional heterogeneity in MPCs (e.g. in terms of R-squared). One reason for this, we think, is that the relevant notion of *disposable* liquid wealth for determining MPCs, is actually very difficult to measure. It depends on the level, variation and timing of regular income; access to both informal and formal credit; and the level and variation in pre-committed and/or unforeseen expenses. Pre-committed expenses are particularly difficult to measure and are likely to be correlated with income and liquid wealth in complicated ways that make liquid wealth a very noisy proxy of disposable resources.

On the methodological front, we have demonstrated the usefulness of alternative treatments – gains, losses, news and loans – in distinguishing between different models of consumption behavior. Our hope is that future work will seek to identify similar experiments based on actual choice data that can complement our findings based on answers to hypothetical questions.

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Online Appendix for "What Would You Do with $500? Spending Responses to Gains, Losses, News and Loans"

A Further Details on Survey

In this Appendix, we provide the exact language and formatting (emphasis/underlining) used in the survey questions analyzed in the paper.

A.1 MPC Questions

For our MPC elicitation, we rely on questions across five different types of scenarios:

1. GAIN: windfall now ($500, $2,500, $5,000)
2. GAIN-NEWS: windfall in 3 months ($500, $5,000)
3. LOSS: occurring now ($500)
4. LOSS-NEWS: occurring later (500; either in 3 months or in 2 years)
5. LOAN ($5,000)

Parts in square brackets denote different variations depending on the gain amount or the timing (these were not shown to respondents). For scenario categories 2-5, we only provide the parts that differ from the GAIN scenarios. For the LOSS and LOSS-NEWS scenarios, there were minor changes to the wording across survey waves, as noted below. Example screenshots are provided at the end of this section.

<u>1. GAIN</u>

Now consider a hypothetical situation where you unexpectedly receive a one-time payment of **$500 [or 2,500, or 5,000]** today.

We would like to know whether this extra income would cause you to change your spending behavior in any way over the **next 3 months**.

Please select only one

• Over the next 3 months, I would **spend/donate** more than if I hadn't received the $500 [or 2,500, or 5,000]

• Over the next 3 months, I would **spend/donate** the same as if I hadn't received the $500 [or 2,500, or 5,000]

• Over the next 3 months, I would **spend/donate** less than if I hadn't received the $500 [or 2,500, or 5,000]

Please select only one

• Over the next 3 months, I would **pay off more debt (or borrow less)** than if I hadn't received the $500 [or 2,500, or 5,000]

• Over the next 3 months, I would **pay off the same amount of debt** as if I hadn't received the $500 [or 2,500, or 5,000]

• Over the next 3 months, I would **pay off less debt (or borrow more)** than if I hadn't received the $500 [or 2,500, or 5,000]

Please select only one

• Over the next 3 months, I would **save more** than if I hadn't received the $500 [or 2,500, or 5,000]

• Over the next 3 months, I would **save the same** as if I hadn't received the $500 [or 2,500, or 5,000]

• Over the next 3 months, I would **save less** than if I hadn't received the $500 [or 2,500, or 5,000]

[If selected "more" ["less"] spending]:
You indicated that you would increase [reduce] your spending/donations over the next 3 months following the receipt of the $500 [or $2,500, or $5,000] payment.

How much more [less] would you spend/donate than if you hadn't received the $500 [or $2,500, or $5,000]? [Enter value]

[Spending composition follow up (see screenshot below):]
And how much of these $(entered value) would you spend on each of the following: [And how much of this $(entered value) would come from a reduction in spending on each of the following:]
(Please note: The numbers need to add up to [entered value].)
Traveling / vacation / eating out / other leisure activities: $
Donation / gifts: $
General living expenses: $
Purchase of durables typically costing $1,000 or less (eg. electronics, sports equipment, clothing etc.): $
Purchase of durables typically costing more than $1,000 (such as a car, etc.): $
Renovations or improvements to my home: $
Pay for college / education / training for members of my household (including myself): $
Other (please specify:): $

[Spending timing follow up, asked for increases only (see screenshot below):]
You indicated that you would increase your spending/donations over the next 3 months by $(entered value) following the receipt of the $2,500 [or 5,000] payment. How would your spending change over time? I would increase my spending in...

(Please note: The numbers need to add up to [entered value].)

the next 2 weeks by $

the 2 weeks after that by $

the second month by $

the third month by $

[If selected pay off more debt (or borrow less)]

You indicated that you would pay off more debt (or borrow less) over the next 3 months following the receipt of the $500 [or 2,500, or 5,000] payment. How much more debt would you pay off (or how much less would you borrow) than if you hadn't received the $500 [or 2,500, or 5,000]?

[If selected pay off less debt (or borrow more)]

You indicated that you would pay off less debt (or borrow more) over the next 3 months following the receipt of the $500 [or 2,500, or 5,000] payment. How much less debt would you pay off (or how much more would you borrow) than if you hadn't received the $500 [or 2,500, or 5,000]?

[If selected "more" ["less"] saving]:
You indicated that you would increase [reduce] your saving over the next 3 months following the receipt of the $500 [or 2,500, or 5,000] payment. How much more [less] would you save than if you hadn't received the $500 [or 2,500, or 5,000] ?

2. GAIN-NEWS

Now consider a hypothetical situation where you learn that you will receive a guaranteed one-time payment of **$500 [or $5,000]** exactly three months from today**.**

We would like to know whether this extra income would cause you to change your spending behavior in any way over the **next 3 months (that is, before you receive the money)**.

Please select only one

- Over the next 3 months, I would **spend/donate more** than if I did not expect the guaranteed $500 [or 5,000] (in 3 months' time)
- Over the next 3 months, I would **spend/donate the same** as if I did not expect the guaranteed $500 [or 5,000]
- Over the next 3 months, I would **spend/donate less** than if I did not expect the guaranteed $500 [or 5,000]

Please select only one

- Over the next 3 months, I would **pay off more debt (or borrow less)** than if I did not expect the guaranteed $500 [or 5,000] (in 3 months' time)

• Over the next 3 months, I would **pay off the same amount of debt** as if I did not expect the guaranteed $500 [or 5,000]

• Over the next 3 months, I would **pay off less debt (or borrow more)** than if I did not expect the guaranteed $500 [or 5,000]

Please select only one

• Over the next 3 months, I would **save more** than if I did not expect the guaranteed $500 (in 3 months' time)

• Over the next 3 months, I would **save the same** as if I did not expect the guaranteed $500 [or 5,000]

•Over the next 3 months, I would **save less** than if I did not expect the guaranteed $500 [or 5,000]

[If selected "more" ["less"] spending]:
You indicated that you would increase [reduce] your spending/donations over the next 3 months after learning that you will receive a $500 [or $5,000] payment in 3 months.

How much more [less] would you spend/donate than if you did not expect to receive the $500 [or $5,000] in 3 months? [Enter value]

[Spending composition follow up]

[If selected "more" ["less"] borrowing]:
You indicated that you would pay off less debt (or borrow more) [pay off more debt (or borrow less)] over the next 3 months after learning that you will receive a $500 [or $5,000] payment in 3 months.

How much less debt would you pay off (or how much more would you borrow) [more debt would you pay off (or how much less would you borrow)] than if you did not expect to receive the $500 [or $5,000] in 3 months?[Enter value]

[If selected "more" ["less"] saving]:
You indicated that you would increase [reduce] your saving over the next 3 months after learning that you will receive a $500 [or $5,000] payment in 3 months.

How much more [less] would you save than if you did not expect to receive the $500 [or $5,000] in 3 months? [Enter value]

3. LOSS

March 2016 wave:

Now consider a hypothetical situation in which you **unexpectedly** lose $500 today. Note that this is a one-time loss – it does not in any way affect your income going forward. You have simply found yourself suddenly to have $500 less than you previously had.

We would like to know whether this one-time $500 loss would cause you to change your spending behavior in any way over the **next 3 months.**
[The rest is identical to the March 2017 wave below]

March 2017 wave:

Now consider a hypothetical situation in which you **unexpectedly** lose $500 **today**. Note that this is a one-time loss – it does not in any way affect your income going forward. You have simply found yourself suddenly to have $500 less than you previously had.

We would like to know whether this one-time $500 loss would cause you to change your spending behavior in any way over the **next 3 months (that is, between now and June 2017).**

Please select only one

- Over the next 3 months, I would **spend/donate more** than if I hadn't lost $500
- Over the next 3 months, I would **spend/donate the same** as if I hadn't lost $500
- Over the next 3 months, I would **spend/donate less** than if I hadn't lost $500

Please select only one

- Over the next 3 months, I would **pay off more debt (or borrow less)** than if I hadn't lost $500
- Over the next 3 months, I would **pay off the same amount of debt** as if I hadn't lost $500
- Over the next 3 months, I would **pay off less debt (or borrow more)** than if I hadn't lost $500

Please select only one

- Over the next 3 months, I would **save more** than if I hadn't lost $500
- Over the next 3 months, I would **save the same** as if I hadn't lost $500
- Over the next 3 months, I would **save less** than if I hadn't lost $500

[If selected "more" ["less"] spending]:
You indicated that you would increase [reduce] your spending/donations over the next 3 months following the one-time loss of $500.

How much more [less] would you spend/donate than if you hadn't lost $500? [Enter value]

[Spending composition follow up]

[If selected “more” [“less”] borrowing]:
You indicated that you would pay off less debt (or borrow more) [pay off more debt (or borrow less)] over the next 3 months following the one-time loss of $500.

How much less debt would you pay off (or how much more would you borrow) [more debt would you pay off (or how much less would you borrow)] than if you hadn’t lost $500? [Enter value]

[If selected “more” [“less”] saving]:
You indicated that you would increase [reduce] your saving over the next 3 months following the one-time loss of $500.

How much more [less] would you save than if you hadn’t lost $500? [Enter value]

4. LOSS-NEWS

January 2017 wave:

Now consider a hypothetical situation where you learn today that you will lose **$500** exactly **three months** from today. Note that this is a one-time loss – it will not in any way affect your income otherwise.

We would like to know whether this one-time $500 loss would cause you to change your spending behavior in any way over the **next upcoming 3 months (that is, before you lose the $500).** [The rest is identical to the March 2017 wave below]

March 2017 wave:

Now consider a hypothetical situation where you learn today that you will lose **$500** exactly **three months** from today (in June 2017) [exactly **two years** from today (in March 2019)]. Note that this is a one-time loss – it will not in any way affect your income otherwise.

We would like to know whether this one-time $500 loss would cause you to change your spending behavior in any way over the **next upcoming 3 months (that is, between now and June 2017 – before you lose the $500).**

Please select only one

• Over the next 3 months, I would **spend/donate more** than if I did not expect the guaranteed $500 loss (in 3 months' time) [(in 2 years' time)]

• Over the next 3 months, I would **spend/donate the same** as if I did not expect the guaranteed $500 loss

• Over the next 3 months, I would **spend/donate less** than if I did not expect the guaranteed $500 loss

Please select only one

• Over the next 3 months, I would **pay off more debt (or borrow less)** than if I did not expect the guaranteed $500 loss (in 3 months' time) [(in 2 years' time)]

• Over the next 3 months, I would **pay off the same amount of debt** as if I did not expect the guaranteed $500 loss

• Over the next 3 months, I would **pay off less debt (or borrow more)** than if I did not expect the guaranteed $500 loss

Please select only one

• Over the next 3 months, I would **save more** than if I did not expect the guaranteed $500 loss (in 3 months' time) [(in 2 years' time)]

• Over the next 3 months, I would **save the same** as if I did not expect the guaranteed $500 loss

•Over the next 3 months, I would **save less** than if I did not expect the guaranteed $500 loss)

[If selected "more" ["less"] spending]:

You indicated that you would increase [reduce] your spending/donations over the next 3 months after learning that you will lose $500 in 3 months [in 2 years].

How much more [less] would you spend/donate than if you did not expect the $500 loss in 3 months [2 years]? [Enter value]

[Spending composition follow up]

[If selected "more" ["less"] borrowing]:

You indicated that you would pay off less debt (or borrow more) [pay off more debt (or borrow less)] over the next 3 months after learning that you will lose $500 in 3 months [in 2 years].

How much less debt would you pay off (or how much more would you borrow) [more debt would you pay off (or how much less would you borrow)] than if you did not expect the $500 loss in 3 months [2 years]? [Enter value]

[If selected “more” [“less”] saving]:

You indicated that you would increase [reduce] your saving over the next 3 months after learning that you will lose $500 in 3 months [in 2 years].

How much more [less] would you save than if you did not expect the $500 loss in 3 months [2 years]? [Enter value]

5. LOAN

Now consider a hypothetical situation where you are unexpectedly given **$5,000** today which you will have to return after 12 months. So this $5,000 is **a interest-free loan** that is to be repaid in a year’s time.

We would like to know whether this $5,000 would cause you to change your spending behavior in any way over the **next 3 months.**

Please select only one

• Over the next 3 months, I would **spend/donate more** than if I hadn’t received the $5,000 loan

• Over the next 3 months, I would **spend/donate the same** as if I hadn’t received the $5,000 loan

• Over the next 3 months, I would **spend/donate less** than if I hadn’t received the $5,000 loan

Please select only one

• Over the next 3 months, I would **pay off more debt (or borrow less)** than if I hadn’t received the $5,000 loan

• Over the next 3 months, I would **pay off the same amount of debt** as if I hadn’t received the $5,000 loan

• Over the next 3 months, I would **pay off less debt (or borrow more)** than if I hadn’t received the $5,000 loan

Please select only one

• Over the next 3 months, I would **save more** than if I hadn’t received the $5,000 loan

• Over the next 3 months, I would **save the same** as if I hadn’t received the $5,000 loan

•Over the next 3 months, I would **save less** than if I hadn’t received the $5,000 loan

[If selected “more” [“less”] spending]:

You indicated that you would increase [reduce] your spending/donations over the next 3 months following the receipt of the $5,000 loan.

How much more [less] would you spend/donate than if hadn't received the $5,000 loan? [Enter value]

[Spending composition follow up]

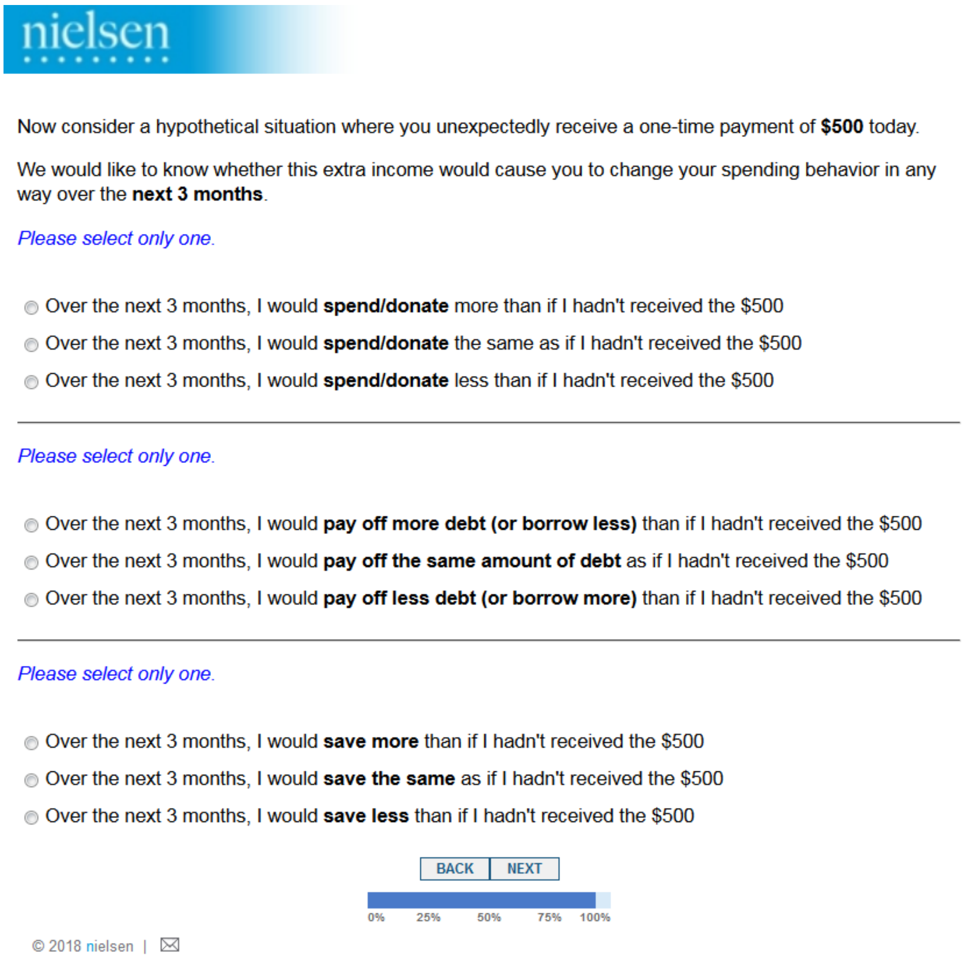
[Spending timing follow up]

[If selected "more" ["less"] borrowing]:
You indicated that you would pay off less debt (or borrow more) [pay off more debt (or borrow less)] over the next 3 months following the receipt of the $5,000 loan.

How much less debt would you pay off (or how much more would you borrow) [pay off more debt (or borrow less)] than if you hadn't received the $5,000 loan? [Enter value]

[If selected "more" ["less"] saving]:
You indicated that you would increase [reduce] your saving over the next 3 months following the receipt of the $5,000 loan.

How much more [less] would you save than if you hadn't received the $5,000 loan? [Enter value]


nielsen

Now consider a hypothetical situation where you unexpectedly receive a one-time payment of **$500** today.

We would like to know whether this extra income would cause you to change your spending behavior in any way over the **next 3 months**.

Please select only one.

- Over the next 3 months, I would **spend/donate** more than if I hadn't received the $500
- Over the next 3 months, I would **spend/donate** the same as if I hadn't received the $500
- Over the next 3 months, I would **spend/donate** less than if I hadn't received the $500

Please select only one.

- Over the next 3 months, I would **pay off more debt (or borrow less)** than if I hadn't received the $500
- Over the next 3 months, I would **pay off the same amount of debt** as if I hadn't received the $500
- Over the next 3 months, I would **pay off less debt (or borrow more)** than if I hadn't received the $500

Please select only one.

- Over the next 3 months, I would **save more** than if I hadn't received the $500
- Over the next 3 months, I would **save the same** as if I hadn't received the $500
- Over the next 3 months, I would **save less** than if I hadn't received the $500

BACK NEXT

0% 25% 50% 75% 100%

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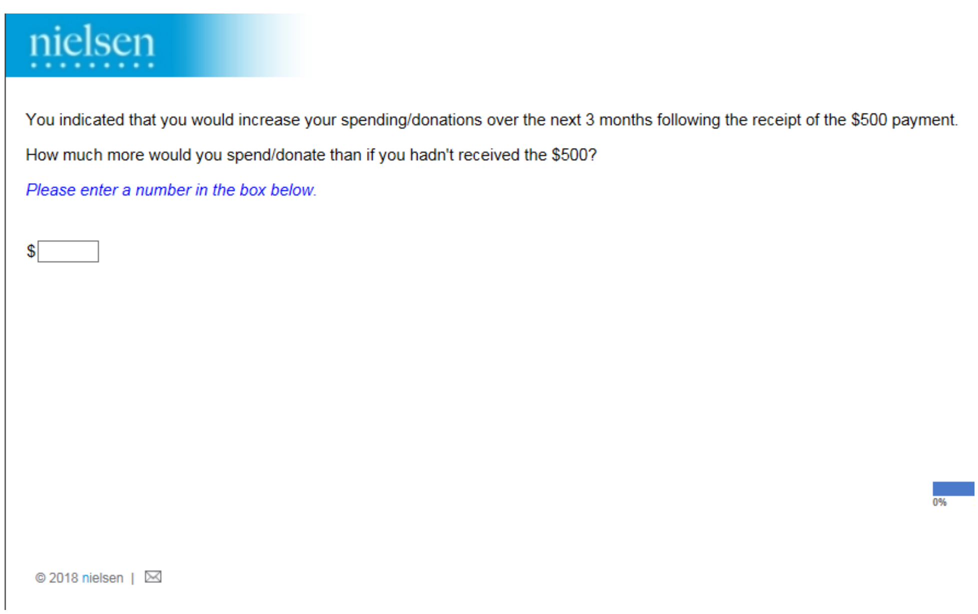
(a) Screen 1


nielsen

You indicated that you would increase your spending/donations over the next 3 months following the receipt of the $500 payment.

How much more would you spend/donate than if you hadn't received the $500?

Please enter a number in the box below.

$

0%

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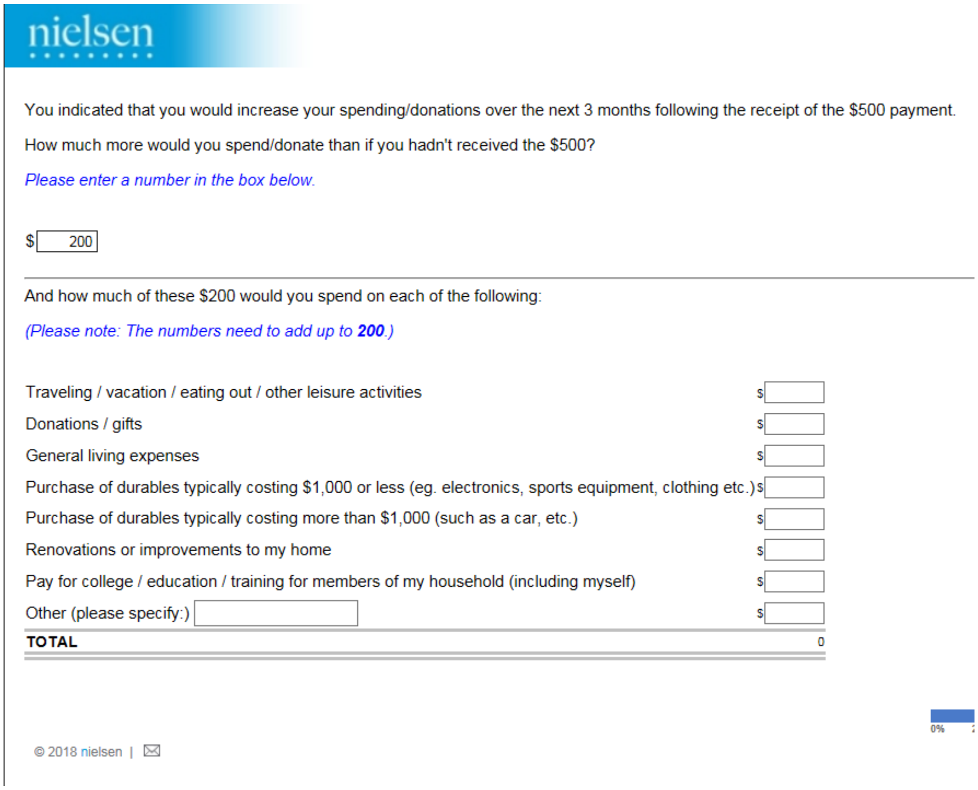
(b) Screen 2, part 1



nielsen

You indicated that you would increase your spending/donations over the next 3 months following the receipt of the $500 payment.

How much more would you spend/donate than if you hadn't received the $500?

Please enter a number in the box below.

$ 200

And how much of these $200 would you spend on each of the following:

*(Please note: The numbers need to add up to **200**.)*

Traveling / vacation / eating out / other leisure activities	$
Donations / gifts	$
General living expenses	$
Purchase of durables typically costing $1,000 or less (eg. electronics, sports equipment, clothing etc.)	$
Purchase of durables typically costing more than $1,000 (such as a car, etc.)	$
Renovations or improvements to my home	$
Pay for college / education / training for members of my household (including myself)	$
Other (please specify:)	$
TOTAL	0

0%

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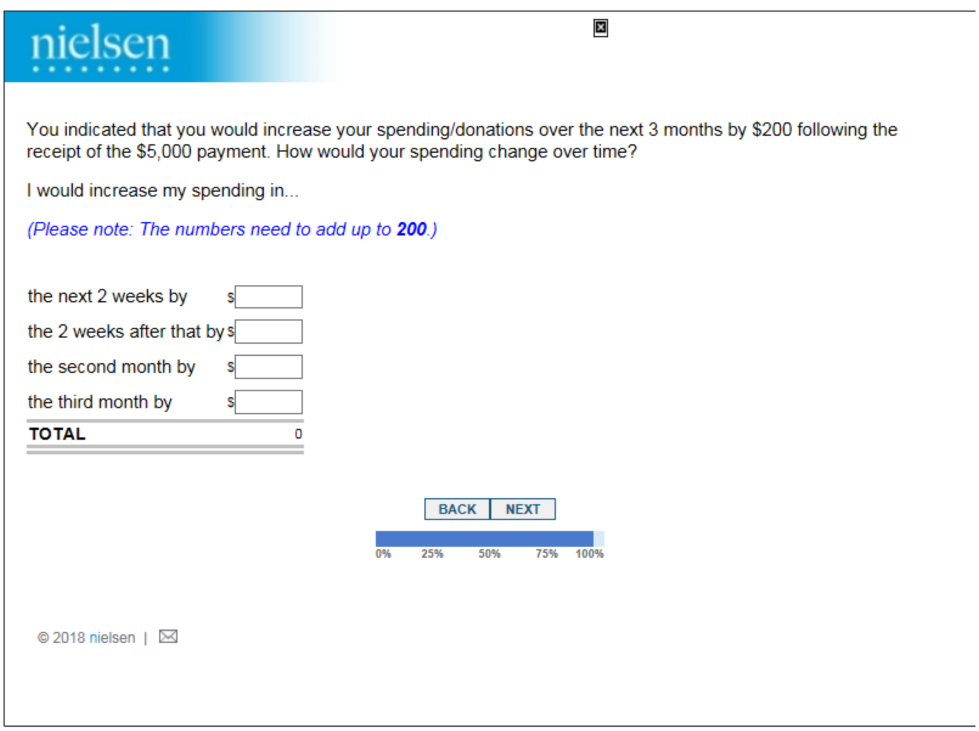
(c) Screen 2, Part 2 (spending composition follow-up – pops up after part 1 answered)



nielsen

You indicated that you would increase your spending/donations over the next 3 months by $200 following the receipt of the $5,000 payment. How would your spending change over time?

I would increase my spending in...

*(Please note: The numbers need to add up to **200**.)*

the next 2 weeks by	$
the 2 weeks after that by	$
the second month by	$
the third month by	$
TOTAL	0

BACK NEXT

0% 25% 50% 75% 100%

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(d) Screen 3 (spending timing follow-up — only for some treatments, and only for increases)

A.2 Respondent Characteristics

Here, we provide the questions on which the cuts in Tables A-6 and A-7 below are based.

Age: the age of the respondent.

Bachelor's degree+: equals one if the respondent indicates that their highest completed education is a bachelor's degree, master's degree, doctoral degree, or professional degree.

Income: based on the following question:

> Which category represents the total combined pre-tax income of all members of your household (including you) during the past 12 months?
>
> Please include money from all jobs, net income from business, farm or rent, pensions, interest on savings or bonds, dividends, social security income, unemployment benefits, Food Stamps, workers compensation or disability benefits, child support, alimony, scholarships, fellowships, grants, inheritances and gifts, and any other money income received by members of your household who are 15 years of age or older.

[Respondents select from: Less than $10,000 / $10,000 to $19,999 / $20,000 to $29,999 / $30,000 to $39,999 / $40,000 to $49,999 / $50,000 to $59,999 / $60,000 to $74,999 /$75,000 to $99,999 / $100,000 to $149,999 / $150,000 to $199,999 / $200,000 or more]

Liquid Assets: based on the following two-part question:

> Which of the following do you or anyone in your family living with you have any money invested in?
>
> Please do NOT include any investments in retirement accounts (401k, 403b, 457, IRA, thrift savings plans etc.) or employer-sponsored pensions.

[Respondents select (possibly several) from: Checking accounts or cash / Savings accounts / Money market funds / CDs (Certificates of Deposit) / Government/Municipal Bonds or Treasury Bills / Stocks or bonds in publicly held corporations, stock or bond mutual funds, or investment trusts / None of the above]

[If did not select "None of the above":]

> If you added up all the money in these categories that you and your family members living with you have invested in, how much would it total?

[Respondents select from: Less than $500 / $500 to $999 / $1,000 to $1,999 / $2,000 to $4,999 / $5,000 to $9,999 / $10,000 to $19,999 / $20,000 to $29,999 / $30,000 to $49,999 / $50,000 to $99,999 / $100,000 to $249,999 / $250,000 to $499,999 / $500,000 to $749,999 / $750,000 to $999,999 / $1,000,000 or more]

Credit score: based on the question:

"What would you say is your credit score? [Respondents select from: Below 620 / 620-679 / 680-719 / 720-760 / above 760 / Don't know]

"Below Median" includes respondents in the first three categories (and we do not include in this cut the ones that answered "Don't know").

(Do not) have 2 months of funds: based on the response to the yes/no question (asked in February 2016) "In case of an unexpected decline in income or increase in expenses, do you [or your spouse/partner] have at least two months of covered expenses available in cash, bank accounts, or easily accessible funds?"

Tend to spend now / in future: based on the response to the question (asked in February 2016) "In general, are you the sort of person who would rather spend your money and enjoy it today or save more for the future? Where would you place yourself on the scale below?" [Respondents are asked to select on a scale from 1 to 5, where 1 is "Spend now" and 5 is "Save for the future". We classify as "Tend to spend now" those that select values 1-3.]

Time discounting questions: Respondents that were in the survey in February 2016 were asked the following:

> In this final part of the survey, 5 respondents will be randomly chosen to win a significant amount of money, as explained below.
>
> In completing this final part of the survey, you have to decide whether you want a smaller amount of money sooner, or a larger amount of money later. Specifically, in each of the following rows, please choose the option you prefer. For example, in row 1, you have a choice between $150 today versus $160 in a month from today.
>
> If you are selected as a winner, one of these rows will be randomly picked and you will receive the money on the date indicated. (Note that "today" means within one business day.)

> You will know immediately after the end of this survey whether you were chosen as a winner or not. If you are chosen as one of the winners, you would also be informed about the row for which you will be paid.

They were then asked to make choices across three blocks of question (following Meier and Sprenger, 2010):

1. Money TODAY or IN A MONTH: they were presented with 6 rows, where in each row they chose between "$X guaranteed today" or "$160 guaranteed in a month", where X took the following values: 150, 140, 130, 120, 100, 80.

2. Money TODAY or IN 6 MONTHS: they were presented with 7 rows, where in each row they chose between "$X guaranteed today" or "$160 guaranteed in 6 months", where X took the following values: 150, 140, 130, 120, 100, 80, 60.

3. Money IN 6 MONTHS or IN 7 MONTHS: they were presented with 6 rows, where in each row they chose between "$X guaranteed in 6 months" or "$160 guaranteed in 7 months", where X took the following values: 150, 140, 130, 120, 100, 80.

We define a respondent to have a "Low 1-month Discount Factor" if in the first block, they prefer any of the smaller amounts today to the $160 in a month.

We define a respondent to exhibit "Inconsistent Time Discounting" if their implied discount factor in the first block is lower than in the third block (e.g. they prefer $150 today over $160 in a month, but prefer $160 in 7 months over $150 in 6 months).

B Further Analyses

B.1 Testing for Selection in Initial Participation and Attrition

In Table A-1, we examine possible selection in terms of who participates in the survey modules that feature our treatments the first time they are invited to do so, and also in terms of who remains in the panel from one survey to the next (while still eligible and invited for participation). As noted in the main text, Binder (2019) has documented selection in retention for the SCE overall (i.e., the regular monthly module).

The first two columns show that older and college-educated individuals are more likely to participate in the initial survey, and older individuals are more likely to be retained.[44] We find no evidence for differential participation or retention probabilities by income or homeownership (a proxy for wealth). Importantly, for our purposes, the last two columns show that retention is not related to respondents' reported MPC in the initial survey or to the scenarios they were assigned to. For example, column (4) shows that we cannot reject the null that the scenarios an individual was assigned to have no impact on retention (as indicated by a p-value of 0.385 for a joint F-test). The low R2 of 0.01 shows that selection on observables explains hardly any of the variation in retention in the sample, suggestive of attrition being largely due to idiosyncratic factors. It is also worth noting that our first-time response rate of 80% and retention rate (from one survey wave to the next) of 89% is strikingly high, for any online survey.

B.2 Composition and Timing of Spending

As noted in Section 3.2 of the main text, we asked respondents who indicated that they would adjust their spending in response to the treatment about how much of that change in spending would come from different spending categories.[45] The exact wording of seven possible spending categories can be found in Appendix A. We group the categories into non-durable spending ("traveling/vacation/eating out/other leisure activities"; "donations/gifts"; "general living expenses"), durables ("purchase of durables typically costing $1,000 or less"; "...typically costing more than $1,000"; "renovations or improvements to my home'; "pay for college/education/training

[44]These regressions are unweighted, since the purpose is to analyze which characteristics may be correlated with participation/retention.

[45]Spending composition was asked in all waves except March 2017.

Table A-1: Regression analysis of selective participation and sample attrition

	(1) First Participation	(2) Retention	(3) Retention	(4) Retention
Positive MPC in prev. survey			-0.013	-0.008
			(0.014)	(0.014)
Age over 60	0.056***	0.058***		0.058***
	(0.017)	(0.015)		(0.015)
Age under 40	-0.016	-0.020		-0.020
	(0.019)	(0.020)		(0.020)
HH income 30-60k	0.007	0.007		0.008
	(0.022)	(0.021)		(0.021)
HH income 60-100k	0.019	0.015		0.015
	(0.023)	(0.021)		(0.021)
HH income over 100k	-0.000	-0.003		-0.004
	(0.024)	(0.023)		(0.023)
Some College/AD	0.005	-0.023		-0.024
	(0.026)	(0.024)		(0.024)
College Grad	0.068***	0.005		0.003
	(0.025)	(0.024)		(0.024)
Homeowner	-0.003	0.004		0.004
	(0.018)	(0.018)		(0.018)
White	0.024	-0.006		-0.007
	(0.020)	(0.019)		(0.019)
Male	0.019	0.025*		0.026*
	(0.015)	(0.014)		(0.014)
Constant	0.747***	0.868***	0.822***	0.793***
	(0.033)	(0.030)	(0.049)	(0.057)
Date FEs?	Yes	Yes	Yes	Yes
Scenario FEs?	No	No	Yes	Yes
p-val. F-test Scenario FEs = 0			0.441	0.385
Avg. Y	0.80	0.89	0.89	0.89
Adj. R2	0.02	0.01	0.00	0.01
Obs.	3107	2194	2194	2194

Robust standard errors clustered by respondent in parentheses.

Table reports regressions of whether an invited SCE panelist participated in a first MPC survey (column 1) or whether a participant from a first survey participated in a subsequent MPC survey they were invited for (columns 2-4) on panelist characteristics, past MPC responses, and fixed effects for dates and the MPC scenarios seen in previous surveys. Robust standard errors clustered by respondent in parentheses. Significance: * < 0.1, ** < 0.05, *** < 0.01.

Table A-2: Average Spending Shares by Category

	Population-Weighted			Population&MPC-Weighted			
	Nondurables	Durables	Other	Nondurables	Durables	Other	N
Gain							
\$500	0.72 (0.02)	0.23 (0.02)	0.05 (0.01)	0.65 (0.02)	0.28 (0.02)	0.06 (0.01)	410
\$2500	0.61 (0.03)	0.34 (0.03)	0.05 (0.01)	0.54 (0.03)	0.39 (0.03)	0.07 (0.02)	179
\$5000	0.60 (0.01)	0.35 (0.01)	0.05 (0.01)	0.53 (0.01)	0.40 (0.01)	0.06 (0.01)	830
Loss							
\$500	0.79 (0.02)	0.19 (0.02)	0.02 (0.01)	0.78 (0.02)	0.21 (0.02)	0.01 (0.01)	195
News-Gain							
\$500 in 3 months	0.53 (0.05)	0.40 (0.05)	0.07 (0.03)	0.50 (0.05)	0.43 (0.05)	0.07 (0.03)	49
\$5000 in 3 months	0.64 (0.03)	0.30 (0.03)	0.06 (0.02)	0.53 (0.03)	0.41 (0.03)	0.06 (0.02)	111
Loan							
\$5000	0.63 (0.03)	0.35 (0.03)	0.03 (0.01)	0.61 (0.03)	0.38 (0.03)	0.01 (0.01)	108

Nondurables and Durables definitions provided in text. N is the number of respondents with non-zero MPC for which the spending shares were elicited. In the first three columns, shares are weighted by the each respondent's population weights. In the second three columns, the weights are given by the product of a respondent's population weight and the absolute value of their MPC. Standard errors in parentheses.

for members of my household"), and "other". Table A-2 shows average shares of spending responses for individuals with non-zero MPCs, in each of the three categories. In all cases, most of the adjustment in spending in response to the treatment comes from non-durables.

In the May 2016 and January 2017 survey waves, we further asked respondents who indicated they would increase their spending about the timing of spending within the following three month period. In May 2016, this was asked for all three treatments (\$5000 GAIN, \$2500 GAIN, \$5000 LOAN) while in January 2017 it was asked for the \$5000 GAIN treatment. The average shares of the spending increase happening in different time intervals are shown in Table A-3 (pooling the two \$5000 GAIN waves). More than half of the increased spending (for those with MPC>0) occurs in the first month.

B.3 Within-Respondent MPC Changes

In Table A-4, we study correlates of changes (or absolute changes) in MPCs reported by a respondent in the same scenario across two waves. As noted in Section 4.6, the total number of repeat responses is 452. Of those, 280 come from the \$5000 gain scenario in March 2016 and May 2016, 131 from the \$5000 gain scenario in May 2016 and January 2017, and 41 from the \$500 gain scenario in March 2016 and January 2017.

Since we observe certain characteristics of the respondents repeatedly in each monthly SCE, we can examine whether changes in individual observable characteristics are correlated with within-respondent changes in MPCs across waves. However,

Table A-3: Distribution of Timing

$5000 Gain	Mean	Median	25th Percentile	75th Percentile
Next Two Weeks	37.8	25.0	10.0	50.0
Next Two-Four Weeks	20.0	20.0	0.0	30.0
Second Month From Now	22.5	20.0	0.0	33.3
Third Month From Now	19.8	10.0	0.0	30.0
$2500 Gain	Mean	Median	25th Percentile	75th Percentile
Next Two Weeks	46.7	40.0	20.0	80.0
Next Two-Four Weeks	20.9	20.0	0.0	25.0
Second Month From Now	18.8	13.3	0.0	25.0
Third Month From Now	13.6	0.0	0.0	22.2
$5000 Loan	Mean	Median	25th Percentile	75th Percentile
Next Two Weeks	38.2	30.0	20.0	60.0
Next Two-Four Weeks	21.9	20.0	10.0	25.0
Second Month From Now	18.8	20.0	10.0	25.7
Third Month From Now	21.1	12.5	4.0	33.3

Table reports (population-weighted) average and quartiles of spending shares (in %) over different time intervals, for the three scenarios where the timing distribution was elicited.

given the rather small sample size, for many such observables there simply is not enough variation. For instance, we observe whether a respondent is employed full-time or not, but only in 18 cases do we observe changes across two waves in which a respondent answers the same question. We do get some more variation when looking at household income changes (income is elicited in each wave based on the question in Appendix A.2): in 73 (66) cases, a respondent reports a higher (lower) household income in the second wave where a scenario is asked than in the first wave.

Columns (1) and (2) of Table A-4 show that income increases between the two waves are associated with an increase in the MPC in the second wave, relative to the first wave (while for those with an income decrease we observe no difference relative to the omitted category with no change). At first glance, this positive relationship between income increases and MPC is surprising, although it could of course reflect various omitted variables: for instance, perhaps a respondent is working longer hours in order to be able to meet a specific expense, and that is also why they have a higher MPC. Thus, we do not want to attach too strong of an interpretation to the direction of the correlation. However, the fact that MPCs do respond to some extent to a change in circumstances suggests that variation from one wave to the next is not just noise. We also see strong evidence for mean reversion in MPCs in column (2): the MPC in the first wave is strongly negatively correlated with the change across

Table A-4: Within-respondent MPC Changes

	(1) ΔMPC	(2) ΔMPC	(3) $\lvert\Delta MPC\rvert$	(4) $\lvert\Delta MPC\rvert$	(5) $\lvert\Delta MPC\rvert$	(6) $\lvert\Delta MPC\rvert$
MPC survey 1		-0.556***		0.415***		0.413***
		(0.068)		(0.056)		(0.057)
Income Increase (0/1)	0.135***	0.142***				
	(0.041)	(0.038)				
Income Decrease (0/1)	0.014	0.014				
	(0.042)	(0.036)				
Some College/AD (0/1)			0.011	-0.017		
			(0.047)	(0.041)		
College Grad (0/1)			0.006	-0.019		
			(0.044)	(0.039)		
Numeracy Score (0-5)					0.015	0.002
					(0.010)	(0.009)
Constant	-0.047*	0.030	0.158***	0.157***	0.077*	0.069*
	(0.025)	(0.021)	(0.056)	(0.052)	(0.046)	(0.040)
Treatment FEs?	Yes	Yes	Yes	Yes	Yes	Yes
Avg. Y	-0.01	-0.01	0.16	0.16	0.16	0.16
Adj. R2	0.02	0.27	-0.00	0.20	0.00	0.20
Obs.	452	452	452	452	452	452

Robust standard errors clustered by respondent in parentheses.
Significance: * < 0.1, ** < 0.05, *** < 0.01.

the two waves.

In columns (3) to (6), we instead use within-person absolute changes in MPCs as the dependent variable, and study the relationship with respondent education and numeracy. These variables are proxies of respondent cognitive ability; numeracy is also a measure of how seriously a given respondent takes the survey. A measurement error interpretation of the results would predict negative relationship between absolute changes and education/numeracy. However, we do not find evidence of this, suggesting that the changes we observe are not entirely due to measurement error.

B.4 Order Effects

In Table A-5, we present a regression-based test for whether the order in which a given treatment was seen by a respondent (first or second within a given survey) affects the provided MPCs. We regress indicators for whether a respondent indicated they would change their spending (column 1), increase their spending (column 2), or the MPC itself (column 3) in a given treatment on treatment-date fixed effects and an indicator for whether the respondent was exposed to this treatment first. Column (1) shows some (weak) evidence that seeing a treatment first makes the respondent slightly more likely to report that they will change their spending. However, column (2) shows that there is no evidence that seeing a treatment first makes it more likely

Table A-5: Testing for Order Effects

	(1) MPC $\neq 0$	(2) MPC > 0	(3) MPC
First Treatment Seen	0.019*	0.012	0.002
	(0.011)	(0.010)	(0.006)
Treatment X Date FEs?	Yes	Yes	Yes
Avg. Y	0.37	0.30	0.14
Adj. R2	0.07	0.11	0.12
Obs.	8467	8467	8467

Observations weighted by population weights.

Robust standard errors clustered by respondent in parentheses.

Significance: * < 0.1, ** < 0.05, *** < 0.01.

that the respondent states they will increase their spending, and column (3) shows that the average MPC is also not (economically or statistically) significantly affected by the treatment order.

Figure A-1 shows graphically how the shares of responses with zero and positive MPCs vary for each treatment with the question order. Only in one treatment (GAIN \$2500) is the share of zero responses significantly higher at $p<0.1$ when the treatment is seen first.

B.5 Consistency of Responses with Budget Constraint

Our survey instrument elicited changes in each of the three categories: spending, saving, and paying off debt (the focus in this paper is on changes in the first category only). The survey did not force respondents to give answers that satisfied the current period budget constraint (for example, for the \$500 gain treatment, the responses for spending, saving, and paying off debt should sum to \$500 for a given respondent; likewise, in the news treatments, the responses to the three categories for the current period—before the money is received—should sum to zero). In all of our treatments, we find that the responses to the three categories do not sum to the current period budget constraint for the majority of the respondents. For example, in the \$500 gain treatment, responses summed up to exactly \$500 for only 28 percent of the respondents.

There is some evidence that this may be related to the attention/care with which respondents take the survey. Amongst respondents who answer all 5 numeracy questions correctly (and hence either have high cognitive ability and/or are the types of respondents who are more likely to take the survey seriously), responses satisfy the budget constraint for 42 percent of the cases. This proportion is 20 percent among

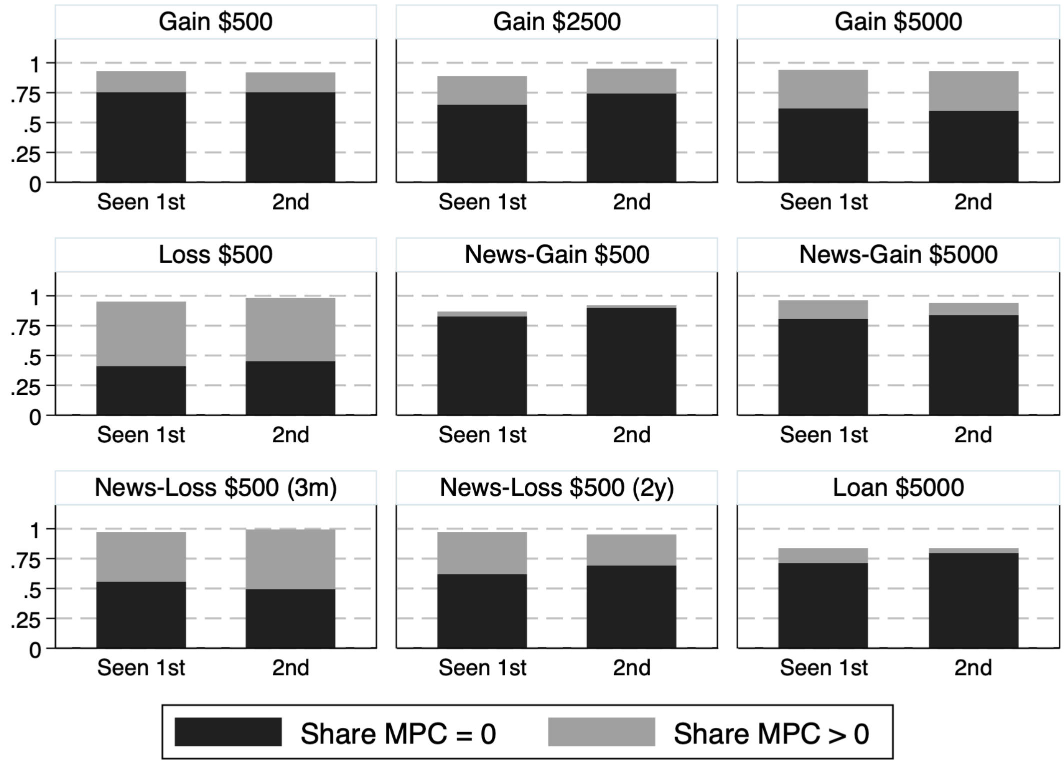


Figure A-1: Share of Responses with Zero and Positive MPC in Each Treatment, by Question Order

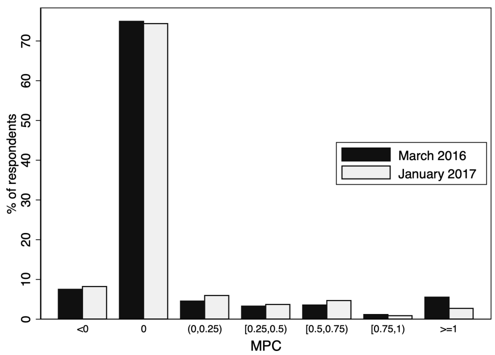
respondents who answer one or two of the 5 questions correctly. However, even amongst the most numerate individuals, answers are not consistent with the budget constraint for the majority of the cases. There is good reason for why this might have been the case. Individuals are asked for the amounts by which they would change their spending, saving, and paying off debt on separate screens. In addition, they are not reminded of their responses to the sub-categories.

One further concrete hypothesis is that in the majority of the cases where responses do not satisfy the constraint, the respondent would "not be doing anything" with the remaining money, which is effectively saving it (in the case of a positive shock). In other words, respondents may think of "saving" as an active act (e.g., moving money to a dedicated savings account), not realizing that "not spending" is equivalent to "saving" in an economic sense. If that is the case, it would understate the propensity to save that we measure (but that we do not focus on), while the MPC numbers would be largely unaffected. In order to test this, we fielded a short module in the September 2019 SCE. The module was fielded to 1,139 individuals. More precisely, we fielded the $500 gain treatment. The only difference from our main survey instrument was that respondents whose answers (for spending, saving, and paying off debt) summed to less than $500 were then asked "*Note that your changes only sum to $Y. However, since you received $500, the combined changes above should be $500. Your current answers imply that you will not be doing anything with the remaining $(500 – Y). This effectively means you would be saving the remaining $(500 – Y), for instance by keeping that money as cash. Is that what you would want to do? [Yes or No]*". Those who chose "No" were then asked to adjust their answers to the amounts they would spend, save, and pay off debt such that they summed to $500.

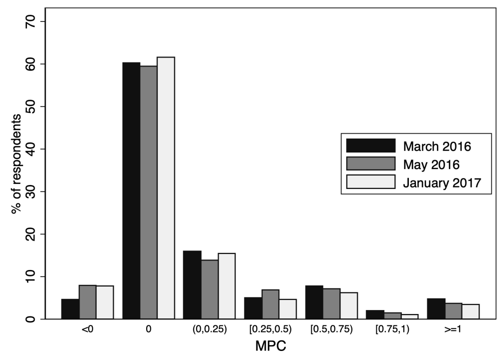
We find that as in the main study, answers did not sum to $500 for the majority of the respondents. Only 37.8% of the sample gave responses that summed to $500. As in the main study, this proportion was higher for high-numeracy individuals (42.9% versus 24.6% for low-numeracy individuals). Importantly for us, of those whose answers did not sum to $500 and were asked "*This effectively means you would be saving the remaining $(500 – Y), for instance by keeping that money as cash. Is that what you would want to do?*", 90.1% answered Yes. That is, for these individuals, the amount they expect to spend (which is the MPC) was unchanged. This suggests that the idea that the majority of individuals whose answers were not summing up to the budget constraint were planning to save the rest was correct. Including these individuals as also giving "consistent" responses increases the share of individuals with consistent answers to 84%. This still leaves some measurement error in our MPC estimates. In the September 2019 module, we initially get an average MPC of 6.8% (close to the 7% estimate for the $500 gain in the paper). Forcing people to give responses that sum to $500 changes this number slightly to 8.1%. In sum, our qualitative results on MPCs should not be affected by the not-summing-up issue, since the vast majority of respondents expect to save the "missing" amount.

B.6 Additional Tables and Figures

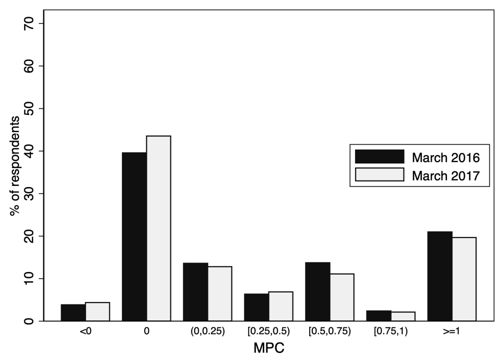
The following tables and figures are referred to in footnotes of the main text.



(a) $500 Gain



(b) $5,000 Gain



(c) $500 Loss

Figure A-2: Stability of responses across survey waves

Table A-6: MPC Statistics (Average MPC)

	Gain			Loss	News-Gain		News-Loss		Loan
	$500 Gain	$2500 Gain	$5000 Gain	$500 Loss	$500 Gain in 3 months	$5000 Gain in 3 months	$500 Loss in 3 months	$500 Loss in 2 years	$5000 Loan
Full Sample	0.08 [1679]	0.11 [540]	0.14 [2040]	0.30 [1536]	-0.00 [362]	0.04 [594]	0.27 [586]	0.14 [589]	0.01 [541]
Age <=50	0.08 [829]	0.10 [265]	0.13 [977]	0.33 [741]	-0.00 [179]	0.05 [266]	0.28 [293]	0.17 [279]	0.01 [263]
Age > 50	0.08 [850]	0.13 [275]	0.15 [1063]	0.27 [795]	-0.01 [183]	0.04 [328]	0.26 [293]	0.12 [310]	0.02 [278]
P-value	0.72	0.14	0.06	0.00	0.76	0.25	0.59	0.03	0.12
< Bachelor's Degree	0.07 [717]	0.09 [224]	0.12 [885]	0.34 [704]	-0.02 [148]	0.03 [278]	0.30 [259]	0.17 [271]	0.01 [238]
Bachelor's Degree+	0.08 [961]	0.13 [316]	0.15 [1155]	0.27 [831]	0.01 [214]	0.05 [316]	0.24 [326]	0.12 [318]	0.02 [303]
P-value	0.39	0.04	0.01	0.00	0.09	0.08	0.08	0.07	0.42
Inc <=$75k	0.08 [946]	0.12 [294]	0.14 [1137]	0.34 [850]	-0.01 [213]	0.04 [334]	0.31 [314]	0.16 [333]	0.01 [320]
Inc > $75k	0.08 [712]	0.10 [241]	0.15 [880]	0.25 [669]	0.00 [147]	0.04 [257]	0.22 [266]	0.13 [250]	0.01 [213]
P-value	0.79	0.27	0.42	0.00	0.48	0.74	0.00	0.26	0.79
Liquid Assets <= $20k	0.08 [690]	0.12 [203]	0.14 [798]	0.37 [605]	0.01 [162]	0.05 [232]	0.33 [229]	0.16 [229]	0.02 [212]
Liquid Assets > $20k	0.09 [679]	0.11 [215]	0.14 [823]	0.23 [621]	-0.00 [142]	0.04 [244]	0.22 [249]	0.13 [229]	0.01 [217]
P-value	0.72	0.55	0.94	0.00	0.55	0.29	0.00	0.22	0.43
Below Median Credit Score	0.07 [691]	0.12 [218]	0.14 [834]	0.35 [611]	-0.01 [169]	0.05 [250]	0.30 [231]	0.17 [229]	0.01 [219]
Above Median Credit Score	0.10 [589]	0.12 [172]	0.15 [683]	0.24 [542]	0.01 [112]	0.04 [193]	0.23 [217]	0.11 [203]	0.01 [188]
P-value	0.13	0.82	0.51	0.00	0.48	0.53	0.05	0.06	0.58
Do not have 2 months of funds	0.08 [250]	0.13 [103]	0.14 [278]	0.47 [83]	0.00 [84]				0.01 [91]
Have 2 months of funds	0.10 [655]	0.12 [237]	0.15 [718]	0.26 [213]	-0.00 [224]				0.01 [261]
P-value	0.48	0.72	0.33	0.00	0.70				0.90
Tend to Spend Now	0.11 [582]	0.14 [214]	0.17 [615]	0.33 [195]	-0.00 [197]				0.01 [210]
Tend to Save for Future	0.07 [322]	0.08 [125]	0.12 [380]	0.29 [99]	0.00 [112]				0.02 [142]
P-value	0.07	0.04	0.01	0.37	0.62				0.59
Low 1-month Discount Factor	0.09 [362]	0.11 [214]	0.18 [526]	0.34 [124]	-0.01 [125]				0.02 [198]
High 1-month Discount Factor	0.09 [522]	0.13 [198]	0.13 [609]	0.30 [165]	0.01 [176]				0.00 [228]
P-value	0.86	0.58	0.00	0.47	0.21				0.08
Consistent Time Discounting	0.09 [714]	0.12 [265]	0.14 [790]	0.31 [236]	0.00 [242]				0.01 [287]
Inconsistent Time Discounting	0.09 [168]	0.14 [66]	0.20 [181]	0.34 [53]	-0.00 [58]				0.04 [57]
P-value	0.81	0.61	0.00	0.66	0.86				0.04

Note: Positive MPC corresponds to a negative change in spending for the loss treatments. Statistics are unweighted. Sample size reported in square brackets.

Table A-7: MPC Statistics (Proportion with MPC>0, in %)

	Gain			Loss	News-Gain		News-Loss		Loan
	$500 Gain	$2500 Gain	$5000 Gain	$500 Loss	$500 Gain in 3 months	$5000 Gain in 3 months	$500 Loss in 3 months	$500 Loss in 2 years	$5000 Loan
Full Sample	18.64 [1679]	27.41 [540]	36.23 [2040]	49.15 [1536]	5.52 [362]	14.48 [594]	43.17 [586]	28.52 [589]	8.13 [541]
Age <=50	19.30 [829]	27.17 [265]	36.13 [977]	53.58 [741]	7.26 [179]	15.79 [266]	46.42 [293]	29.75 [279]	6.84 [263]
Age > 50	18.00 [850]	27.64 [275]	36.31 [1063]	45.03 [795]	3.83 [183]	13.41 [328]	39.93 [293]	27.42 [310]	9.35 [278]
P-value	0.49	0.90	0.93	0.00	0.15	0.41	0.11	0.53	0.29
< Bachelor's Degree	18.13 [717]	23.21 [224]	30.73 [885]	56.11 [704]	4.05 [148]	12.95 [278]	49.42 [259]	36.16 [271]	7.56 [238]
Bachelor's Degree+	19.04 [961]	30.38 [316]	40.43 [1155]	43.20 [831]	6.54 [214]	15.82 [316]	38.04 [326]	22.01 [318]	8.58 [303]
P-value	0.64	0.07	0.00	0.00	0.31	0.32	0.01	0.00	0.67
Inc <=$75k	19.98 [946]	29.25 [294]	36.15 [1137]	59.06 [850]	5.63 [213]	15.87 [334]	52.87 [314]	35.44 [333]	8.75 [320]
Inc > $75k	17.13 [712]	25.31 [241]	36.36 [880]	36.77 [669]	5.44 [147]	12.84 [257]	31.95 [266]	19.60 [250]	7.04 [213]
P-value	0.14	0.31	0.92	0.00	0.94	0.30	0.00	0.00	0.48
Liquid Assets <= $20k	21.74 [690]	29.06 [203]	37.84 [798]	63.80 [605]	8.64 [162]	16.81 [232]	56.77 [229]	35.81 [229]	10.85 [212]
Liquid Assets > $20k	15.76 [679]	26.05 [215]	35.24 [823]	35.27 [621]	2.82 [142]	13.52 [244]	32.93 [249]	22.71 [229]	5.53 [217]
P-value	0.00	0.49	0.28	0.00	0.03	0.32	0.00	0.00	0.04
Below Median Credit Score	19.54 [691]	31.19 [218]	37.89 [834]	58.76 [611]	6.51 [169]	15.60 [250]	53.25 [231]	34.50 [229]	7.31 [219]
Above Median Credit Score	17.83 [589]	26.16 [172]	35.14 [683]	38.38 [542]	5.36 [112]	13.99 [193]	34.10 [217]	21.18 [203]	7.45 [188]
P-value	0.44	0.28	0.27	0.00	0.69	0.64	0.00	0.00	0.96
Do not have 2 months of funds	20.00 [250]	33.01 [103]	39.21 [278]	74.70 [83]	9.52 [84]				10.99 [91]
Have 2 months of funds	18.17 [655]	25.74 [237]	37.05 [718]	40.85 [213]	4.46 [224]				6.90 [261]
P-value	0.53	0.17	0.53	0.00	0.09				0.22
Tend to Spend Now	20.27 [582]	30.84 [214]	41.14 [615]	51.28 [195]	5.58 [197]				8.10 [210]
Tend to Save for Future	15.53 [322]	23.20 [125]	32.11 [380]	49.49 [99]	6.25 [112]				7.75 [142]
P-value	0.08	0.13	0.00	0.77	0.81				0.91
Low 1-month Discount Factor	18.51 [362]	25.23 [214]	41.63 [526]	53.23 [124]	4.00 [125]				12.12 [198]
High 1-month Discount Factor	18.39 [522]	30.81 [198]	34.48 [609]	48.48 [165]	7.39 [176]				4.39 [228]
P-value	0.96	0.21	0.01	0.43	0.22				0.00
Consistent Time Discounting	18.07 [714]	27.92 [265]	36.08 [790]	49.15 [236]	6.61 [242]				6.27 [287]
Inconsistent Time Discounting	20.24 [168]	28.79 [66]	44.20 [181]	56.60 [53]	3.45 [58]				15.79 [57]
P-value	0.51	0.89	0.04	0.33	0.36				0.01

Note: Positive MPC corresponds to a negative change in spending for the loss treatments.
Statistics are unweighted. Sample size reported in square brackets.

Table A-8: MPC Differences $500 Loss vs. Gain, by Respondent Wealth

Dep. var.: MPC	
Loss X Liq. Wealth 0 to 5k	0.295***
	(0.039)
Loss X Liq. Wealth 5k to 30k	0.225***
	(0.044)
Loss X Liq. Wealth 30k to 250k	0.184***
	(0.043)
Loss X Liq. Wealth over 250k	0.024
	(0.045)
Respondent FEs?	Yes
Order X Date FEs?	Yes
Adj. R2	0.13
Obs.	1372

Robust standard errors clustered by respondent in parentheses.
Significance: * < 0.1, ** < 0.05, *** < 0.01.

Table A-9: Summary statistics, only keeping respondents with numeracy score ≥ 3, and MPCs in [0,1]

		MPC	Share of Respondents with MPC			MPC | MPC > 0	
	Count	Mean (SE)	< 0	$=0$	> 0	Mean (SE)	Median
Gain							
$500	1396	0.11 (0.007)	0.00	0.80	0.20	0.53 (0.007)	0.50
$2500	463	0.12 (0.012)	0.00	0.74	0.26	0.45 (0.012)	0.40
$5000	1383	0.15 (0.007)	0.00	0.58	0.42	0.37 (0.007)	0.30
Loss							
$500	1317	0.35 (0.011)	0.00	0.46	0.54	0.64 (0.011)	0.60
News-Gain							
$500 in 3 months	301	0.02 (0.006)	0.00	0.95	0.05	0.43 (0.006)	0.50
$5000 in 3 months	505	0.04 (0.006)	0.00	0.87	0.13	0.31 (0.006)	0.40
News-Loss							
$500 in 3 months	516	0.30 (0.017)	0.00	0.53	0.47	0.64 (0.017)	0.60
$500 in 2 years	503	0.15 (0.014)	0.00	0.72	0.28	0.55 (0.014)	0.50
Loan							
$5000	428	0.04 (0.006)	0.00	0.89	0.11	0.34 (0.006)	0.46

Note: Weighted statistics reported. Positive MPC corresponds to a negative change in spending for the loss treatments.

C Further Details on Model

C.1 Details of Income Process Estimation

We estimate the following process for *quarterly* log labor income for household i in period t, $\log y_{i,t}$

$$\begin{aligned} \log y_{it} &= z_{it} + \varepsilon_{it} \\ z_{it} &= \rho z_{it-1} + \eta_{it} \end{aligned}$$

where ε_{it} and η_{it} are persistent and transitory shocks that are drawn from the following distributions independently across households and over time

$$\eta_{it} \sim \begin{cases} 0 & \text{wp } 1-\lambda_\eta \\ N\left(0,\sigma_\eta^2\right) & \text{wp } \lambda_\eta \end{cases} \qquad \varepsilon_{it} \sim \begin{cases} 0 & \text{wp } 1-\lambda_\varepsilon \\ N\left(0,\sigma_\varepsilon^2\right) & \text{wp } \lambda_\varepsilon \end{cases}$$

We estimate the model by matching moments of log *annual* household labor income. When matching moments, we simulate the quarterly earnings model for each candidate set of parameters, and then construct annual labor income by aggregating the quarterly realizations.

The moments that we match are as follows. Define annual income growth at lag d as

$$\Delta_d y_{it} \coloneqq \begin{cases} y_{i,t} & \text{if } d=0 \\ y_{i,t+d} - y_{i,t} & \text{if } d>0 \end{cases}$$

and the corresponding cross-sectional moments as

$$m_{d,j} = E\left[(\Delta_d y)^j\right]$$

for $j = 2, 4$ and $d = 1, 2, \ldots D$. For $j = 2$, these moments are equivalent to the variance of log annual income and the auto-covariance function of log annual income (either in levels or first-differences). Thus, when using only moments with $j = 2$, this approach is the same as in a large literature that estimates income process by matching auto-covariance functions. However, using only moments with $j = 2$, the arrival rates $(\lambda_\eta, \lambda_\varepsilon)$ are not identified. For the calibration used in our model, we simply set both parameters equal to 0.25, which implies that earnings shocks arrive on average once per year. We do this in order to remain as close as possible to the annual models used in most of the existing literature, in which shocks are restricted to arrive exactly once a year. For completeness, we also report results assuming $\lambda_\varepsilon = \lambda_\eta = 1$, which is the assumption that some authors have imposed when estimating quarterly models on annual data.

However, it is also possible to estimate $(\lambda_\eta, \lambda_\varepsilon)$ by including higher order moments of earnings changes at different lags. Including the moments with $j = 4$ is equivalent to matching the kurtosis of log annual income growth at different horizons, which identifies the arrival rates of the shocks. Comparing the implied $j = 4$ moments from the models that fix the arrival rates with the corresponding moments in the data, is informative about the fit of those models. Table A-10 reports the moments we match in the PSID data, the fit of the model and the estimated parameters for each specification.

Lag	Data	Model $\lambda_\eta = \lambda_\varepsilon = 0.25$	Model $\lambda_\eta = \lambda_\varepsilon = 1$	Model Estimate $(\lambda_\eta, \lambda_\varepsilon)$
Moments: Variance (equivalent to $j = 2$)				
0	0.505	0.505	0.505	0.505
1	0.142	0.142	0.142	0.143
5	0.295	0.295	0.295	0.317
Moments: Kurtosis (equivalent to $j = 4$)				
1	10.90	4.49	3.01	11.46
5	6.39	3.50	3.00	4.95
Parameter Estimates				
ρ		0.988	0.988	0.986
σ_η^2		0.044	0.011	0.050
λ_η		0.25	1	0.248
σ_ε^2		0.638	0.209	1.64
λ_ε		0.25	1	0.071

Table A-10: Estimated parameters from earnings processes

C.2 Wealth Distributions

Table A-11 reports additional statistics on the wealth distributions in the alternative versions of the model discussed in the main text.

Table A-11: Adjustment Costs and Wealth Statistics in Models

	With Cons. Adjustment Costs			Without Cons. Adjustment Costs		
	Het β	Common β	Common β	Het β	Common β	Common β
	Baseline	Wealth = 3.2	Frac< \$1000		Wealth = 3.2	Frac< \$1000
Mean β (annual)	0.877	0.984	0.867	0.877	0.984	0.865
Consumption Adjustment						
Adjustment cost	4.0e-4	1.3e-5	5.8e-4	0.000	0.000	0.000
Frac with cons change	0.540	0.650	0.627	1.000	1.000	0.991
Earnings Statistics						
Mean annual earnings	1.000	1.000	1.000	1.000	1.000	1.000
Stdev log annual earnings	0.713	0.713	0.713	0.713	0.713	0.713
Wealth Statistics						
Mean wealth	3.191	3.200	0.168	3.197	3.200	0.167
Wealth <= 0	0.057	0.005	0.068	0.057	0.005	0.068
Wealth <= 0.005	0.132	0.009	0.113	0.135	0.009	0.112
Wealth <= 0.01	0.175	0.015	0.168	0.179	0.015	0.166
Wealth <= 0.02	0.252	0.034	0.259	0.251	0.034	0.264
Wealth <= 0.05	0.384	0.090	0.474	0.386	0.090	0.482
Wealth <= 0.1	0.491	0.156	0.646	0.492	0.155	0.647
Wealth <= 0.15	0.551	0.203	0.736	0.550	0.203	0.736
Wealth <= \$1000	0.229	0.028	0.230	0.230	0.028	0.230
Wealth <= \$5000	0.462	0.134	0.594	0.460	0.134	0.596
Wealth <= \$10000	0.561	0.213	0.751	0.560	0.212	0.751
Wealth, 10th percentile	0.002	0.057	0.004	0.002	0.057	0.004
Wealth, 25th percentile	0.020	0.215	0.019	0.020	0.216	0.019
Wealth, 50th percentile	0.105	1.110	0.055	0.105	1.114	0.054
Wealth, 75th percentile	1.341	4.448	0.161	1.380	4.452	0.161
Wealth, 90th percentile	12.21	9.683	0.421	12.25	9.678	0.419
Wealth, 99th percentile	34.93	19.65	1.674	34.82	19.61	1.672
Wealth, 99.90th percentile	44.73	26.72	3.761	44.61	26.66	3.759
Top 10% wealth share	0.707	0.444	0.560	0.704	0.443	0.562
Top 1% wealth share	0.124	0.071	0.151	0.124	0.071	0.151
Gini coefficient (wealth)	0.840	0.662	0.704	0.839	0.662	0.705