Review of Economic Studies (2021) **0**, 1–36 doi:10.1093/restud/rdaa076

Advance Access Publication 9 November 2020



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

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*First version received March* 2018*; Editorial decision August* 2020*; Accepted October* 2020 (*Eds.*)

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 (1) responses to unanticipated gains are vastly heterogeneous (either zero or substantially positive); (2) responses increase in the size of the gain, driven by the extensive margin of spending adjustments; (3) responses to losses are much larger and more widespread than responses to gains; and (4) 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 behaviour. We also find that (5) 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 (6) 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.

*Key words*: Consumption, Savings, Marginal propensity to consume, Survey.

*JEL Codes*: D12, D14, E21.

## 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

*The editor in charge of this paper was Dirk Krueger.*

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 article, 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 behaviour. We demonstrate these by implementing the survey treatments inside simple theoretical models, 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 behaviour 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

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.

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 behaviour.[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 behavioural 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 behavioural 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 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

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.

(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, 2020; 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, 2020)—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, 2020; 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; Ameriks *et al.*, 2019).

This article 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 U.S. 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 article 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 behaviour. 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 that generates a sufficient fraction of households with limited access to disposable liquid resources.

The remainder of the article 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 analyses 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 U.S. The goal of the survey is to elicit expectations about a variety of economic variables, such as inflation and labour 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%.[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 panellists (*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 panellists.[7]

Demographic and financial characteristics of respondents in the sample align well with corresponding characteristics of the U.S. population. We report several of these characteristics

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.

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 panellists, with 25 non-responses (corresponding to less than 0.3% of observations).

TABLE 1
*Sample characteristics*

| | Overall | March 16 | May 16 | January 17 | March 17 | U.S. Pop. |
|---|---|---|---|---|---|---|
| Sample size | 2,586 | 1,086 | 1,087 | 1,190 | 1,180 | |
| 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 $\leq$ 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 $\geq$ 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 |

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

in Table 1, along with their population counterparts from the 2015 American Community Survey (ACS; Ruggles *et al.* 2020) or the 2013 Survey of Consumer Finances (SCF; Board of Governors of the Federal Reserve System 2020). 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 U.S. 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 article 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 behaviour in response to an unexpected gain in resources. Respondents are first asked in what

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 Supplementary Appendix B.1 for an analysis of retention and response rates.

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, and 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 behaviour 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 had not received the $500
> - Over the next 3 months, I would **spend/donate the same** as if I had not received the $500
> - Over the next 3 months, I would **spend/donate less** than if I had not received the $500
>
> *Please select only one*
> - Over the next 3 months, I would **pay off more debt (or borrow less)** than if I had not received the $500
> - Over the next 3 months, I would **pay off the same amount of debt** as if I had not received the $500
> - Over the next 3 months, I would **pay off less debt (or borrow more)** than if I had not received the $500
>
> *Please select only one*
> - Over the next 3 months, I would **save more** than if I had not received the $500
> - Over the next 3 months, I would **save the same** as if I had not received the $500
> - Over the next 3 months, I would **save less** than if I had not received the $500

Respondents are then asked by how much they would change their behaviour 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 had not 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

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.

tax rebates. They ask respondents to choose between three uses of their tax rebate: (1) mostly spend; (2) mostly save; or (3) 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 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 behaviour 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 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

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).

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

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 labour 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 behaviour 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:

**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 behaviour differs across alternative scenarios, thus providing a way to control for fixed unobserved individual characteristics. Finally, for some treatments we asked follow-up questions

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

TABLE 2
*Treatments and survey months*

| | March 16 | May 16 | January 17 | March 17 |
|---|---|---|---|---|
| Gain | | | | |
| $500 Gain | $n = 1{,}085$ | | $n = 594$ | |
| $2,500 Gain | | $n = 540$ | | |
| $5,000 Gain | $n = 361$ | $n = 1{,}084$ | $n = 595$ | |
| $500 Gain in 3 months | $n = 362$ | | | |
| $5,000 Gain in 3 months | | | $n = 594$ | |
| Loss | | | | |
| $500 Loss | $n = 362$ | | | $n = 1{,}174$ |
| $500 Loss in 3 months | | | $n = 594$ | $n = 586$ |
| $500 Loss in 2 years | | | | $n = 589$ |
| Loan | | | | |
| $5,000 Loan | | $n = 541$ | | |

*Notes*: Table shows number of respondents in each treatment and survey month. In January 2017, 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.

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 Supplementary 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 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

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 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 | 1,638 | 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 | 1,629 | 0.12 (0.006) | 0.08 | 0.56 | 0.36 | 0.36 (0.006) | 0.30 |
| LOSS | | | | | | | |
| $500 | 1,536 | 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 | | | | | | | |
| $5,000 | 541 | 0.01 (0.005) | 0.17 | 0.75 | 0.08 | 0.34 (0.005) | 0.40 |

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

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 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, $2,500, or $5,000. 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 behaviour 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

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%.

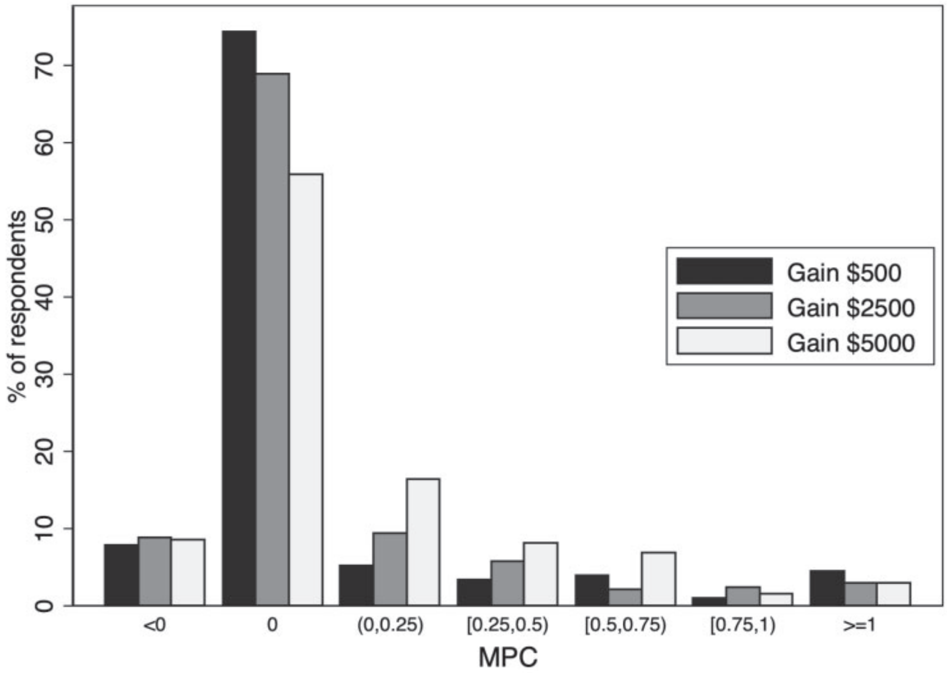


FIGURE 1
Histogram of MPCs for different Gain scenarios

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 *e.g.* Bunn *et al.*, 2018; 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).

This size effect in reported MPCs can be seen in Figure 1 by comparing the black histogram ($500 windfall) with the grey 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 behaviour. 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 distributions for hypothetical positive shocks corresponding to one 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 behaviour), but not on the extensive margin (fraction of respondents who indicate they would change behaviour).

### 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, 2020) 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; Aydin, 2018; Baker, 2018).

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

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

18. See Jappelli and Pistaferri (2020) 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).

We did find some evidence that respondents with (1) lower discount factors, as measured from an incentivized choice experiment; or (2) 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 indeed have higher MPCs out of gains.[20]

## 4. ADDITIONAL TREATMENTS: NEWS, LOSSES, AND 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 behavioural 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 behaviour 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 (grey histograms) is displayed in Figure 2a and b. 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 grey histograms compared with the black 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

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.

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.

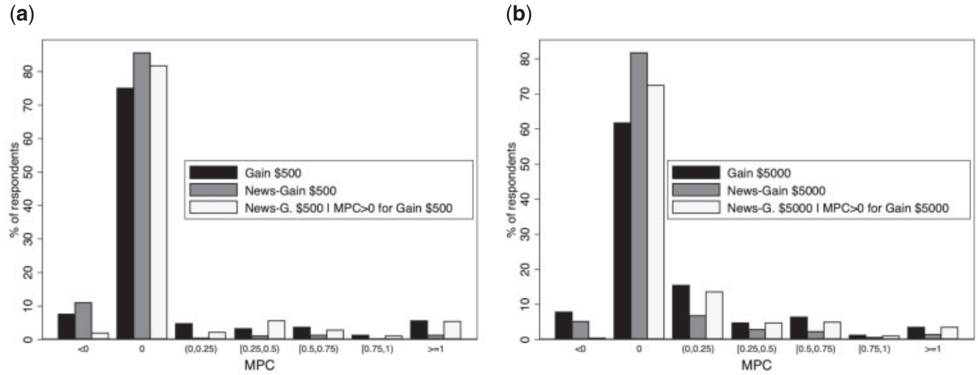


FIGURE 2

Spending response to news about future gains versus response to gains today. (a) Gain versus news of gain: $500. (b) Gain versus news of gain: $5,000.

that they would indeed increase their spending in the GAIN treatment (the white bars in Figure 2a and b). 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 *et al.* (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 (2020) 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.

### 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 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

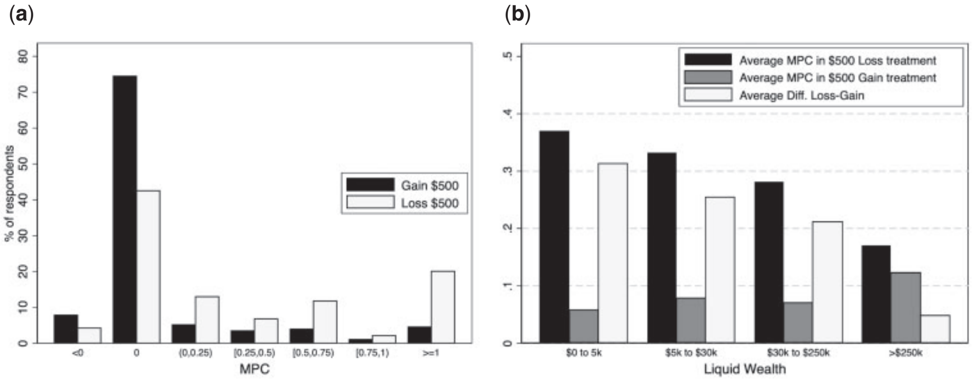


FIGURE 3

Spending response to losses. (a) Losses versus gains. (b) Losses versus gains, by liquid wealth.

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 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

22. This asymmetric response to gains and losses is consistent with evidence from the expiration of the 2013 payroll tax cuts (see Zafar *et al.*, 2013; Bracha and Cooper, 2014; 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.

23. See Tables A-6 and A-7 in the Supplementary 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 Supplementary 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.

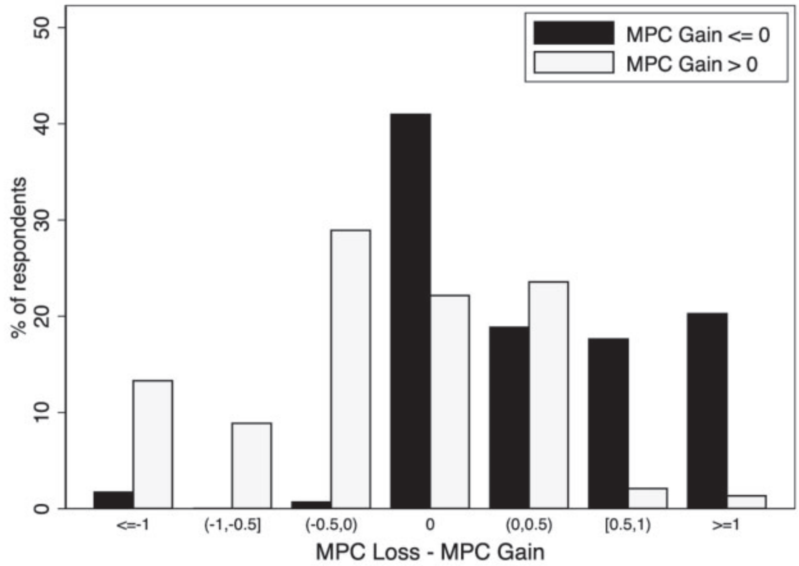


FIGURE 4
Distribution of difference between MPCs out of losses and gains.

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 behaviour 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 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 grey 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

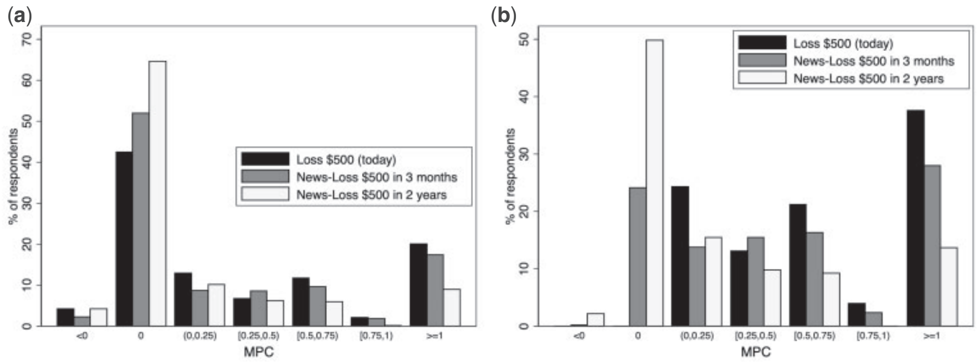


FIGURE 5

News-Loss. (a) News about a $500 loss at different horizons. (b) News-loss effect for those respondents who have MPC>0 out of loss today.

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 behaviour among individuals with high propensities to consume. Despite having a high MPC out of an immediate loss, they are willing 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 not fully understand the concept of an interest free loan, or are worried about their ability to refrain from spending part

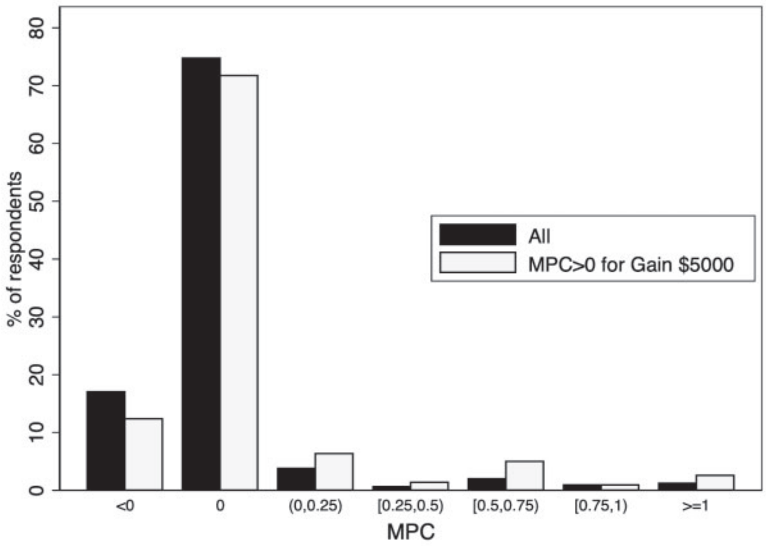


FIGURE 6

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

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]

### 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.

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.

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 versus \$500 LOSS treatments (panel B); the \$5,000 GAIN vs. \$5,000 LOAN treatments (panel C); the GAIN versus NEWS-GAIN treatments[28] (panel D); and the LOSS versus 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 shows 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 behaviour (*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 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

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

29. Figure A-2 in the Supplementary 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 \$5,000 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.

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 × Order FEs? | No | Yes | No | Yes |
| Panel A. Size effect for gains (omitted category: Gain = $500) | | | | |
| Gain = $2,500 | 0.024* | 0.025 | 0.047** | 0.052 |
| | (0.015) | (0.019) | (0.023) | (0.034) |
| Gain = $5,000 | 0.061*** | 0.056*** | 0.149*** | 0.155*** |
| | (0.010) | (0.017) | (0.016) | (0.029) |
| $p$-value $\beta_{2,500} = \beta_{5,000}$ | 0.003 | 0.005 | 0.000 | 0.000 |
| Obs. | 4,259 | 3,094 | 4,259 | 3,094 |
| 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. | 3,215 | 1,653 | 3,215 | 1,653 |
| Panel C. Gain versus loan (omitted category: Gain = $5,000) | | | | |
| Loan | −0.115*** | −0.129*** | −0.244*** | −0.259*** |
| | (0.011) | (0.016) | (0.022) | (0.027) |
| Obs. | 1,625 | 1,082 | 1,625 | 1,082 |
| Panel D. Gain now versus news ($500 and $5,000; 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. | 2,636 | 1,937 | 2,636 | 1,937 |
| 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. | 2,349 | 2,344 | 2,349 | 2,344 |

*Notes*: 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: $^{*}p<0.1$, $^{**}p<0.05$, $^{***}p<0.01$.

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° 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]

30. In Supplementary 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.

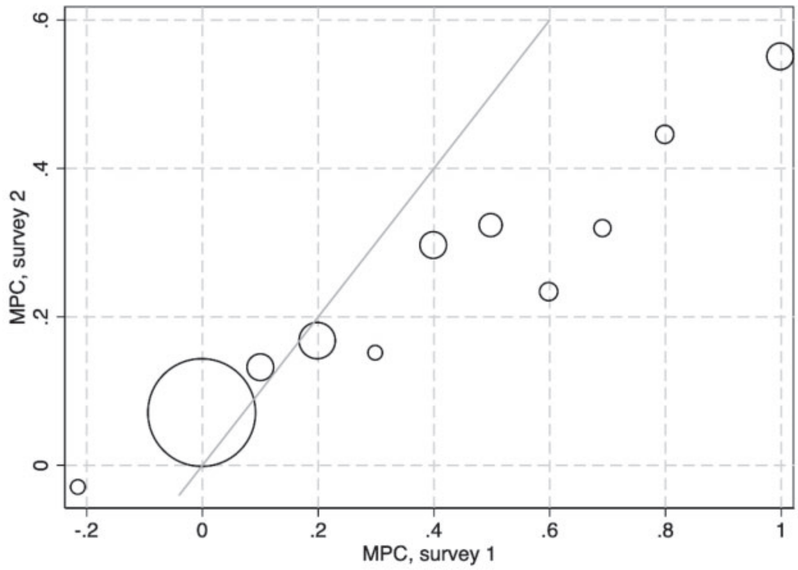


FIGURE 7
Within-respondent response stability.

**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 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 one 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, are 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

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 Supplementary Appendix B.4 for the details of this analysis.

have high numeracy (a proxy for both 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.
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 behaviour—the polar extremes of rule-of-thumb and permanent income behaviour—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.

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 Supplementary 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 (*e.g.* 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 Supplementary Appendix Section B.5, which also presents analysis of additional data that we collected for this purpose. The additional data indicate 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.

### 5.1. *Definition of the treatments*

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

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

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

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

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

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

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

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

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 $\Delta$ as

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

where $c_{it}^{\Delta}$ 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 behaviour.

**5.2.1. 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:

$$\text{MPC}^{G} = \text{MPC}^{L} = \text{MPC}^{LN} = 1$$
$$\text{MPC}^{NG} = \text{MPC}^{NL} = 0.$$

Rule-of-thumb behaviour is thus not consistent with the substantial fraction of respondents who report not changing their consumption behaviour in the GAIN and LOAN treatments, nor does it generate a size effect or sign asymmetry. Moreover, rule-of-thumb behaviour 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.

**5.2.2. 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:

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

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 and 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.

**5.2.3. 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 $\alpha$ 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

$$\text{MPC}^{G} = \text{MPC}^{L} = \text{MPC}^{LN} = \alpha$$
$$\text{MPC}^{NG} = \text{MPC}^{NL} = 0.$$

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.

**5.2.4. Precautionary savings models.** Modern workhorse models for understanding consumption behaviour 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

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

$$\text{subject to}$$

$$c+s=x$$

$$x'=Rs+y'$$

$$s \geq 0.$$

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 labour 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 $\text{MPC}^L > \text{MPC}^G$, 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 $\gamma$ 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 $\beta R$ is close to 1 or $\gamma$ 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.*, $\text{MPC}^L=1$. Whether $\text{MPC}^G$ 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 $\Delta$. This means that a borrowing constrained agent has $\text{MPC}^G=1$ for small windfalls and $\text{MPC}^G<1$ 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 $\text{MPC}^G=\text{MPC}^L=1$ and another for whom have $\text{MPC}^G=\text{MPC}^L\approx 0$. 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 analyse the NEWS treatment separately for constrained and unconstrained agents. For unconstrained agents, the first-order condition for consumption is

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

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

$$\text{MPC}^{NG}=R^{-1}\text{MPC}^G$$

$$\text{MPC}^{NL}=R^{-1}\text{MPC}^L.$$

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 $\text{MPC}^G-\text{MPC}^{NG}$ 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 $\text{MPC}^G-\text{MPC}^{NG}$ is far from zero and hence may be hand-to-mouth. On the other hand, the informativeness of the corresponding gap for losses, $\text{MPC}^L-\text{MPC}^{NL}$ 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

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.

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

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.*, $\text{MPC}^{NL} > \text{MPC}^{NG}$, 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 later, the loan has a negligible effect on their inter-temporal budget constraint, and hence $\text{MPC}^{LN} \approx 0$.[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.*, $\text{MPC}^{LN} \approx \text{MPC}^{G}$. 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*

**5.3.1. 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.

Expressed in recursive form, the model is

$$V(x,c,y) = \max\left\{V^A(x,y) - \psi, V^N(x,c,y)\right\}$$

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

subject to

$$c' + s = x$$

$$x' = Rs + y'$$

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

$$s \geq 0$$

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

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 $\psi$ 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, $\psi$, 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 $\beta$, adjustment cost parameter $\psi$, and the stochastic process for idiosyncratic income.

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

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

where $\varepsilon_{it}$ and $\eta_{it}$ 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 $\left(\lambda_\eta, \lambda_\varepsilon\right)$ 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

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 $\left(\lambda_\eta, \lambda_\varepsilon\right)$ by explicitly targeting these higher-order moments in estimation. See Supplementary Appendix C.1 for details.

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 Supplementary 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 $\beta$, 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 labour 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 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 \$1,000 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 $\beta$ to match the mean wealth target, and one in which we choose $\beta$ to match the fraction of households with financial wealth less than \$1,000.

The remaining parameter is the adjustment cost $\psi$. 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. Supplementary Appendix C.2 contains additional statistics on the wealth, income and consumption distributions in each version of the model discussed below.

**5.3.2. 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.

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% versus 15%) and the fraction with less than \$10,000 in total wealth (55% versus 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 Supplementary Table A-9, when we exclude respondents with MPCs outside the [0,1] range, the fraction is 20%.

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 $\beta$ heterogeneity | | | | | | | |
| GAIN | $500 | 0.13 | 0.00 | 0.80 | 0.20 | 0.65 | 0.56 |
| | $2,500 | 0.20 | 0.00 | 0.44 | 0.56 | 0.36 | 0.33 |
| | $5,000 | 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 |
| | $5,000 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 | $5,000 | 0.08 | 0.00 | 0.58 | 0.42 | 0.19 | 0.14 |
| Panel B: No $\beta$ heterogeneity, targeting mean wealth | | | | | | | |
| GAIN | $500 | 0.04 | 0.00 | 0.80 | 0.20 | 0.19 | 0.16 |
| | $2,500 | 0.04 | 0.00 | 0.55 | 0.45 | 0.10 | 0.07 |
| | $5,000 | 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 |
| | $5,000 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 | $5,000 | 0.01 | 0.00 | 0.86 | 0.14 | 0.04 | 0.03 |
| Panel C: No $\beta$ Heterogeneity, targeting households with < $1000 | | | | | | | |
| GAIN | $500 | 0.14 | 0.00 | 0.80 | 0.20 | 0.69 | 0.61 |
| | $2,500 | 0.24 | 0.00 | 0.31 | 0.69 | 0.34 | 0.31 |
| | $5,000 | 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 |
| | $5,000 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 | $5,000 | 0.08 | 0.00 | 0.52 | 0.48 | 0.17 | 0.12 |

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

The median and mean intensive margin responses in 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 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.

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 treatment. 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 behaviour 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.

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 collection 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%,

43. In addition, behavioural 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).

TABLE 7
*Mean MPCs from treatments in models without consumption adjustment costs*

| | Heterogeneous $\beta$ | Common $\beta$ Target mean wealth | Common $\beta$ Target frac. $<$\$1000 |
|---|---|---|---|
| GAIN | | | |
| $500 | 0.23 | 0.05 | 0.29 |
| $2,500 | 0.21 | 0.05 | 0.27 |
| $5,000 | 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 |
| $5,000 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 | | | |
| $5,000 | 0.08 | 0.01 | 0.10 |

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 5.2.2.

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.

Overall, these quantitative findings suggest that two features are important in generating the observed pattern of MPCs across the survey treatments: (1) a meaningful extensive margin of consumption response and (2) 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 behaviour.

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: (1) a meaningful extensive margin of consumption responses, which we generate through a utility cost of changing consumption and (2) 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 behaviour 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 behaviour. 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.

*Acknowledgments*. Nima Dahir, Brian Livingston, and Alberto Polo provided outstanding research assistance, for which we are grateful. We thank our discussant Monica Paiella, four anonymous referees and the editor, Dirk Krueger, for extremely helpful comments and suggestions. Fuster and Zafar were employed at the Federal Reserve Bank of New York while much of this paper was written. The views expressed herein are those of the authors and not necessarily those of the Federal Reserve Bank of New York, the Federal Reserve System, or the Swiss National Bank.

**Supplementary Data**

Supplementary data are available at *Review of Economic Studies* online. And the replication packages are available at https://dx.doi.org/10.5281/zenodo.4115399.

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