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Eliciting the Marginal Propensity to Consume in Surveys*

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Abstract

Different methods of eliciting the marginal propensity to consume give very different distributions. Mean MPCs range from below 0.1, indicating life-cycle consumers, to over 0.5, consistent with consumers being hand-to-mouth. We conducted a randomized survey experiment to test if this difference arises because of question wording: we compare using a direct question and a filtered q uestion. Survey wording has large effects on (1) the mean MPC, (2) the extensive margin, and (3) how MPCs vary with payment size, spending horizon, and liquidity. MPCs elicited using a filtered question are much closer to results from using a covariance restriction approach.

Keywords: MPCs, survey experiment

JEL codes: C83, D12, D14, E21

*The views expressed do not necessarily reflect the views of the Federal Reserve Bank of Chicago or the Federal Reserve System.

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1 Introduction

The marginal propensity to consume (MPC) is one of the most studied objects in economics. At the macro level, it is central for understanding the economy's responses to monetary and fiscal policies in aggregate. At the micro level, it is used to measure the degree to which households are insured against transitory income shocks. A vast literature has aimed to measure the size of the MPC, how and why it varies among individuals, and how it varies with the size of the income shocks that individuals experience. However, different papers have generated very different answers: the aim of this paper is to understand the source of these differences.

The survey elicitation approach, pioneered by Shapiro and Slemrod (1995, 2003), is an increasingly commonly used tool to measure MPCs and their dispersion. This approach asks survey respondents how they will or would respond to payments (or bills) of a given amount and within a given time period. Payments are often hypothetical. This approach gives researchers the ability to study, at low cost and in large samples, how MPCs vary across income changes and circumstances that they specify. It also allows researchers to uncover and investigate the full distribution of consumers' MPCs. Survey questions eliciting MPCs in hypothetical scenarios have now been included in a number of surveys including the Survey of Consumer Expectations fielded by the Federal Reserve Bank of New York, the Household Finance and Consumption Survey (HFCS) asked across Eurozone countries, the Bank of Italy's Survey of Household Income and Wealth (SHIW) and the Bank of England's Survey of Household Finances. These are the basis of current papers including Jappelli, Savoia, and Sciacchetano (2024), Koşar et al. (2025) and Andreolli and Surico (2026).

A puzzle is that different surveys elicit very different distributions of MPCs. Across different surveys, mean MPCs range from below 0.1, indicating an economy of almost entirely life-cycle consumers, to 0.5, consistent with a large fraction of consumers being hand-to-mouth. We hypothesize that this is due to question wording. The key distinction we draw is between survey questions which use a filter question and those which are direct.

The direct questions used to elicit MPCs in the Italian SHIW and the various country surveys of the HFCS imply large MPCs. The mean MPC in the SHIW reported by Jappelli and Pistaferri (2014) is 48%. Drescher, Fessler, and Lindner (2020) report mean MPCs from the HFCS for 17 European countries. These range from 33% to 57% with an overall mean of 47%. The question studied by Drescher, Fessler, and Lindner (2020) is:

> Imagine you unexpectedly receive money from a lottery, equal to the amount of income your household receives in a month. What percent would you spend over the next 12 months on goods and services, as opposed to any amount you would save for later or use to repay loans?

The SHIW question is similar, but omits a spending horizon.[1]

Questions in the filtered style imply much lower MPCs: Fuster, Kaplan, and Zafar (2021) estimate 8% and Crossley et al. (2021) 11%. The first part of the question studied by Fuster, Kaplan, and Zafar (2021) is:

> 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

The survey methods literature cautions against questions that are "leading" or presumptive, and advocates the use of filter questions. For example, in summarizing key considerations in questionnaire design, Krosnick and Presser (2010) advise "avoid leading or loaded questions that push respondents towards an answer" and "filter questions should be included, to avoid asking respondents questions that do not apply to them." Moser and Kalton (2017) argue that a question should not imply that the respondent engages in the behavior being asked about. For example, a question like "how many cigarettes a day do you smoke?" should be preceded by a filter question which establishes whether the respondent smokes at all. The effects of leading or presumptive questions, and of the absence of a filter question, have been documented in

[1] Orchard, Ramey, and Wieland (2025) posit that the omission of a spending horizon may be the reason for high elicited MPCs in the SHIW: a forward-looking rational consumer will fully spend a windfall over the remainder of life. However, the fact that the HFCS question which does specify a 12-month spending horizon also results in very high MPCs suggests the explanation must lie elsewhere.

the survey methods literature across a range of content domains including religious affiliation (Brenner, LaPlante, and Reed, 2023) and reporting crime (Knäuper, 1998).[2]

The survey method literature does offer two caveats to the preference for filtered questions. Direct questions may be desirable when the behavior under study is under-reported due to social desirability bias (Freedman, Aykan, and Kleban, 2003). Repeated use of filtered questions can also lead to motivated under-reporting if respondents learn that an affirmative response leads to additional questions (Eckman et al., 2014). We return to these issues below.

Filtered and direct MPC questions are asked in different countries and at different times, making it difficult to know whether MPC estimates vary because of the way questions were asked or because they were posed to individuals in different economic contexts (for example, Gross, Notowidigdo, and Wang (2020) find that MPCs increase in recessions and with the local unemployment rate). Therefore we conducted a survey experiment in which households were randomly assigned different types of question designed to elicit their MPCs out of hypothetical payments.[3] The types of question reflect the direct vs filtered approaches. It is common to find that MPCs are larger for households with less liquidity (Bunn et al., 2018; Ganong et al., 2020; Fuster, Kaplan, and Zafar, 2021), that MPCs are smaller for larger payments (Fuster, Kaplan, and Zafar, 2021), and that MPCs are larger when measured over longer time horizons (Boehm, Fize, and Jaravel, 2025). Accordingly, we also examine how question wording affects the way responses vary with the size and duration of the payments and with household liquidity.

We find that the wording of the question can have dramatic effects on the size of measured MPCs, their relationship with liquidity and how MPCs differ according to the size and duration of payments. Across different combinations of windfall size and spending horizon, a two-part question (including a filter question) resulted in mean MPCs ranging from 0.11 to 0.13. A direct (unfiltered) question resulted in mean MPCs from 0.51 to 0.79. Most of the difference is on the extensive margin: with a filtered question, far more respondents report that they would not change spending at all.

[2] As one example, Brenner, LaPlante, and Reed (2023) conducted a randomized survey experiment in which one group was only asked "What is your current religious affiliation, if any?" with multiple options including no affiliation, and a second group were first asked a yes/no filter question ("do you currently have a religious affiliation"). They found that the filter question increased the fraction of unaffiliated respondents by almost half.

[3] Survey experiments have been used to study other aspects of question design when eliciting MPCs. Sahm, Shapiro, and Slemrod (2015) show that consumers are 7 ppt more likely to choose an option to "mostly spend" a hypothetical tax cut, and less likely to choose an option to mostly reduce debt, when the option to reduce debt is expressed as "mostly to decrease borrowing" than when it is "mostly to pay off debt". Crossley et al. (2023) show that reported MPCs are 11% greater, and reported transfers to other households smaller, when the question is explicit that the source of the hypothetical payment is a government transfer paid to everyone than when this is left ambiguous.

With the direct question, we find lower MPCs with larger payments, and higher MPCs with longer horizon and lower liquidity. This heterogeneity is not found with the filtered question. However, when we condition on a positive spending change (and hence a positive MPC), the negative size, positive horizon effects emerge in the responses to the filtered question.

The question our results raise is what is the preferred way to elicit an MPC. The survey methods literature advocates the filtered question. We use the approximation method of Blundell, Pistaferri, and Preston (2008) also using the *Understanding Society* data to show that a model of partial insurance is consistent with the estimates from the filtered MPC questions. Similarly, recent re-evaluations of the natural experiment results lead to estimates more in line with estimates from the filtered question (Havranek and Sokolova, 2020; Borusyak, Jaravel, and Spiess, 2024).

2 Data and Design

Our data is drawn from the 15th wave of the UK Understanding Society Innovation Panel (IP15), which was collected between June and October 2022 (University of Essex, Institute for Social and Economic Research, 2023). This nationally representative panel of 1,500 households was first surveyed in 2008, with refreshment samples of around 500 respondent households added in waves 4, 7, 10, 11, and 14. The Panel attempts to interview all adults in sample households, and is mixed mode, with interviews conducted by either web or face to face interview. The core aim of the Panel is to run experiments and methodological tests separately from the much larger main Understanding Society panel.

We contributed a set of questions to the Innovation Panel on households' liquidity and on individuals' spending, debt and saving responses to a hypothetical payment. Households were randomly assigned (with equal probability) to one of eight groups, which were each given different versions of the hypothetical payments questions as we describe below. The experiment was preregistered.[4] Randomization was clustered at the household level and stratified by allocated mode (web or face to face), incentive group, and sample (original sample, and each refreshment sample.) All inferences make appropriate adjustment for clustering and pre-stratification. The randomization delivered groups that are balanced on observables. A selection of balance checks are presented in the Supplemental Materials (Appendix A). Households were assigned different

[4]RCT ID AEARCTR-0009231 initial registration date May 08, 2022

wordings of the question and asked about payment sizes of different amounts and responses over different time periods.

2.1 Filter question

Half of the sample were asked a filter question: they were first asked whether a windfall would lead them to increase their spending, decrease spending or keep spending the same; before then being asked for the amount of their spending changes in pounds. Finally they were asked whether unspent funds would be used to save, pay down debt, give more to friends and family or receive less from friends and family. This version of the question was based on the questions asked in Fuster, Kaplan, and Zafar (2021) (see above). We added in an option to report using funds to adjust intra-household transfers as analysed in Crossley et al. (2021) and Crossley et al. (2023). The exact question is:

> Now consider a hypothetical situation where you unexpectedly receive a one-time payment of £[AMOUNT] today. We would like to know whether this extra income would cause you to change your spending, borrowing and saving behaviour in any way over the next [DURATION].
>
> If you received the one-time £[AMOUNT] payment, over the next [DURATION] would you...
>
> - Spend more than if I hadn't received the £[AMOUNT]
> - Spend the same as if I hadn't received the £[AMOUNT]
> - Spend less than if I hadn't received the £[AMOUNT]

Respondents who responded that they would spend more or spend less were then asked a follow-up question, which depended on their answer.

> You indicated that you would [increase/reduce] your spending over the next [DURATION] following the receipt of the £[AMOUNT] payment. How much [more/less] would you spend than if you hadn't received the £[AMOUNT]?

Finally, those who responded they would not spend the full amount of the payment (i.e. those with an MPC less than one) were asked:

> You have indicated that you would not spend all of the £[AMOUNT] payment. We are interested in what else you would do over the next [DURATION] with the amount that you do not spend?
>
> - I would pay off more debt (or borrow less) than if I hadn't received the £[AMOUNT]
> - I would save more than if I hadn't received the £[AMOUNT]
> - I would receive less financial help from friends or family than if I hadn't received the £[AMOUNT]
> - I would give more financial help to friends or family than if I hadn't received the £[AMOUNT]

Respondents were invited to select all options that applied.

2.2 Direct question

The other half of the sample were asked how much they would spend without the initial filter question. They were then asked whether unspent funds would be used to save, pay down debt, give more to friends and family or receive less from friends and family in the same way as the first half of the sample. We refer to this sample as having received the 'direct' question. The exact wording is:

> Imagine you unexpectedly received a one-time payment of £[AMOUNT] today. How much of it would you spend over the next [DURATION] months?

In asking for amounts spent directly without the initial filter question, the wording of the question is closer to the questions asked in the SHIW and HFCS (analysed in Jappelli and Pistaferri (2014, 2020) and Drescher, Fessler, and Lindner (2020)). These surveys do not specify the precise value of the windfall and instead ask individuals to imagine they received an amount from the lottery "equal to the amount of income your household receives in a month" and then asked what percentage of that windfall they would spend. These other aspects of the question's framing – that is, specifying that the winnings came from a lottery, asking respondents to calculate a percentage, and allowing the size of the payment to vary according to households' monthly income – could of course also affect responses. We kept the question similar to that asked in Fuster, Kaplan, and Zafar (2021) in order to isolate the importance of the filter question

in particular. Those who responded with an MPC less than one were asked the same follow-up question on the use of unspent funds as respondents who were assigned the filter question (whether they would pay off more debt, save more, receive less financial help, and/or give more financial help).

2.3 Question permutations and liquidity question

Within each of the filter and direct arms of the experiment, individuals were further randomly assigned to be asked either about a smaller payment of £500 or a larger payment of £2,500. They were also randomly assigned questions asking about their responses over the next three months or over the next 12 months. The $2 \times 2 \times 2$ different possible permutations of this question gives eight different treatment groups.

An individual's MPC is calculated as the elicited spending change divided by the amount of the transfer (either £500 or £2500). Of the 2,719 responding individuals in the experiment, 2,496 reported valid MPC and so comprise the final sample. The share with item non-response to the MPC question is similar across questions types: 9 percent (filtered) and 7.5 percent (direct). We censored the MPC at 0 and 1 but this only affected a small number of cases (32 and 4 respectively).

To study how question wording affects how reported MPCs vary with liquidity, we collected a measure of liquidity. Participants were asked how long they could cover their living expenses if they lost their main source of income. Responses were recorded on a five-point scale ("less than a week", "1 week to less than 1 month", "1 month to less than 3 months", "3 months to less than 6 months", "6 months or longer") which we subsequently grouped into three categories: up to 3 months, 3 to 6 months, and more than 6 months. The full question was:[5]

> If you lost your main source of household income, how long could your household continue to cover living expenses, without having to borrow any money or ask for help from friends or family?

[5]This question was taken from the Financial Conduct Authority's Financial Lives survey (Financial Conduct Authority, 2023).

3 Results

Baseline Results. Table (1) presents the distribution of elicited MPCs in each of our eight treatment arms. We first arrange the results in four groups defined by the randomly assigned windfall size and spending horizon. Within each group we contrast those who received the direct question with the filtered. Thus, we have four treatment effects, corresponding to each of the four assigned combinations of windfall size and spending horizon.

The first column of numbers in Table (1) presents the mean MPC for each treatment arm, and the difference in means between direct and filtered questions within each combination of windfall size and spending horizon. MPCs are much larger when elicited with a direct question. Across the combinations of size and horizon, average MPCs from the direct question range from 0.51 to 0.79, whereas average MPCs from the filtered question range from 0.11 to 0.13. The treatment effects (differences) range from 0.39 to 0.67, with confidence intervals bounded well away from zero in all cases, and an overall combined treatment effect of 0.54. These results suggest a very strong effect of question wording.

The next two columns of Table (1) show that this question wording effect is largely on the extensive margin. The probability of reporting a strictly positive MPC is much higher with the direct question: $P(MPC > 0)$ always exceeds 0.8, whereas with the filtered question this probability ranges from 0.15 to 0.25 across possible combinations of size and horizon. Conditional on reporting a strictly positive MPC, the means are quite similar across question types. For example, with a windfall of £500 and a spending horizon of 3 months, the unconditional means differ by 0.53 but the conditional means only by 0.1.

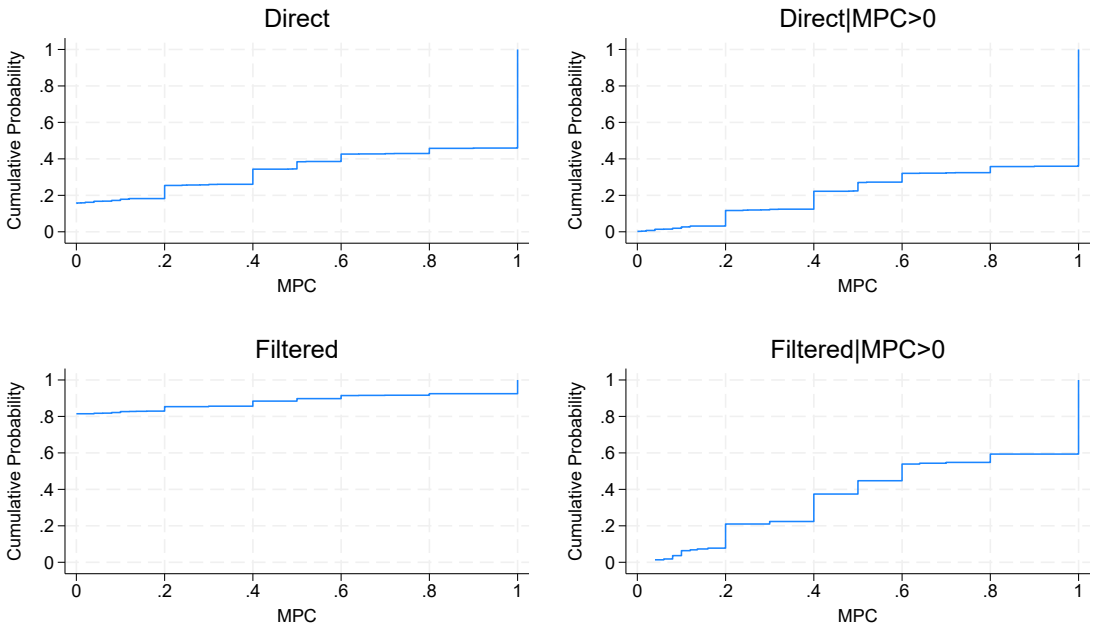
Figure (1) shows the CDF of MPCs for each question type (pooling across combinations of size and horizon). The two panels on the left show the unconditional distributions, with the direct question at the top and the filtered question below. The two panels on the right show the same distributions but conditional on a positive reported MPC. Conditional on a positive MPC, the two distributions are more similar, but not identical. For example, the 50th percentile of positive MPCs obtained by the direct question is 1, while for the filtered question the same percentile is 0.6. A formal statistical (Kolmogorov-Smirnov) test of equality of the two conditional distributions is strongly rejected ($p<0.001$).

Table 1: Question Effects on Elicited MPCs

	Method	Mean	Prob MPC>0	Mean (if MPC>0)	N
£500, 3 months	Direct	0.66	0.80	0.83	315
	Filtered	0.13	0.17	0.72	327
	Difference	**0.53**			
		[0.47, 0.60]			
£500, 12 months	Direct	0.79	0.87	0.91	328
	Filtered	0.12	0.14	0.81	291
	Difference	**0.67**			
		[0.61, 0.73]			
£2,500, 3 months	Direct	0.51	0.85	0.60	303
	Filtered	0.12	0.25	0.48	314
	Difference	**0.39**			
		[0.33, 0.44]			
£2,500, 12 months	Direct	0.68	0.86	0.79	332
	Filtered	0.11	0.16	0.67	286
	Difference	**0.58**			
		[0.52, 0.64]			
Average Difference		**0.54**			
		[0.51, 0.57]			

Notes: Standard errors take account of the clustered and pre-stratified randomization. 95% confidence intervals are shown in square brackets.

Figure 1: CDFs by question method



Notes: We drop observations whose reported MPC is less than zero or greater than one. Sample of 2460 individuals.

The Effect of Windfall Size and Spending Horizon. Table (2) addresses the question of how size and horizon effects in elicited MPCs are moderated by question wording. The top panel reports the effects on the mean MPC. The responses obtained via the direct question, in column 1, replicate well-known findings in the literature. Mean MPCs are smaller with a larger windfall, and higher with a longer spending horizon. These effects are not present in mean MPCs obtained from the filtered question. These differences in size and horizon effects across question types are statistically significant at conventional levels. Thus, question wording seems to affect not only the level of MPCs but also how MPCs vary with parameters such as the size of the hypothetical windfall and the spending horizon.

The middle panel of Table (2) reports the effect of size and horizon on the probability of reporting a positive MPC. Size and horizon do not affect the extensive margin using the direct question, but the extensive margin is affected when using the filter question. The bottom panel of Table (2) reports the analysis conditional on a report of a strictly positive MPC. There are significant size and horizon effects for those answering the filtered question if they give a strictly positive response. Moreover, conditional on a positive MPC, the size and horizon effects ob-

tained from the two question types are statistically and economically indistinguishable. Our conclusion is that the effect of question wording on size and horizon seems to be because the direct question leads to large numbers of non-zero responses.

Table 2: Interaction Effects on estimated MPC: Size and Spending Horizon

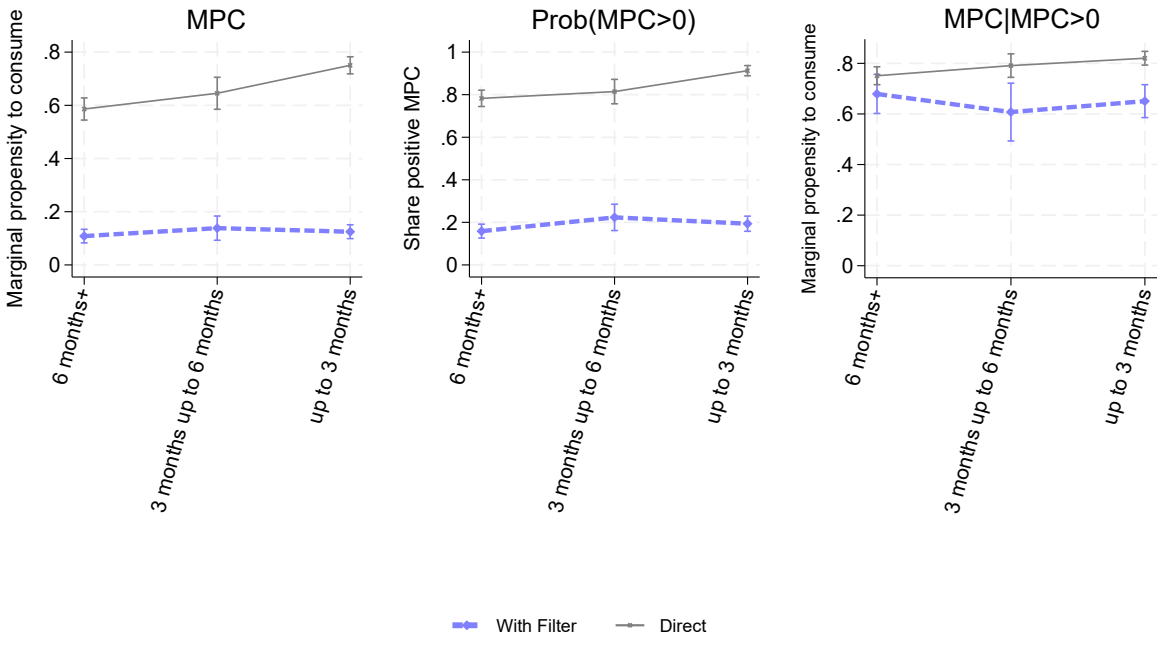
	Direct	Filtered	Difference
		MPC	
Size	-0.13 [-0.18,-0.08]	-0.01 [-0.04,0.03]	-0.12 [-0.18,-0.06]
Spending Horizon	0.15 [0.1,0.2]	-0.01 [-0.04,0.02]	0.16 [0.10,0.22]
		Prob(MPC>0)	
Size	0.02 [-0.02,0.07]	0.05 [0.00,0.09]	-0.02 [-0.09,0.04]
Spending Horizon	0.04 [0.00,0.09]	-0.06 [-0.10,-0.01]	0.10 [0.04,0.16]
		$MPC \mid MPC > 0$	
Size	-0.17 [-0.21,-0.13]	-0.19 [-0.28,-0.1]	0.02 [-0.07,0.12]
Spending Horizon	0.14 [0.10,0.18]	0.13 [0.05,0.22]	0.00 [-0.09,0.1]

Notes: The first row reports the effect of windfall size on the MPC by type of question. The second row does the same for the duration of spending. The second panel reports the effect on whether $MPC > 0$, and third panel the MPC conditional on $MPC > 0$. Variation in size and duration are experimentally manipulated. Standard errors take account of the clustered and pre-stratified randomization. 95% confidence intervals are shown in square brackets.

Figure (2) takes up the question of how reported MPCs vary with liquidity and how this differs by question type. Of course, unlike size and horizon, liquidity is not randomly assigned but is obtained from respondents with a question on how many months of regular expenses they could cover if they lost their main source of income. For this exercise, we pool across payment

size and duration within each arm. Respondents were divided into three groups: those who could cover more than 6 months; 3-6 months; and less than 3 months. The solid line in Figure (2) shows how the mean MPC differs with liquidity for those who respond to the direct question. These responses replicate the liquidity effect documented by previous authors (Bunn et al., 2018; Ganong et al., 2020; Fuster, Kaplan, and Zafar, 2021). The mean MPC for those with low liquidity (less than three months) is approximately 0.2 greater than the mean for those with high liquidity (more than 6 months).

Figure 2: MPC by Liquidity and Question Type



Notes: Presented are elicited MPCs by liquidity. Sample of 2,435 individuals with non-missing responses to the liquidity question. Estimates from regressing the MPC on treatment arm, interacted with the time the household can sustain current spending with liquid assets. The estimates are therefore averages across payment size and duration. Standard errors take account of the clustered and pre-stratified randomization. The error bars are 95% confidence intervals.

Source: Authors' calculations based on Understanding Society.

The dotted line in Figure (2) shows how the mean MPC differs with liquidity for those who

respond to the filtered question. In contrast to the direct question, there is no liquidity effect.[6] The difference in difference estimate contrasting low and high liquidity across question types is 0.146 with a confidence interval of (0.087, 0.206) and $p < 0.001$. The relationship of the MPC to liquidity is also sensitive to question type.

Additional Analysis. We report two analyses that were not pre-registered.[7] First, we tested whether allocated mode of interview (web or face-to-face) had a main effect on mean MPCs or an interaction effect with question wording. The reason to do this is the suggestion that direct (or leading) questions may be desirable where responses may be inhibited by social desirability bias (Freedman, Aykan, and Kleban, 2003). If reporting spending in response to a windfall was perceived as socially undesirable by respondents, we would expect to find a mode effect, and for that mode effect to differ by question type. However, we obtained a null result for both main and interaction effects.

Second, we tested whether being a member of the wave 14 refreshment sample had a main effect on mean MPCs or an interaction effect with question wording. These tests compare a sample that has completed the interview in just one previous wave with respondents (from the original sample and earlier refreshment samples) that have completed the interview multiple times (in some cases many times). This is of interest because one potential problem with filter questions is "motivated mis-reporting" (Eckman et al., 2014; Tourangeau and Kreuter, 2015). Respondents may learn that positive answers to filter questions lead to follow up questions, and misreport (give a "no") to reduce interview length and burden. While previous waves of the Innovation Panel did not pose MPC questions, they did contain a variety of filtered questions, particularly regarding income. If learning about filter questions is a problem in these data, one would expect a larger question wording effect in samples which have been in the panel longer. The wave 14 refreshment sample had a mean MPC (across all groups) 0.06 above the average of other samples. However, we found no evidence of an interaction between sample and question wording.

[6] This result is consistent with Fuster, Kaplan, and Zafar (2021). They use only a filtered question, and find no relationship between MPCs and liquidity for gains (but do find a relationship between liquidity and MPCs for losses).

[7] These additional analyses were conducted in response to seminar suggestions. The pre-stratification of randomization by mode and by sample make these analyses feasible.

4 Assessment of MPC Responses

The one-part and two-part questions give very different results, both with respect to the distribution of MPCs and, to some extent, the correlation of MPCs with key covariates, such as liquidity. The natural question is then which set of answers is more likely to be correct, and which question should future survey designers deploy.

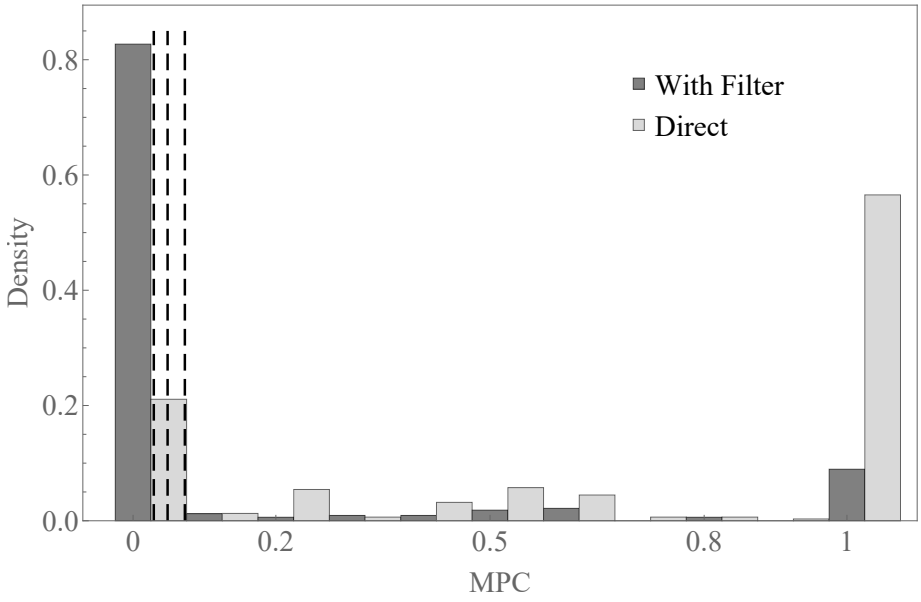
One approach to choosing between formats for eliciting MPCs is to compare the elicited MPCs to other methods for estimating MPCs. As noted in the introduction, numerous papers following Blundell, Pistaferri, and Preston (2008) use covariance restrictions on panel data on income and consumption to estimate the "pass-through" of transitory income shocks.

We implemented the BPP procedure on a sample of 12,148 households observed in the Understanding Society Main Panel from 2013 to 2020 (waves 5-12). The Innovation Panel and Main Panel are based on probability samples of the same population, with surveys conducted in a closely aligned fashion and with considerable overlap in content.[8] We compare our elicited estimates of the MPC to estimates using the GMM procedure of Blundell, Pistaferri, and Preston (2008). Further details of our implementation of this approach are given in Appendix B in the Supplemental Materials.

Figure 3 compares the elicited values of the MPC to the pass-through parameters from the procedure of Blundell, Pistaferri, and Preston (2008). We convert the pass-through parameter, which is in logs, to the level of the MPC by multiplying by $\frac{c}{y}$. Higher values of $\frac{c}{y}$ imply greater values of the MPC for a given pass-through elasticity. The vertical lines show the estimated MPC for $\frac{c}{y} \in \{0.6, 1.0, 1.5\}$. The estimates of the MPC using Blundell, Pistaferri, and Preston (2008) are close to those using the filter question and substantially different from the direct question.

[8] Both panels get the same "core" interview. The Main Panel gets additional substantive modules (that rotate from wave to wave) while the Innovation Panel gets several survey experiments that change from wave to wave

Figure 3: Comparing Elicited versus estimated MPCs



Notes: The dashed vertical lines represent the MPC implied from the variance-covariance restrictions, using Blundell, Pistaferri, and Preston (2008). The pass-through estimate from transitory income into (food) consumption from the Blundell, Pistaferri, and Preston (2008) procedure is estimated as 0.019. This is converted into an MPC by using values of $\frac{c}{y} \in \{0.6, 1.0, 1.5\}$. This is converted into an overall MPC by using a food income elasticity of 0.4. This gives three values of the MPC: {0.029, 0.048, 0.073}

5 Conclusion

Our conclusion is that direct hypothetical questions on MPCs overstate MPCs compared either to asking about MPCs in two parts with a filter question or compared to using covariance restrictions. More generally, the distribution of the MPC is not robust to the way the question is asked.

Various macroeconomic papers have drawn partly on particular elicited choice studies and on evidence from tax rebates to conclude that MPCs are large, at around 0.25 or higher for quarterly nondurable spending. These estimates underpin, for example, heterogeneous macro models based on the idea of wealthy-hand-to-mouth following (Kaplan and Violante, 2014, 2018) and Auclert, Bardóczy, and Rognlie (2020). This literature was developed partly to match a distribution of estimated MPC that was generated by use of a leading question. Or at best, to match a distribution that is not robust to the way the question is asked. A symptom of the

problem this generates is the difficulty of reconciling these large MPCs with the observed wealth distribution.

Using a different approach to estimation, Fuster, Kaplan, and Zafar (2021) find a large fraction of individuals report no response to the income change. This leads Fuster, Kaplan, and Zafar (2021) to build a model with non-pecuniary costs of consumption adjustment to match this extensive margin feature of their data. Our results indicate that this feature of the data (the high number of reported MPCs equal to zero) is specific to the filtered question. Our general point is that we need to have a clearer consensus on the MPC survey response before building models to match particular responses.

An alternative to using the direct elicitation or covariance restrictions is to use natural experiments. The evidence from these natural experiments has recently undergone a re-evaluation. An extensive meta-study by Havranek and Sokolova (2020) finds that micro-estimates of the MPC are subject to substantial publication bias, as inferred from the positive relationship between the size of estimates and their standard errors. While the mean MPC estimate from micro-studies using observed income changes in their sample is 0.21, after correcting for publication bias this estimate falls to 0.11. Past studies using a difference-in-difference design, and random differences in the timing of receipt of stimulus checks, have also been found to overstate MPCs once staggered treatment and heterogeneous treatment effects are accounted for. Borusyak, Jaravel, and Spiess (2024) find mean MPCs for nondurable consumption that are roughly half as large as those originally found in Broda and Parker (2014), at between 0.08-0.11 over 3 months (implying marginal propensities to spend on all goods of 0.25-0.37). Finally, Orchard, Ramey, and Wieland (2025) show that the original estimates of MPCs from these tax rebates are not consistent with observed macro fluctuations in consumption.[9]

Our findings suggest that caution should be applied to estimates of large MPCs arising from reported preference studies, particularly those that employ questions with potentially leading or presumptive wording. Our experiment allows us to identify the impact differences in the wording of the question have on reported MPCs, and we find that they are substantial. Results from questions more similar to those used in Fuster, Kaplan, and Zafar (2021) are significantly smaller than questions used in Jappelli and Pistaferri (2014, 2020) and the implied MPCs are much closer

[9] Commault (2022) also observes that MPC estimates from natural experiments are not consistent with estimates of pass-through parameters from Blundell, Pistaferri, and Preston (2008). Her argument is that the pass-through estimates are underestimates of the true MPC because of assumptions about the MA structure of transitory shocks. The difficulty is that using the correct MA structure leads to very large standard errors so that it is hard to rule out a wide range of MPC values.

to the publication bias-corrected MPC estimates from Havranek and Sokolova (2020).

A limitation of this study is that power considerations led us to trial only two question variants (while also randomizing windfall size and spending horizon). Future work might usefully explore a wider range of question variations. Nonetheless, our results point to the importance of framing in survey design.

Declarations

Ethics approval and consent to participate

The University of Essex Ethics Committee has approved all data collection on *Understanding Society* main study and innovation panel waves.

Availability of data and materials

Data from the *Understanding Society* Innovation Panel used for this study are available for download from the UK Data Service SN: 6849, http://doi.org/10.5255/UKDA-SN-6849-14.

Competing interests

The authors declare that they have no competing interests.

Pre-registration

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Online Supplemental Materials

A Balance Checks

Table A1: Balance Check

	Male	Age	Degree	Ethnic Minority	Children	Food Spend
Constant	0.451***	52.026***	0.465***	0.081***	0.174***	387.537***
	(0.020)	(1.247)	(0.029)	(0.023)	(0.026)	(14.674)
Filtered £500 12 months	0.007	-0.607	0.046	-0.008	0.025	-41.016
	(0.028)	(1.752)	(0.043)	(0.031)	(0.038)	(21.587)
Filtered £2500 3 months	-0.000	-2.576	0.009	-0.002	0.056	-16.632
	(0.027)	(1.776)	(0.042)	(0.032)	(0.039)	(22.163)
Filtered £2500 12 months	-0.005	1.242	-0.004	-0.005	0.037	-17.287
	(0.028)	(1.767)	(0.043)	(0.032)	(0.039)	(23.868)
Direct £500 3 months	-0.039	-0.216	-0.005	0.001	0.041	3.132
	(0.028)	(1.810)	(0.042)	(0.030)	(0.038)	(22.693)
Direct £500 12 months	0.039	-0.170	-0.022	0.008	0.039	-4.003
	(0.027)	(1.688)	(0.042)	(0.033)	(0.039)	(22.011)
Direct £2500 3 months	-0.019	-0.140	0.004	-0.009	0.053	-34.171
	(0.029)	(1.715)	(0.043)	(0.031)	(0.039)	(19.374)
Direct £2500 12 months	0.015	-1.131	-0.007	-0.027	0.056	-20.047
	(0.027)	(1.666)	(0.042)	(0.027)	(0.038)	(19.753)
Observations	2719	2685	2610	2005	2719	2483

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes: Outcomes are binary variables regressed on a set of treatment group dummies. Robust standard errors in parentheses.

Source: Authors' calculations based on Understanding Society.

B Implementation of Blundell, Pistaferri, and Preston (2008)

Define after tax log income of household i in period t, $\log Y_{i,t}$, be composed of a persistent component, $P_{i,t}$, and a transitory income shock, $\varepsilon_{i,t}$, according to the following expression:

$$\log\ Y_{i,t} = \mathbf{Z'_{i,t}}\mathbf{\Psi_t} + P_{i,t} + \varepsilon_{i,t} \tag{1}$$

where $Z_{i,t}$ is a set of observable characteristics that predict income, and $P_{i,t}$ evolves according to

$$P_{i,t} = P_{i,t-1} + u_{i,t} \tag{2}$$

We define unexplained log income as $y_{i,t} := \log Y_{i,t} - \mathbf{Z'_{i,t}}\mathbf{\Psi_t}$ and unexplained log consumption as $c_{i,t} := \log C_{i,t} - \mathbf{Z'_{i,t}}\mathbf{\Phi_t}$. The change in unexplained consumption is given by the following linear function of the income shocks:

$$\Delta c_{i,t} = c_{i,t} - c_{i,t-1} = \alpha u_{i,t} + \beta \varepsilon_{i,t} + \eta_{i,t} \tag{3}$$

where α measures the degree of transmission of permanent income shocks, β measures the degree of transmission of transitory income shocks, and $\eta_{i,t}$ corresponds to any other shocks that might affect consumption, but are independent of income.

We assume that the shocks ε, u, and η are all i.i.d. with variances σ^2_ε, σ^2_u, and σ^2_η, respectively. Commault (2022) discusses how the validity of this procedure depends on the transitory process having an MA(k) structure. She shows that a higher order MA structure generates higher estimates of β than if the process is an MA(0). However, estimates with the higher order MA process produce very noisy estimates.

We observe the variances and covariances of income and consumption in the data, and use

the model structure in equations 1 and 3 to give:

$$\begin{aligned}
\text{Var}\left(\Delta y_t\right) &= \sigma_u^2 + 2\sigma_\epsilon^2 && (4)\\
\text{Cov}\left(\Delta y_t, \Delta y_{t-1}\right) &= -\sigma_\epsilon^2 && (5)\\
\text{Var}\left(\Delta c_t\right) &= \alpha^2\sigma_u^2 + \beta^2\sigma_\epsilon^2 + \sigma_\eta^2 && (6)\\
\text{Cov}\left(\Delta y_t, \Delta c_t\right) &= \alpha\sigma_u^2 + \beta\sigma_\epsilon^2 && (7)\\
\text{Cov}\left(\Delta y_t, \Delta c_{t-1}\right) &= -\beta\sigma_\epsilon^2 && (8)
\end{aligned}$$

The parameter of interest is β which we can identify once we have the parameters for the transitory and permanent income process, which are pinned down by the variances of income growth and the intertemporal co-variances. Our data is for food expenditure and so we adjust the pass-through parameter to calculate the pass-through to total non-durable expenditure.