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# MPCs in an Economic Crisis: Spending, Saving and Private Transfers\*

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## Abstract

MPCs were directly elicited from a representative sample of UK adults in July 2020 using receipt of a hypothetical unanticipated, one-time income payment. Reported MPCs are modest, around 11% on average. They are higher, but still modest, for individuals in households with high current needs. These low MPCs may be a consequence of the prevailing economic uncertainty. Significant fractions of respondents report they would use a windfall to pay down debt, or that they would change their transfer payments to or from family and friends. The latter means that the aggregate MPC out of a stimulus payment need not equal the population-average MPC.

Keywords: spending, MPC, crowding out, COVID-19

JEL codes: D12, D14, E21

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## 1 Introduction

Governments often respond to recessions and economic crises with measures to stimulate household consumption spending. These include targeted reductions in VAT, expansions in the coverage and generosity of welfare benefits, and direct stimulus payments to individuals or households. The propensity of individuals and households to increase consumption spending out of any transitory increase in income (the marginal propensity to consume or MPC) is a key policy variable determining the effectiveness of many of these measures, and of direct stimulus payments in particular.

The aim of this paper is to understand how consumers react to fiscal stimulus during an economic crisis. It has been argued that the MPC out of a one-off payment may be particularly high during the pandemic because earnings falls have been particularly large (Alon et al. (2020)). We distinguish between responses through increased spending, through saving and debt reduction and through private transfers to others.

We draw on a large-scale, high-quality panel survey of individuals conducted during the COVID pandemic in the United Kingdom to characterise the level and distribution of MPCs across UK adults. The fourth wave of this survey in July 2020 included questions directly eliciting the MPC: individuals were asked how they would respond to a hypothetical unanticipated and one-time payment of £500 (\$640). Those who reported they would not spend the entire windfall were asked about alternative uses of the payment. Further, the COVID sample is based on the long-running *Understanding Society* panel and so we combine data on how individuals have fared during the crisis with panel data on economic position prior to the pandemic.

Theory suggests that the MPC out of transitory income will either be close to zero or close to one (Jappelli and Pistaferri (2010)). For unconstrained consumers, who have many periods left to live and who face little uncertainty, the increase in consumption should be roughly equal the annuitised value of the increase in lifetime income, which will typically be near zero ( $\approx r/(1+r)$  where  $r$  is the interest rate). By contrast, consumers who face binding constraints on borrowing, and have little liquid wealth, would be expected to have MPCs close to one (Kaplan and Violante (2014)). Models of precautionary savings, in which consumers are both highly impatient and highly risk averse, predict high MPCs for consumers who are below their target ‘buffer-stock’ of savings. (Carroll (1997)).

As predicted by theory, we find that most individuals report either that they would make small or zero changes in spending in response to the hypothetical payment, or that they would spend the full amount. 81% of individuals report they would not increase spending at all in the three months following receipt of the additional £500. 7% of individuals report they would spend the full

amount. This translates into an average MPC of 11p for each £1. Few observable characteristics are strongly associated with MPC heterogeneity but we do find higher average MPCs for the college educated and those who do not own their homes outright.

There is also important heterogeneity in the use of unspent funds: Of the 93% who reported they would not spend the full amount, 66% of individuals reported they would use the payment to increase savings and 22% would use the payment to reduce debts. This heterogeneity is more strongly associated with observable characteristics. Respondents with children, those who do not own their housing outright, those who were not regular savers in the past, those in financial arrears, and those expecting future financial difficulties are more likely to report they would use the extra funds to pay down debt, and are less likely to report that they would increase saving. In other words, those in more financially fragile situations would use the payments to pay down debt.

An important and novel feature of the survey is that it explores how private transfers between households are affected by the receipt of the extra income. Individuals were asked if they would reduce or increase transfers to other households following receipt of the payment. 9% of individuals report that they would give more and 3% report they would receive less. The crowding out of private support is higher among those in the lowest income quintile, and this highlights the potential interaction of windfall income gains with transfer behaviour.

This paper contributes to a literature estimating how individuals respond to one-off income payments, and the extent of heterogeneity in responses across individuals. This literature includes the direct elicitation of the MPC using either hypothetical scenarios (Drescher, Fessler, and Lindner (2020); Bunn et al. (2018); Sahm, Shapiro, and Slemrod (2012); Jappelli and Pistaferri (2014); Fuster, Kaplan, and Zafar (2021); Christelis et al. (2019); Jappelli and Pistaferri (2020)) as well as questions about responses to past or future windfalls (such as tax rebates, Shapiro and Slemrod (1995, 2003); Broda and Parker (2014)). Alternative approaches have used income and spending data in conjunction with natural experiments (such as differences in the timing of receipt of tax rebates and stimulus cheques (Johnson, Parker, and Souleles, 2006; Parker et al., 2013; Baker et al., 2020), lottery wins (Fagereng, Holm, and Natvik, 2018)), wealth shocks (Christelis, Georgarakos, and Jappelli, 2015), or statistical decompositions of income shocks and covariance restrictions on the joint distributions of income and consumption growth (Blundell, Pistaferri, and Preston (2008)).

A small number of papers estimate MPCs during the current pandemic-induced recession. Baker et al. (2020) use spending data estimate the impact of stimulus payments made in the U.S. in April 2020. They find that each \$1 received resulted in increases in spending of 0.25 cents when their sample is weighted using the Current Population Survey. Again in the U.S., Coibion et al. (2020) use

direct elicitation and find that 15% respondents “mostly spent” their stimulus cheques. Christelis et al. (2020) find MPCs for nondurables around 0.3-0.5 for hypothetical windfalls of €3000 in a survey of six Eurozone countries conducted between April and October 2020.

There are two main contributions of our paper. First, it is unique in the broad MPC literature in showing that some consumers would respond to a one-time income payment by transferring more to friends and family, and that others would have support from friends and family reduced. This implies that targeting of payments to high-MPC individuals could be partly undone by private transfers. Further, this implies that the aggregate MPC out of a stimulus payment need not equal the population-average MPC, even if all individuals receive the same payment.

Second, our paper differs from much of the MPC literature in that we estimate MPCs in a time of a severe crisis, when MPCs may be different and when fiscal stimulus packages are most likely to be deployed. The crucial difference between our paper and the others that measured MPCs in 2020 is that those papers evaluate MPCs out of stimulus checks distributed during the first wave of infection when there were many restrictions on spending. In contrast, our data (the July 2020 wave of the *Understanding Society* COVID-19 study) were collected after the first wave of the pandemic, during a period when the UK had come *out* of lockdown and many social-distancing, travel and commercial restrictions had been lifted.

Despite this, our average estimated MPCs are low compared to the average MPCs from most of the literature cited above, which typically report MPCs around 0.3-0.5. In our concluding section we discuss possible explanations for this finding, and implications for policy responses to the pandemic and other economic crisis.

## 2 Data and Methods

### 2.1 Understanding Society COVID-19 Web Survey

This paper is based on data collected in the fourth wave of the *Understanding Society* COVID-19 Study (Institute for Social and Economic Research (2020a); henceforth COVID-19 Study), fielded in late July of 2020. The UK went into “lockdown” on 23rd March. By July, the first peak of the pandemic had past in the UK. The retail and hospitality industries had reopened as had domestic and some international travel. The UK economy contracted substantially in March and April, but began to grow again in May. GDP grew 6.4% in July but was still 11% down on February<sup>1</sup>. In terms of economic support policies, the UK government introduced the Job Retention Scheme

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<sup>1</sup><https://www.ons.gov.uk/economy/grossdomesticproductgdp/bulletins/gdpmonthlyestimateuk/august2020>

on March 20th, soon followed by the Self-Employment Support Scheme. Both policies were still in place in July. July saw the announcement of the “eat out to help out” scheme (a subsidy to restaurant meals) though this did not come into effect until the start of August. July also saw the introduction of a temporary cut in VAT specific to the hospitality and domestic tourism industries which took effect from the 15th of the month.<sup>2</sup>

The COVID-19 study is a new component of *Understanding Society*: the UK Household Longitudinal Study (henceforth Main Study). *Understanding Society* (University of Essex Institute for Social and Economic Research, NatCen Social Research, and Kantar Public, 2019) is the UK’s main longitudinal Household Survey, and a sister study to the PSID in the U.S. and the GSOEP in Germany, among others. The Main Study began in 2009 and attempts to interview all adults in sample households annually using a mixed mode design.<sup>3</sup> The COVID-19 Study began in April 2020 and uses more frequent web surveys to capture the experiences and behaviour of Main Study participants during the COVID-19 pandemic.

The fact that the COVID-19 Study is based on the *Understanding Society* Main Study has several key advantages. First the COVID-19 Study inherits the properties of the Main Study that ensure reliable population inferences. Second, data collected in the COVID-19 Study can be linked to data on the same participants, and their households, collected in past waves of the Main Study. Data from earlier waves of the Main Study provides context for COVID-19 Study, and for modelling nonresponse to the COVID-19 Study. All members of the Main Study who, in April 2020, belonged to active households and were aged sixteen or over were eligible for the COVID-19 Study.<sup>4</sup>

Our analysis focuses on those who had previously responded to Wave 9 of the Main Study. For this group, the response rate was 39%. Because data collection was restricted to web mode, and the fieldwork period was short, this response rate is significantly below what is achieved in the Main Study, though not out of line with web studies. However, very rich background information is available from the Main Study on both respondents *and* nonrespondents. This allows for very careful modelling of nonresponse. A Post-Lasso procedure is used to select predictors of response from a very large set of candidate variables and to estimate the inverse-probability weights that are

<sup>2</sup>The temporary cut as from 20% to 5% and was announced to last until early January 2021.

<sup>3</sup>*Understanding Society* carried on from and incorporates the sample of the earlier *British Household Panel* survey which ran from 1991 to 2008.

<sup>4</sup>The July 2020 wave was issued to all of these individuals except those who attrited from the Main Study after Wave 9 but prior to April 2020, and those that had adamantly refused to participate in the COVID-19 Study (either after the initial invitation, or at an earlier wave). A total of 36,268 individuals were invited to participate in the July wave, and invitations were sent by email and/or SMS text message, or by post. Respondents were offered a small incentive to participate. The seven-day fieldwork period opened on July 24th, and reminders were sent on days 2, 3 and 6. The web questionnaire was designed to take approximately 20 minutes to complete. Further details about the COVID-19 Study fieldwork can be found in Institute for Social and Economic Research (2020b).

provided with the data. Many other surveys correct for nonresponse with simple calibration weights based on a small number of observables such as age, gender and education. In contrast, the Main Study contains information such frequency of internet use, which is a strong predictor of web survey response.<sup>5</sup> The Main Study also provides population targets that can be used to test the ability of the COVID-19 Study weights to eliminate nonresponse biases. Both the development and testing of the inverse-probability weights is described in detail in Crossley, Fisher, and Low (2021) and (Benzeval et al., 2021). Those papers demonstrated the the inverse-probability weights developed for the COVID-19 are very effective at removing nonresponse biases, and that they significantly outperform simple calibration weights.

## 2.2 Sample and Covariates

We use a sample of 11,223 individuals who had positive Wave 9 sample weights, for whom COVID-19 sample weights can be derived. We then exclude from our analysis 248 individuals who did not provide an answer to the initial MPC question. This gives a final analysis sample of 10,975 individuals.

To explore heterogeneity in spending responses we focus primarily on characteristics measured in the Main Study, prior to the pandemic. In addition to demographics (age, gender, education, family type), we look at several measures of financial position. We created an income measure which assigns respondents to quintiles of household income based on average annual income in the three years prior to COVID. Income includes earned and unearned income, net of tax and inclusive of any benefits received and is equivalized by household composition. We also look at whether the individual lives in housing which is owned outright (so that there are neither mortgage nor rental payments).

In addition to these pre-COVID variables, we use six covariates from the Jule wave of the COVID study, which are therefore potentially co-determined with the spending response. The first is whether the individual reduced their working hours, or lost their job during the pandemic prior to July 2020. The second is about individuals' saving behaviour:<sup>6</sup> we group individuals according to whether they are current savers and whether they reported saving in the 2018-2019 wave of the Main Study (“new saver”, “was saver”, “never saver” and “always saver”). The third is whether

<sup>5</sup>As one example, the New York Fed Survey of Consumer Expectations, studied by Fuster, Kaplan, and Zafar (2021), response rates hover around 55%. However, the weights provided in that survey calibrate only to population totals on age, income, education and region.

<sup>6</sup>Savers are those who answered affirmatively to the question “Do you save any amount of your income, for example by putting something away now and then in a bank, building society, or Post Office account, other than to meet regular bills?”.

individuals are currently in arrears on household bills, mortgage rent or council tax. The fourth is whether the individual expects financial difficulties over the next three months. Respondents were asked *On a scale of 0-100% how likely do you think it is that you will have difficulty paying your usual bills and expenses in the next three months?*. We group responses to this question into two categories (0, 1-100). The three month period that these expectations are defined over coincides with the timing of the spending questions discussed below. The final covariates are whether individuals are currently ‘shielding’ to avoid infection risk (individuals identified as at risk of severe risk of coronavirus by the National Health Service were asked stay at home where possible, even as restrictions were relaxed for the general population) and about individuals’ expectations of catching COVID over the next month.

The first two columns of Table 1 show the distribution of these covariates in our sample, both unweighted and weighted. This highlights the importance of using the appropriate weights, as discussed in detail in Crossley, Fisher, and Low (2021). A comparison of the weighted and unweighted statistics reveals that women, those over the state pension age, and those with a degree are over-represented in the unweighted data. The estimates suggest that about a third of the population own their homes outright. In July of 2020 just over a third expected financial difficulties over the next three months. Less than 10% were shielding due to health vulnerabilities and more than 90% of the population considered themselves unlikely or very unlikely to contract COVID-19.

### 2.3 Measuring the Marginal Propensity to Consume

The July 2020 wave of the COVID-19 Study includes new questions designed to directly elicit respondents’ MPC. The exact wording of the questions is given in the Appendix. Respondents are asked to consider a hypothetical situation in which they receive an unexpected and one-time payment of £500 on the day they are completing the survey.<sup>7</sup> They were first asked if this would lead them to spend more, less or the same over the next three months. If they responded either more or less, a second question asked quantitatively how much more or less.<sup>8</sup> Much of the previous literature *only* asks for qualitative responses (“mostly spend”, “mostly save” etc. as in Shapiro and Slemrod (1995, 2003, 2009)). An obvious advantage of our approach is that it allows us to directly calculate the value of the MPC for different individuals rather than having to infer it. In terms of the hypothetical payment, the questions in the COVID-19 are most similar to questions asked in the New York Fed’s Survey of Consumer Expectations analysed by Fuster, Kaplan, and Zafar

<sup>7</sup>About \$640 at the time of the survey.

<sup>8</sup>As noted above, some of the previous literature asks about a a real-world windfall (such as a tax rebate as in Shapiro and Slemrod (1995)) rather than a hypothetical payment.

Table 1: Summary Statistics

|  | Sample means |          | How would spending change? |      | MPCs |            |
|--|--------------|----------|----------------------------|------|------|------------|
|  | Unweighted   | Weighted | Increase                   | Same | Mean | $\geq 0.9$ |
| <b>Gender:</b> Men                     | 0.42         | 0.48     | 0.21                       | 0.73 | 0.12 | 0.08       |
| Women                                  | 0.58         | 0.52     | 0.17                       | 0.76 | 0.10 | 0.07       |
| <b>Age:</b> 19-65                      | 0.71         | 0.78     | 0.21                       | 0.72 | 0.12 | 0.09       |
| 66+                                    | 0.29         | 0.22     | 0.12                       | 0.84 | 0.06 | 0.03       |
| <b>Education:</b> GCSE or lower        | 0.29         | 0.37     | 0.17                       | 0.74 | 0.09 | 0.06       |
| A-level                                | 0.20         | 0.23     | 0.17                       | 0.75 | 0.09 | 0.06       |
| Degree                                 | 0.50         | 0.40     | 0.21                       | 0.75 | 0.13 | 0.09       |
| <b>Family type:</b> Couple, child      | 0.24         | 0.23     | 0.24                       | 0.68 | 0.15 | 0.11       |
| Couple, no child                       | 0.49         | 0.38     | 0.15                       | 0.81 | 0.08 | 0.05       |
| Single, child                          | 0.03         | 0.04     | 0.27                       | 0.59 | 0.16 | 0.11       |
| Single, no child                       | 0.25         | 0.35     | 0.19                       | 0.74 | 0.11 | 0.07       |
| <b>Housing:</b> Not owned              | 0.56         | 0.67     | 0.22                       | 0.70 | 0.13 | 0.09       |
| Owned                                  | 0.44         | 0.33     | 0.13                       | 0.84 | 0.07 | 0.04       |
| <b>Income quintile:</b> q1             | 0.14         | 0.18     | 0.22                       | 0.67 | 0.11 | 0.07       |
| q2                                     | 0.16         | 0.19     | 0.19                       | 0.72 | 0.10 | 0.07       |
| q3                                     | 0.20         | 0.20     | 0.19                       | 0.74 | 0.11 | 0.08       |
| q4                                     | 0.23         | 0.21     | 0.19                       | 0.77 | 0.12 | 0.08       |
| q5                                     | 0.27         | 0.22     | 0.17                       | 0.80 | 0.11 | 0.08       |
| <b>Labour shock:</b> No shock          | 0.88         | 0.87     | 0.18                       | 0.75 | 0.11 | 0.07       |
| Hours loss                             | 0.09         | 0.10     | 0.23                       | 0.70 | 0.12 | 0.07       |
| Job loss                               | 0.03         | 0.04     | 0.13                       | 0.76 | 0.07 | 0.05       |
| <b>Saver:</b> Never saver              | 0.32         | 0.37     | 0.21                       | 0.70 | 0.11 | 0.08       |
| New saver                              | 0.11         | 0.11     | 0.19                       | 0.77 | 0.11 | 0.08       |
| Always saver                           | 0.37         | 0.33     | 0.16                       | 0.80 | 0.10 | 0.06       |
| Was saver                              | 0.19         | 0.19     | 0.18                       | 0.76 | 0.11 | 0.08       |
| <b>Arrears:</b> In arrears             | 0.06         | 0.10     | 0.23                       | 0.58 | 0.13 | 0.09       |
| Up-to-date                             | 0.94         | 0.90     | 0.18                       | 0.76 | 0.11 | 0.07       |
| <b>Chance financial difficulty:</b> 0% | 0.71         | 0.63     | 0.16                       | 0.80 | 0.10 | 0.07       |
| > 0 & $\leq$ 100 %                     | 0.29         | 0.37     | 0.23                       | 0.65 | 0.13 | 0.08       |
| <b>COVID shielding:</b> Shielding      | 0.06         | 0.08     | 0.15                       | 0.78 | 0.08 | 0.05       |
| Not shielding                          | 0.94         | 0.92     | 0.19                       | 0.74 | 0.11 | 0.08       |
| <b>Expects to get COVID:</b> Likely    | 0.04         | 0.04     | 0.23                       | 0.70 | 0.13 | 0.09       |
| Unlikely                               | 0.60         | 0.59     | 0.20                       | 0.74 | 0.12 | 0.08       |
| Very unlikely                          | 0.37         | 0.36     | 0.16                       | 0.77 | 0.09 | 0.06       |
| All (weighted)                         | -            | -        | 0.19                       | 0.75 | 0.11 | 0.07       |
| All (unweighted)                       | -            | -        | 0.17                       | 0.78 | 0.10 | 0.07       |

*Notes:* Sample size ('All') is 10,975 in columns 1-4 and 10,759 in columns 5-6. Statistics are weighted for nonresponse except where otherwise noted. MPCs are calculated on the basis of questions asking if spending would change upon receipt of £500 and by how much. MPCs are trimmed to be at least zero and at most one. Income quintiles are of household incomes that were collected in 2017-18 (wave 9) and are equivalised using the OECD modified scale. The variables 'Labour shock', 'arrears', 'expects financial difficulties', 'COVID shielding' and 'expects to get COVID' are collected in the July 2020 survey. The 'Labour shock' categories are defined according to changes between February and July 2020, where an hours loss is defined as working positive hours in February and being employed but working zero hours in July. 'Saver' categories are defined according to saving behaviour in 2018-19 (wave 10) and July 2020. 'COVID shielding' refers to being identified as someone at risk of severe illness from coronavirus by the NHS. 'Expects to get COVID' has a reference period of the next month while 'expects financial difficulties' the next three months.

(2021).<sup>9</sup>

Two further aspects of the MPC questions bear noting. First, the questions ask about total spending, rather than just about nondurable consumption spending. While some studies, such as Christelis et al. (2019), separate durable and nondurable spending, this is rare in the literature. Moreover, from the point of view of fiscal stimulus, total spending (including on durables) is the measure of primary interest to policy makers. Second, our questions are explicit about the time-horizon over which additional spending will occur (the next three months). This is likely to be the time-frame of most interest to policymakers. Questions that are not explicit about the time-frame may capture longer-run spending responses that are less relevant for short term stimulus policies.<sup>10</sup>

A perennial issue with questions about hypothetical windfalls is that consumers stated responses may differ from their true spending responses, as measured in studies such as Parker et al. (2013) and Baker et al. (2020). Parker and Souleles (2019) find that those with higher self-reported propensities to consume out of stimulus checks increased their spending by more when their stimulus checks were actually received.

If the categorical and quantitative questions about spending indicated that the respondent would not spend the full £500, a final question asked what they would do with the unspent amount. Options included paying off debt, saving, giving more financial help to friends and family; and additionally, whether they would receive less financial help from friend or family. These latter options capturing private transfers in response to transfer payments are a novel addition to the literature that directly elicits MPCs. Further, private transfers seem to be an important way in which individuals have dealt with economic shocks associated with the pandemic in the UK. Crossley, Fisher, and Low (2021) show that 12.5% of working age adults reported receiving a private transfer over the first two waves of the COVID-19 Study (covering April and May, 2020), more than reported new borrowing or new benefit claims. Moreover, the incidence of such transfers was much higher for some groups. For example, 40% of single parents reported receiving a private transfer in this period. Thus the potential interaction of a windfall income gains with transfer behaviour is of considerable interest.

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<sup>9</sup>Jappelli and Pistaferri (2014) ask for a quantitative amount spent, but do so directly, without a preliminary categorical question, or allowance for negative amounts.

<sup>10</sup>A limitation of the data is that it only considers gains. The MPC out of gains is of course the relevant parameter for policy makers considering direct income payments to consumers as a fiscal stimulus measure. However, some past research has found that larger MPCs are elicited when considering income losses (Bunn et al. (2018), Fuster, Kaplan, and Zafar (2021)).

### 3 Results

#### 3.1 Spending Responses

The penultimate row in the central panel of Table 1, reports the fraction of individuals receiving an additional £500 that would increase spending, keep spending the same or decrease spending. While this ‘extensive margin’ response does not estimate the MPC, it provides a clear indication of individuals’ intentions, and the extent of heterogeneity across groups. Further, this extensive margin is likely to be less subject to measurement error. Overall, only 19% of individuals report that they would spend more. 75% report that they would not change their spending. 7% reported that they would spend less.<sup>11</sup>

The penultimate column of Table 1 shows average MPCs. These combine both the intensive and extensive margins of response. An individual’s MPC is calculated as the elicited consumption change divided by £500. MPCs are capped to be between zero and one. This means, for example, that we have re-coded as having MPCs of zero those individuals who reported that they would spend less as a result of the £500. On average each £1 increase in income results in an increase in spending of 11p. This is to the low end of figures reported in the literature. For example, Drescher, Fessler, and Lindner (2020) report country-average MPCs in Europe that range from 32.9% to 57.3% with an overall mean of 46.9%. The last row of Table 1 also reports the fraction who would increase spending and average reported MPC *without* weights. Although, as noted above, the weights do significantly adjust the socio-demographic composition of the sample, weighting raises the average MPC only from 0.10 to 0.11, and the fraction that would increase spending at all from 0.17 to 0.19. Given that the correction for selection on a very rich set of observables leads to such a small change in the average MPC, it is unlikely that any residual selection on unobservables could lead to a significantly larger average MPC.

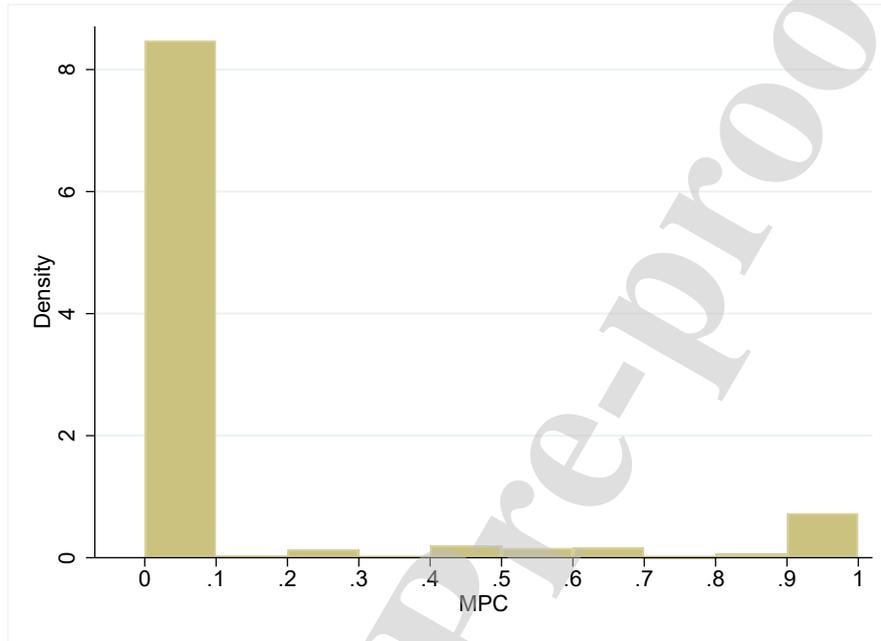
Substantial heterogeneity underlies the average MPC. Figure 1 shows the full distribution of reported MPCs in our sample. Consistent with the theory outlined in the introduction, individuals tend to either reported low MPCs (below 0.1) or high MPCs (above 0.9) with very few individuals reporting intermediate values: 85% have an MPC of 0; 7% have an MPC of 1 and would spend the full amount of the windfall.

Table 1 also shows how spending responses vary by observable characteristics. Reported intention to spend a windfall, and higher MPCs, are associated with being of retirement age, the

<sup>11</sup>It may be surprising to see a fraction spending less, but this is similar to the proportion who reported they would spend less in response to a cash windfall in Fuster, Kaplan, and Zafar (2021), where 6% reported they would spend less.

presence of children, not owning one's home outright (and so obliged to make either rent or mortgage payments.) However, we do not find an average MPC above 15% in any subgroup.

Figure 1: Histogram of MPCs



*Notes:* Sample size is 10,759. Statistics are weighted for nonresponse. MPCs are calculated on the basis of questions asking if spending would change upon receipt of £500 and by how much. MPCs are trimmed to be at least zero and at most one.

### 3.2 Exploring MPC Heterogeneity

Table 2 reports results from regressions of MPCs on multiple characteristics. Given the bi-modal nature of the data, we report both OLS models of the individual MPC, and probit models for a binary indicator for whether individuals report a high MPC (0.9 or above). In both cases we report three specifications. The first includes sociodemographic characteristics measured pre-COVID. The second adds long-run income quintiles, again measured pre-COVID (in the Main Study). In the third specification we include variables for changes in individuals' circumstances over the pandemic which may be co-determined with MPCs. The only robust and statistically significant results in Table 2 are that those with degrees and those that do not own their homes outright tend to have higher MPCs. We find no evidence that those with lower long-run incomes have higher MPCs after conditioning on other demographics. This is at odds with the theoretical prediction that MPCs are greater for poorer individuals. However, previous studies have also found either no clear rela-

tionship between MPCs and income (Shapiro and Slemrod, 1995) or that poorer individuals have *lower* MPCs than the rich (Sahm, Shapiro, and Slemrod (2012)). Another theoretical prediction is that MPCs should be greater for ‘hand-to-mouth’ individuals with low liquid wealth (Kaplan and Violante, 2014). While we do not observe liquid wealth stocks directly, we observe several proxies (saving behaviour, whether individuals are in arrears and whether individuals expect financial difficulties). However, none of these variables are correlated with the MPCs individuals report. Fuster, Kaplan, and Zafar (2021) also note that the search for observable correlates of MPC heterogeneity has yielded few robust results.

Table 2: MPC Regressions

|                         | OLS               |                   |                   | Probit<br>$\geq 0.9$ |                   |                   |
|-------------------------|-------------------|-------------------|-------------------|----------------------|-------------------|-------------------|
| Female                  | -0.02**<br>(0.01) | -0.02*<br>(0.01)  | -0.02*<br>(0.01)  | -0.02*<br>(0.01)     | -0.02*<br>(0.01)  | -0.01<br>(0.01)   |
| Age 19-65               | 0.04**<br>(0.01)  | 0.03**<br>(0.01)  | 0.03*<br>(0.01)   | 0.03**<br>(0.01)     | 0.03**<br>(0.01)  | 0.03*<br>(0.01)   |
| GCSE or lower           | 0.01<br>(0.01)    | 0.01<br>(0.01)    | 0.01<br>(0.01)    | 0.02<br>(0.01)       | 0.02<br>(0.01)    | 0.02<br>(0.01)    |
| Degree                  | 0.04***<br>(0.01) | 0.04***<br>(0.01) | 0.05***<br>(0.01) | 0.03***<br>(0.01)    | 0.03**<br>(0.01)  | 0.04***<br>(0.01) |
| Couple, child           | 0.02<br>(0.01)    | 0.02<br>(0.01)    | 0.02<br>(0.01)    | 0.02<br>(0.01)       | 0.02<br>(0.01)    | 0.02<br>(0.01)    |
| Couple, no child        | -0.01<br>(0.01)   | -0.01<br>(0.01)   | -0.01<br>(0.01)   | -0.01<br>(0.01)      | -0.01<br>(0.01)   | -0.01<br>(0.01)   |
| Single, child           | 0.04<br>(0.03)    | 0.05<br>(0.03)    | 0.04<br>(0.03)    | 0.03<br>(0.03)       | 0.03<br>(0.03)    | 0.02<br>(0.03)    |
| Home not owned outright | 0.04***<br>(0.01) | 0.04***<br>(0.01) | 0.04***<br>(0.01) | 0.03***<br>(0.01)    | 0.03***<br>(0.01) | 0.03***<br>(0.01) |
| Income q1               |                   | 0.00<br>(0.02)    |                   |                      | -0.01<br>(0.01)   |                   |
| Income q2               |                   | -0.01<br>(0.02)   |                   |                      | -0.01<br>(0.01)   |                   |
| Income q4               |                   | 0.00<br>(0.01)    |                   |                      | 0.00<br>(0.01)    |                   |
| Income q5               |                   | 0.00<br>(0.02)    |                   |                      | 0.01<br>(0.01)    |                   |

Table 2: MPC Regressions (Cont'd)

|                              | OLS   |                  |      | Probit<br>$\geq 0.9$ |                 |      |
|------------------------------|-------|------------------|------|----------------------|-----------------|------|
| Hours loss                   |       | -0.02<br>(0.01)  |      |                      | -0.02<br>(0.01) |      |
| Job loss                     |       | -0.03<br>(0.02)  |      |                      | -0.02<br>(0.01) |      |
| Never saver                  |       | 0.00<br>(0.02)   |      |                      | -0.01<br>(0.01) |      |
| New saver                    |       | -0.00<br>(0.02)  |      |                      | -0.01<br>(0.01) |      |
| Always saver                 |       | -0.02<br>(0.02)  |      |                      | -0.02<br>(0.01) |      |
| In arrears                   |       | -0.00<br>(0.02)  |      |                      | 0.00<br>(0.02)  |      |
| Expects financial difficulty |       | 0.02<br>(0.01)   |      |                      | 0.00<br>(0.01)  |      |
| Shielding                    |       | -0.02<br>(0.02)  |      |                      | -0.01<br>(0.02) |      |
| Likely get COVID             |       | -0.01<br>(0.03)  |      |                      | -0.01<br>(0.02) |      |
| Very unlikely get COVID      |       | -0.02*<br>(0.01) |      |                      | -0.01<br>(0.01) |      |
| Observations                 | 10507 | 10507            | 9691 | 10507                | 10507           | 9691 |

*Notes:* Columns 1-3 contain OLS coefficient estimates and columns 4-6 contain marginal effects from probit models for a discrete change of dummy variable from 0 to 1 (col 3-4). Regressions are weighted for nonresponse. Standard errors in parentheses. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ . Omitted categories are: male; age 66+; educated to a-level; single with no child; income quintile three; 'no (labour market) shock'; 'never saver'; 'not in arrears', '0 % chance of financial difficulty', 'not shielding' and 'unlikely to get COVID'. MPCs are calculated on the basis of questions asking if spending would change upon receipt of £500 and by how much. MPCs are trimmed to be at least zero and at most one. Income quintiles are of household incomes that were collected in 2017-18 (Wave 9) and are equivalised using the OECD modified scale. The variables 'Labour shock', 'arrears', 'expects financial difficulties', 'COVID shielding' and 'expects to get COVID' are collected in the July 2020 survey. The 'Labour shock' categories are defined according to changes between February and July 2020, where an hours loss is defined as working positive hours in February and being employed but working zero hours in July. 'Saver' categories are defined according to saving behaviour in 2018-19 (wave 10) and July 2020. 'COVID shielding' refers to being identified as someone at risk of severe illness from coronavirus by the NHS. 'Expects to get COVID' has a reference period of the next month while 'expects financial difficulties' the next three months.

### 3.3 The Uses of Unspent Payments

In Table 3, we take the sample of those who have an MPC of less than one, and analyse the use of the money that is not spent. Respondents were asked if, on receipt of a £500 payment, they would pay off debt, save more, give more financial help to friends and family, or receive less financial

help from friends or family (respondents were invited to indicate all that apply). Columns 1 and 2 of Table 3 report the fractions who would reduce debt and the fraction who would save. Columns 3 and 4 report the hypothetical effect of an unexpected one-time income receipt on financial transfers to and from family and friends. The single most reported use of unspent payments is saving, which was indicated by 66% of the 93% of respondents with MPCs less than one (or 61% overall), but 22% reported they would pay down debt. Crossley, Fisher, and Low (2021) shows that financial assistance between friends and family has been an important mechanism by which people in the UK have coped with financial hardship associated with the pandemic: 15% of households received transfers from friends in the three months after March, and 12% made such transfers. Table 3 shows that 9% report an increase in the transfers they would make, and 3% of people report that their financial transfers would be reduced by the £500 payment.

There are substantial differences in the report use of unspent receipts by observable characteristics. Lower income individuals are more likely to report reducing debts: 26% of those in the lowest income quintile report they would pay down debt compared to 18% in the richest quintile. Further, lower income individuals are less likely to report saving any of the additional funds (53% of those in the bottom quintile compared to 73% in the top quintile). Those with children are also much more likely to report paying down debt than those without, as are those who do not own their homes outright or who rent, and those of working age individuals relative to those of retirement age. Single parents and those in the lowest income quintiles are most likely to receive reduced private transfers: in other words, the additional income payment crowds out private support. Those over 66 or owning their own home are most likely to report that they would use the payment to give more financial support to friends or family.

To further explore these associations, we ran probit regressions of binary indicators for different uses of any unspent funds on individual characteristics. These are reported in Table 4. For each outcome, we report two specifications. The first specification includes characteristics measured pre-COVID only, including long run income quintile. The second includes variables capturing individuals' circumstances and expectations in July 2020.

All else equal, working age individuals, those with children and those who do not own their homes outright (including renters) are more likely to use unspent funds to reduce debt. These same groups are also less likely to report saving unspent funds, with the exception of those of working age. Those of working age, those in the bottom income quintile and those who do not own their homes are more likely to report that the receipt of funds would lead to reduced transfers from other households, and that they would increase transfers to other households. Turning to

covariates measured in July 2020, those who do not report saving, those currently in arrears and those expecting future financial difficulties are more likely to use their unspent funds to reduce debts (and less likely to use these funds to increase savings). Those expecting financial difficulties and those those who are no longer saving are more likely to see a decline in transfers from other households. Those who reduced their hours worked are significantly less likely to transfer unspent funds to other households.

## 4 Discussion

One policy option to boost spending during COVID is a lump-sum income transfer, or alternatively a tax credit, that is paid to households. This was carried out in the US with payments received in April - May 2020 (Coibion et al. (2020)). In this paper, we provide evidence on the implications for private spending in the UK of making such a payment in the middle of the pandemic by directly eliciting marginal propensities to consume from individuals in the UK in July 2020. Individuals were asked how their spending would change in response to an unexpected one-time payment of £500 payment.

Our key conclusion is that the fraction of households that would spend part of such a payment is low: only 19% say they would increase spending at all as a result. Further, the percentage of payments that would be spent, as measured by marginal propensities to consume, is very small at 11% on average and we do not find an average MPC above 15 % for any socioeconomic group.

The permanent income hypothesis predicts small spending responses to transitory income shocks. One straightforward interpretation of our results is that the majority of individuals behave in a way consistent with this theory. However, the average MPC we find is much lower than that reported in other studies that directly elicit MPCs. It is natural to ask why this is the case. MPCs may have been low because of the pandemic or responses to it. In July of 2020 the UK had come out of lockdown and many supply constraints and travel and commercial restrictions had been lifted. Nevertheless, economic uncertainty or perceived health risk are candidate explanations for the low report MPCs. However, we find little evidence in support of either hypotheses. Our data indicate that few individuals considered themselves at high risk of infection at the time of the survey. Moreover, when we look at the average MPC for those who consider themselves likely to get COVID, or who have been told by the authorities to “shield” (for example because of a pre-existing health condition) we see very modest differences in MPCs.

Another possibility is that either the constraints on spending due to the first lockdown in the

spring of 2020, or policies to support incomes through the pandemic, increased liquidity among those who might otherwise had higher MPCs. It is certainly the case that the aggregate saving rate in the UK rose significantly in the spring of 2020 (Office for National Statistics (2020)). However, in other work (Crossley, Fisher, Levell, and Low (2021)) we have shown that the extra saving was concentrated among affluent individuals and households, whom we would expect to have low MPCs in any event. Further, our analysis in this paper shows very little variation in average MPCs between savers and nonsavers in July 2020. In contrast to the U.S., pandemic income support policies in the U.K. did not focus on direct payments to individuals and households. There was an increase of £20 per week in Universal Credit (the main out of work benefit) but the largest expenditure was through the Coronavirus Job Retention Scheme (or “furlough” scheme) which was a wage subsidy aimed at preserving employment relationships. Moreover we do not find higher MPCs for those who nevertheless lost their jobs.

A third possibility is that our low estimates of average MPCs are due either to something about the sample or the way the question was asked. There was significant nonresponse to the COVID-19 study. However, as noted above, the sample is ultimately based on probability samples, and nonresponse has been very carefully modelled using the very rich set of observables available from the Main Study. The resulting combination of data and inverse-probability weights has been tested and shown capable of hitting many population targets (including variables not included in the non-response modelling). Moreover, although the weights do significantly adjust the socio-demographic composition of the sample, weighting has a very small effect on the average MPC (raising it by 1 percentage point). Given that the correction for selection on a very rich set of observables leads to such a small change in the average MPC, we conclude that sample selection is not a credible explanation for our low average MPC. The COVID-19 study asked about a hypothetical payment of £500 and spending over a three month horizon. Some previous surveys asked about larger windfalls or longer time horizons, and size and horizon effects have been documented in this literature. However, where these effects have been estimated in a controlled way, they are not large. For example, Fuster, Kaplan, and Zafar (2021) randomly allocate respondents to hypothetical payments of \$500, \$2500, and \$5000 and find that going from the smallest to the largest hypothetical payment raises the average reported MPC only by 5 percentage points. Moreover, we think the payment size and spending horizon in the COVID-19 Study are appropriate to realistic policy scenarios.

One aspect of the hypothetical spending questions posed in the COVID-19 Study maybe important. Respondents are first asked about the extensive margin (whether spending would increase, decrease or stay the same) and then, if spending would increase or decrease, by how much. These

questions follow the pattern adopted by the New York Fed Survey of Consumer Expectations. In those data, Fuster, Kaplan, and Zafar (2021) estimate average MPCs at the lower end of the range reported in the prior literature. Fuster, Kaplan, and Zafar (2021) postulate that this neutral formulation does not prime respondents to give a positive spending amount (in contrast to asking directly “how much would you spend”).

Finally, a striking feature our analysis of the hypothetical uses of unspent windfall income is the strong desire to pay down debt, particularly among individuals that theory and the prior literature would suggest should have high MPCs. We find that the desire to pay down debt is particularly high among single parents, among “never-savers”, among those in arrears, and among those expecting financial difficulties over the last three months. In other work (Crossley, Fisher, and Low (2021), Crossley et al. (2021)), we have shown that the economic consequences of the pandemic in the U.K. tended, at least initially, to exacerbate pre-existing inequalities. The top of the long-run income distribution increased saving, while the bottom of the distribution took on debt.

In the end, we think the two most credible explanations for the low average MPC in these data are the neutral wording of the question, and the possibility that the economic uncertainties associated the pandemic induced a strong desire to deleverage among those at the bottom of the distribution, and this kept their MPCs low. For now, these remain hypotheses. We are aware of no randomized test of a neutral, two-part MPC question against alternatives, and we have no pre-pandemic baseline data on the desire to deleverage.

Importantly though, in contrast to what has been suggested elsewhere (for example, Alon et al. (2020)), we find no evidence that MPCs were high during the pandemic, at least in the UK. This suggests that income transfers would not be an effective way to stimulate spending. Even those who expect financial difficulties in the immediate future, or anticipate job loss, do not expect to spend the additional payments over this period, and rather choose to pay off debts. This decision is despite the relaxation of opportunities to spend at the time of the Study.

In addition to paying off debts or saving, the £500 payments induce changes in private transfers. For those that have not spent all of the payment, we find that 12% report that their financial transfers would be affected by the £500 payment, either giving more financial support to family or receiving less. Single parents and those in the lowest income quintiles are most likely to receive less transfers, in other words the additional income payment crowds out private support. Those over 66 or owning their own home are most likely to report that they would use the payment to give more financial support to friends or family.

The fact that one-time payments affect transfer behaviour has important implications for policy. In particular, it means that the aggregate MPC (the amount of a stimulus that is spent) need not be equal to the average MPC, even if the cash stimulus is distributed equally. If low MPC individuals (such as older people) pass on some or all of the stimulus to higher MPC individuals, this can raise the aggregate MPC above the average. On the other hand, the crowding out of private transfers will have the opposite effect so that high MPC households receive less (net of private transfers) than the additional income payment, and low MPC individuals households receive more. This would reduce the aggregate MPC. Crowding out of private transfers could also mean that stimulus payments targeted at groups with high individual MPCs increase spending less than predicted.

Our data do not allow us to fully characterise how these effects on transfer payments will translate into the aggregate MPC, but this is an important question for future research. Nevertheless, given the modest fraction whose transfers change, and the very low MPCs across all groups in our data, the aggregate MPC, like the average MPC, was likely to be very low through this crisis.

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Table 3: Use of amount not spent (if any not spent)

|   | Reduce Debt | Save | Give More | Receive Less |
|---|-------------|------|-----------|--------------|
| <b>All spending &lt;£500</b>                    | 0.22        | 0.66 | 0.09      | 0.03         |
| <i>All spending &lt;£500 (unweighted)</i>       | 0.20        | 0.68 | 0.10      | 0.02         |
| <b>Gender:</b> Men                              | 0.21        | 0.68 | 0.07      | 0.02         |
| Women   | 0.23        | 0.64 | 0.10      | 0.03         |
| <b>Age:</b> 19-65                               | 0.26        | 0.64 | 0.07      | 0.03         |
| 66+   | 0.08        | 0.71 | 0.16      | 0.01         |
| <b>Education:</b> GCSE or lower                 | 0.22        | 0.62 | 0.09      | 0.03         |
| A-level   | 0.23        | 0.68 | 0.08      | 0.03         |
| Degree  | 0.21        | 0.68 | 0.10      | 0.02         |
| <b>Family type:</b> Couple, child               | 0.35        | 0.57 | 0.05      | 0.03         |
| Couple, no child                                | 0.14        | 0.71 | 0.11      | 0.02         |
| Single, child                                   | 0.44        | 0.42 | 0.07      | 0.06         |
| Single, no child                                | 0.19        | 0.69 | 0.09      | 0.03         |
| <b>Housing (2018-19):</b> Not owned             | 0.29        | 0.61 | 0.06      | 0.04         |
| Owned   | 0.08        | 0.74 | 0.14      | 0.01         |
| <b>Income quintile (2018-19):</b> q1            | 0.26        | 0.53 | 0.11      | 0.07         |
| q2  | 0.24        | 0.63 | 0.10      | 0.03         |
| q3  | 0.25        | 0.66 | 0.07      | 0.01         |
| q4  | 0.19        | 0.70 | 0.08      | 0.02         |
| q5  | 0.18        | 0.73 | 0.09      | 0.00         |
| <b>Labour shock:</b> No shock                   | 0.21        | 0.66 | 0.10      | 0.02         |
| Hours loss                                      | 0.29        | 0.63 | 0.05      | 0.04         |
| Job loss  | 0.23        | 0.62 | 0.08      | 0.07         |
| <b>Saver:</b> Never saver                       | 0.31        | 0.51 | 0.09      | 0.05         |
| New saver                                       | 0.21        | 0.74 | 0.09      | 0.01         |
| Always saver                                    | 0.13        | 0.79 | 0.09      | 0.00         |
| Was saver                                       | 0.21        | 0.67 | 0.09      | 0.04         |
| <b>Arrears:</b> In arrears                      | 0.51        | 0.33 | 0.07      | 0.07         |
| Up to date                                      | 0.19        | 0.69 | 0.09      | 0.02         |
| <b>Expects financial difficulty:</b> 0 % chance | 0.14        | 0.72 | 0.10      | 0.01         |
| > 0 & ≤ 100 % chance                            | 0.36        | 0.55 | 0.07      | 0.06         |
| <b>COVID shielding:</b> Shielding               | 0.17        | 0.61 | 0.14      | 0.04         |
| Not shielding                                   | 0.22        | 0.66 | 0.09      | 0.03         |
| <b>Expects to get COVID:</b> Likely             | 0.30        | 0.60 | 0.10      | 0.06         |
| Unlikely  | 0.23        | 0.66 | 0.08      | 0.03         |
| Very unlikely                                   | 0.19        | 0.66 | 0.10      | 0.03         |

*Notes:* Sample size ('All') is 9,844. Statistics are weighted for nonresponse. Respondents can report multiple uses, so the columns do not sum to one. Income quintiles are of household incomes that were collected in 2017-18 (wave 9) and are equivalised using the OECD modified scale. The variables 'Labour shock', 'arrears', 'expects financial difficulties', 'COVID shielding' and 'expects to get COVID' are collected in the July 2020 survey. The 'Labour shock' categories are defined according to changes between February and July 2020, where an hours loss is defined as working positive hours in February and being employed but working zero hours in July. 'Saver' categories are defined according to saving behaviour in 2018-19 (wave 10) and July 2020. 'COVID shielding' refers to being identified as someone at risk of severe illness from coronavirus by the NHS. 'Expects to get COVID' has a reference period of the next month while 'expects financial difficulties' the next three months.

Table 4: Use of amount not spent, Probit marginal effects

|                         | Reduce debt       |                   | Save               |                    | Receive less      |                  | Give more          |                    |
|-------------------------|-------------------|-------------------|--------------------|--------------------|-------------------|------------------|--------------------|--------------------|
| Female                  | 0.01<br>(0.01)    | 0.02<br>(0.01)    | -0.03<br>(0.02)    | -0.04*<br>(0.02)   | 0.00<br>(0.00)    | 0.00<br>(0.00)   | 0.03***<br>(0.01)  | 0.03***<br>(0.01)  |
| Age 19-65               | 0.10***<br>(0.02) | 0.08***<br>(0.02) | 0.00<br>(0.02)     | 0.01<br>(0.02)     | 0.02***<br>(0.00) | 0.01**<br>(0.00) | -0.06***<br>(0.01) | -0.05***<br>(0.01) |
| GCSE or lower           | 0.00<br>(0.02)    | -0.02<br>(0.02)   | -0.05*<br>(0.02)   | -0.03<br>(0.02)    | -0.00<br>(0.01)   | 0.00<br>(0.00)   | -0.01<br>(0.01)    | -0.01<br>(0.01)    |
| Degree                  | -0.01<br>(0.02)   | -0.00<br>(0.02)   | -0.00<br>(0.02)    | -0.02<br>(0.02)    | -0.00<br>(0.01)   | -0.00<br>(0.00)  | 0.01<br>(0.01)     | 0.01<br>(0.01)     |
| Couple, child           | 0.10***<br>(0.02) | 0.09***<br>(0.02) | -0.11***<br>(0.02) | -0.10***<br>(0.02) | -0.01<br>(0.00)   | -0.01*<br>(0.00) | -0.01<br>(0.02)    | -0.01<br>(0.02)    |
| Couple, no child        | 0.00<br>(0.02)    | 0.01<br>(0.02)    | -0.03<br>(0.02)    | -0.02<br>(0.02)    | 0.00<br>(0.01)    | -0.00<br>(0.00)  | 0.01<br>(0.01)     | 0.01<br>(0.01)     |
| Single, child           | 0.17***<br>(0.05) | 0.13**<br>(0.05)  | -0.22***<br>(0.05) | -0.16**<br>(0.05)  | 0.00<br>(0.01)    | 0.00<br>(0.01)   | -0.02<br>(0.03)    | 0.01<br>(0.03)     |
| Home not owned outright | 0.15***<br>(0.01) | 0.12***<br>(0.01) | -0.10***<br>(0.02) | -0.05*<br>(0.02)   | 0.01**<br>(0.00)  | 0.01<br>(0.00)   | -0.04**<br>(0.01)  | -0.04**<br>(0.01)  |
| Income q1               | 0.02<br>(0.03)    |                   | -0.12***<br>(0.03) |                    | 0.05**<br>(0.02)  |                  | 0.05*<br>(0.02)    |                    |
| Income q2               | -0.01<br>(0.02)   |                   | -0.02<br>(0.03)    |                    | 0.02<br>(0.01)    |                  | 0.03*<br>(0.02)    |                    |
| Income q4               | -0.03<br>(0.02)   |                   | 0.02<br>(0.03)     |                    | 0.01<br>(0.01)    |                  | 0.00<br>(0.01)     |                    |
| Income q5               | -0.03<br>(0.02)   |                   | 0.03<br>(0.02)     |                    | -0.01**<br>(0.00) |                  | 0.01<br>(0.01)     |                    |

Table 4: Use of amount not spent, Probit marginal effects (Cont'd)

|                              | Reduce debt |      | Save     |      | Receive less |      | Give more |      |
|------------------------------|-------------|------|----------|------|--------------|------|-----------|------|
| Hours loss                   | -0.01       |      | 0.03     |      | 0.01         |      | -0.04***  |      |
|                              | (0.02)      |      | (0.03)   |      | (0.00)       |      | (0.01)    |      |
| Job loss                     | -0.01       |      | -0.01    |      | 0.03         |      | -0.03     |      |
|                              | (0.04)      |      | (0.05)   |      | (0.02)       |      | (0.02)    |      |
| Never saver                  | 0.07**      |      | -0.12*** |      | 0.00         |      | 0.00      |      |
|                              | (0.02)      |      | (0.03)   |      | (0.00)       |      | (0.01)    |      |
| New saver                    | 0.01        |      | 0.05*    |      | -0.01***     |      | -0.01     |      |
|                              | (0.03)      |      | (0.03)   |      | (0.00)       |      | (0.01)    |      |
| Always saver                 | -0.06**     |      | 0.10***  |      | -0.02***     |      | -0.01     |      |
|                              | (0.02)      |      | (0.02)   |      | (0.00)       |      | (0.01)    |      |
| In arrears                   | 0.12***     |      | -0.26*** |      | 0.00         |      | 0.03      |      |
|                              | (0.04)      |      | (0.04)   |      | (0.00)       |      | (0.03)    |      |
| Expects financial difficulty | 0.13***     |      | -0.10*** |      | 0.02***      |      | -0.01     |      |
|                              | (0.02)      |      | (0.02)   |      | (0.01)       |      | (0.01)    |      |
| Shielding                    | -0.02       |      | -0.04    |      | -0.00        |      | 0.05      |      |
|                              | (0.03)      |      | (0.04)   |      | (0.00)       |      | (0.03)    |      |
| Likely get COVID             | -0.01       |      | 0.01     |      | 0.01         |      | 0.03      |      |
|                              | (0.04)      |      | (0.06)   |      | (0.01)       |      | (0.03)    |      |
| Very unlikely get COVID      | -0.01       |      | -0.01    |      | 0.01         |      | 0.00      |      |
|                              | (0.01)      |      | (0.02)   |      | (0.00)       |      | (0.01)    |      |
| Observations                 | 9605        | 8829 | 9605     | 8829 | 9605         | 8829 | 9605      | 8829 |

*Notes:* Presented are marginal effects for a discrete change of dummy variable from 0 to 1. Standard errors in parentheses. \*  $p < 0.05$ , \*\*  $p < 0.01$ , \*\*\*  $p < 0.001$ . Regressions are weighted for nonresponse. Omitted categories are: male; age 66+; educated to a-level; single with no child; income quintile three; 'no (labour market) shock'; 'never saver'; 'not in arrears', '0 % chance of financial difficulty', 'not shielding' and 'unlikely to get COVID'. MPCs are calculated on the basis of questions asking if spending would change upon receipt of £500 and by how much. MPCs are trimmed to be at least zero and at most one. Income quintiles are of household incomes that were collected in 2017-18 (wave 9) and are equivalised using the OECD modified scale. The variables 'Labour shock', 'arrears', 'expects financial difficulties', 'COVID shielding' and 'expects to get COVID' are collected in the July 2020 survey. The 'Labour shock' categories are defined according to changes between February and July 2020, where an hours loss is defined as working positive hours in February and being employed but working zero hours in July. 'Saver' categories are defined according to saving behaviour in 2018-19 (wave 10) and July 2020. 'COVID shielding' refers to being identified as someone at risk of severe illness from coronavirus by the NHS. 'Expects to get COVID' has a reference period of the next month while 'expects financial difficulties' the next three months.

**Declaration of interests**

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

The authors declare the following financial interests/personal relationships which may be considered as potential competing interests:

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