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Marginal Propensity to Consume in Recessions: a Meta-analysis*

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Abstract

This paper assembles a dataset of 1244 estimates of marginal propensities to consume (MPC) out of stimulus checks and other small transitory or predictable payments—as reported by 40 studies. I use meta-regressions to uncover the sources of systematic variation in estimates and provide fitted MPC for a number of policy-relevant scenarios. An increase in unemployment by one percentage point is associated with an MPC estimate that is higher by 4-5 percentage points. MPC estimates systematically vary depending on payment characteristics: they decrease with the size of the payments; MPCs out of stimulus checks are higher than those out of some other payments that are recurring. MPCs are lower for households holding ample liquidity. These results imply that the effects of stimulus payments and other policy interventions crucially depend on the circumstances and the manner in which funds are disbursed, and highlight the importance of considering state-dependent multipliers, liquidity constraints, near rationality and mental accounting.

Keywords: Consumption, household finance, marginal propensity to consume, fiscal stimulus, liquidity constraints.

JEL Codes: D12, E21, H31, G4

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1 Introduction

When recessions hit, policymakers act. In 2001, in the aftermath of the dot-com crash the US government disbursed about 38 billion dollars worth of tax rebates to US consumers in payments of $300-$600 per household.[1] In 2008, at the height of the Great Recession the government intervened with about 100 billion dollars of Economic Stimulus payments, sending US households checks of between $300-$600 per adult.[2] In 2020, once again, amid the COVID-19 pandemic and a global economic collapse, the government stepped in—this time, with payments of $1200 per adult, totaling about 300 billion dollars.[3] These measures were followed by two more rounds of Economic Impact Payments in 2021.[4]

But how much do these measures actually help the economy? The answer to this question depends on the fraction of the check that gets consumed by the households, that is, on the households' Marginal Propensity to Consume (MPC). In general, the larger the MPC, the stronger the partial equilibrium effect these measures can have on aggregate demand. For all three interventions mentioned above, there is evidence that households did respond with an increase in consumption—however, there is variation in the estimates of the corresponding MPCs.[5]

Interpreting the differences in reported MPCs is not straightforward. On the one hand, it is possible that average consumption responses were fundamentally different across the policy experiments due to unique circumstances in which the money was disbursed: the depth of the crises at the time and the sizes of the payments households received were not the same. If that is the case, then this variation needs to be studied and understood in order to improve future policy outcomes. On the other hand, the observed variation in estimates could stem from the differences in how the studies were conducted. For example, some of the more recent studies use high frequency financial account data, whereas others rely on surveys—if one data tends to produce systematically different results, this could explain part of the observed variation.[6] It is difficult to shed light on this issue by examining a single policy experiment: typically, the amounts of money disbursed per individual do not randomly vary across population; furthermore, the disbursement happens over a relatively short period of time without much variation in the aggregate economic conditions. While a narrative approach of comparing results ob-

[1] This was implemented as part of the Economic Growth and Tax Relief Reconciliation Act. See details in e.g. Shapiro & Slemrod (2003).

[2] The money was disbursed through the Economic Stimulus Act. See details in e.g. Sahm *et al.* (2010).

[3] This followed the adoption of the CARES act. See more details in e.g. Karger & Rajan (2020).

[4] Second round was disbursed through the COVID-related Tax Relief Act of 2020 (with checks of $600 per eligible adult), the third round followed the adoption of the American Rescue Plan Act of 2021 (with payments of $1400 per person). See details at home.treasury.gov.

[5] Johnson *et al.* (2006) find that consumers spent between 20 to 40 percent of the 2001 tax rebates on nondurable consumption during the quarter in which the checks arrived, while Parker *et al.* (2013) document the quarterly MPC out of the 2008 Economic Stimulus Payments to be between 12 to 30 percent. For the CARES act payments of 2020, there is evidence of a more prominent response: Baker *et al.* (2020) find the spending MPC of .25-.3 in the 10 days following check arrival, while Karger & Rajan (2020) document an MPC of .46 for the two weeks after check receipt.

[6] For example, Johnson *et al.* (2006) and Parker *et al.* (2013) use data from household surveys, while Baker *et al.* (2020) and Karger & Rajan (2020) use high frequency financial account data.

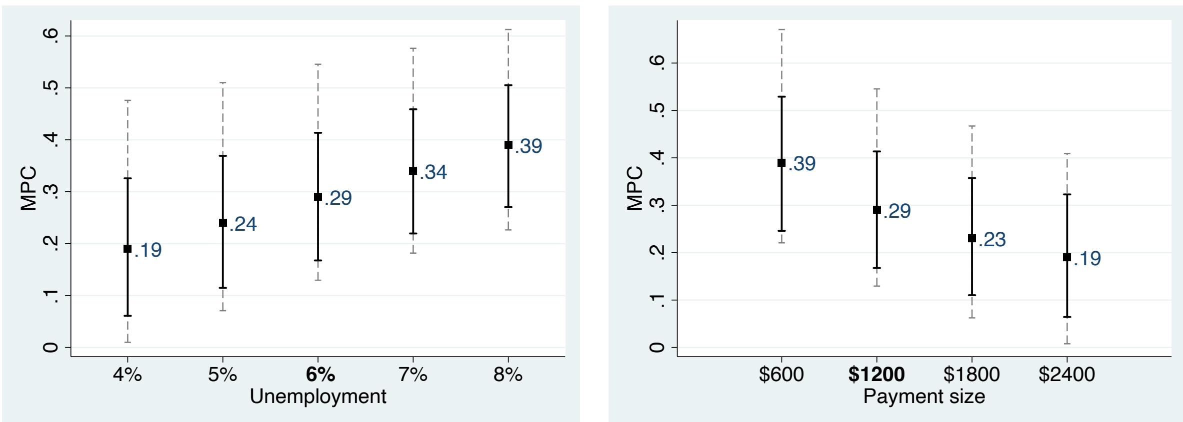
tained by different studies can be helpful, it cannot, ultimately, establish sources of systematic variation in estimates, nor provide quantitative policy recommendations.

In this paper I address this problem with a quantitative approach, by pooling MPC estimates produced by studies that examine different time periods and payments of different sizes. I include MPCs out of stimulus checks (disbursed during times of relatively high unemployment), as well as MPCs out of other payments with similar properties (disbursement of which is not connected to the phase of the business cycle). Specifically, I include estimates of MPC out of payments that, similar to the stimulus checks, should not affect consumers' expected lifetime income upon receipt—that is, payments that are either small and transitory or anticipated in advance. I thus obtain a sample of 1244 estimates from 40 studies. I ask whether the MPC estimates depend systematically on the circumstances in which the corresponding payments are received, and use meta-regression analysis to pin down the sources of this variation.

I find that the reported marginal propensities to consume vary systematically with the unemployment rate at the time of payment disbursement. An increase in the unemployment rate by one percentage point leads to a quarterly MPC that is higher by about 4-5 percentage points. Figure 1(a) plots the fitted estimates of MPC out of stimulus checks for nondurable consumption against the unemployment rate. An unemployment rate of 4% corresponds to an estimated three-month MPC of around .19, while for an 8% unemployment rate the MPC estimate doubles, becoming .39.

Figure 1: Fitted 'best practice' MPC. Unemployment rates and Payment sizes

(a) MPC and *Unemployment* (b) MPC and *Payment size*



Notes: The figure shows point estimates of quarterly MPC based on the meta-regression model described in the right panel of Table 5, see discussion in Section 4. The baseline estimates denoted in bold reflect the marginal propensities to raise nondurable consumption upon receiving a $1200 stimulus check, conditional on unemployment rate of 6%, and on the estimate being obtained with US data and reported in a widely cited study. Estimates not marked in bold have the same interpretation except for the dimensions denoted on the horizontal axes of the graphs. The two sets of bands reflect 95% confidence intervals, the narrower solid band is computed with conventional clustering at the study level, the wider dashed band uses wild bootstrap cluster—see details in Section 3.

The connection between MPCs and the associated unemployment rates may arise because of tighter liquidity constraints in recessions. When unemployment is on the rise, more households lose regular paycheck income and are thus forced to finance consumption by spending their accumulated liquid assets. In these circumstances a payment receipt elicits stronger average consumption response, as households that are short on cash use it to catch up consumption to the levels in line with their expected lifetime income. This interpretation is supported by further findings in the present paper, suggesting that MPC estimates are lower for households holding ample liquid assets. These results are also in line with Gross *et al.* (2020) who document a countercyclical marginal propensity to consume out of changes in credit card limits.

Estimates of MPC out of salient payments tend to be lower compared to those out of payments that are small. For example, marginal propensities to raise nondurable consumption upon receiving stimulus checks of \$600, \$1200, \$1800 are estimated to be around .39, .29, .23, respectively—see fitted estimates on Figure 1b. This finding is in line with Fagereng *et al.* (2021) and Olafsson & Pagel (2021) who report that marginal propensities to consume out of lottery winnings decline with the size of the payment. It is also in line with Havránek & Sokolova (2020), who document a similar result for a subset of 17 papers included in the present dataset. One explanation for the link between MPCs and payment sizes is that re-calculating the optimal consumption path may be costly—households may optimally choose to do so for large payments, but not for payments that are small (see Reis 2006). Another possibility is that households may behave near-rationally, following the prescriptions of the canonical rational agent model only when the deviations from the optimal path are too costly in terms of welfare losses, and consuming a constant share of transitory income otherwise, see Caballero (1995), Browning & Crossley (2001). Either feature would lead to small payments getting mostly consumed, and large payments being mostly saved.

Estimates of marginal propensities to consume differ depending on the types of payments under consideration. First, there is evidence suggesting that consumption response to stimulus check receipt is greater than that following receipt of payments that are recurring (i.e. tax refunds, changes in regular payments and other recurring payments). At the same time, I do not find much evidence of systematic differences between MPC out of stimulus checks and MPC out of unemployment benefits or lottery winnings. Second, there seems to be an asymmetry between consumption responses to increases and decreases in income: the MPC out of income declines is lower.

It therefore appears that consumers respond differently to an income receipt depending on its source and characteristics—payments received through different sources are not fungible. One possible explanation is that households use mental accounting when making consumption decisions (see Thaler 1999). Households may assign different uses to income coming from different sources, mostly saving some cash inflows (e.g. tax refunds) and mostly consuming others (e.g. stimulus checks). Furthermore, if preferences are characterized by loss aversion (Tversky & Kahneman 1991), then consumers may be more eager to smooth out income declines than income increases, which would explain larger consumption fluctuations when income rises. This reasoning echoes conclusions of Card & Ransom (2011) and Baugh *et al.* (2021).

Baugh *et al.* (2021) document an asymmetry in consumption response to income increases and declines and argue that the behaviors they observe can be best explained through mental accounting. Card & Ransom (2011) find that college and university professors use mental accounting when making saving decisions.

Reported marginal propensities to consume depend on the consumption group they pertain to. Generally, the more broad the definition of consumption, the more prominent the consumption response—which should be the case by construction.[7] In the US, for a stimulus check of $1200 and given unemployment rate of 6%, marginal propensities to raise consumption of food, nondurables and total consumption are estimated at .08, .29 and .47, respectively. For countries other than the US, consumption responses to stimulus checks are somewhat less prominent: under the above conditions, the marginal propensity to raise nondurable consumption would only be about .18. Finally, there is some (weak) evidence suggesting that studies using account data may come up with estimates that are higher compared to studies relying on data from other sources (such as survey data).

The fitted MPC estimates calculated in this paper characterize only a partial equilibrium consumption response—this is a limitation for both this paper and the literature it investigates. To glean the general equilibrium implications, one must consider multiplier effects. Some of the 40 studies investigated in this paper are often cited when calibrating Heterogeneous Agent New Keynesian (HANK) models; the calibrated models are then used to gauge the effects of fiscal and monetary policy (e.g. Kaplan *et al.* 2018, Auclert *et al.* 2020). The fitted estimates reported in this paper can be used as additional reference for such calibrations, as they reflect the entirety of the relevant empirical evidence. Furthermore, the connection between the average MPC and the unemployment rate uncovered here is one more argument in favor of considering state-dependent multipliers that vary with the phase of the business cycle (see discussion in Parker 2011 and empirical evidence in Auerbach & Gorodnichenko 2012).

The results reported here are obtained within the framework of meta-analysis. Meta-analysis has been used to study a large number of topics in economics—examples include the link between minimum wage and employment (Card & Krueger 1995, Doucouliagos & Stanley 2009), the impacts of active labor market programs (Card *et al.* 2018), monopsony in labor markets (Sokolova & Sorensen 2021), the elasticity of intertemporal substitution and habit formation in consumption (Havránek 2015, Havránek *et al.* 2017), and the effects of IMF programs on economic growth (Balima & Sokolova 2021).

The present work is most closely related to the meta-analysis of Havránek & Sokolova (2020) who study evidence of excess sensitivity in consumption to predictable changes in income. Out of the 144 studies examined by Havránek & Sokolova (2020), 17 use observable plausibly exogenous variation in income to estimate marginal propensities to consume. The present paper includes evidence from these 17 studies, but conducts analysis that is different along a number of important dimensions.[8] First, the present study of MPC estimates is larger in scale: in addition

[7] Havránek & Sokolova (2020) document similar variation in MPC estimates across consumption groups.

[8] The majority of estimates examined in Havránek & Sokolova (2020) cannot be used for the purposes of the present paper: they come from studies that use instruments to predict changes in income, and then examine

to the 17 papers mentioned above I include 23 other studies that provide estimates with similar interpretation, effectively doubling the sample size. Second, this study investigates a number of policy-relevant problems not considered in Havránek & Sokolova (2020) (e.g. cyclical properties of MPC and the differences across payment types) and models variation in MPC estimates with a finer level of detail, capturing variation across estimates with 30 control variables (as opposed to 18 controls in Havránek & Sokolova 2020). Third, the present study provides fitted MPC estimates for a number of policy-relevant scenarios.

The paper is organized as follows. Section 2 details the data collection strategy and describes the resulting dataset. Section 3 studies the sources of systematic variation in MPC estimates. I build and evaluate a number of empirical models, addressing model uncertainty with the frequentist approach as well as with Bayesian Model Averaging. I discuss the estimation results and what they imply for our understanding of variation in MPC estimates reported by the literature. Section 4 constructs fitted MPC estimates for a number of scenarios, based on the entirety of the empirical literature on the topic; policy implications are discussed. Section 5 summarizes key takeaways.

2 Data

One way to gauge how households spend cash inflows is to estimate the following specification (as in e.g. Johnson *et al.* 2006, Parker *et al.* 2013):

$$\Delta C_{i,t+1} = mpc \cdot Payment_{i,t+1} + \sum_s \beta_{1s} \cdot month_{i,s} + \beta_2' X_{i,t} + \epsilon_{i,t+1} \tag{1}$$

where $C_{i,t+1}$ is consumption of household i in period $t+1$, $Payment_{i,t+1}$ is the dollar amount of the payment the household receives, $month_{i,s}$ are controls meant to absorb seasonality, $X_{i,t}$ are other controls, $\epsilon_{i,t+1}$ is noise. The parameter mpc captures the marginal propensity to consume, i.e. by how many dollars households raise their consumption upon receiving one extra dollar—within the specified time period (e.g. a quarter).

If households are rational and unburdened by liquidity constraints, their consumption should equal a fraction of their expected lifetime income. For such households, payments that do not have a prominent effect on the expected lifetime income—that is, predictable payments or payments that are relatively small and transitory—should not elicit a strong consumption response. Therefore, if all households behaved rationally and had ample liquidity to smooth out income fluctuations, payments like fiscal stimulus checks would have a very modest effect on economic activity, and the estimate of mpc in regression (1) would be close to zero.

Yet, many studies find evidence of sizable positive MPCs in response to both receiving a stimulus check and to changes in other predictable or small transitory payments.[9] For example, Johnson *et al.* (2006) and Agarwal *et al.* (2007), Parker *et al.* (2013), Broda & Parker (2014)

whether consumption responds to these predictable changes. These studies test the permanent income hypothesis rather than provide estimates of marginal propensities to consume.

[9] These observed patterns gave rise to models that feature hand-to-mouth households whose consumption largely tracks current income. Uncovering the drivers behind the hand-to-mouth behavior—such as bounded

document prominent consumption responses to 2001 and 2008 stimulus payments; Souleles (1999), Baugh *et al.* (2021) and Gelman (2021b) observe that consumers respond to receiving tax refunds. A number of studies find systematic responses to receiving regular income, such as social security payments (Stephens 2003) and paychecks (e.g. Gelman *et al.* 2014, Baker & Yannelis 2017, Olafsson & Pagel 2018). One explanation for these findings is that the premiss discussed above is wrong: at least some households are either liquidity constrained, or not fully rational, or both.[10]

The goal of this paper is to gauge MPCs out of income changes that should not generate a large consumption response within the context of the life cycle/buffer stock saving models—unless the households face binding liquidity constraints (see e.g. Deaton 1991, Carroll 1997). There are several types of data that can be used to produce MPCs out of payments with these properties. As discussed above, there are studies that estimate households' response to one-time stimulus payments. Aside from the works examining US policy experiments, there are also studies of stimulus tax rebates in Japan (LaPoint & Unayama 2020) and growth dividends in Singapore (Agarwal & Qian 2014).

In addition to the stimulus studies, there are other works examining predictable or small and transitory payments. Examples include papers that study consumption responses to other one-time or temporary income, such as lottery winnings (Fagereng *et al.* 2021, Olafsson & Pagel 2021) and unemployment benefits (Ganong & Noel 2019, Gerard & Naritomi 2021); payments that are regular and should be perfectly predictable by the households, such as regular tax refunds/payments (e.g. Gelman 2021b, Souleles 1999, Baugh *et al.* 2021), Alaska permanent fund payments (Hsieh 2003, Kueng 2018), social security and paycheck receipts (Stephens 2003, Gelman *et al.* 2014, Olafsson & Pagel 2018, Ganong *et al.* 2020, Gelman 2021a); changes in mortgage and tax payments (e.g. Jappelli & Scognamiglio 2018, Surico & Trezzi 2018). All of these and similar income changes, like the stimulus checks, should not have a strong effect on consumption within the context of conventional models.

I collect all estimates of *mpc* that characterize consumption response to receipt of such payments; the full list of payment types and corresponding studies is available in Table A2. I only collect estimates for which the authors report some measure of precision, such that the associated standard error can be deduced, and estimates for which there is some information about the corresponding payment amount.[11] I only include MPCs out of observable plausibly exogenous income changes for which there is a clearly defined horizon (e.g. consumption response over a day, a month, a quarter). I thereby exclude a prominent part of the literature

rationality, binding liquidity constraints, heterogeneity in preference parameters—is one of the key challenges in the literature (see e.g. Kaplan *et al.* 2014, Aguiar *et al.* 2020).

[10] Parker (2017) points out that the same households may both exhibit rule-of-thumb-like behavior and face binding liquidity constraints due to persistent household traits, such as preferences or behavioral patterns.

[11] The precise information regarding the payment amount is not always available—I include proxies for the payment amount when possible. For example, there are many MPC estimates calculated for sample splits of the original dataset; while there is typically information about the payment amount for the overall dataset, there may not be information specific to each subsample. In such cases I use payment size for the original dataset as proxy for subsamples, provided that the subsampling is unlikely to be highly correlated with the payment size. If high correlation is suspected (e.g. as in paycheck size with subsampling by income) and precise information about payment amount is not available, the corresponding MPC estimate is dropped.

that examines MPC out of *hypothetical* income changes obtained through household surveys (e.g. Christelis *et al.* 2019, Jappelli & Pistaferri 2020).

I employ the following data collection strategy. First, I include 17 studies from Havránek & Sokolova (2020) that fit the above criteria. I collect additional relevant information for each reported estimate. Second, I use Google Scholar to locate relevant studies that came out after 2017 (which is when Havránek & Sokolova 2020 completed their data collection). The last few years have seen a great increase in studies of excellent quality that use financial account data which was previously not available. Some such studies are not yet published. For this reason I chose to include both published and unpublished papers from this period.[12] The search was run and the results were saved on April 1 2021; the search returned 33 pages. Third, I check references of the collected studies to find papers the previous two steps may have missed. This strategy yields 1244 estimates collected from 40 studies.[13]

The studies in the sample estimate MPCs over different horizons. Some deal with monthly or quarterly data to gauge consumption response over a month or a quarter. Others have access to high frequency data and study changes in consumption that occur on daily basis around the time of payment receipt. Even when the household is intent on spending the payment in full, the payment is unlikely to be fully spent on the first day upon receipt: it takes time for spending to occur. The more time passes, the larger fraction of the payment would likely be spent. Therefore, other things being equal, consumption response over longer horizons should be greater.

Table 1: MPC and the response horizons. Raw data

	Mean	Median	5%	95%	N	N studies
all	0.22	0.08	-0.02	0.85	1244	40
1 day	0.01	0.00	0.00	0.07	55	4
2-7 days	0.08	0.02	0.00	0.45	279	10
8 days - 1 month	0.21	0.08	-0.00	0.51	352	17
2 months	0.11	0.10	0.00	0.26	20	5
3 months	0.31	0.21	-0.03	0.98	378	14
4-6 months	0.30	0.20	-0.20	1.24	50	5
over 6 months	0.35	0.30	-0.15	0.97	110	8

Table 1 summarizes MPC estimates in our dataset and the respective horizons of consumption responses. The biggest group of estimates describes consumption response over a quarter. At the same time, there are many estimates that relate to shorter horizons, and some estimates for horizons that are longer. Overall, it does appear that consumption responses over longer horizons are somewhat higher—though the relationship is not straightforward. This is not surprising: after all, Table 1 combines estimates that may differ along many dimensions aside from the horizon of consumption response.

Before proceeding with the analysis, I work to convert the estimates into comparable formats,

[12] This is unlike Havránek & Sokolova (2020) who only include published studies.

[13] See search query and the list of studies in Appendix C.

such that the underlying consumption responses pertain to the same horizon. I choose the quarterly frequency as the baseline. I use the following transformation to convert an estimate $\widehat{mpc}_f$ of consumption response over the horizon of f months:[14]

$$\widehat{mpc}_3 = 1 - (1 - \widehat{mpc}_f)^{\frac{3}{f}} \tag{2}$$

A similar conversion approach is employed in e.g. Carroll *et al.* (2014), Carroll *et al.* (2017); theoretical justification is offered in the Online Appendix of Auclert (2019).[15,16] I apply this transformation to all MPC estimates between 0 and 1 of frequency other than quarterly.[17] The sample statistics for the scaled dataset is reported in Table 2. The mean and median MPC for scaled estimates (.35 and .21) are close to those for quarterly MPCs reported in Table 1 (.31 and .21).

Table 2: MPC sample statistics. Scaled data

	Mean	Median	5%	95%	N	N studies
All	0.35	0.21	-0.02	1.00	1244	40
Unemployment$<$ .06	0.26	0.18	-0.00	0.90	629	18
Unemployment$\geqslant$ .06	0.43	0.25	-0.05	1.00	615	22
Payment amount$<$ \$1200	0.35	0.21	-0.06	1.00	824	25
Payment amount$\geqslant$ \$1200	0.33	0.20	-0.00	0.98	420	22
Pay: stimulus	0.37	0.25	0.01	0.98	532	12
Pay: unemp. benefits	0.33	0.34	0.20	0.61	9	2
Pay: lottery and other	0.60	0.17	0.04	2.64	52	3
Pay: paycheck	0.46	0.32	-0.00	1.05	219	11
Pay: reg. payments	0.19	0.02	-0.16	0.97	117	4
Pay: refunds	0.16	0.13	-0.04	0.47	145	4
Pay: other recurring	0.29	0.24	-0.00	0.93	170	5
LC binding	0.37	0.25	-0.02	1.13	225	25
LCB: liquid assets	0.31	0.25	0.00	0.96	100	18
LC not binding	0.20	0.07	-0.07	0.88	281	27
LCNB: liquid assets	0.17	0.09	-0.02	0.83	96	18

Notes: See details about payment types in Table A2. 'LC' refers to 'liquidity constraints'. See details about estimates pertaining to constrained and unconstrained households in Table A3.

[14]For simplicity I assume 4 weeks in a month, so e.g. a weekly MPC would have $f = 0.25$, a daily MPC would have $f = 0.04$.

[15]The author is very grateful to Greg Kaplan for suggesting this conversion.

[16]One alternative to this procedure is to use a linear transformation $\widehat{mpc}_3 = \widehat{mpc}_f \cdot \frac{3}{f}$. Though straightforward, the linear scaling has a serious drawback: it assumes the consumption response is constant over time. For example, if a consumer spends 5% of the payment on the day of receipt, the linear scaling would convert this to $0.05 \cdot 28 \cdot 3 = 4.2$, under an assumption that with each passing day another 5% would be spent. With linear scaling the sample mean is .65, far from the sample mean for quarterly estimates (.31)—see Table B1 for more sample statistics under linear scaling. The scaling described in (2) ensures that, whenever the estimate is between 0 and 1, the scaled quarterly MPC does not exceed unity.

[17]There are estimates that lie outside this interval, for which the above conversion is inappropriate. First, there are negative estimates of the MPC suggesting that households do not respond to payment receipt by increasing consumption. Second, there are estimates that exceed unity, suggesting that households spend more than 100% of the payment. I do not scale these estimates, keeping them in the dataset as they are. There are 84 such unscaled estimates in the dataset. I later perform a robustness check in which these estimates are dropped (see subsection 3.4).

The estimates included in this dataset differ along a number of important dimensions. Roughly half of the estimates characterize consumption responses during times of relatively low unemployment (629 estimates from 18 studies), while the other half correspond to periods when unemployment was relatively high (615 estimates from 22 studies).[18] Marginal propensities to consume for periods of high unemployment appear larger: mean MPC during periods with average unemployment over 6% is .43 compared to .26 for other periods.

The estimates considered here measure consumption responses to payments of different sizes.[19] The sample includes 420 estimates of MPC out of salient payments over $1200 (the payment per individual according to the CARES act), as well as 824 estimates for payments that are smaller. The sample combines estimated marginal propensities to consume out of different types of payments. The largest group of 532 estimates describes MPC out of stimulus checks. The rest of the estimates refer to consumption responses to unemployment benefits, lottery payouts, paycheck receipts, changes in regular payments (mortgage, taxes), receipts of tax refunds and other recurring payments. Mean MPCs vary across these categories.

The majority of estimates in our sample characterize marginal propensities to consume for the general population of consumers considered by the study. However, some estimates are obtained for subsamples of the general population that are deemed likely (or unlikely) to be facing binding liquidity constraints. Most commonly, a study would split the dataset into subgroups based on a measure of liquid assets and compare MPCs for consumers with high/low liquidity (e.g. households in top and bottom quartile of cash on hand). In our sample, mean MPC estimate for consumers holding substantial liquidity (.17) is half the size of the average MPC for the general population (.35). The MPC for consumers that are unlikely to be facing binding liquidity constraints (based on liquid assets as well as other indicators) is also relatively low—about .2.

So far, according to Table 2, mean marginal propensities to consume appear to vary across payment types and liquidity characteristics of the underlying population; the magnitudes of the MPC estimates also seem to depend on the average unemployment rate during the period under consideration. On the one hand, these apparent differences may result from a systematic variation in the underlying 'true' marginal propensities to consume across these dimensions. On the other hand, some of the observed differences may arise coincidentally. For example, studies that consider MPCs out of stimulus checks likely focus more on periods of relatively high unemployment—compared to studies that look at other payments. If, indeed, marginal propensities to consume are higher during times of economic distress, then the relatively high average MPC out of stimulus checks may not signify any behavioral differences in how consumers spend stimulus checks compared to, for example, tax refunds. Therefore, simply comparing

[18] Average unemployment corresponding to each MPC estimate was obtained by 1) recording the time period during which the payment was received by consumers, and 2) averaging unemployment rates for this time period, for the corresponding country. Most MPC estimates in this sample (901) come from the US; for these estimates I use unemployment data from the Federal Reserve Bank of St. Louis. For estimates from other countries I use similar data from FRED, World Bank or the IMF—depending on availability.

[19] Payment sizes were converted to 2021 US dollars using CPI data from Federal Reserve Bank of St. Louis. Payments in foreign currencies were first converted to US dollars based on average exchange rates for the period under consideration (also from FRED), and then deflated to 2021 dollars.

average estimates out of stimulus checks and tax refunds (or across other dimensions) will not yield a good answer to whether the underlying 'true' MPCs differ systematically.

In the next section I will attempt to disentangle the source of systematic variation in the MPC estimates by using meta-regression analysis, a method employed by many previous studies to answer the question of why do estimates of the same parameter vary.

3 Why do MPC estimates vary?

I now turn to investigate the sources of systematic variation in the reported MPC estimates. I employ the following meta-regression model:

$$\widehat{mpc}_{ij} = \overline{mpc} + \sum_{l=1}^{N} \beta_l X_{l,ij} + \epsilon_{ij}, \tag{3}$$

where $\widehat{mpc}_{ij}$ is the MPC estimate i reported by study j; the term $\sum_{l=1}^{N} \beta X_{l,ij}$ includes control variables that are meant to capture systematic variation in the reported estimates; $\overline{mpc}$ is the constant term; ϵ_{ij} is noise.[20] Among the $X_{l,ij}$ controls, the non-binary explanatory variables are centered such that the constant term $\overline{mpc}$ can be interpreted as a 'good' estimate of mean MPC based on the existing literature.[21]

The dataset I explore contains estimates reported in 40 studies, with each study reporting multiple results. The estimates coming out of the same study are likely correlated. I therefore cluster the standard errors at the study level. However, as the number of clusters is relatively small, the resulting standard errors might exhibit a downward bias. Following Cameron *et al.* (2008), I remedy this by additionally computing p-values with the wild bootstrap cluster.[22]

3.1 A bird's eye view

I start by estimating a simple empirical model that accounts for a few key potential sources of variation in estimates; the results are summarized in Table 3.

First, reported MPC estimates may depend on the size of the payments that the authors are considering, particularly if some households were rationally inattentive, or if their preferences were near-rational. If households are rational, have ample liquidity and can calculate the optimal consumption path at no cost, then their period consumption should equal a small fraction of their expected lifetime income. For such households, only income changes that affect the lifetime income would trigger a prominent consumption response; the marginal propensity to consume out of all other cash flows would be close to 0—regardless of the size of the payment. But if re-calculating the optimal consumption path is costly, households would choose to do so only in response to large changes in income, while remaining rationally inattentive to changes that

[20] Similar meta-regression models have been employed by e.g. Havránek (2015), Card *et al.* (2018), Doucouliagos *et al.* (2018), Havránek & Sokolova (2020), Balima & Sokolova (2021).

[21] The summary of the centering choices is available in Table A1; more discussion is provided in the following subsections.

[22] A similar approach has previously been taken in Sokolova & Sorensen (2021) and Balima & Sokolova (2021).

are small (see Reis 2006). Households would then display a high MPC out of small changes in income, but not out of changes that are substantial enough to trigger a re-optimization. In a similar vein, if households exhibit near-rationality, they would allow their consumption to track current income as long as this does not cause too big of a deviation from the optimal consumption path (Caballero 1995, Browning & Crossley 2001); they would then have high MPCs out of small income changes and MPCs close to 0 out of cash inflows that are large.

According to Table 3, as the log of the payment size goes up, the reported MPC decreases—thus, more salient payments are associated with lower MPCs. This result is consistent with rational inattention and near-rationality.

Second, consumer response to increases in income and the corresponding MPC may be different compared to their response to an income reduction. In particular, if households' preferences exhibit loss aversion (Tversky & Kahneman 1991), then they would be more incentivized to smooth consumption in response to income declines than to income increases. So far, the evidence in Table 3 does not suggest a prominent difference between responses to income increases and declines: while the point estimate of the coefficient on *Decrease* is negative, it is not statistically different from zero.

Third, consumers' response to an income receipt may vary depending on the income source. If consumers use mental accounting, then payments received through different sources may not be fungible (Thaler 1999). Income that is considered a windfall may be consumed more readily relative to regular income. As a first attempt to address this possibility, I control for whether the income flow the MPC pertains to is recurring, i.e. if the payment is likely to be received/made more than once, over several years. Close to half of estimates considered here pertain to one-time stimulus payments, such as the Economic Stimulus Payments in the US. I divide the remaining estimates into two categories: first, MPCs out of non-recurring payments other than stimulus (e.g. lottery) and payments that are received in clusters over short horizons (unemployment benefits); second, MPCs out of payments that are recurring (e.g. tax refunds, paychecks, payments from Alaska permanent fund).[23]

Results in Table 3 reveal that, while MPCs out of stimulus are not statistically different from those out of other non-recurring payments, they do appear to differ from payments that are recurring, with MPC out of recurring payments being lower by about 12 percentage points. This perceived non-fungibility between different sources of income (e.g. one-time stimulus checks and recurring payments, such as yearly tax refunds) may serve as a form of self-control, a commitment devise that helps keep spending within the desired limits. Card & Ransom (2011) observe that saving decisions of college and university professors depend on how the compensation is labeled. Baugh *et al.* (2021) document that households draw on liquid assets to smooth consumption fluctuations when making payments, but not when expecting a cash inflow; they conclude that the behaviors they observe are best explained within the framework of mental accounting.

[23] See details in Table A2.

Table 3: Variation in $\widehat{mpc}$: Key factors

Variable	Coef.	p-value	p-value (wild)
Payment characteristics			
Payment amount	-0.127	0.00	0.02
Decrease	-0.162	0.25	0.30
Pay: not recurring, other	0.172	0.12	0.52
Pay: recurring	-0.120	0.01	0.04
Liquidity constraints			
LC: binding	0.040	0.42	0.46
LC: not binding	-0.089	0.04	0.07
Unemployment	4.484	0.00	0.00
Consumption definition			
Total cons.	0.197	0.01	0.04
Food	-0.152	0.03	0.04
Indiv. category	-0.177	0.01	0.02
Constant ($\overline{mpc}$)	0.321	0.00	0.00
N studies	40		
N	1244		

Notes: Estimation is done via OLS; standard errors are clustered at the study level. In addition to the conventional p-values I compute p-values resulting from wild bootstrap clustering—'p-value (wild)'—which is done in STATA using `boottest` routine (see Roodman 2018) with Rademacher weights and 9999 replications.

Fourth, households' ability to smooth consumption may be impeded by binding liquidity constraints. Therefore, households that do not have access to sufficient liquid funds are more likely to live paycheck-to-paycheck and exhibit higher MPCs compared to households with ample liquidity. The majority of MPC estimates considered here pertain to samples of households close to general population. Some estimates correspond to subsamples of households that are likely to face binding liquidity constraints, while others relate to subsamples of likely unconstrained households.[24] Table 3 reports on the differences between these three groups of estimates. Consumption response of households that likely have sufficient liquidity appears to be lower by about 9 percentage points compared to that of the general population. For likely constrained households, the point estimate is higher than general population, but is not statistically different.

As noted above, most estimates in our sample are obtained for the general population that combines consumption responses of both constrained and unconstrained households. But the share of liquidity constrained households in the general population is unlikely to be the same across datasets. In particular, when unemployment is on the rise, more households are likely to hit a binding liquidity constraint due to job loss. Thus, one might expect higher average MPCs in recessions. Results presented in Table 3 support this notion: as the unemployment rate rises by one percentage point, the reported MPC increases by about 4.5 percentage points.

[24]See details in Table A3.

Finally, the reported MPCs should differ depending on the consumption category they describe. By construction, the more broad the definition of the consumption category is, the higher the MPC should be. Most estimates in our sample refer to consumption of nondurable goods. Some estimates pertain to total consumption—according to Table 3, these estimates are higher by about 20 percentage points. Other estimates relate to the response of food and individual consumption categories; the estimates from the latter groups are smaller than estimates for nondurable consumption.

As mentioned above, I center the non-binary variables to give meaningful interpretation to the constant term $\overline{mpc}$. Specifically, the control for payment size is centered such that the $\overline{mpc}$ reflects propensity to consume for a payment of $1200, i.e. the payment per person disbursed under the 2020 CARES act. Furthermore, the unemployment rate is centered around 6%, which roughly corresponds to the mean US unemployment over the twenty years preceding the crisis of 2020. Therefore, the estimate of the constant term in Table 3, 0.32, describes the marginal propensity to raise nondurable consumption in response to a stimulus check receipt of $1200 for the general population, given unemployment rate of 6%.

3.2 A detailed frequentist investigation

We now turn to investigate the variation in MPC estimates in a greater detail. To this end, I construct an extensive list of additional controls meant to capture the precise context in which each MPC estimate was obtained, arriving at the total of 30 explanatory variables. I discuss these controls below. The full list of variables, their precise definitions and sample statistics are available in Table A1.

Payment characteristics
As discussed above, the payments in our dataset correspond to categorically different income sources. If households use mental accounting (Thaler 1999) to choose how much to consume out of income receipts, the specific source of income may matter. I break *Pay: not recurring, other* and *Pay: recurring* into finer categories. Non-recurring income is now split into unemployment benefits, lottery winnings and other non-recurring income—as before, stimulus checks are the reference group. Recurring payments are now represented by paycheck income, changes in regular payments, refunds and other recurring payments.

Among the studies examining MPC out of paychecks, there are two distinct categories. Some studies examine how monthly/quarterly consumption responds to predictable or transitory changes in take-home pay (e.g. Parker 1999, Souleles 2002). A positive MPC uncovered by such studies would indicate a violation of the canonical life-cycle/permanent-income hypothesis model without binding liquidity constraints, possibly due to bounded rationality or the lack of sufficient liquidity.

Other studies that have access to high frequency data examine short-term responses to paycheck receipt, comparing consumption in the days/weeks following paycheck receipt to consumption at other times (e.g. Stephens 2003, Baker & Yannelis 2017, Gelman *et al.* 2020). For these studies, a positive MPC in the days following paycheck receipt may be interpreted in the same vein as similar evidence from lower-frequency studies discussed above—however, there are also other possible interpretations that are unique to this particular strand of literature.

Paychecks and social security payments are income that arrives at regular intervals (monthly, bimonthly). Meanwhile, household spending is not continuous over time—it is lumpy, concentrated. Households may choose to time some of the recurring spending (e.g. big shopping trips) to around the time of regular income arrival. Gelman *et al.* (2014) find that some of the apparent excess sensitivity in their high frequency data is due to this coincidental timing. Another possible interpretation of a positive MPC upon paycheck arrival is that households use budgeting rules (defined over months or weeks), allowing themselves to spend fixed amount per budgeting period (Thaler 1999). This would make high consumption at the beginning of the budgeting period more likely—if the beginning of budgeting cycle corresponds to paycheck arrival, we would observe positive MPC out of paychecks. Importantly, if either of these interpretation is correct, then a high MPC observed for high frequency data would not mean that a similarly high MPC would be detected for data of lower frequency discussed above. I create controls to distinguish between the two types of data used to estimate MPC out of paychecks.

Liquidity constraints

As discussed above, households that have limited access to liquid funds may struggle to smooth consumption fluctuations in response to income changes. Authors can test this conjecture by splitting their samples into groups based on some indicator that is correlated with liquidity, to distinguish between those with ample liquidity and those likely constrained. One such indicator is the amount of liquid assets that the household holds—splitting the sample into quartiles (or halves, terciles, quintiles) by liquid assets and comparing consumption responses between top and bottom quartiles (halves, terciles, quintiles) can shed light on the importance of liquidity constraints. But when information about liquid assets is unavailable, other indicators that may be correlated with access to credit can be used. Household income or age may be correlated with credit access: wealthier older households are more likely to have access to liquid funds compared to their low-income younger counterparts; homeowners that payed off their mortgage may have access to more credit than renters. I assemble a set of controls to capture whether the sample examined can be classified as constrained or unconstrained, distinguishing between different proxies for liquidity constraints. The details are provided in Table A3.[25]

Data characteristics

Households residing in different countries may have different consumption patterns—for cultural, as well as economic reasons. I construct a control to distinguish MPCs that relate to consumers residing outside of the US. Households' consumption responses to payments may

[25]The observation is classified as 'binding' or 'not binding' liquidity constraints whenever it is based on a sample corresponding to top or bottom quantiles in the partition of the sample. For example, if a sample is split into quintiles based on the value of liquid assets, the MPC estimate corresponding to the bottom quintile would be marked as *LCB: liquid assets*=1, the estimate corresponding to the top quintile would be recorded as *LCNB: liquid assets*=1, while the estimates corresponding to the other quintiles would not be recorded as indicating anything about liquidity constraints. Some authors perform two-way partitions of their sample, splitting it e.g. by both income and assets; in such cases partition along each dimension is recorded.

have changed over time, e.g. due to increased access to credit and other improvements in financial instruments. To account for this possibility I record the midyear of each sample.

The majority of studies of consumption responses rely on data from households' surveys, such as the Consumer Expenditure (CE) Survey (e.g. Souleles 2002, Johnson *et al.* 2006, Parker *et al.* 2013), or the Nielsen Consumer Panel (e.g. Broda & Parker 2014, Parker 2017). Many of the more recent studies are able to observe consumption responses directly, due to the increased availability of the financial account data (e.g. Agarwal *et al.* 2007, Gelman *et al.* 2014, Olafsson & Pagel 2018, Baugh *et al.* 2021). I construct a control to reflect this distinction. Furthermore, some of the datasets are relatively short panels with large cross-sectional dimension (e.g. the CE Survey)—studies that use these datasets identify MPCs from cross-sectional variation, by comparing consumption of households that received payments over a given period to those that did not. By contrast, other studies use long panels (particularly studies using account data) and identify MPCs by comparing each households' consumption upon payment receipt to its own consumption at other times. I construct a control to captures the relative importance of these two dimensions of the datasets.

Method and publication characteristics

The majority of estimates in our sample are obtained using exogenous variation in timing and/or amounts of payment receipts across individuals for identification. While most estimates are produced with OLS or difference-in-differences, some are generated with other methods—in particular, using instruments to predict payment amount (e.g. some estimates in Johnson *et al.* 2006, Parker *et al.* 2013). I construct a control to reflect this distinction.

Havránek & Sokolova (2020) show that the literature estimating consumption responses to income changes is prone to selective reporting: there is some underreporting of negative results and results of low statistical power. This bias gives rise to a correlation between estimates and their standard errors.[26] To account for this bias, Havránek & Sokolova (2020) include the estimates' standard error in their meta-regression—as a result, the constant term in their model captures the effect corrected for the bias associated with selective reporting.[27] Similar to Havránek & Sokolova (2020), I account for this selective reporting by including estimates' standard errors among the list of controls. Since the estimates used in this analysis have been scaled to quarterly frequency (see Section 2), I scale the standard errors as well.[28] This, however, results in a caveat. Publication bias tests draw their conclusions by examining the correlation between the estimates and their standard errors. The non-linear scaling does not preserve the original ratio of the estimate to the standard error, which makes publication bias tests on scaled data less precise. Nevertheless, I include the standard error to at least partially capture the effects of potential selective reporting.

[26]See more on publication bias in e.g. Stanley (2005), Stanley (2008).

[27]Technically, the constant term only captures the 'true' unbiased effect if publication bias is proportional to the standard error—otherwise, it is only an approximation. Nevertheless, it was shown to work well as such in Monte Carlo simulations (Stanley 2008).

[28]Because the transformation described in (2) is non-linear, I approximate the standard errors using the delta method.

The heterogeneity across the reported MPC estimates may also be driven by differences in study quality that are not directly observable. I attempt to catch some of these difference by controlling for how well the paper has been cited.[29]

I start with a specification similar to that shown in Table 3, accounting for finer differences across payment types and subsamples of population considered by the studies—the results are reported in specification (1) of Table 4. The control *Pay: Not Recurring, other* is replaced with controls for when the MPC pertains to unemployment benefits, or lottery winnings and other non-recurring payments (excluding stimulus checks, which remain the reference group). Recurring cash flows are now split into paychecks (with a distinction between high and low frequency responses), regular payments (such as mortgage), tax refunds, and other recurring cash flows. For estimates corresponding to likely constrained and likely unconstrained households, I now specify the liquidity indicator used to perform the sub-sampling. The variables *LC: binding* and *LC: not binding* are now replaced with five new variables each, based on whether the sample split was performed using households' liquidity, income, age, home ownership status or other indicators likely correlated with liquidity constraints. In specification (2) of Table 4 I add controls for a number of features of the data with which the MPC estimates were obtained, most notably, for whether the MPC was calculated on data from countries other than the US, and whether the financial account data was used. In specification (3) I add controls for estimation method and publication characteristics.

Once again, the spending behavior depends on the size of the payment consumers receive: MPCs out of larger payments are smaller. This is in line with Fagereng *et al.* (2021) and Olafsson & Pagel (2021) as well as Havránek & Sokolova (2020) who, too, report bigger MPCs for smaller payment sizes. As discussed above, one possible explanation for this finding is near rationality: households re-calculate the optimal consumption path upon receiving large payments, but not for payments that are small. In consequence, consumers mostly save large payments, and mostly consume small ones. This interpretation is further supported by Kueng (2018), who finds the behavior of consumers in Alaska consistent with near rationality.

The results reported in Table 4 suggest that households that hold high levels of liquid assets tend to have MPCs that are lower by about 9-10 percentage points compared to the general population. Furthermore, household consumption behavior is affected by the state of the economy: as the unemployment rate goes up by one percentage point, MPC increases by about 4-5 percentage points. One plausible explanation for these findings is the link between binding liquidity constraints and unemployment.

[29]Specifically, I include the log number of citations per year, counting from the year the paper first appeared on Google Scholar. In what follows I also include a robustness check that only includes data from studies that have been published, see subsection 3.4.

Table 4: Variation in $\widehat{mpc}$: A frequentist investigation

Variable	(1) Coef.	(1) p-value	(1) p-value (wild)	(2) Coef.	(2) p-value	(2) p-value (wild)	(3) Coef.	(3) p-value	(3) p-value (wild)
Payment characteristics									
Payment amount	-0.153	0.00	0.00	-0.153	0.00	0.00	-0.145	0.00	0.01
Decrease	-0.182	0.04	0.26	-0.166	0.07	0.26	-0.197	0.02	0.17
Pay: unemp. benefits	0.112	0.37	0.58	0.019	0.93	0.95	0.141	0.50	0.66
Pay: lottery and other	0.187	0.07	0.74	0.231	0.06	0.52	0.266	0.00	0.25
Pay: paycheck, high fr.	0.179	0.02	0.14	0.147	0.31	0.46	0.181	0.19	0.35
Pay: paycheck, low fr.	-0.173	0.14	0.28	-0.179	0.31	0.50	-0.204	0.21	0.43
Pay: reg. payments	-0.290	0.00	0.14	-0.193	0.14	0.20	-0.287	0.06	0.17
Pay: refunds	-0.191	0.01	0.18	-0.242	0.09	0.27	-0.227	0.11	0.32
Pay: other recurring	-0.174	0.04	0.21	-0.182	0.12	0.38	-0.133	0.20	0.38
Liquidity constraints									
LCB: liquid assets	0.038	0.31	0.36	0.039	0.30	0.36	0.021	0.58	0.60
LCB: income	0.033	0.61	0.63	0.050	0.46	0.50	0.034	0.61	0.66
LCB: age	-0.031	0.72	0.72	-0.023	0.77	0.77	-0.050	0.51	0.53
LCB: home own.	0.201	0.27	0.65	0.228	0.21	0.56	0.155	0.38	0.74
LCB: other	0.137	0.31	0.56	0.117	0.35	0.61	0.071	0.55	0.75
LCNB: liquid assets	-0.093	0.01	0.04	-0.092	0.01	0.03	-0.107	0.00	0.01
LCNB: income	-0.001	0.97	0.98	0.014	0.63	0.66	0.009	0.70	0.70
LCNB: age	0.120	0.29	0.49	0.151	0.25	0.46	0.133	0.31	0.55
LCNB: home own.	0.001	1.00	1.00	-0.072	0.54	0.65	-0.103	0.34	0.51
LCNB: other	-0.100	0.43	0.51	-0.124	0.34	0.47	-0.191	0.08	0.23
Unemployment	4.935	0.00	0.00	3.870	0.01	0.02	4.633	0.00	0.01
Consumption definition									
Total cons.	0.213	0.00	0.02	0.209	0.00	0.02	0.180	0.01	0.03
Food	-0.242	0.00	0.00	-0.230	0.00	0.00	-0.202	0.01	0.03
Indiv. category	-0.183	0.01	0.04	-0.169	0.03	0.07	-0.162	0.06	0.13
Data features									
non-USA data				-0.082	0.29	0.48	-0.135	0.02	0.16
Midyear of data				-0.013	0.88	0.91	0.027	0.75	0.81
Account data				0.109	0.49	0.60	0.077	0.62	0.70
Cross-section ratio				0.004	0.56	0.84	0.006	0.46	0.78
Method & publication characteristics									
IV							0.047	0.49	0.58
SE							0.191	0.26	0.36
Citations							-0.059	0.01	0.07
Constant ($\overline{mpc}$)	0.299	0.00	0.01	0.274	0.00	0.02	0.216	0.00	0.04
Studies	40			40			40		
Observations	1244			1244			1244		

Notes: Estimation is done via OLS; standard errors are clustered at the study level. In addition to the conventional p-values I compute p-values resulting from wild bootstrap clustering—'p-value (wild)'—which is done in STATA using `boottest` routine (see Roodman 2018) with Rademacher weights and 9999 replications. Variable definitions are available in Table A1.

When households are out of work, they may have to rely on accumulated liquid assets to maintain their consumption as cash inflows cease. When unemployment is on the rise, more households may drain down their liquidity and come to face binding liquidity constraints. As a result, on average, marginal propensity to consume out of cash inflows may go up. This conjecture is in line with the recent work of Gross *et al.* (2020) who study how consumption decisions change with an increased access to credit, and report the MPC out of liquidity to be counter-cyclical.

There are some differences across MPCs depending on the income source, although the evidence is not statistically strong. First, with the exception of *Pay: paycheck, high fr.*, all of the subcategories of the former variable *Pay: recurring* are estimated to have negative signs, matching the negative effect on *Pay: recurring* uncovered in Table 3. Second, there is some evidence suggesting that the MPC may be lower for tax refunds, regular payments (mortgage, taxes) and other recurring payments—although this evidence lacks statistical power, particularly according to p-values from the wild bootstrap cluster. Third, there is some asymmetry in household responses to increases and decreases in income: the MPCs associated with income decreases are smaller. For this latter result, the conventional p-values indicate significance, but the p-values from wild bootstrap clustering do not.

The differences in MPC across income sources may arise if households employ mental accounting, labeling some cash flows as appropriate to be mostly consumed, while directing others into their savings accounts. The evidence here suggests that there may be some such non-fungibility between different cash flows. For example, consumers may perceive regular payments as an expense to be smoothed using liquid assets—changes in this expense are thus inconsequential to the consumption dynamics. Consumers may also prefer to smooth out decreases in income due to loss aversion, which results in smaller MPCs associated with income declines. The asymmetry between MPCs out of income increases in declines uncovered here is in line with the findings in Baugh *et al.* (2021).

The seeming (albeit not statistically strong) discord between MPCs out of paychecks measured on data of different frequencies can result from a combination of factors. As previously discussed, a jump in consumption on payday/payweek does not necessarily mean that monthly consumption tracks current income: it may arise because of coincidental timing of cash inflows and big regular spending, or due to the use of budgeting rules. If this is the case, then scaling payday (payweek) responses to quarterly frequency could exaggerate the feedback between cash inflows and consumption.

While households holding high liquid assets tend to have lower MPCs, there is not much evidence that households with low liquid assets behave systematically different compared to the general population. The same is true for groups of households with high/low income, that are old/young, that have/do not have mortgages. Sample splits based on other indicators do not seem to produce systematic differences either. It is possible that, while a large stock of liquid assets clearly identifies a portion of unconstrained households, other indicators are not as reliable at capturing how constrained/unconstrained the underlying population might feel. On the one hand, some older/wealthier households may choose to hold their wealth in illiquid assets, which may result in them displaying hand-to-mouth behavior (see Kaplan *et al.* 2014). On the other hand, households with low liquid assets in their financial accounts may still be able to smooth consumption fluctuations if they hold cash, or are able to adjust the timing of mortgage and credit card payments (Gelman *et al.* 2020).

As expected, the response of total consumption exceeds that of nondurable consumption, while MPC of food and goods of individual categories are lower. This is not surprising: the broader the definition of consumption, the larger the MPC we should observe. There is also some evidence suggesting that US households display more prominent consumption responses compared to households from other countries. Finally, papers that generate more citations tend to report lower MPC estimates.

Once again, the constant terms in each of the specifications reported in Table 4 correspond to point estimates of MPC out of stimulus checks. For specification (1), the precise interpretation of $\overline{mpc} = 0.299$ is the same as that in Table 3: it is an MPC for nondurable consumption out of a \$1200 check for the general population, conditional on a 6% unemployment rate. In specification (2), $\overline{mpc} = 0.274$ has similar interpretation, though it is further conditioned on a number of qualifiers that do not seem to (statistically) matter.[30] In specification (3), $\overline{mpc} = 0.216$ is also conditional on the associated paper having a high citation count (I center *Citations* around the 90th percentile).[31]

Overall, the key results in Table 4 are fairly consistent across the three specifications considered—the effects associated with *Payment size*, *Unemployment* and *LCNB: liquid assets* are very similar and statistically strong. That being said, coefficient estimates for a few variables differ notably, and so do the estimates of the constant term $\overline{mpc}$, i.e. the 'best practice' MPC. For example, the effect of non-USA data appears statistically insignificant in specification (2), but looks more prominent in specification (3). This is not surprising, as each set of results is conditional on the specific choice of explanatory variables included.[32] So far we have only considered three out of 2^{30} possible combinations of the explanatory variables that could affect reported MPC estimates, making a value judgement as to what the 'true' data generating process might be. But given the large number of possible empirical models, it is unlikely that either of these three specifications represents the closest possible description of the underlying process. In the next section I address this problem of model uncertainty by considering the whole space of the 2^{30} possible empirical models.

3.3 A Bayesian investigation

In the previous section we came up with 30 explanatory variables that could affect reported MPC estimates. For some variables there are strong *a priori* arguments as to why they should be included (e.g. definitions of consumption), while for others the reasoning is less clear (e.g. *Midyear of data*). It is unlikely that all 30 variables contribute to the variation in MPC estimates

[30] Technically, it is also conditional on data coming from the US and being recent (I center *Midyear of data* around its 90th percentile), being based on a survey (as opposed to account data), having a mean ratio of cross-sectional units versus periods (*Cross-section ratio* centered around mean).

[31] Again, technically $\overline{mpc} = 0.216$ is also conditional on the estimate's standard error being close to 0 and the estimate not being obtained via IV—but these distinctions are not statistically significant.

[32] There is also some correlation across the explanatory variables included, although the overall multicollinearity is manageable: the VIF is 9.3.

in a meaningful way—the empirical model containing all of these controls is likely misspecified. The models considered in the previous section were chosen based on a value judgement. In this section I will follow an agnostic approach instead, and use Bayesian Model Averaging (BMA) to assess all 2^{30} possible models without imposing preferences for the inclusion of specific controls.

The BMA approach considers all 2^{30} possible combinations of explanatory variables—or 'models'. Each model gets assigned a metric based on its relative performance, the Posterior Model Probability (PMP). For each model its PMP reflects the likelihood that the model captures the 'true' underlying data generating process of MPC estimates. The coefficient estimates are then averaged across all models weighted by posterior model probabilities (see introduction to BMA in Koop 2003).

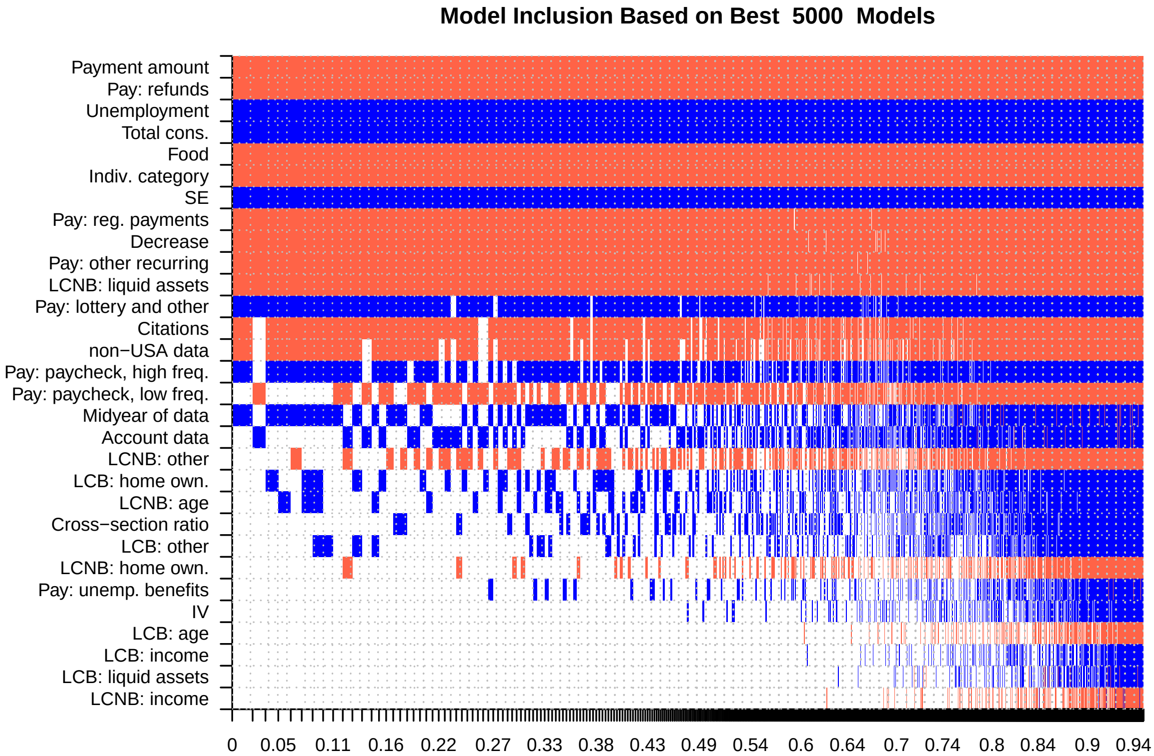
Bayesian Model Averaging has been used to study a variety of topics in economics (see overview in Steel 2020). It has also been applied to address model uncertainty in meta-analyses (e.g. Havránek & Irsova 2017, Cazachevici *et al.* 2020, Balima & Sokolova 2021), as well as in meta-analyses on consumption theory (Havránek *et al.* 2015, Havránek *et al.* 2017 and Havránek & Sokolova 2020). I implement BMA in the `BMS` package for `R` developed by Zeugner & Feldkircher (2015). The baseline specification I consider relies on unit information prior (UIP) for parameters and the uniform prior for model space, a combination shown to have good predictive performance (see Eicher *et al.* 2011). Results for other prior combinations are summarized in subsection 3.4.

Figure 2 presents estimation results for 5000 'best' models—according to BMA. On Figure 2, each column corresponds to one model. The models are sorted by their posterior model probabilities, from most likely to capture the data generating process on the left to least likely on the right. The 30 explanatory variables are listed along the vertical axis. For each model, a white cell opposite an explanatory variable indicates that the variable is not included in the model; a blue (red) cell means it is included and the estimated coefficient is positive (negative). The explanatory variables that are included in 'good' models likely belong to the 'true' data generating process of MPC estimates. For each explanatory variable, the likelihood of it belonging in the underlying data generating processed is captured by its Posterior Inclusion Probability (PIP), i.e. the sum of posterior model probabilities of all models in which the variable is included. On Figure 2, the explanatory variables are sorted by their PIPs, from most likely to be relevant at the top, to least likely on the bottom.

Examining the colors on Figure 2, we note that for variables included in the best models (e.g. *Payment amount*, *Unemployment*), the signs of the estimated effects are consistent across the model space. Furthermore, for all variables in the top half of the graph, the direction of the effects across 'good' models matches the signs reported in Table 4—that is, the Bayesian Model Averaging results are generally in line with those obtained with the frequentist approach of the previous section.

The left panel of Table 5 displays BMA numerical results; it lists the posterior means of the coefficients, that is, coefficient estimates averaged across all models and weighted by posterior model probabilities. The table also reports the standard deviations of the coefficients, along

Figure 2: Variation in $\widehat{mpc}$: Bayesian Model Averaging



Notes: The figure illustrates BMA estimation results for 5000 'best' models (each model represented by a column), conditional on the unit information prior for parameters and the uniform prior for model space. The models are sorted by posterior model probabilities, from most likely (left) to least likely (right). Horizontal axis depicts cumulative posterior model probabilities. Red color (lighter in grayscale) indicates that a variable is included with negative sign, blue (darker in grayscale)—included with positive sign.

with the variables' posterior inclusion probabilities. I also perform a frequentist check, for which I only keep the variables that are more than 50% likely to belong in the data generating process, i.e. variables with PIP over 0.5. The results for the frequentist specification are reported in the right panel of Table 5. Overall, despite a large number of models considered by the BMA, the effects of the key variables are very close to those reported in Table 4.

Payment characteristics systematically affect the reported MPC estimates: with the exception of *Pay: unemp. benefits*, all controls in this category have posterior inclusion probabilities close to 1; some have low p-values in the frequentist check. First, once again, larger payments are shown to be associated with smaller MPCs: as the log of payment amount increases by one, the MPC goes down by about 14 percentage points—a statistically strong result that is very similar to the findings of the previous sections. Second, there is some evidence of consumption responses to increases and decreases in income being asymmetric: the MPC out of increases exceeds that out of decreases by about .16.

Table 5: Variation in $\widehat{mpc}$: Bayesian Model Averaging and frequentist check

Variable	BMA Post. mean	BMA Post. st.d.	BMA PIP	post-BMA Coef.	post-BMA p-value	post-BMA p-value (wild)
Payment characteristics						
Payment amount	-0.137	0.012	1.000	-0.140	0.001	0.013
Decrease	-0.157	0.046	0.989	-0.162	0.015	0.102
Pay: unemp. benefits	0.023	0.077	0.113			
Pay: lottery and other	0.210	0.082	0.945	0.235	0.002	0.307
Pay: paycheck, high freq.	0.124	0.083	0.756	0.165	0.023	0.223
Pay: paycheck, low freq.	-0.122	0.102	0.649	-0.130	0.346	0.483
Pay: reg. payments	-0.326	0.065	0.997	-0.350	0.000	0.046
Pay: refunds	-0.232	0.057	1.000	-0.217	0.021	0.194
Pay: other recurring	-0.161	0.062	0.987	-0.131	0.038	0.181
Liquidity constraints						
LCB: liquid assets	0.000	0.006	0.026			
LCB: income	0.001	0.008	0.028			
LCB: age	-0.001	0.014	0.029			
LCB: home own.	0.063	0.096	0.350			
LCB: other	0.025	0.058	0.197			
LCNB: liquid assets	-0.115	0.041	0.955	-0.126	0.001	0.010
LCNB: income	-0.000	0.006	0.022			
LCNB: age	0.039	0.069	0.293			
LCNB: home own.	-0.021	0.056	0.158			
LCNB: other	-0.075	0.097	0.430			
Unemployment	4.375	0.607	1.000	4.864	0.000	0.001
Consumption definition						
Total cons.	0.173	0.027	1.000	0.176	0.009	0.038
Food	-0.201	0.036	1.000	-0.210	0.008	0.023
Indiv. category	-0.174	0.027	1.000	-0.194	0.011	0.051
Data features						
non-USA data	-0.087	0.057	0.765	-0.109	0.047	0.188
Midyear of data	0.042	0.042	0.539	0.057	0.192	0.339
Account data	0.059	0.069	0.469			
Cross-section ratio	0.002	0.003	0.240			
Method and publication characteristics						
IV	0.003	0.013	0.065			
SE	0.209	0.045	1.000	0.208	0.215	0.276
Citations	-0.040	0.019	0.887	-0.049	0.007	0.037
Constant ($\overline{mpc}$)	0.270		1.000	0.291	0.000	0.010
Studies	40			40		
Observations	1244			1244		

Notes: The left panel reports the BMA estimation results: the posterior means of the coefficients, their standard deviations and posterior inclusion probabilities. The estimation is performed assuming the unit information prior for parameters and the uniform prior for model space. The right panel reports results for a frequentist check in which only the variables with PIP higher than 50% are included, the OLS is performed and the standard errors are clustered at the study level. 'P-value (wild)' refers to p-values resulting from wild bootstrap clustering; this is done in STATA using `boottest` routine (see Roodman 2018) with Rademacher weights and 9999 replications. Variable definitions are available in Table A1.

Third, the MPCs vary systematically across some payment types. On the one hand, MPCs out of stimulus payments are statistically similar to those out of unemployment benefits. On the other hand, MPCs out of regular payments are lower; MPCs out of tax refunds and other recurring payments appear lower as well, though these results are less precise as the p-values calculated with wild bootstrap cluster are relatively high. Finally, there is some evidence that MPC out of lottery and other payments and MPCs that measure high-frequency responses upon paycheck receipt might be higher than those out of stimulus checks, but these results lack statistical power.

When unemployment rate goes up by one percentage point, the MPC increases by about 4-5 percentage points. As discussed in the previous sections, this could be due to how loss of job affects consumers' access to liquidity. Households that have access to ample liquidity display MPCs that are lower by about 12-13 percentage points than the average response for the general population.

Once again, the precise definition of consumption group matters: compared to MPC for consumption of nondurables, marginal propensity to raise total consumption is higher by about .17-.18, while the propensities to increase consumption of food and of individual categories of goods are lower (by .20-.21 and .17-.19, respectively). The marginal propensities to consume for US households appear higher than those of households in other countries by about .09-.11.

The standard error is picked out by the BMA among variables likely to belong to the data generating process—however, the corresponding p-values in the frequentist check are high. Although this could be indicative of publication selection bias being minor in the present sample, the current specification does not amount to a powerful test for selective reporting: the conversion discussed in Section 2 skews the ratio of the estimates to the standard errors making the publication bias test less reliable. Finally, papers that generate more citations tend to report MPCs that are lower.

The interpretation of the constant term appearing in Table 5 is similar to that discussed in the previous section: it is an estimate of MPC out of a $1200 stimulus check for the general US population, conditional on a 6% unemployment rate and on being reported in a highly cited piece of research. Thus, according to Table 5, the marginal propensity to raise nondurable consumption in response to a $1200 stimulus check is about .27-.29. But as shown above, the MPCs vary widely depending on the precise context in which they are obtained. In Section 4 I use the model appearing in the frequentist check of Table 5 to construct fitted MPC estimates describing a variety of circumstances.

3.4 Robustness

In this section I consider a number of possible caveats and discuss the robustness of the results reported so far. The key results are found to be robust: the effects of *Unemployment*, *Payment size* and *LCNB: liquid assets* remain virtually unchanged in all specifications considered here. At the same time, some of the weaker results are affected by the specification choice. For example, the effects of *Pay: reg. payments*, *Pay: refunds*, *Pay: other recurring* appear much stronger when additional assumptions are made to reduce multicollinearity, but become less prominent once the working papers are dropped from the sample under consideration.

Multicollinearity

The baseline BMA specification includes two regressors—*Midyear of data* and *Account data*—that are relatively highly correlated with each other. The data on households' financial account transactions is only available for the most recent time periods, but not for older datasets. This correlation makes it hard to discern the individual contributions of these two variables, and adds uncertainty to the estimation results. I now perform a robustness check in which I exclude *Midyear of data* from the list of controls, thus assuming the 'true' MPC to be unchanged over the years. This leads to much less multicollinearity in the dataset examined (the VIF drops from 9.3 to 5.3). The results are reported in Table 6. With this amendment to the baseline specification the effects associated with *Pay: reg. payments*, *Pay: refunds*, *Pay: other recurring* become much more statistically powerful (such that even p-values obtained with wild bootstrap cluster are small). Furthermore, the BMA results indicate that the use of account data tends to be associated with MPCs that are higher by about 12-14 percentage points. Overall, these findings are not surprising: the assumption that the 'true' average MPC has remained constant over time eliminates the need to disentangle the effects of *Midyear of data* and *Account data*; this results in less overall noise and the coefficient estimates that are more precise—conditional on the assumption being correct.

There are multiple possible interpretations for the positive effect associated with the use of *Account data*. Unlike studies that base MPC estimates on data from surveys, studies that have access to financial account data can observe consumption patterns directly, without having to rely on respondents' ability to correctly recall and accurately report the pertinent information. At the same time, the observed financial accounts may provide an incomplete picture of consumers' finances, omitting information about e.g. cash holdings, or the use of credit cards not linked to the accounts observed. Furthermore, the sample of households that have financial accounts within a particular organization, the data provider, may not be representative of the general population. On the one hand, the discrepancy between estimates may stem from these differences. On the other hand, estimates based on financial account data often pertain to high-frequency responses, which then get converted to quarterly frequency for the purposes of this study—if this conversion introduces a bias that exaggerates the underlying quarterly effect, in could be responsible for the observed gap between estimates.

Outliers

I examine the extent to which the results uncovered in the previous sections are driven by the outliers in the data. Outside of the transformations discussed in Section 2, the original data has not been modified. I now apply two outlier treatments to the data on MPC estimates and the respective standard errors: first, I winsorize the outliers at 2% (1% each tail); second, winsorizing at 5% (2.5% each tail). I also consider a dataset where the outliers in payment size and unemployment rate are winsorized at 5% (2.5% each tail). Figure B1 compares the Posterior

Inclusion Probabilities and the posterior coefficient estimates for each of these specifications to the baseline reported in subsection 3.3.

Table 6: Variation in $\widehat{mpc}$: BMA and frequentist check, *Midyear of data* excluded

Variable	BMA			post-BMA		
	Post. mean	Post. st.d.	PIP	Coef.	p-value	p-value (wild)
Payment characteristics						
Payment amount	-0.132	0.011	1.000	-0.129	0.002	0.014
Decrease	-0.132	0.043	0.975	-0.117	0.123	0.337
Pay: unemp. benefits	0.007	0.041	0.049			
Pay: lottery and other	0.165	0.083	0.882	0.174	0.003	0.201
Pay: paycheck, high freq.	0.058	0.068	0.485			
Pay: paycheck, low freq.	-0.210	0.048	0.999	-0.209	0.040	0.162
Pay: reg. payments	-0.312	0.072	0.993	-0.335	0.000	0.053
Pay: refunds	-0.275	0.040	1.000	-0.301	0.001	0.056
Pay: other recurring	-0.209	0.047	1.000	-0.221	0.002	0.067
Liquidity constraints						
LCB: liquid assets	0.001	0.008	0.033			
LCB: income	0.001	0.008	0.031			
LCB: age	-0.002	0.016	0.033			
LCB: home own.	0.053	0.090	0.305			
LCB: other	0.011	0.037	0.105			
LCNB: liquid assets	-0.104	0.044	0.918	-0.122	0.000	0.015
LCNB: income	-0.000	0.006	0.024			
LCNB: age	0.022	0.052	0.188			
LCNB: home own.	-0.031	0.068	0.213			
LCNB: other	-0.087	0.102	0.482			
Unemployment	3.952	0.538	1.000	3.865	0.000	0.000
Consumption definition						
Total cons.	0.166	0.027	1.000	0.159	0.019	0.060
Food	-0.200	0.037	1.000	-0.187	0.006	0.013
Indiv. category	-0.165	0.027	1.000	-0.170	0.030	0.071
Data features						
non-USA data	-0.057	0.058	0.550	-0.074	0.164	0.269
Account data	0.124	0.044	0.957	0.138	0.021	0.069
Cross-section ratio	0.001	0.003	0.213			
Method and publication characteristics						
IV	0.005	0.018	0.100			
SE	0.215	0.046	1.000	0.219	0.173	0.231
Citations	-0.030	0.021	0.773	-0.039	0.041	0.102
Constant ($\overline{mpc}$)	0.253		1.000	0.254	0.000	0.016
Studies	40			40		
Observations	1244			1244		

Notes: See notes for Table 5. Results for BMA with *Midyear of data* excluded, i.e. assuming that MPC did not change over time.

The key results do not appear to be driven by outliers: the PIPs and coefficients associated with the unemployment rate, the payment size, the effect of having high liquid assets and examining decreases in income, refunds, regular payments or other recurring payments are virtually unchanged. At the same time, some of the weaker results are sensitive to the outlier treatments: the posterior inclusion probabilities of *Pay: lottery and other*, *Pay: paycheck, high freq.* and *Pay: paycheck, low freq.* diminish when some of the outlier treatments are used, and the respective coefficient estimates move closer to zero.

Subsamples

I consider two subsamples of the original dataset. First, as discussed in Section 2, the raw data collected for this paper has been converted to the quarterly frequency by means of the transformation (2)—however, this method does not work well for estimates that are negative or exceed unity. There are 84 such estimates in the baseline dataset that have been left unscaled. I now consider a subsample in which these estimates are excluded. Second, the original dataset is comprised of estimates reported by both published studies (33) and the recent unpublished working papers (7). I now separately consider the subsample of estimates reported in published work.

The results for both subsamples are summarized on Figure B2. Most of the statistically stronger results remain unchanged, with a notable exception of the coefficients on *Pay: reg. payments* and *Pay: other recurring*: it appears that additional data provided by the inclusion of working papers crucially contributes to the more prominent negative associated effect observed in the baseline dataset. Some of the other results are also sensitive to this subsampling: in particular, the effects of *Decrease*, *SE* and *Pay: paycheck, low freq.* diminish in the subsamples. In the meantime, the effect of *Pay: paycheck, high freq.* is more prominent in the subsamples than in the original dataset.

BMA priors

The Bayesian Model Averaging results are conditional on a particular choice of priors for parameters and the model space. I now consider how the choice of priors might affect the estimation results (see Figure B3). The baseline BMA results were obtained under the unit information prior for parameters and the uniform prior for model space; I repeat the BMA procedure under a combination of benchmark g-prior ('BRIC' on Figure B3) suggested by Fernandez *et al.* (2001) and the beta-binomial model prior proposed by Ley & Steel (2009) ('Random' on Figure B3); I also consider flexible data-dependent priors for parameters (Liang *et al.* 2008, Feldkircher & Zeugner 2012) denoted as 'HyperBRIC' on Figure B3. Some additional discussion of these priors and an implementation similar to ours can be found in Balima & Sokolova (2021); see Hasan *et al.* (2018) and Steel (2020) for more context and details. The choice of priors has a very modest effect on the BMA estimation results: the key findings remain essentially unchanged under the alternative combinations of priors, and the weaker results appear more statistically significant (judging by higher corresponding PIPs).

4 Fitted MPC and policy implications

We have shown that marginal propensities to consume vary systematically depending on the context in which they were obtained. We also generated a fitted 'best practice' MPC estimate of around .29—for nondurable consumption of US households receiving a stimulus payment of $1200, given the unemployment rate of 6% and being reported by a highly referenced study. But because consumption responses vary along a number of dimensions, this estimate may not be best suited for predicting MPCs under different circumstances. In this section we will construct fitted MPC estimates that describe consumption response in a variety of alternative scenarios.

To calculate the fitted 'best practice' MPC estimates I use the specification shown in the right panel of Table 5, the frequentist check for the Bayesian Model Averaging exercise. The results are displayed on Figure 1 and Figure 3. The figures show fitted estimates, 95% confidence intervals based on conventional clustering at the study level (solid bands), as well as intervals based on wild bootstrap cluster (dashed bands). On each figure, the baseline scenario with the estimate of .29 is marked in bold.

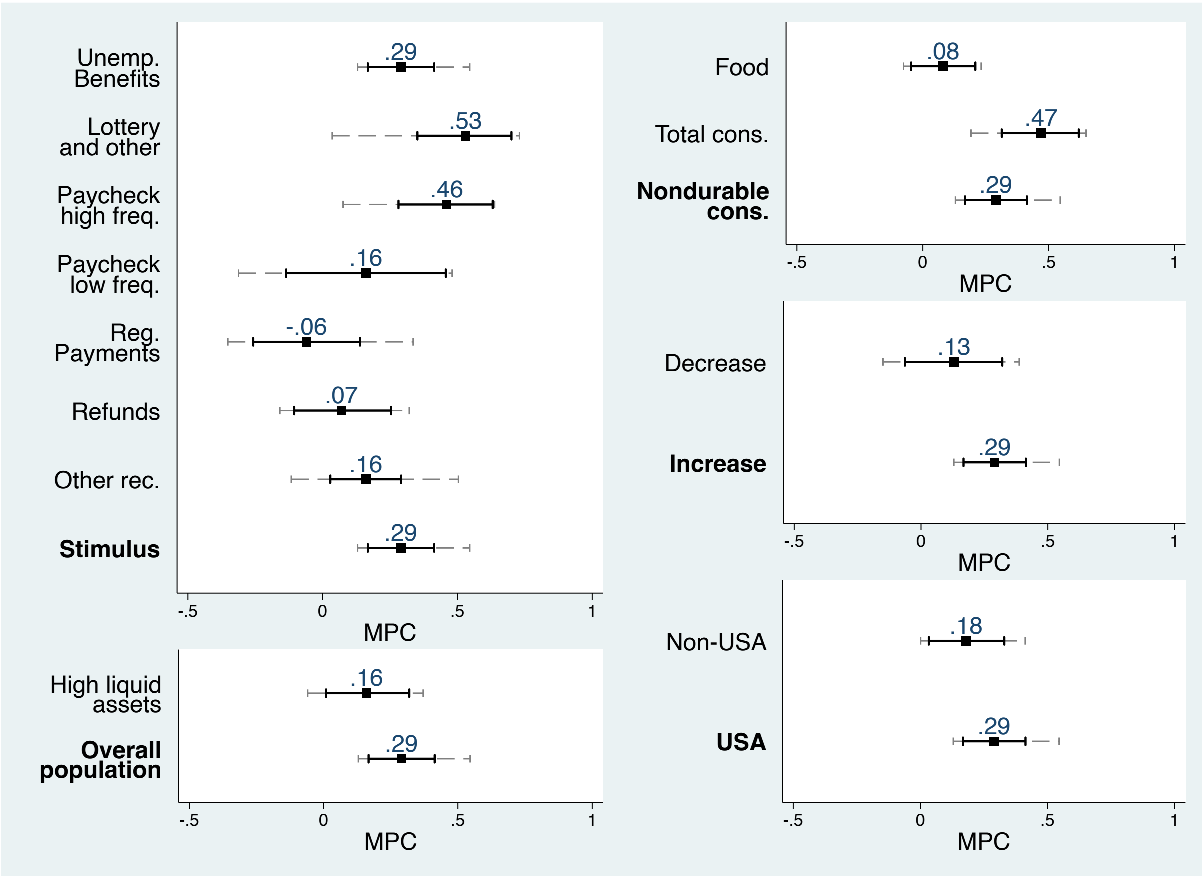
The estimates of marginal propensity to consume are higher in times of economic distress. As the unemployment rate rises from 6% to 8%, the MPC goes up from .29 to .39. When the economy is booming and the unemployment rate is around 4%, the MPC drops to .19. One interpretation of this finding is that, in recessions, as more households loose jobs and are forced to drain down their liquid savings, liquidity constraints become binding for a larger fraction of population; as a result, households spend larger fractions of stimulus checks upon receipt. This interpretation is in line with Gross *et al.* (2020), who find that household consumption is more responsive to an increased access to credit during economic downturns.

This countercyclical property of MPCs could mean that fiscal stimulus is more effective in recessions because it elicits larger consumption response. One caveat is in order: the MPCs considered here are partial equilibrium responses that ignore general equilibrium effects. Nevertheless, these estimates can serve as a basis for calibrating general equilibrium models and a reference point for comparing model responses to those observed in micro data.

In general equilibrium models, higher MPCs typically mean higher fiscal multipliers and more effective fiscal policy. Parker (2011) points out that most studies of fiscal multipliers that use linear methods—such as VARs without regime switches, or DSGE models linearized around one steady state—obtain multipliers that are averaged over the business cycle, as opposed to multipliers that arise in recessions. Yet it is the latter that are pertinent for making decisions about fiscal policy. Auerbach & Gorodnichenko (2012) document prominent differences between fiscal multipliers in times of economic booms and in recessions, and find recession multipliers to be much larger. The findings in this paper offer an additional argument in favor of considering state-dependent multipliers, and provide an insight into why fiscal policy may be more effective in times of economic turmoil.

The right panel of Figure 1 depicts MPC estimates for different payment sizes. The estimates of marginal propensity to consume are smaller for payments that are more salient. The fitted MPC for a stimulus payment of $600 is .39, while for a payment of $1800 it is .23. Thus, policy experiments that distribute smaller payments may be more effective in producing an immediate consumption response per dollar spent—and be associated with higher fiscal multipliers.

Figure 3: Fitted 'best practice' MPC. Payment and data characteristics



Notes: the figure shows point estimates based on the meta-regression model described in the right panel of Table 5, see model details in Section 3. The baseline estimates denoted in bold reflect the marginal propensities to raise nondurable consumption upon receiving a $1200 stimulus check, conditional on unemployment rate of 6%, and on the estimate being obtained with US data and reported in a widely cited study. Estimates not marked in bold have the same interpretation except for the dimensions denoted on the vertical axes of the graphs. The two sets of bands reflect 95% confidence intervals, the narrower solid band is computed with conventional clustering on the study level, the wider dashed band uses wild bootstrap cluster.

Fitted estimates of the marginal propensity to consume differ across payment types (see Figure 3). The marginal propensity to consume out of stimulus checks (.29) is higher compared to MPC out of changes in regular payments (-.06), refunds (.07), changes in the take-home pay (.16) and other recurring payments (.16)—these latter estimates are statistically indistinguishable from zero (though some of these results change once working papers are excluded, see subsection 3.4). Point estimates are higher for lottery and other one-time payments (a result that is sensitive to outliers, see subsection 3.4) and when measuring the immediate response to paycheck receipt. Marginal propensity to consume out of unemployment benefits was not found to be statistically different from MPC out of stimulus checks.

As discussed in the previous sections, these disparities in how consumers respond to different types of payments may be a product of mental accounting, which renders inflows coming from different sources non-fungible in the eyes of the consumers. This non-fungibility can have profound implications for how economic policy affects consumer behavior, and through it, the economy. Overall, it appears that consumer responses to fluctuations in recurring inflows and outflows of cash are more muted, whereas the responses to one-time income receipts are more pronounced. Thus, for example, a stimulus check disbursement may elicit stronger partial equilibrium consumption response than a comparable decreases in property taxes or mortgage payments.

Other things equal, MPC should be larger for broader definitions of consumption: the more categories of consumer goods are included, the higher the overall spending, the larger the apparent consumption response to payment receipt. This reasoning is corroborated by the findings depicted on Figure 3: consumption response is more prominent for total consumption (with MPC of .47 as opposed to .29 for nondurables) and less pronounced for spending on food (MPC of .08).

Marginal propensities to consume are lower for households with high values of liquid assets—about .16 compared to .29 for general population. This result provides additional support for the conjecture that the negative association between unemployment rates and MPCs arises because liquidity dries up when households loose jobs. There is also some asymmetry between responses to income increases and declines: when income falls, consumption does not seem to follow, as MPC out of income declines is only .13 (albeit not robust to some of the subsampling, see subsection 3.4). This latter result is in line with Baugh *et al.* (2021) who find that, while households respond by raising consumption when receiving a tax refund, they do not lower consumption when making payments.

I also compute fitted estimates based on the specification displayed in the right panel of Table 6 that assumes that average MPCs do not change over time—the results are depicted on Figure B4 and Figure B5. The key results are similar, albeit the fitted estimates for 'recurring' payment types are somewhat closer to zero. Importantly, in Table 6 the control *Account data* ends up among the variables picked out by the BMA and is thus included in the frequentist check, i.e. the specification the fitted estimates are based on. This changes the interpretation of the constant term: its baseline estimate of .25 now reflects the 'best practice' MPC estimate that is not based on account data. The fitted estimate of MPC based on data on financial accounts is higher: it is .39. As discussed in the previous sections, both types of data are associated with unique challenges which may give rise to this discrepancy. Finally, I compute and compare fitted estimates of the MPC for the modified samples explored in subsection 3.4—the results are depicted on Figure B6.[33] The MPC point estimates are fairly similar across specifications, ranging between $.25 - .31$.

[33] To obtain these estimates, I use the same strategy as before (see Table 5 and Table 6). First, I estimate BMA; second, I run a frequentist check that only includes variables with PIP higher than .5; third, I record the constant term with the corresponding confidence intervals from the frequentist check on Figure B6.

5 Conclusion

When recessions hit, governments deploy policy measures meant to boost the economy, including in the form of one-time payments to households. Whether or not such measures are effective at achieving that goal depends on the fraction of the payment that is being consumed shortly after the payment is received. This paper brought forth a dataset comprised of all estimates of marginal propensities to consume out of transitory or predictable payments reported by the existing literature. I pin down several sources of systematic variation in the MPC estimates and show that the immediate results of a policy intervention crucially depend on the particular context in which it is carried out. I provide fitted MPC values for a number of different policy-relevant scenarios. The baseline point estimate of the quarterly MPC out of a \$1200 stimulus check is found to be around .29 (assuming a 6% unemployment rate), but the estimates vary widely depending on a number of parameters.

Estimates of MPC are higher in times of high unemployment, possibly owing to the role played by liquidity constraints: as more households loose jobs, liquidity constraints become binding for a larger fraction of population—on average, consumption becomes more sensitive to cash flows. This interpretation is further corroborated by the finding of lower MPCs for subsamples of population holding ample liquid assets. The countercyclical feature of MPC estimates is quite prominent: conditional on the unemployment rate of 4%, the MPC is around .19, while it is .39 given an 8% unemployment. While I only provide estimates for a partial-equilibrium consumption response to cash inflows, these findings highlight the importance of considering general equilibrium models that allow for state-dependent multipliers.

Marginal propensities to consume differ depending on the features of the particular cash flows they pertain to. More salient cash flows are associated with lower MPCs: a payment of \$600 is linked with an MPC of .39, while a \$1800 payment results in an MPC of about .23. This relationship may arise due to consumers having near-rational preferences or facing costs of re-calculating the optimal consumption path. Thus, policy interventions that distribute smaller payments may elicit a stronger immediate consumption response per dollar spent. I also find some evidence of systematic variation in MPC estimates depending on the type of payment considered: MPC out of tax refunds and some other recurring cash flows appear smaller compared to those out of one-time payments, such as stimulus checks. These differences may be a result of consumers using a form of mental accounting, assigning different uses to income coming from different sources, which then renders some cash inflows non-fungible. This feature implies that the manner in which households receive a particular cash inflow matters for the corresponding consumption response.

The estimates of marginal propensities to consume also seem to be asymmetrical across income increases and declines, possibly due to loss aversion. There is also some (weaker) evidence suggesting that MPC estimates may differ across countries and depend on the sources of data used to obtain them.

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Appendix A: Description of Variables

Table A1: Definitions and summary statistics of explanatory variables

Variable	Description	Mean	Std. dev.
Payment characteristics			
Payment amount	Log of the absolute dollar value of the income change deflated to 2021 dollars. **Centered** at ln(1200).	-0.366	1.056
Decrease	=1 if MPC estimate pertains to an income decrease (reference group: income increase).	0.105	0.307
Pay: unemp. benefits	=1 if the income source is unemployment benefits (reference group: stimulus checks). See details in Table A2.	0.007	0.085
Pay: lottery and other one-time	=1 if the income source is lottery winnings or other one-time payments (reference group: stimulus checks). See details in Table A2.	0.042	0.200
Pay: paycheck, high frequency	=1 if the income source is recurring paychecks, and the MPC refers to short-term response following paycheck receipt (reference group: stimulus checks). See details in Table A2.	0.101	0.302
Pay: paycheck, low frequency	=1 if the income source is recurring paychecks, and the MPC refers to response to changes in take-home pay (reference group: stimulus checks). See details in Table A2.	0.075	0.263
Pay: regular payments	=1 if the source of the cash flow change is a change in regular payments (reference group: stimulus checks). See details in Table A2.	0.094	0.292
Pay: refunds	=1 if the income source is tax refunds (reference group: stimulus checks). See details in Table A2.	0.117	0.321
Pay: other recurring	=1 if the income source is some other recurring payment (reference group: stimulus checks). See details in Table A2.	0.137	0.344
Liquidity constraints			
LCB: liquid assets	=1 if the estimate pertains to a subsample of population with low liquid assets (reference group: general population). See details in Table A3 Panel A.	0.080	0.272
LCB: income	=1 if the estimate pertains to a subsample of population with low income (reference group: general population). See details in Table A3 Panel A.	0.055	0.229
LCB: age	=1 if the estimate pertains to a subsample of population that is young (reference group: general population). See details in Table A3 Panel A.	0.016	0.126
LCB: home ownership	=1 if the estimate pertains to a subsample of population that has a mortgage or is renting (reference group: general population). See details in Table A3 Panel A.	0.016	0.126
LCB: other	=1 if the estimate pertains to a subsample of population that is likely to face binding liquidity constraints based on other indicators (reference group: general population). See details in Table A3 Panel A.	0.024	0.153
LCNB: liquid assets	=1 if the estimate pertains to a subsample of population with high liquid assets (reference group: general population). See details in Table A3 Panel B.	0.077	0.267

Continued on next page

Table A1: Definitions and summary statistics of explanatory variables (continued)

Variable	Description	Mean	Std. dev.
LCNB: income	=1 if the estimate pertains to a subsample of population with high income (reference group: general population). See details in Table A3 Panel B.	0.058	0.234
LCNB: age	=1 if the estimate pertains to a subsample of population that is old (reference group: general population). See details in Table A3 Panel B.	0.027	0.161
LCNB: home ownership	=1 if the estimate pertains to a subsample of population of households with no mortgage (reference group: general population). See details in Table A3 Panel B.	0.058	0.234
LCNB: other	=1 if the estimate pertains to a subsample of population that is likely not to face binding liquidity constraints based on other indicators (reference group: general population). See details in Table A3 Panel B.	0.017	0.129
Unemployment	Average country unemployment rate over the period during which payments were received by households. Expressed as a share. **Centered** at .06.	0.011	0.029
Consumption definition			
Total cons.	=1 if MPC refers to propensity to raise total consumption/spending (reference group: consumption of nondurables).	0.368	0.483
Food	=1 if MPC refers to propensity to raise consumption of food; this includes both total food expenditure and spending on food subcategories, e.g. dining, food at home (reference group: consumption of nondurables).	0.082	0.274
Indiv. category	=1 if MPC refers to propensity to raise spending on an individual consumption category (excluding food subcategories), e.g. apparel, durables (reference group: consumption of nondurables).	0.252	0.435
Data features			
non-USA data	=1 if data comes from countries other than the US.	0.271	0.445
Midyear of data	Log of the average year of data used to obtain the estimate less first average year in the dataset (1982.5). **Centered** around the 90th percentile corresponding to 2018.	-0.561	0.845
Account data	=1 if the estimate is obtained from financial account data.	0.448	0.497
Cross-section ratio	A ratio of the log number of cross-sectional units in the sample to the log number of periods. **Centered** around the mean value, i.e. 4.97.	0.000	3.747
Method & Publication characteristics			
IV	=1 if estimate is obtained with an instrumental variable technique.	0.162	0.369
SE	The standard error associated with the estimate, scaled to quarterly frequency.	0.143	0.256
Citations	Log citations per year. Citations per year are computed using $Citations/(2022 - First\ year)$, where *Citations* are the overall number of citations on Google Scholar and *First year* is the year a (working paper) version of the paper first appeared on Google Scholar. **Centered** at the 90th percentile, i.e. 2.8.	-1.451	1.075

Table A2: Controlling for payment types

Non-recurring			
One-time stimulus payments			
1994	Japan	Stimulus tax rebate	LaPoint & Unayama (2020)
2001	USA	Income tax rebates	Johnson *et al.* (2006), Agarwal *et al.* (2007)
2008	USA	Economic stimulus payments	Parker *et al.* (2013), Broda & Parker (2014), Parker (2017), Parker & Souleles (2019), Boutros (2021)
2011	Singapore	Growth dividend program	Agarwal & Qian (2014)
2020	USA	CARES act payment	Misra *et al.* (2022), Karger & Rajan (2020), Baker *et al.* (2020)
Unemployment benefits			
2014-2016	USA	Unemployment benefits	Ganong & Noel (2019)
2011-2013	Brazil	Unemployment benefits	Gerard & Naritomi (2021)
Lottery and other			
1994-2006	Norway	Lottery winnings	Fagereng *et al.* (2021)
2011-2017	Iceland	Lottery winnings	Olafsson & Pagel (2021)
2011-2014	UK	Reported yearly unanticipated income	Bunn *et al.* (2018)
Recurring			
Paycheck, high frequency			
1986-1996	USA	Social security check receipt	Stephens (2003)
2012-2016	USA	Paycheck receipt	Gelman (2021a)
2009-2012	USA	Paycheck receipt	Kuchler & Pagel (2021)
2013	USA	Paycheck receipt	Baker & Yannelis (2017), Gelman *et al.* (2020)
2011-2015	Iceland	Regular income receipt	Olafsson & Pagel (2018)
2017-2019	Germany	Regular income receipt	Bräuer *et al.* (2022)
2012-2013	USA	Regular income receipt	Gelman *et al.* (2014)
Paycheck, low frequency			
1980-1993	USA	Changes in take home pay	Parker (1999)
1982-1983	USA	Changes in take home pay	Souleles (2002)
2012-2018	USA	Changes in monthly pay	Ganong *et al.* (2020)
Regular payments			
2009-2013	Italy	Mortgage payments	Jappelli & Scognamiglio (2018)
1988-2001	USA	Mortgage payments	Coulibaly & Li (2006)
2010-2014	China	Mortgage payments	Zhao *et al.* (2020)
2012-2012	Italy	Property tax payments	Surico & Trezzi (2018)
Tax refunds			
2012-2016	USA	Tax refunds	Gelman (2021b), Gelman *et al.* (2022)
1980-1991	USA	Tax refunds	Souleles (1999)
2011-2015	USA	Tax refunds receipt/payment	Baugh *et al.* (2021)
Other regular			
1986-1990	Japan	Public pension benefits	Stephens & Unayama (2011)
2017-2019	Germany	Dividend payments	Bräuer *et al.* (2022)
1999-2016	Germany	Capital gains from mutual fund liquidations	Meyer *et al.* (2020)
2010-2014	USA	Alaska permanent Fund payment	Kueng (2018)
1980-2001	USA	Alaska permanent Fund payment	Hsieh (2003)

Table A3: Controlling for severity of liquidity constraints

Panel A. Liquidity Constraints Binding	
LCB: liquid assets	
Low liquid assets	Johnson *et al.* (2006), Parker *et al.* (2013), Kueng (2018), Gelman (2021b), Fagereng *et al.* (2021), Baugh *et al.* (2021), Baker *et al.* (2020), Bräuer *et al.* (2022), Gelman (2021a), Boutros (2021)
Low liquid assets normalized by income	Souleles (1999), Souleles (2002), Bräuer *et al.* (2022)
Low liquid assets normalized by consumption	Kueng (2018), Ganong *et al.* (2020)
Low liquid assets relative to own average liquid assets	Gelman (2021b)
Low net liquid assets	Bräuer *et al.* (2022)
Low net liquid assets normalized by consumption	Olafsson & Pagel (2021)
Low total assets	Ganong & Noel (2019), Bräuer *et al.* (2022)
The household does not have at least two months of income available in liquid wealth	Broda & Parker (2014), Parker (2017)
Less than enough assets to finance one month of non-durable consumption	Parker (1999)
LCB: Income	
Low income	Souleles (2002), Johnson *et al.* (2006), Parker *et al.* (2013), Broda & Parker (2014), Parker (2017), Kueng (2018), Olafsson & Pagel (2018), Ganong & Noel (2019), Baker *et al.* (2020), Olafsson & Pagel (2021)
Low income relative to own average income	Olafsson & Pagel (2021)
Low age-adjusted income	LaPoint & Unayama (2020)
Income has decreased	Parker (2017)
LCB: Age	
Young	Parker (1999), Souleles (2002), Johnson *et al.* (2006), Agarwal *et al.* (2007), Parker *et al.* (2013), Ganong & Noel (2019), Fagereng *et al.* (2021), Bräuer *et al.* (2022), LaPoint & Unayama (2020), Meyer *et al.* (2020)
LCB: Home ownership	
Mortgagors	Parker *et al.* (2013), Jappelli & Scognamiglio (2018), Surico & Trezzi (2018), LaPoint & Unayama (2020)
Renters	LaPoint & Unayama (2020)
LCB: other	
Small credit card limit	Agarwal *et al.* (2007)
High credit card utilization	Agarwal *et al.* (2007)
Young or have a small credit limit with high utilization	Agarwal *et al.* (2007)
No credit card	Baker & Yannelis (2017)
Low saving	Baker & Yannelis (2017)
Low debt service ratio	LaPoint & Unayama (2020)
Debtors	Surico & Trezzi (2018)
Low deposits around payment receipt	Bräuer *et al.* (2022)
Low permanent income	Kueng (2018)
Unemployed households	Ganong & Noel (2019)
Payment is high relative to income	Boutros (2021)

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Table A3: Controlling for severity of liquidity constraints (continued)

Panel B. Liquidity Constraints Not Binding	
LCNB: liquid assets	
High liquid assets	Johnson *et al.* (2006),Parker *et al.* (2013), Kueng (2018), Ganong & Noel (2019), Fagereng *et al.* (2021), Baker *et al.* (2020), Baugh *et al.* (2021), Bräuer *et al.* (2022), Gelman (2021a), Boutros (2021)
High liquid assets normalized income	Souleles (1999), Souleles (2002), Bräuer *et al.* (2022)
High liquid assets normalized by consumption	Ganong *et al.* (2020)
High liquid assets relative to own average liquid assets	Gelman (2021b)
High net liquid assets	Bräuer *et al.* (2022)
High net liquid assets normalized by consumption	Olafsson & Pagel (2021)
High total assets	Bräuer *et al.* (2022)
The household has at least two months of income available in liquid wealth	Broda & Parker (2014), Parker (2017)
More than enough assets to finance six months of nondurable consumption	Parker (1999)
LCNB: Income	
High income	Parker (1999), Souleles (2002), Johnson *et al.* (2006), Parker *et al.* (2013), Broda & Parker (2014), Parker (2017), Olafsson & Pagel (2018), Kueng (2018), Baker *et al.* (2020), Olafsson & Pagel (2021)
High income relative to own average income	Olafsson & Pagel (2021)
High age-adjusted income	LaPoint & Unayama (2020)
Income has increased	Parker (2017)
LCNB: Age	
Old	Parker (1999), Souleles (2002), Johnson *et al.* (2006), Agarwal *et al.* (2007), Stephens & Unayama (2011), Parker *et al.* (2013), Fagereng *et al.* (2021), Bräuer *et al.* (2022), Meyer *et al.* (2020), LaPoint & Unayama (2020)
LCNB: Home ownership	
Homeowners with no mortgage	Coulibaly & Li (2006), Parker *et al.* (2013), LaPoint & Unayama (2020)
Households with no mortgage	Surico & Trezzi (2018)
Homeowners with other residential properties	Surico & Trezzi (2018)
LCNB: Other	
Has credit card	Baker & Yannelis (2017)
Old or have a high credit limit with low utilization	Agarwal *et al.* (2007)
High credit card limit	Agarwal *et al.* (2007)
Low credit card utilization	Agarwal *et al.* (2007)
High saving	Baker & Yannelis (2017)
Payment is small relative to income	Ganong & Noel (2019), Boutros (2021)
High deposits around payment receipt	Bräuer *et al.* (2022)
High permanent income	Kueng (2018)
No debt	LaPoint & Unayama (2020), Surico & Trezzi (2018)
Payweek spending is affordable without reducing the user's resources below the 5th percentile of their average resources	Kuchler & Pagel (2021)

Appendix B Robustness

Appendix B.1 Robustness of main results

Figure B1: Outlier treatments

Panel A: Posterior Inclusion Probabilities

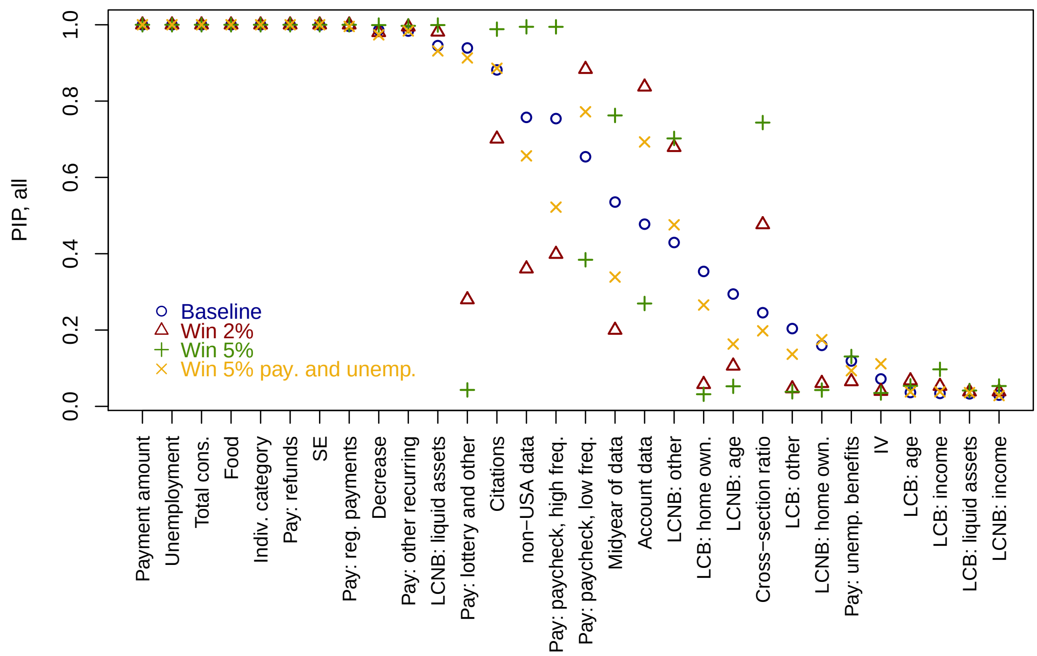

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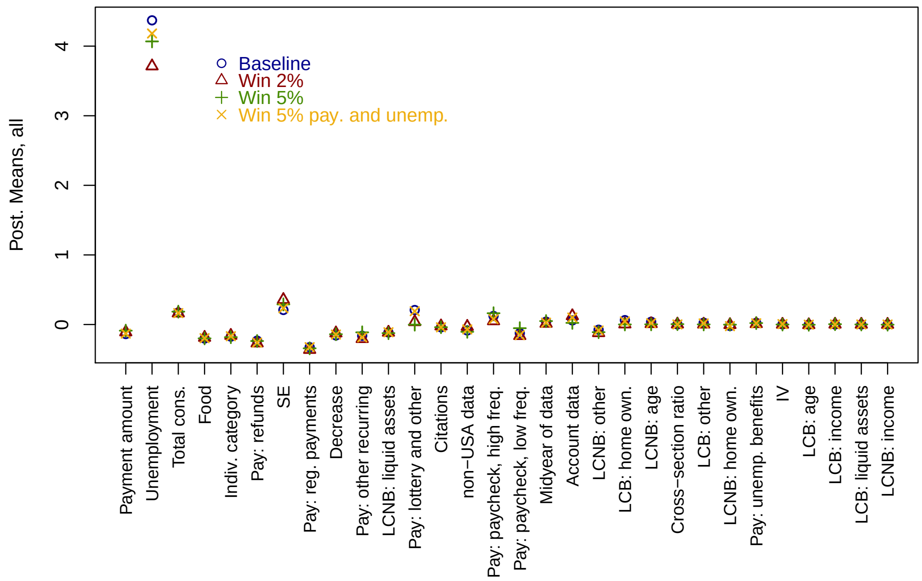
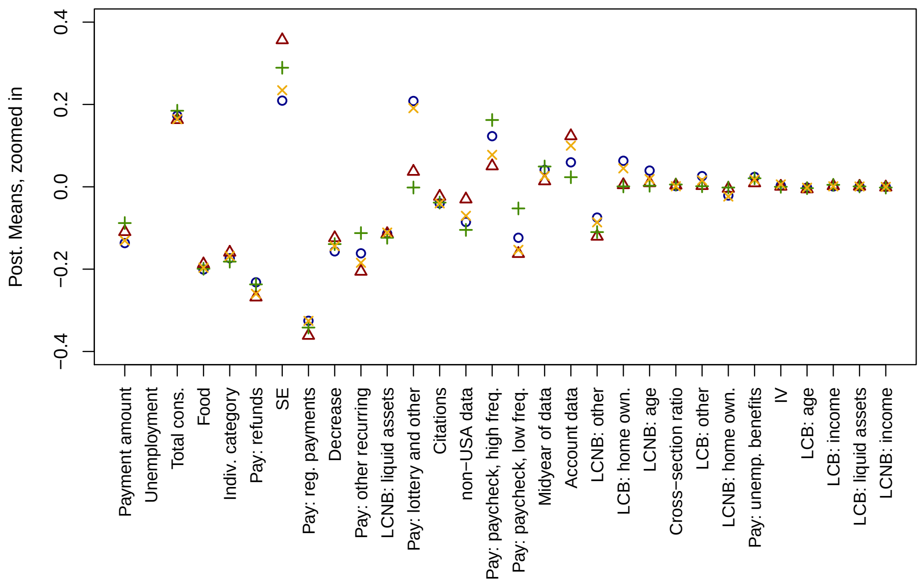
Figure B1: Outlier treatments

Panel B: Posterior Means

(a) All



(b) Zoomed in



Notes: The figure shows BMA estimation results given alternative outlier treatments. The estimation procedure is the same as the one used to obtain the results reported on Figure 2 and summarized in the left panel of Table 5. Panel A shows posterior inclusion probabilities; Panel B(a) shows all posterior means; Panel B(b) zooms in on the means between -.4 and .4. *Baseline*=dataset used in the main body of the paper, estimates same as those in Table 5; *Win 2%*=dataset with outliers in MPC estimates and respective standard errors winsorized at 2%; *Win 5%*=dataset with outliers in MPC estimates and respective standard errors winsorized at 5%; *Win 5% pay. and unemp.*=dataset with outliers in *Payment amount* and *Unemployment* winsorized at 5%. Variable definitions are available in Table A1.

Figure B2: Subsamples

Panel A: Posterior Inclusion Probabilities

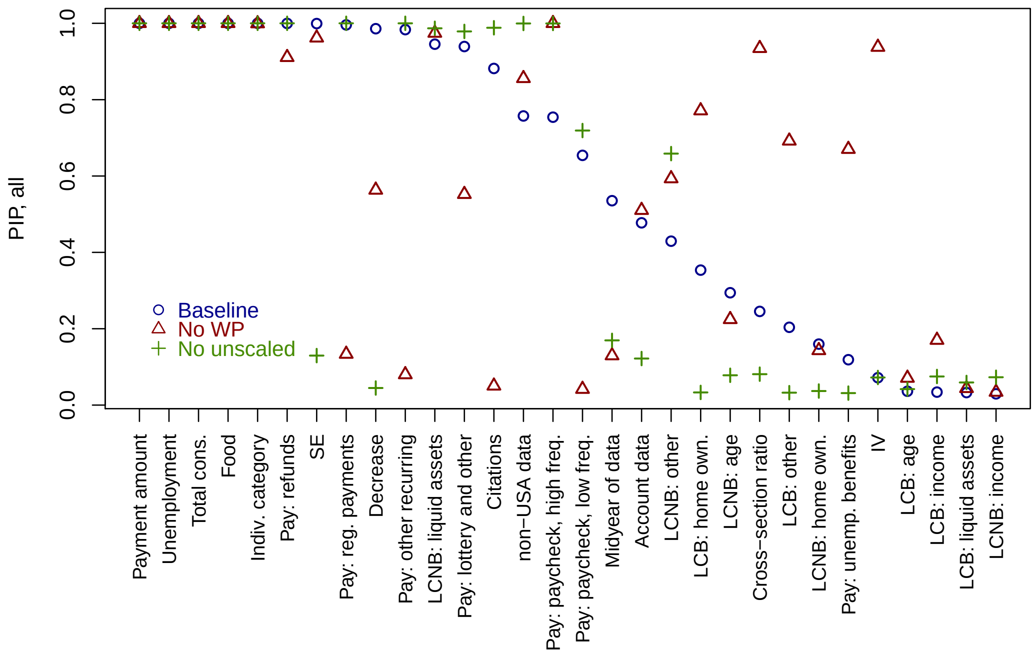


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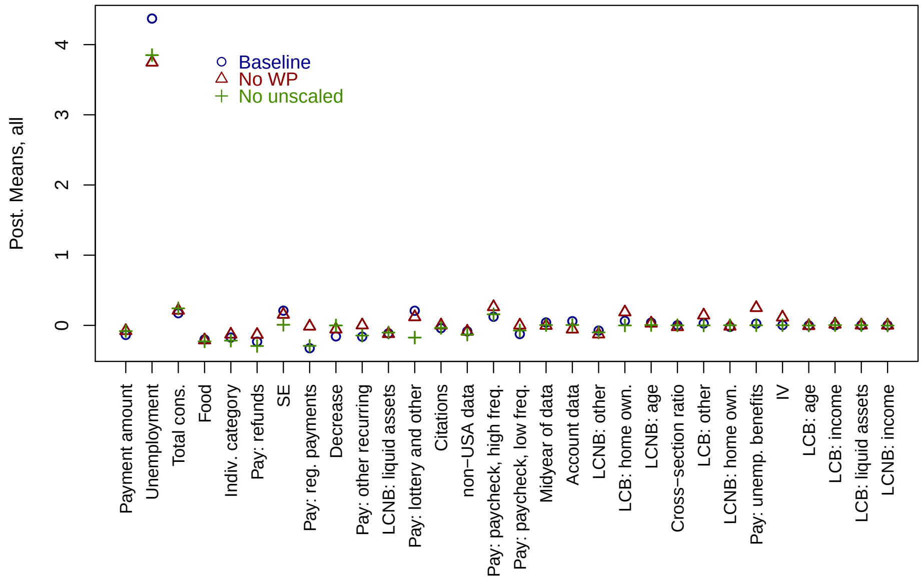
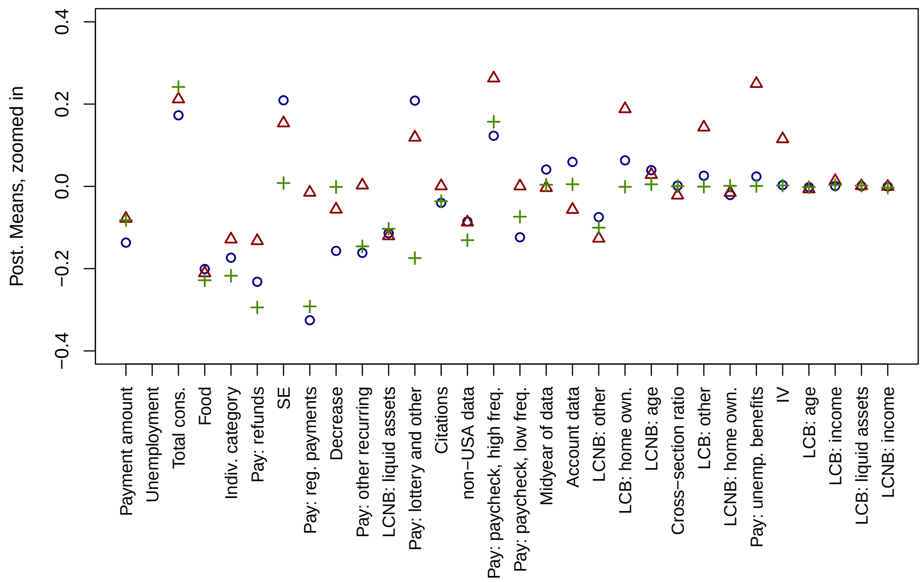
Figure B2: Subsamples

Panel B: Posterior Means

(a) All



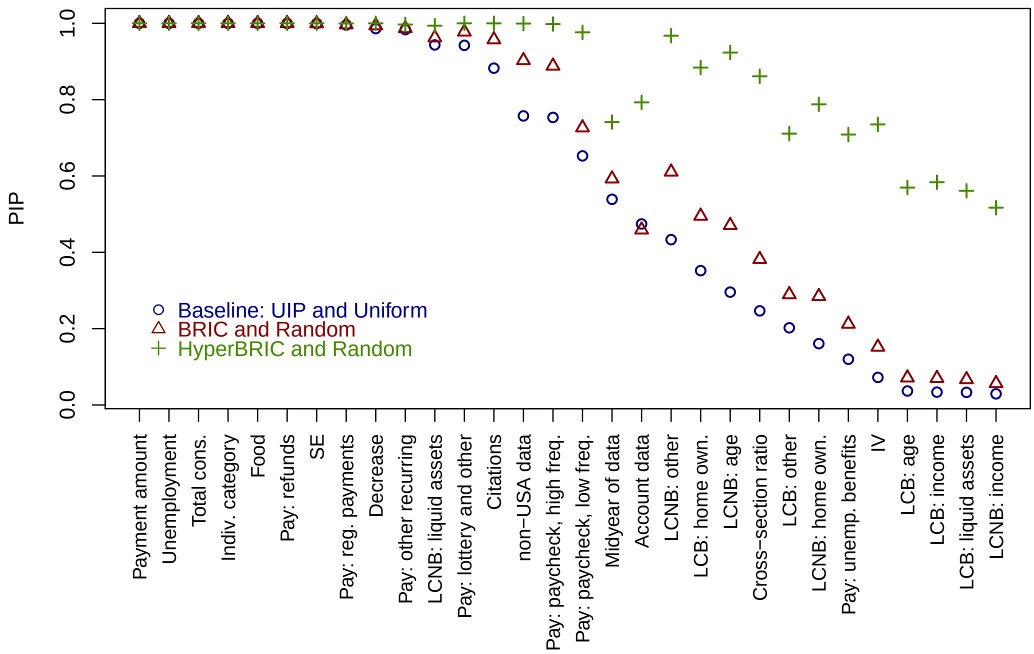
(b) Zoomed in



Notes: The figure shows BMA estimation results for alternative subsampling of the original dataset. The estimation procedure is the same as the one used to obtain the results reported on Figure 2 and summarized in the left panel of Table 5. Panel A shows posterior inclusion probabilities; Panel B(a) shows all posterior means; Panel B(b) zooms in on the means between -.4 and .4. *Baseline*=dataset used in the main body of the paper, estimates same as those in Table 5; *No WP*=subsample of 1019 estimates with results from unpublished work excluded; *No unscaled*=subsample of 1160 estimates with unscaled estimates excluded. Variable definitions are available in Table A1.

Figure B3: Priors

Panel A: Posterior Inclusion Probabilities

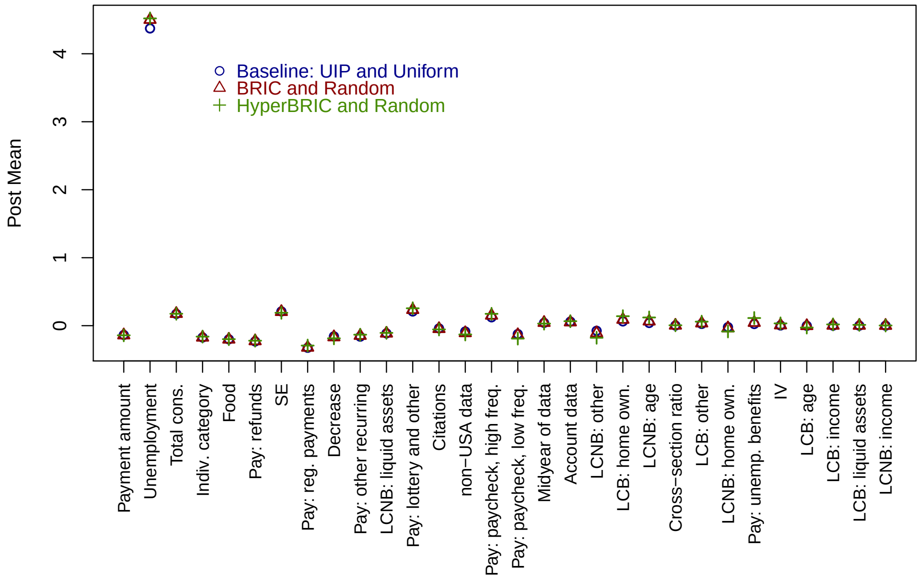


Continued on next page

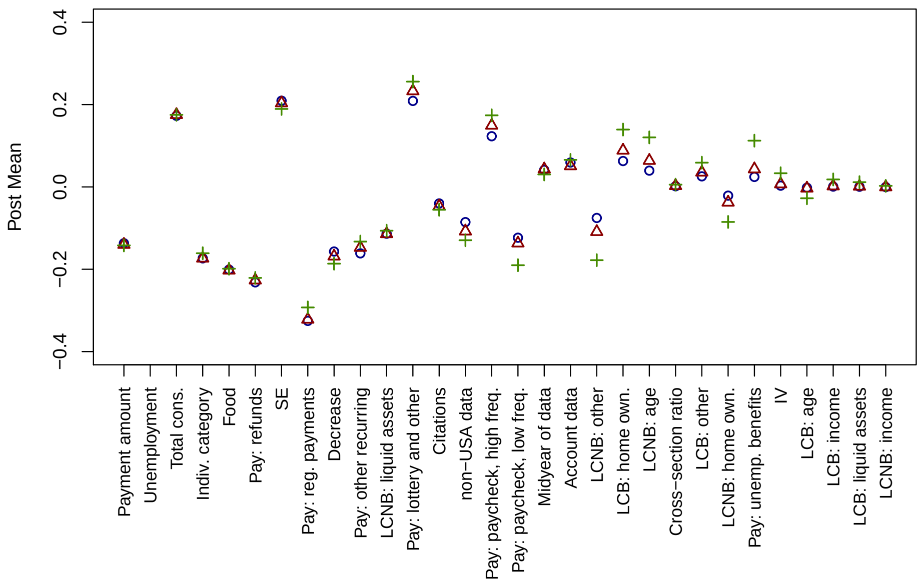
Figure B3: Priors

Panel B: Posterior Means

(a) All

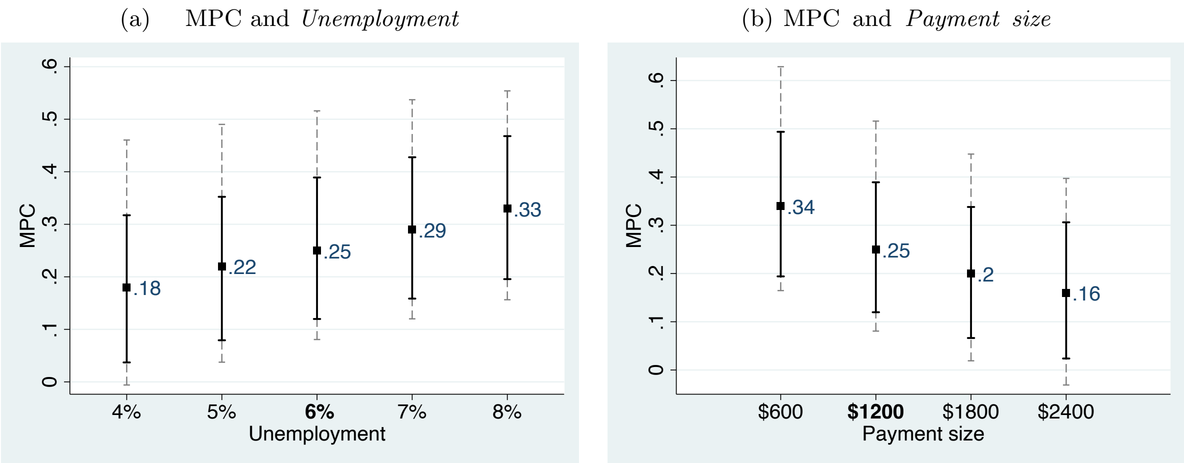


(b) Zoomed in



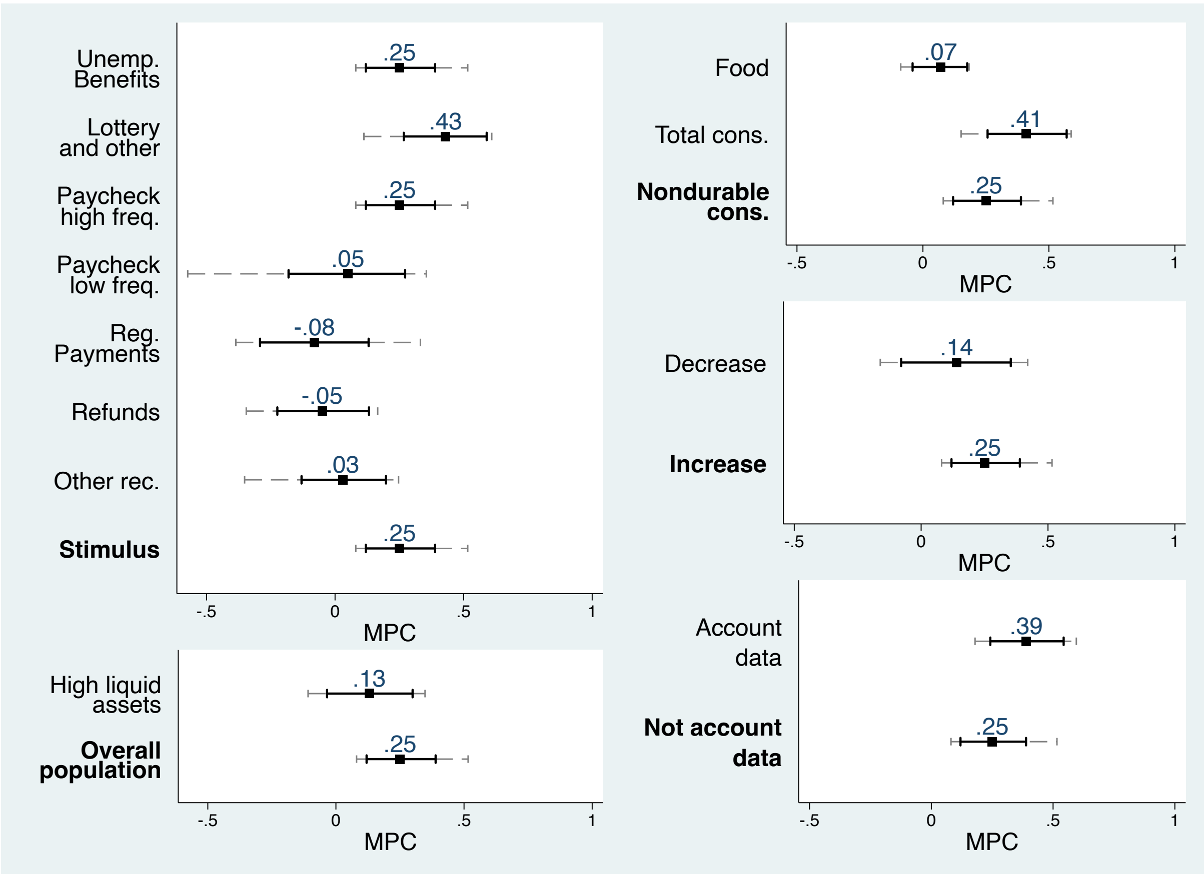
Notes: The figure shows BMA estimation results under alternative priors for parameters and model space. Panel A shows posterior inclusion probabilities; Panel B(a) shows all posterior means; Panel B(b) zooms in on the means between -.4 and .4. *Baseline: UIP and Uniform*=specification used in the main body of the paper with Unit Information Prior for model parameters and Uniform prior for model space, estimates same as those in Table 5; *BRIC and Random*= benchmark g-prior suggested by Fernandez *et al.* (2001) for parameters and the beta-binomial model prior proposed by Ley & Steel (2009) for model space. *HyperBRIC and Random*= flexible data-dependent priors for parameters (Liang *et al.* 2008, Feldkircher & Zeugner 2012) and the beta-binomial prior for model space.

Figure B4: Fitted 'best practice' MPC. Unemployment rates and Payment sizes. No *Midyear of data*



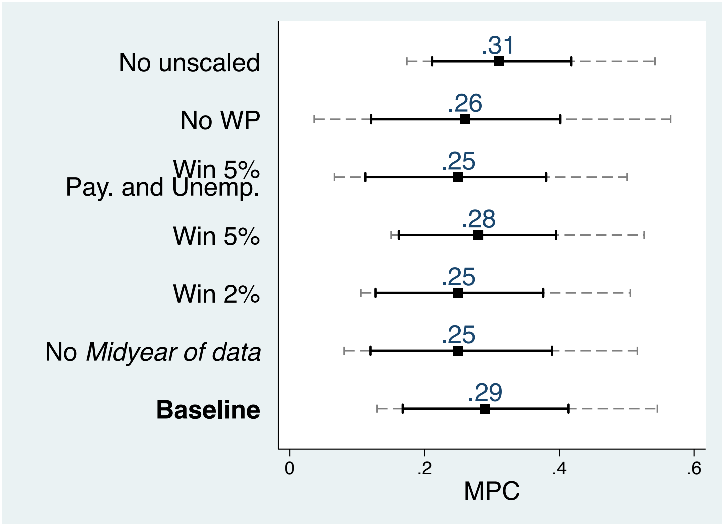
Notes: See notes for Figure 1. The figure shows point estimates based on the meta-regression model described in the right panel of Table 6 that excludes *Midyear of data*, see discussion in subsection 3.4.

Figure B5: Fitted 'best practice' MPC. Payment and data characteristics. No *Midyear of data*



Notes: See notes for Figure 3. The figure shows point estimates based on the meta-regression model described in the right panel of Table 6 that excludes *Midyear of data*, see discussion in subsection 3.4.

Figure B6: Fitted 'best practice' MPC. Payment and data characteristics. Robustness of the baseline



Notes: See notes for Figure 3. The figure shows point estimates of the constant term for the modified samples considered in subsection 3.4. These 'best practice' MPC are obtained by 1) running a BMA and 2) estimating an OLS that only includes variables with PIP higher than .5 in the BMA. *Baseline*=dataset used in the main body of the paper, estimate same as $\overline{mpc}$ in Table 5; *No Midyear of data*=estimate same as $\overline{mpc}$ in Table 6; *Win 2%*=dataset with outliers in MPC estimates and respective standard errors winsorized at 2%; *Win 5%*=dataset with outliers in MPC estimates and respective standard errors winsorized at 5%; *Win 5% pay. and unemp.*=dataset with outliers in *Payment amount* and *Unemployment* winsorized at 5%; *No WP*=subsample of 1019 estimates with results from unpublished work excluded; *No unscaled*=subsample of 1160 estimates with unscaled estimates excluded.

Appendix B.2 Linear scaling

Table B1: MPC sample statistics. Linear scaling

	Mean	Median	5%	95%	N	N studies
All	0.65	0.21	-0.02	2.92	1244	40
Unemployment$<$ .06	0.38	0.18	-0.00	1.26	629	18
Unemployment$\geqslant$ .06	0.93	0.26	-0.05	3.72	615	22
Payment amount$<$ \$1200	0.59	0.22	-0.06	2.51	824	25
Payment amount$\geqslant$ \$1200	0.77	0.20	-0.00	3.24	420	22
Pay: stimulus	0.73	0.26	0.01	2.94	532	12
Pay: unemp. benefits	0.37	0.36	0.21	0.81	9	2
Pay: lottery and other	0.61	0.13	0.04	2.64	52	3
Pay: paycheck	1.38	0.20	-0.00	5.40	130	8
Pay: reg. payments	0.36	0.13	-0.13	1.33	200	6
Pay: refunds	0.18	0.14	-0.04	0.57	145	4
Pay: other recurring	0.61	0.24	-0.00	2.45	176	6
LC binding	0.59	0.27	-0.02	2.60	225	25
LCB: liquid assets	0.38	0.27	0.00	1.12	100	18
LC not binding	0.21	0.07	-0.07	0.93	281	27
LCNB: liquid assets	0.17	0.09	-0.02	0.83	96	18

Notes: This statistics is based on data scaled with linear transformation $\widehat{mpc}_3 = \widehat{mpc}_f \cdot \frac{3}{f}$ (as opposed to the transformation in 2). See details about payment types in Table A2. 'LC' refers to 'liquidity constraints'. See details about estimates pertaining to constrained and unconstrained households in Table A3.

Appendix C Studies Used in Meta-analysis

I used the following search query to find the relevant studies in Google Scholar:

> Consumption estimate ('mpc' OR 'marginal propensity to consume') ('rule of thumb' OR 'hand-to-mouth') transitory income

I ran the search on April 1 2021, for studies that came out in or after 2018. The search returned 33 pages with 10 papers per page. The results were saved.

Papers in Study

Agarwal, S., C. Liu, & N. Souleles (2007): "The reaction of consumer spending and debt to tax rebates?evidence from consumer credit data." *Journal of Political Economy* **115(6)**: pp. 986–1019.

Agarwal, S. & W. Qian (2014): "Consumption and Debt Response to Unanticipated Income Shocks: Evidence from a Natural Experiment in Singapore." *American Economic Review* **104(12)**: pp. 4205–4230.

Baker, S. R., R. A. Farrokhnia, S. Meyer, M. Pagel, & C. Yannelis (2020): "Income, liquidity, and the consumption response to the 2020 economic stimulus payments." *Working Paper 27097*, National Bureau of Economic Research.

Baker, S. R. & C. Yannelis (2017): "Income Changes and Consumption: Evidence from the 2013 Federal Government Shutdown." *Review of Economic Dynamics* **23**: pp. 99–124.

Baugh, B., I. Ben-David, H. Park, & J. A. Parker (2021): "Asymmetric consumption smoothing." *American Economic Review* **111(1)**: pp. 192–230.

Boutros, M. (2021): "Bounded Intertemporal Rationality and the Marginal Propensity to Consume." *Technical report.*

Bräuer, K., A. Hackethal, & T. Hanspal (2022): "Consuming Dividends." *The Review of Financial Studies* Hhac010.

Broda, C. & J. A. Parker (2014): "The Economic Stimulus Payments of 2008 and the Aggregate Demand for Consumption." *Journal of Monetary Economics* **68**: pp. 20–36.

Bunn, P., J. Le Roux, K. Reinold, & P. Surico (2018): "The consumption response to positive and negative income shocks." *Journal of Monetary Economics* **96**: pp. 1–15.

Coulibaly, B. & G. Li (2006): "Do Homeowners Increase Consumption after the Last Mortgage Payment? An Alternative Test of the Permanent Income Hypothesis." *The Review of Economics and Statistics* **88(1)**: pp. 10–19.

Fagereng, A., M. B. Holm, & G. J. Natvik (2021): "Mpc heterogeneity and household balance sheets." *American Economic Journal: Macroeconomics* **13(4)**: pp. 1–54.

Ganong, P., D. Jones, P. J. Noel, F. E. Greig, D. Farrell, & C. Wheat (2020): "Wealth, race, and consumption smoothing of typical income shocks." *Working Paper 27552*, National Bureau of Economic Research.

Ganong, P. & P. Noel (2019): "Consumer spending during unemployment: Positive and normative implications." *American Economic Review* **109(7)**: pp. 2383–2424.

Gelman, M. (2021a): "The Self-Constrained Hand-to-Mouth." *The Review of Economics and Statistics* pp. 1–45.

Gelman, M. (2021b): "What drives heterogeneity in the marginal propensity to consume? temporary shocks vs persistent characteristics." *Journal of Monetary Economics* **117**: pp. 521–542.

Gelman, M., S. Kariv, M. D. Shapiro, & D. Silverman (2022): "Rational Illiquidity and Consumption: Theory and Evidence from Income Tax Withholding and Refunds." *American Economic Review* **(forthcoming)**.

Gelman, M., S. Kariv, M. D. Shapiro, D. Silverman, & S. Tadelis (2014): "Harnessing Naturally Occurring Data to Measure the Response of Spending to Income." *Science* **345(6193)**: pp. 212–215.

Gelman, M., S. Kariv, M. D. Shapiro, D. Silverman, & S. Tadelis (2020): "How individuals respond to a liquidity shock: Evidence from the 2013 government shutdown." *Journal of Public Economics* **189(C)**.

Gerard, F. & J. Naritomi (2021): "Job displacement insurance and (the lack of) consumption-smoothing." *American Economic Review* **111(3)**: pp. 899–942.

Hsieh, C.-T. (2003): "Do Consumers React to Anticipated Income Changes? Evidence from the Alaska Permanent Fund." *American Economic Review* **93(1)**: pp. 397–405.

Jappelli, T. & A. Scognamiglio (2018): "Interest rate changes, mortgages, and consumption: evidence from Italy." *Economic Policy* **33(94)**: pp. 183–224.

Johnson, D. S., J. A. Parker, & N. S. Souleles (2006): "Household Expenditure and the Income Tax Rebates of 2001." *American Economic Review*

96(5): pp. 1589–1610.

Karger, E. & A. Rajan (2020): "Heterogeneity in the Marginal Propensity to Consume: Evidence from Covid-19 Stimulus Payments." *Working Paper Series WP 2020-15*, Federal Reserve Bank of Chicago.

Kuchler, T. & M. Pagel (2021): "Sticking to your plan: The role of present bias for credit card paydown." *Journal of Financial Economics* **139(2)**: pp. 359–388.

Kueng, L. (2018): "Excess Sensitivity of High-Income Consumers." *The Quarterly Journal of Economics* **133(4)**: pp. 1693–1751.

LaPoint, C. & T. Unayama (2020): "Winners, Losers, and Near-Rationality: Heterogeneity in the MPC out of a Large Stimulus Tax Rebate." *Discussion papers 20067*, Research Institute of Economy, Trade and Industry (RIETI).

Meyer, S., M. Pagel, & A. Previtero (2020): "The Consumption Response to Realized Capital Gains: Evidence from Mutual Fund Liquidations." *Technical report.*

Misra, K., V. Singh, & Q. Zhang (2022): "Impact of stay-at-home-orders and cost-of-living on stimulus response: Evidence from the cares act." *Marketing Science* **41(2)**: pp. 211–229.

Olafsson, A. & M. Pagel (2018): "The Liquid Hand-to-Mouth: Evidence from Personal Finance Management Software." *Review of Financial Studies* **31(11)**: pp. 4398–4446.

Olafsson, A. & M. Pagel (2021): "Borrowing in Response to Windfalls." *Technical report.*

Parker, J. A. (1999): "The Reaction of Household Consumption to Predictable Changes in Social Security Taxes." *American Economic Review* **89(4)**: pp. 959–973.

Parker, J. A. (2017): "Why Don't Households Smooth Consumption? Evidence from a $25 Million Experiment." *American Economic Journal: Macroeconomics* **9(4)**: pp. 153–183.

Parker, J. A. & N. S. Souleles (2019): "Reported effects versus revealed-preference estimates: Evidence from the propensity to spend tax rebates." *American Economic Review: Insights* **1(3)**: pp. 273–90.

Parker, J. A., N. S. Souleles, D. S. Johnson, & R. McClelland (2013): "Consumer Spending and the Economic Stimulus Payments of 2008." *American Economic Review* **103(6)**: pp. 2530–53.

Souleles, N. S. (1999): "The Response of Household Consumption to Income Tax Refunds." *American Economic Review* **89(4)**: pp. 947–958.

Souleles, N. S. (2002): "Consumer Response to the Reagan Tax Cuts." *Journal of Public Economics* **85(1)**: pp. 99–120.

Stephens, M. (2003): "3rd of tha Month: Do Social Security Recipients Smooth Consumption Between Checks?" *American Economic Review* **93(1)**: pp. 406–422.

Stephens, M. & T. Unayama (2011): "The Consumption Response to Seasonal Income: Evidence from Japanese Public Pension Benefits." *American Economic Journal: Applied Economics* **3(4)**: pp. 86–118.

Surico, P. & R. Trezzi (2018): "Consumer Spending and Property Taxes." *Journal of the European Economic Association* **17(2)**: pp. 606–649.

Zhao, D., Y. Chen, & J. Shen (2020): "Mortgage payments and household consumption in urban china." *Economic Modelling* **93**: pp. 100–111.