

NBER WORKING PAPER SERIES

QUICK-FIXING: NEAR-RATIONALITY IN CONSUMPTION AND SAVINGS BEHAVIOR

Peter Andre  
Joel P. Flynn  
Georgios Nikolakoudis  
Karthik Sastry

Working Paper 33464  
<http://www.nber.org/papers/w33464>

NATIONAL BUREAU OF ECONOMIC RESEARCH  
1050 Massachusetts Avenue  
Cambridge, MA 02138  
February 2025, Revised September 2025

We are grateful to Kai Barron, Jonathon Hazell, David Laibson, Chen Lian, Lukas Nord, Ryan Oprea, Michael Peters, Oliver Pfäuti, Laura Pilossoph, Charlie Rafkin, Morten Ravn, Matthew Rognlie, Chris Roth, Tony Smith, Matthew White, Johannes Wohlfart, Michael Woodford, Chris Wolf, and seminar participants at the New York Fed, the University of Connecticut, the LAEF Workshop, CREI, Oxford, Cambridge, the Philadelphia Fed, Princeton, Columbia, the 2024 Winter SED Meeting, the 2025 Spring NBER Monetary Economics Meeting, the University of Chicago, Carnegie-Mellon, the University of Rochester, the CIGS Conference on Macroeconomic Theory and Policy, the Bank of Portugal Conference on Monetary Economics, and the 2025 NBER Summer Institute. We thank Zhuokai Huang for excellent research assistance. First posted version: October 25, 2024. Ethics approval: The study obtained ethics approval from the Princeton University Institutional Review Board (#15968). Preregistration: The main data collections were preregistered via OSF (<https://osf.io/sgw9y/registrations>). The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

NBER working papers are circulated for discussion and comment purposes. They have not been peer-reviewed or been subject to the review by the NBER Board of Directors that accompanies official NBER publications.

© 2025 by Peter Andre, Joel P. Flynn, Georgios Nikolakoudis, and Karthik Sastry. All rights reserved. Short sections of text, not to exceed two paragraphs, may be quoted without explicit permission provided that full credit, including © notice, is given to the source.

Quick-Fixing: Near-Rationality in Consumption and Savings Behavior  
Peter Andre, Joel P. Flynn, Georgios Nikolakoudis, and Karthik Sastry  
NBER Working Paper No. 33464  
February 2025, Revised September 2025  
JEL No. E21, E62, E70, G51

### **ABSTRACT**

When changing consumption-savings plans is costly, people may instead rely on quick-fixes: simple rules that avoid these costs. We field a novel survey to measure households' consumption responses to small and large income shocks. Almost 70% of households follow one of four quick-fixes and fully consume or fully save small shocks, abruptly adjust their behavior for large shocks, and thereafter behave similarly. In an incomplete-markets model, quick-fixing is near-rational: the average opportunity cost of quick-fixing is only \$17 per quarter. Yet, this small and empirically realistic deviation from benchmark models significantly alters aggregate consumption responses to income shocks.

Peter Andre  
Goethe University Frankfurt  
Leibniz Institute for Financial Research SAFE  
andre@safe-frankfurt.de

Joel P. Flynn  
Yale University  
Department of Economics  
and NBER  
joel.flynn@yale.edu

Georgios Nikolakoudis  
Princeton University  
gn3@princeton.edu

Karthik Sastry  
Princeton University  
Department of Economics  
and NBER  
ksastry@princeton.edu

# 1 Introduction

Households' consumption and savings choices, particularly their responses to income shocks, are a critical ingredient in essentially all models of the macroeconomy. Standard theory describes households as frictionless optimizers who follow spending and savings plans that maximize lifetime utility subject to budget constraints (Friedman, 1957; Hall, 1978). However, constantly optimizing consumption and savings plans to keep up with frequent changes in economic circumstances is practically challenging and often of limited benefit. For this reason, economists have acknowledged that households' behavior could be prone to *near-rationality* (Akerlof and Yellen, 1985b): suboptimal behavior when the gains from optimization are too small to justify its effort.

Nevertheless, understanding the microeconomic and macroeconomic implications of the near-rationality hypothesis faces a critical and well-recognized challenge (Cochrane, 1989): There is usually only one way to frictionlessly optimize, but the set of "almost optimal" policy functions is immense. Moreover, while many consumption-savings plans may almost achieve the frictionless payoff at the individual level, they can have drastically different macroeconomic consequences (Krusell and Smith, 1996).

This paper combines novel evidence and a near-rational extension of an incomplete-markets model to explore these questions: Do households' responses to income shocks exhibit near-rationality? If so, what forms does this near-rationality take? And how does the revealed nature of near-rationality affect the aggregate response to income shocks?

To make progress in answering these questions, we start by describing a near-rational model in which households can either optimize or use a *quick-fix*: a simple policy function with a potentially small convenience benefit. Quick-fixes have distinct implications for consumption policy functions: they are prevalent for small shocks, where mistakes are less costly, but abandoned for large shocks.

Motivated by this observation, we design a novel survey to elicit households' consumption policy functions by measuring their responses to a wide range of one-time income shocks. We find that most households rely on one of four distinct *quick-fixes* to respond to small shocks but not large shocks. These quick-fixes account for one-third of cross-sectional differences in marginal propensities to consume (MPCs) while being essentially unpredictable from rich economic and demographic information about households. We then combine our empirical estimates with a quantitative model to study the microeconomic and macroeconomic implications of our findings. The opportunity cost of near-rationality (relative to frictionless optimization) that rationalizes our data is only \$17 per quarter per household on average. Yet, we show that near-rational behavior significantly alters the response of aggregate con-

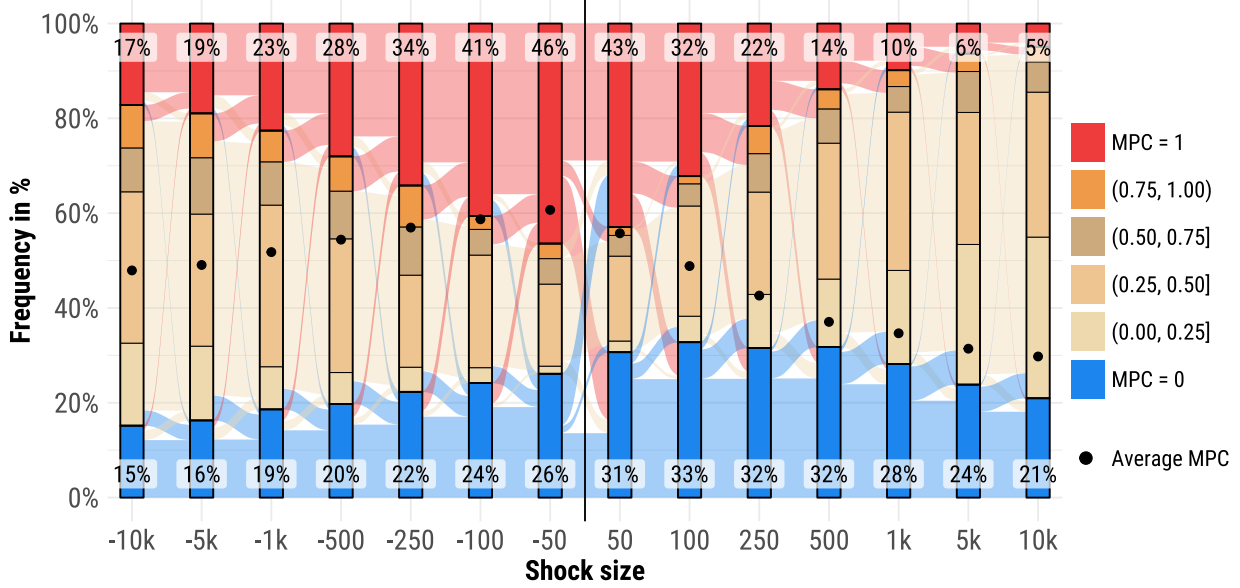
sumption to shocks and resolves important puzzles relating to the weak relationship between MPCs and wealth and the high front-loadedness of intertemporal MPCs.

**Formalizing Quick-Fixing.** To fix ideas and motivate our empirical strategy, we begin with a simple model. We consider a household living for two periods that decides how much to consume and save. A *frictionlessly optimizing* household optimally chooses consumption and savings to maximize payoffs. This defines an optimal policy function that maps income states to consumption levels. A *near-rational* household instead begins with a simple, potentially suboptimal policy function that we call a *quick-fix*. For example, the household might always set consumption at a reference level, letting savings adjust to shocks, or instead always set savings at a reference level, letting consumption adjust to shocks. The household derives a (potentially very small) convenience from using the quick-fix instead of perfectly optimizing, perhaps because the quick-fix requires less thinking, allows sticking to established habits, helps manage psychological urges, or avoids physical or mental transaction costs. The household quick-fixes if the payoff loss from doing so is lower than this convenience benefit and perfectly optimizes otherwise. This generates three key predictions for consumption policy functions: (i) quick-fixes are prevalent for small shocks, where payoff losses are low, but (ii) they are abruptly and discontinuously abandoned for large shocks, and (iii) after abandonment, households that would otherwise use different quick-fixes behave similarly.

A key implication of the model is that the distribution of household-level *consumption policy functions*—consumption responses to income shocks of different sizes—is the critical moment to identify quick-fixing behavior and evaluate the model against alternatives. In particular, these data can reveal what (if any) quick-fixes people use and what triggers people to abandon them. Measuring this object is impossible in observational data absent structural assumptions; even absent measurement error, such data can only reveal how a household responds to a single shock at a given time. This necessitates a survey-based research design that elicits responses to many hypothetical shocks of different sizes.

**Empirical Evidence.** We therefore design a survey to elicit household consumption policy functions. We administered this survey to about 5,000 US households in October and November of 2023. The sample approximates the US adult population in terms of gender, age, income, education, and region. We ask respondents how they would adjust their spending and saving over the next three months in response to multiple hypothetical shocks in random order: seven unexpected one-time payments ranging from \$50 to \$10,000 and seven unexpected one-time income losses of the same amounts. Collecting this *within-respondent* information differentiates our study from existing work using surveys to measure the marginal propensity to consume and allows us to detect and understand near-rational behavior. Our

**Figure 1:** The “bowtie” MPC distribution across shock sizes



*Notes:* The alluvial graph summarizes the MPC data of the 4,981 US households in our sample. Each of the 14 columns displays the distribution of MPCs for one particular shock size, with colors indicating the size of the MPC. The streams between bars indicate how households’ MPCs transition between two neighboring shocks (here, we only distinguish between MPCs of 0, MPCs of 1, and interior MPCs to keep the figure readable). Black dots depict the average MPCs for each shock.

measured MPCs align with those reported in previous observational studies, consistent with evidence that surveys can accurately capture households’ MPCs (see, *e.g.*, Colarieti et al., 2024; Kotsogiannis and Sakellaris, 2025; Parker and Souleles, 2019).

We document five new facts. Figure 1 visualizes the first fact: the “bowtie” distribution of MPCs and shock sizes. For small shocks, households frequently report an MPC of 0 (*i.e.*, fully saving a gain or drawing from savings after a loss) or 1 (*i.e.*, fully consuming a gain or cutting back consumption after a loss). As shocks get larger in absolute value, the fraction of extreme responses declines, and the fraction of interior responses increases. This transition from extreme to interior MPCs, visualized by the shaded flows, generates the “bowtie” appearance of Figure 1. We observe very similar behavior among households across the wealth distribution. The bowtie pattern is at odds with the predictions of standard consumption-savings models. For example, we find that as shocks become *more* negative, *fewer* households report having a high MPC. Instead, the drastic difference in MPCs for small and large shocks is reminiscent of quick-fixing.

Indeed, our second fact is that the distribution of MPCs is well described by a decomposition of households into four *quick-fixing types*. These households use extreme MPCs of 0 or 1 as heterogeneous quick-fixes for small shocks, but abruptly abandon them for larger shocks, and thereafter behave homogeneously. To decompose households into types, we only consider

how they respond to the smallest shocks, the \$50 loss and gain. We say that households are *consumption fixers* if they have a zero consumption response to both shocks; *savings fixers* if they have a zero savings response to both shocks; *consumption prioritizers* if they increase consumption after windfalls but cut savings after losses; and *savings prioritizers* if they increase savings after windfalls but cut consumption after losses. These types span 68% of our respondents. Using within-respondent data, we show that they satisfy all formal properties of quick-fixes: (i) they adopt categorically different policies for small shocks, (ii) abruptly transition to interior MPCs for large shocks, and (iii) have similar MPCs conditional on transitioning to an interior MPC. The remaining 32% of “uncategorized” respondents do not exhibit clear quick-fixing behavior and usually report an interior MPC that is relatively stable for different shock sizes.

Our third fact suggests that quick-fixing helps to explain the empirical puzzle that MPCs are only weakly predicted by households’ financial situation and demographics. In our data, cross-sectional variation in spending, income, income risk, liquid and illiquid wealth, debt, education, age, gender, and household size explains only 13% of the household-level variation in MPCs. This result, consistent with previous findings in the literature (Lewis et al., 2024), poses a challenge for standard models in which such variables should explain *all* MPC variation. By contrast, quick-fixing types account for a significantly larger share, 35%, of the household-level variation in MPCs. This comes while quick-fixing types are essentially unpredictable: the same economic and demographic characteristics predict quick-fixes with  $R^2$  values between 2% and 6%. Moreover, consistent with the near-rational theory, the predictive power of traditional economic factors increases with the magnitude of shocks.

Our fourth fact highlights that quick-fixing accounts for a large part of the variation in aggregate MPCs across small and large shocks as well as gains and losses. In particular, quick-fixing behavior increases the size-dependence of aggregate MPCs (the maximum absolute difference in aggregate MPCs across different shock scenarios) by a factor of 2–3.5.

Finally, our fifth fact supports the proposed mechanism that households quick-fix to avoid costs of changing behavior. In an additional survey, we elicit households’ responses to shocks alongside measures of how carefully they would consider their decisions, how likely they are to assess their overall financial situation when making a decision, and how likely they are to consult another household member. When households quick-fix and have extreme MPCs, they are substantially less likely to report deliberating.

**Quantitative Analysis.** In the final part of the analysis, we leverage our novel data and embed the empirically revealed quick-fixing patterns in an otherwise standard incomplete-markets consumption-savings model. Households in the model are one of five types: the four different quick-fixing types or frictionless optimizers. The fraction of each type in the

model corresponds to the fraction of each type that we empirically categorize. We estimate the size of the cost of changing behavior for each type to rationalize the switching patterns from extreme MPCs to the interior that we observe in the data. We calibrate the remaining preference and income process parameters as in [Kaplan et al. \(2018\)](#), and we estimate the discount factor to match the MPCs of uncategorized households.

At the microeconomic level, the model is able to rationalize the observed patterns of behavior with costs of changing behavior that imply very small opportunity costs of near-rationality: \$17 per quarter per household on average. Moreover, despite the fact that quick-fixing gives rise to materially different predictions for households’ responses to income shocks relative to the frictionlessly optimizing benchmark, patterns of wealth accumulation (a non-targeted moment) are essentially identical across types: even savings prioritizers are not richer and consumption prioritizers are not poorer than other types. Thus, our model rationalizes the puzzlingly weak empirical relationship between MPCs and wealth ([Lewis et al., 2024](#)).

At the macroeconomic level, the patterns of quick-fixing that we empirically uncovered have three implications for the aggregate response to income shocks. First, macroeconomic shocks of the same size but with different *incidence* induce markedly different aggregate responses. For example, the aggregate MPC out of a \$250 that affects everybody equally is more than twice as large as an equally sized aggregate shock that arises from 2.5% of the population experiencing a \$10,000 shock. As some business cycle shocks are concentrated on a few people (such as unemployment) and others are more uniformly experienced (such as fiscal stimulus), our results imply that there is no “one-size-fits-all” aggregate MPC. Second, targeting fiscal transfers by wealth is much less effective than in standard models. In fact, in our model and consistent with the data, there is a very weak and even non-monotone relationship between the MPC and a household’s wealth level. Third, our model implies intertemporal MPCs (iMPCs) that are substantially more front-loaded than standard models suggest, and this front-loading is larger for smaller shocks. This has important implications for the efficacy of fiscal stimulus ([Angeletos et al., 2024](#)) and helps resolve a “front-loadedness” puzzle: state-of-the-art incomplete market models cannot account for the front-loadedness of estimated iMPCs in observational studies ([Boehm et al., 2025](#)).

**Related Literature.** Our article speaks to a large literature on households’ consumption and savings behavior and its aggregate consequences. However, no previous work has used a tailored empirical design to characterize a near-rational model of consumption and savings, contrast it with leading alternatives, empirically discipline the rules that people use, and quantify the macroeconomic implications of the empirically implied near-rational behavior.

Within the literature initiated by [Akerlof and Yellen \(1985a,b\)](#) that studies the near-

rationality hypothesis, [Cochrane \(1989\)](#) and [Krusell and Smith \(1996\)](#) quantitatively evaluate the losses from various rule-of-thumb consumption rules and find that they are small. [Kueng \(2018\)](#) and [Browning and Crossley \(2001\)](#) suggest that empirically observed deviations from consumption smoothing could arise due to their low opportunity costs. Our paper goes further by empirically measuring prevalent near-rational consumption-savings rules through a tailored survey and studying the macroeconomic implications of the implied behavior.

By empirically characterizing these near-rational rules, we contribute to the literature exploring how bounded rationality shapes households’ consumption-savings decisions. Existing studies focus, for example, on the role of mental accounting ([Kőszegi and Matějka, 2020](#); [Lian, 2021](#)), hyperbolic discounting ([Ganong and Noel, 2019](#); [Maxted et al., 2024](#)), inattention and deviations from rational expectations ([Coibion et al., 2024](#); [Pfäuti and Seyrich, 2024](#)), memory ([Ilut and Valchev, 2023, 2024](#)), and learning from experiences ([Malmendier and Nagel, 2011](#)). Unlike this prior work, which typically isolates the role of one specific psychological phenomenon, our approach adopts an agnostic stance by allowing data to reveal the dominant near-rational rules that households follow. Moreover, our model of quick-fixing aligns well with recent research in behavioral economics showing that individuals often resort to simple solutions when faced with complex problems ([Bordalo et al., 2025](#); [Oprea, 2024](#)).

Surveys are our empirical tool of choice because they allow us to simultaneously measure multiple MPCs for each household, which we show is necessary to evaluate near-rational theories. A large literature has used surveys to understand heterogeneity in MPCs ([Christelis et al., 2019](#); [Coibion et al., 2020](#); [Fuster et al., 2021](#); [Jappelli and Pistaferri, 2014, 2020](#)). Moreover, recent empirical analyses show that survey-based measures of MPCs align closely with observational evidence ([Parker and Souleles, 2019](#); [Colarieti et al., 2024](#); [Kotsogiannis and Sakellaris, 2025](#)), and our data reproduces many of the basic patterns identified in observational studies. The closest paper to our analysis is [Fuster et al. \(2021\)](#): they identify one similar regularity in their data, a mass of agents with  $MPC = 0$ , and attribute this to “inertia” in adjusting consumption. Our approach of quick-fixing is conceptually distinct for two reasons. First, inertial consumption behavior (in our language, consumption fixing) is only one of many ways that quick-fixing is implemented, accounting for just 14% of households in our data. Second, our identified distribution of quick-fixes gives rise to different macroeconomic implications relative to inertial behavior, such as population-wide MPCs that decline (as opposed to increase) in shock size. More generally, our approach disciplines the “how” of near-rationality, which is essential for accurately capturing its macroeconomic implications.

## 2 A Simple Model of Quick-Fixing

To fix ideas and motivate our empirical strategy, we describe quick-fixing in a two-period model of consumption and savings. A household has payoffs  $u(c_1) + \beta u(c_2)$  over consumption streams  $(c_1, c_2)$ , where  $u$  is a concave, increasing, and differentiable utility function, and  $\beta > 0$  is a discount factor. The household earns income  $(y_1, y_2)$  in each period and earns return  $R$  on savings. At  $t = 1$ , they choose how much to consume and save.

A *frictionlessly optimizing* household implements the consumption and savings plan that maximizes lifetime utility. In particular, they equate marginal utility from consumption in both periods,  $u'(c_1) = \beta R u'(c_2)$ . We denote this household’s *consumption policy function*, or mapping from income states to consumption choices, as  $c_1 = c^*(y_1, y_2)$ .

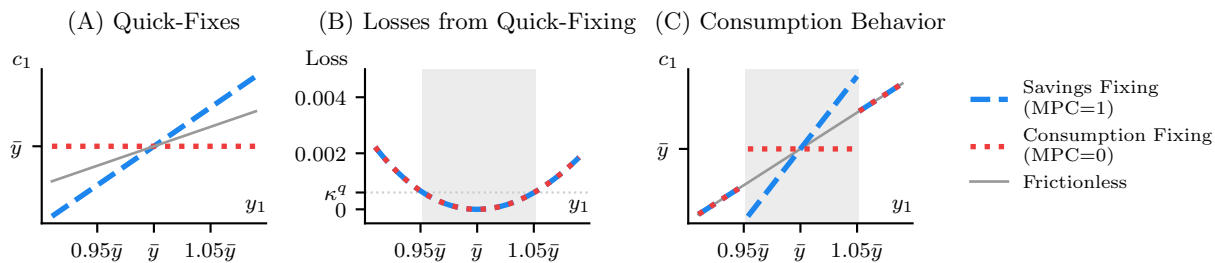
A *near-rational* household might not use the plan described above because they have an alternative that is “good enough.” Such a household, by default, sets its consumption via a *quick-fix*: a policy function  $c_1 = c^q(y_1, y_2)$  that differs from  $c^*$ . Deviating from this quick-fix costs  $\kappa_q$  in utility. This friction captures any conveniences in following the quick-fix: for example, saving cognitive resources, sticking to established habits, managing psychological urges, or avoiding physical or mental transaction costs. Regardless of the precise nature of  $c^q$  or the deep origins of  $\kappa_q$ , behavior follows a common pattern: the household acts “rationally” if the relative benefit from doing so exceeds  $\kappa_q$ ; otherwise, they follow their quick-fix.

To illustrate the key ideas, we present a simple graphical example in Figure 2. We set  $u(c) = \log c$ ,  $y_2 = \bar{y}$ , and  $R = \beta = 1$ . We think of  $\bar{y}$  as the household’s “usual” income, and any deviation at  $t = 1$  as a “shock.” In this example, a household’s quick-fix is to fully spend any shock, corresponding to a marginal propensity to consume (MPC) of 1. Since the household’s savings are held constant, we call this “savings fixing.” The quick-fix is represented by the policy function  $c^q(y_1, y_2) = y_1$  (blue dashed line in Panel A). This contrasts with the frictionless policy function  $c^*(y_1) = \frac{y_1 + \bar{y}}{2}$  (gray solid line in Panel A)—when there is a shock ( $y_1 \neq \bar{y}$ ), the choices diverge.

The payoff loss from following this quick-fix, illustrated in Panel B, is zero when there is no shock and close to flat in small consumption differences. This is a more general result, also noted by [Akerlof and Yellen \(1985a\)](#) and [Cochrane \(1989\)](#): the loss from deviating from an optimal policy function is *second-order* in the difference in consumption. The first-order effects are zero in the neighborhood of consumption choices that solve the Euler equation due to the envelope theorem. Thus, the losses from quick-fixing can be economically very small. For example, in our simple calibration, a 5% shock to income leads to a 2.5% consumption difference and a loss of 0.0006 utils, or the equivalent of 0.03% of lifetime consumption.

The household’s behavior, illustrated in Panel C, features a jump in the level of con-

**Figure 2:** An illustration of quick-fixing



sumption and the MPC. This jump occurs when income deviates sufficiently from  $\bar{y}$  for the household to find it worthwhile to abandon their quick-fix given their (potentially small) cost  $\kappa_q > 0$  of doing so. This basic logic applies to many quick-fixes, including those that imply very different marginal behavior. In the same figure, we also illustrate a quick-fix of zero spending response to income shocks (“consumption fixing”): that is,  $c^q(y_1, y_2) = \bar{y}$ . Under our simplifying assumptions, the loss is almost identical to the loss from savings fixing because the gap between the optimal and quick-fixing consumption response has the same size. Behavior again features a “jump” in consumption and MPCs, but the economic implications are the opposite: large shocks trigger an abrupt *increase* in the MPC (Panel C).

We formally analyze the simple model and illustrate that this basic logic applies for a large class of quick-fixes in Appendix A.1. Quick-fixes have second-order opportunity costs but generate first-order differences in consumption and MPCs—up to a sufficiently sizable shock, at which consumption behavior features a jump.

**From Theory to Measurement.** The model predicts that households: (i) quick-fix in response to small shocks, (ii) abandon their quick-fix once shocks are big enough, and (iii) behave the same way conditional on abandoning their quick-fix, regardless of differences in their quick-fixes. Hence, the critical moment that can empirically discipline the near-rational theory is the *distribution of consumption policy functions*: how consumption choices of heterogeneous households respond to income shocks of different sizes.

To elicit this distribution, we design a survey that allows us to collect *household-specific* data on consumption responses to income shocks of different sizes. These data are essential to measure households’ heterogeneous near-rational strategies. For this task, we emphasize that it would be insufficient to use observational data: at best, such data can reveal households’ response to a single shock at a single moment in time, and each individual’s response remains hard to disentangle from the inherent noise in spending. Moreover, as all models of consumption-savings make predictions for this distribution, we will also be able to use our data to assess the ability of leading existing models to explain household behavior.

## 3 Empirical Evidence

This section presents the design and results of a novel, large-scale household survey tailored to uncover households’ policy functions and quick-fixes. We find that the majority of households apply simple rules that fully consume or fully save out of sufficiently small shocks, and we identify four empirically prevalent quick-fixes (Facts 1 and 2). The quick-fixes that households use are poorly explained by financial and demographic characteristics, yet highly explanatory of MPC heterogeneity across households (Fact 3). Moreover, the presence of these behaviors critically shapes aggregate MPCs (Fact 4). Finally, consistent with our proposed mechanism, households report that they would be less likely to carefully consider their behavior when facing small shocks and using a quick-fix (Fact 5). We argue that these patterns are consistent with near-rationality but not with the predictions of leading alternative models.

### 3.1 Survey Design

We conducted our survey with 4,981 US households in October and November 2023, collaborating with the survey company Bilendi. The sample approximates the adult US population in terms of gender, age, income, education, and region, and broadly captures the wealth distribution across the country (see Appendix C.1 for details).<sup>1</sup>

Our main goal is to uncover households’ consumption policy functions and, in particular, their responses to small versus large shocks. Hence, our innovation is to collect detailed *within-household* information on responses to fourteen shocks of different sizes and signs: gains and losses with a magnitude of \$50, \$100, \$250, \$500, \$1,000, \$5,000, and \$10,000. We focus on one-time shocks, rather than other types of income or wealth shocks, to ensure maximum comparability with the literature. We include very small shocks, such as \$50 or \$100, to isolate circumstances under which quick-fixing behavior is most likely to be prevalent.

Our approach builds on a standard and well-studied procedure to elicit spending and savings responses to one-time income shocks (*e.g.*, Jappelli and Pistaferri, 2014, 2020; Christelis et al., 2019; Fuster et al., 2021; Colarieti et al., 2024). We first provide households with short definitions of consumption and saving. We refer to consumption as “spending” to follow common parlance, and we explicitly stress that we consider debt repayment as part of saving. Next, households are asked to think about an unexpected one-time income gain

---

<sup>1</sup>We slightly oversample respondents with a college education and respondents with lower total debt and lower illiquid wealth, but our results are robust to re-weighting and correcting for these imbalances (Figure B.1).

or loss. For example, respondents read:

Consider a hypothetical situation where your household unexpectedly receives a one-time payment of \$1,000 today.

Then, households answer how they would change their spending and saving in response to the shock. They can respond in two numeric open response fields, and we calculate their MPC by dividing their spending response by the income shock.<sup>2</sup>

How would this one-time extra income cause your household to change its spending and saving over the next three months?

Increase in spending:

(By how much) would your household increase its monthly spending \$ \_\_\_\_\_  
over the next three months?

Increase in saving:

(By how much) would your household increase its monthly saving \$ \_\_\_\_\_  
(which includes increases in debt repayment or decreases in debt-taking over the next three months)?

Appendix C.2 presents the key instructions. The full instructions are available online at <https://osf.io/2s7cf>.

We randomize both whether respondents first face gains or losses and the order of shock magnitudes to avoid any bias or learning effects from the order in which respondents are faced with shocks. We also preclude respondents from going backward in the survey to adjust previous answers. This guards against the possibility that respondents revise the profile of their responses in light of choices made later. In practice, we observe similar results across the randomly assigned question orders (Appendix Figure B.1).

The survey closes with a series of detailed demographic and economic background questions, including questions on income, income risk, future income expectations, and wealth (liquid and illiquid). We keep the survey short to avoid response fatigue. The median response duration is approximately 14 minutes, and most respondents complete the survey within 9 and 24 min (20%-80% quantile range). However, we obtain similar results if we restrict attention to only the first or first five MPCs that each respondent reports or drop the 50% of the sample that read the preparatory instructions most quickly (Appendix Figure B.1).

---

<sup>2</sup>Households' changes in spending and saving need to add up to the income gain. We elicit increases in spending and saving for income gains and decreases for income losses. We explain to respondents that they can enter negative numbers to indicate changes in the reverse direction.

**Statistical Precision.** Due to the large sample of nearly 5,000 respondents and 70,000 MPCs, our estimates are highly precise. For instance, the 95% confidence interval for the estimated average MPC has a width of 0.013. Likewise, the margin of error for population share estimates is 1.4 percentage points, meaning that for any given percentage share (X%) of respondents, a (conservative) 95% confidence interval would be  $[X\% - 1.4\%, X\% + 1.4\%]$ .

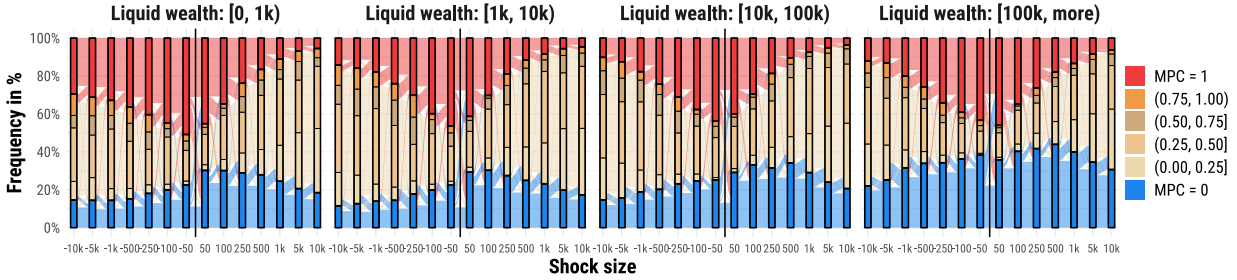
**Comparison of Cross-Sectional Results to Previous Work.** Prior work shows that survey evidence on the consumption response to transitory income shocks aligns well with observational evidence (Kotsogiannis and Sakellaris, 2025; Colarieti et al., 2024; Parker and Souleles, 2019). In our own data, we also verify that several basic patterns in MPCs line up with prior work, both survey and observational (see Appendix C.4). First, average MPCs are relatively high: for example, the one-quarter MPC out of a \$1,000 gain is 0.35. This is comparable to observational estimates, for example, an MPC of about 0.30 out of the 2008 US tax rebate, as estimated in analysis by Borusyak et al. (2024) and Orchard et al. (2025) that revisits prior analysis by Parker et al. (2013) and Broda and Parker (2014). Second, average MPCs decline in the size of shocks: for example, the MPC out of gains is 0.49 for \$100, 0.35 for \$1,000, and 0.30 for \$10,000 ( $p < 0.001$  for all comparisons). This is consistent with observational evidence: Fagereng et al. (2021) find that MPCs out of smaller lottery winnings are higher, and Ganong et al. (2025) find that MPCs out of smaller US stimulus payments in 2008 are higher. Third, the average MPC is larger for losses than for gains ( $p < 0.001$ ), as documented in previous surveys, *e.g.*, Bunn et al. (2018), Christelis et al. (2019), and Fuster et al. (2021). Finally, MPCs vary widely across households, consistent with observational evidence from Misra and Surico (2014), Boehm et al. (2025), and Lewis et al. (2024).

## 3.2 Extreme MPCs and the “Bowtie” Pattern

Our first key finding is the “bowtie” pattern in the distribution of MPCs for different shock amounts, which we illustrated in Figure 1 of the introduction. Extreme MPCs of 0 and 1 are common for small shocks but rare for large shocks. For example, 74% of households consume every dollar ( $MPC = 1$ ) or save every dollar ( $MPC = 0$ ) of the \$50 gain, but only 26% do so for a \$10,000 gain ( $p < 0.001$ ). Likewise, 72% of households fully reduce their consumption ( $MPC = 1$ ) or fully reduce their savings ( $MPC = 0$ ) in response to the \$50 income loss, but only 32% do so for a \$10,000 loss ( $p < 0.001$ ). Consequently, the mass of households with an interior MPC strictly between 0 and 1 increases from small to large shocks, giving Figure 1 its bowtie-like appearance.

This pattern is largely independent of respondents’ current financial situation. Figure 3

**Figure 3:** MPC profiles across the distribution of liquid wealth



*Notes:* The alluvial graphs summarize the MPC profiles of households with varying liquid wealth (see Appendix C.3 for variable definitions). In each panel, each of the 14 columns displays the distribution of MPCs for one particular shock size, and the streams between bars indicate how households’ MPCs transition between two neighboring shocks.

displays the distribution of MPCs conditional on different levels of liquid wealth (*i.e.*, assets held in cash, checking or savings accounts, or easily accessible (non-retirement) investment accounts). Even households whose liquid wealth is more than ten times the size of the largest shocks considered (right-most panel) commonly respond to small shocks by fully adjusting consumption or savings, and they become less likely to do so as the shocks become larger. We find the same bowtie pattern across the distribution of illiquid wealth, debt, net wealth, and the ratio of liquid wealth to income (Figure B.2).

**Fact 1 (The Bowtie):** Many households respond with extreme MPCs of either 0 or 1 to small shocks and transition to interior MPCs for larger shocks. This pattern holds regardless of households’ current liquid and illiquid wealth.

### 3.3 Four Quick-Fixes

This drastic difference in MPCs for small and large shocks is reminiscent of quick-fixing. Using our within-household data, we identify four quick-fixes that underlie this pattern.

**Candidates.** To determine candidates for the quick-fixes households use, we isolate their behavior for the smallest shocks: the \$50 loss and gain. 68% of respondents report an extreme MPC for *both* of these shocks, and we exhaustively categorize them into four types:

**Consumption fixers (14% of households)** respond with an MPC of 0 to small gains and losses, keeping consumption fixed and absorbing shocks with their savings.

**Savings fixers (29% of households)** respond with an MPC of 1 to small gains and losses, keeping their saving fixed and absorbing shocks with their consumption.

**Consumption prioritizers (11% of households)** fully draw on savings to cover a small loss (MPC = 0), but they fully spend a small gain (MPC = 1).

**Savings prioritizers (14% of households)** fully cut back on consumption when they face a small loss ( $MPC = 1$ ), but they fully save a small gain ( $MPC = 0$ ).

The remaining group of **unclassified households (32%)** cannot be assigned to any of the four groups above because they respond with an interior MPC to even the smallest income gains or losses. We do not find clear traces of alternative candidates for quick-fixing behaviors among this group. Unclassified households almost never choose extreme MPCs and instead immediately adopt relatively stable interior MPCs.<sup>3</sup>

**Evaluating the Properties of Quick-Fixes.** The four widespread behaviors described above—fixing consumption, fixing savings, prioritizing consumption, and prioritizing savings—are intuitively plausible quick-fixes. Now, we show that they satisfy the three formal properties of quick-fixes that we identified in Section 2.

First, households use these behaviors to respond to small shocks but abandon them for larger shocks. To visualize this, Figure 4 decomposes the distribution of MPCs by group. For example, Panel A plots the MPCs of consumption fixers for all fourteen shocks. By construction, 100% of consumption fixers start with an MPC of 0 for \$50 income gains or losses. The figure reveals that respondents are increasingly less likely to have an MPC of 0 as the gains or losses become larger. We see this monotonic decline in adherence to the quick-fix as shocks get bigger for all four types.

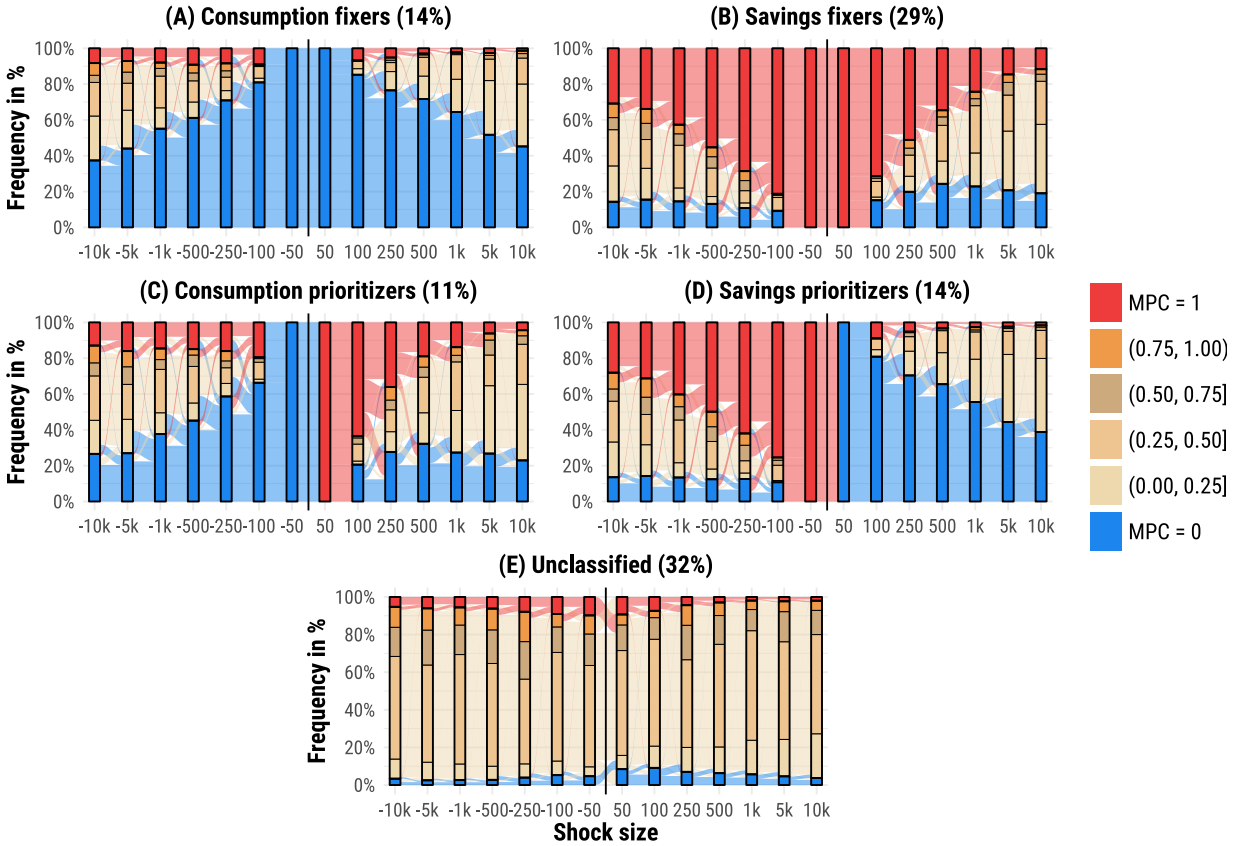
Second, households tend to abruptly abandon the simple response once a critical shock size is reached. By this, we mean that shifts from extreme to interior MPCs rarely occur gradually. Households that start with an MPC of 1 tend to immediately jump from this extreme MPC to an interior MPC that is typically around 0.25 to 0.60 (the 20%-80% quantile range). Their first interior MPCs are thus not unlike those of households who start from the opposite MPC of 0, whose 20%-80% quantile range ranges from 0.20 to 0.50. Figure 5 visualizes the abrupt transition from extreme MPCs (left column) to first interior MPCs (right column) across types for gains.

Third, once households abandon their simple response, their consumption policies are relatively stable *within* respondent and similar *across* respondents, even though their MPC for the smallest shocks differs radically. In the survey, the average absolute difference between two interior MPCs of adjacent shock sizes is 0.14, while the average conditional on a transition from the extremes (0 or 1) to the interior is 0.41. Moreover, households rarely (if at all) transition *back* from the interior to the extremes: conditional on starting from an interior

---

<sup>3</sup>Their most common response to the smallest income shocks is an MPC of 0.5, which a total of 12% of households adopt. However, most of these households also choose an MPC close to 0.5 for larger shocks, so we cannot identify a clear transition pattern. No other consumption response is chosen by more than 5% of households, implying that other quick-fixes—to the extent that they exist—are not very prevalent.

**Figure 4:** Responses to income shocks for different quick-fixing types



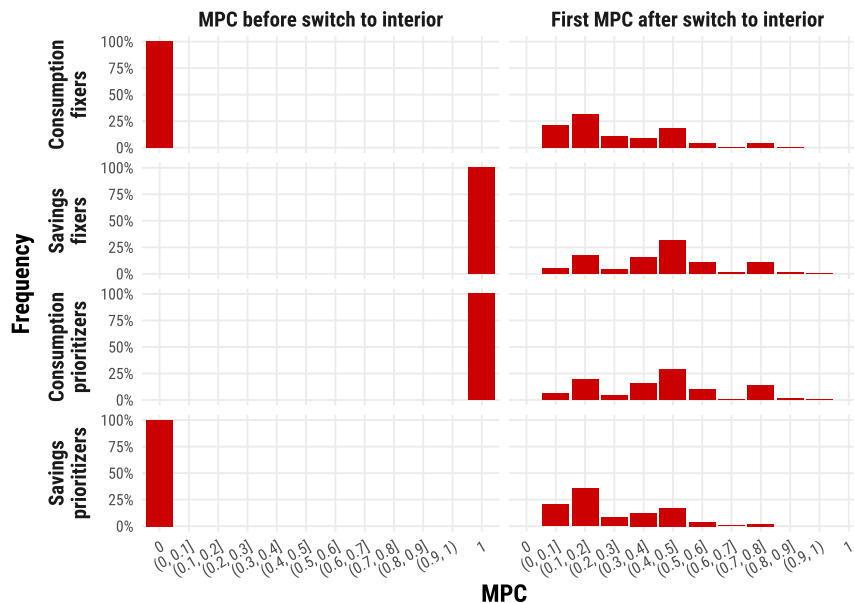
*Notes:* The alluvial graphs summarize the MPC data of four different quick-fixing types, which we define on page 12, and unclassified respondents. In each panel, each of the 14 columns displays the distribution of MPCs for one particular shock size, and the streams between bars indicate how households’ MPCs transition between two neighboring shocks.

MPC, households stay in the interior for 93% of shock size increases. In total, we observe a transition from interior to extreme MPCs only for 3% of shock size increases. Finally, interior MPCs are highly similar across respondents compared to MPCs as a whole: the variation in interior MPCs contributes only 16% to the total variance in MPCs. Figure 5 shows that, while MPCs vary widely across types before switching (left column), their interior MPCs after switching are similar (right column).

Thus, we conclude:

**Fact 2** (Four Quick-Fixes): The majority of households can be categorized as one of four quick-fixing types—consumption fixers, savings fixers, consumption prioritizers, and savings prioritizers—who vary in their extensive margin response to small shocks. Households of all four quick-fixing types tend to abruptly transition from having extreme MPCs to small shocks to having similar interior MPCs for large shocks.

**Figure 5:** Distribution of MPCs for gains before and after switching to interior



*Notes:* The histograms show conditional distributions of MPCs for gains. The rows correspond to the four quick-fixing types. The first column shows the distribution of MPCs before households switch to an interior MPC, which by construction puts all mass at either MPC = 0 or MPC = 1. The second column shows the conditional distribution of MPCs (given type and shock size) for the first shock for which the respondent reports an interior value. An analogous analysis for losses is reported in Figure B.3.

**Discussion of Response Noise.** Some response noise is inevitable in survey data, so we examine whether noise could obscure or falsely generate quick-fixing in Appendix C.5. We focus on the prediction that quick-fixing households should transition to an interior MPC at most once and remain interior thereafter, while unclassified households should never report extreme MPCs. We find that 52% of households behave fully consistently, 71% deviate from the one-switch pattern at most once, and 83% deviate at most twice. Given that we elicited MPCs for 14 different shocks *in random order*, the level of consistency in households’ behavior is high and extremely unlikely to be due to chance. Moreover, we illustrate in a simulation that even modest and empirically realistic levels of response error can explain the imperfect consistency levels that we observe across 14 different questions.

We also conduct an empirical “test-retest” study to gauge the likely response error directly. When we elicit our full MPC module twice—a few hours apart—household reports an identical extensive margin response to identical shocks in 82% of cases (random benchmark: 39%), we assign the same household to the same type in 69% of cases (random benchmark: 24%), and find test-retest correlations close to those of established measures for economic preferences (see Appendix C.5).

### 3.4 Quick-Fixes Account for MPC Heterogeneity

We next explore how well quick-fixes can account for MPC heterogeneity at the household level. Differences in MPCs across households are notoriously hard to predict using observable characteristics of households’ wealth, income, and demographics (*e.g.*, Lewis et al., 2024; Fuster et al., 2021). To study this in our data, we model respondents’ average MPCs as a function of observables in the following simple regression:

$$\text{MPC}_i = \alpha + \beta' X_i + \epsilon_i \tag{1}$$

The outcome  $\text{MPC}_i$  is each respondent’s average MPC across the 12 gain and loss scenarios ranging from \$100 to \$10,000. We exclude the \$50 gain and loss cases, as they are used to define quick-fixing types.

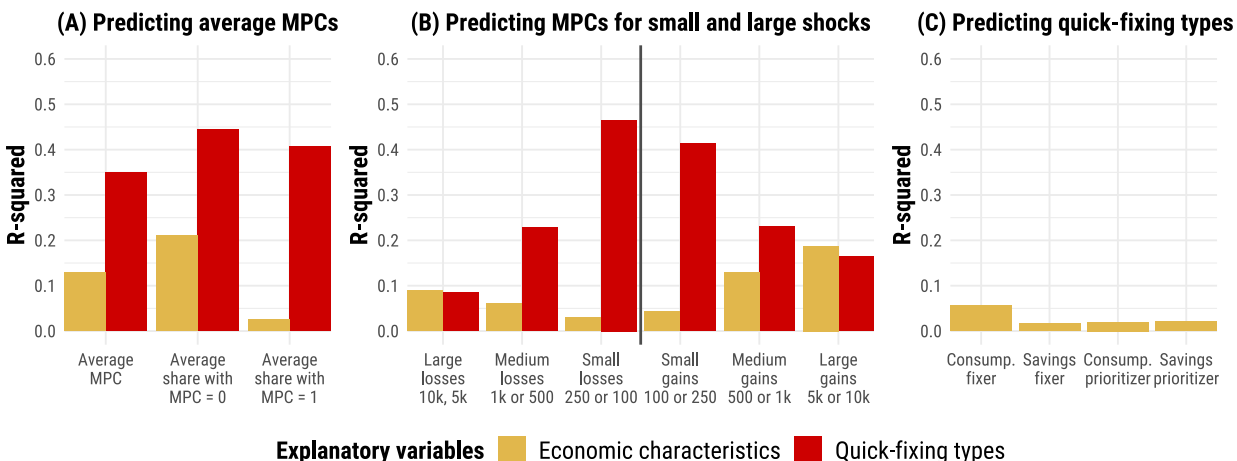
We consider two candidates for the explanatory variables  $X_i$ . First, we consider a full suite of demographic and financial characteristics: households’ monthly spending (log), income (log), and income risk; dummies for low, intermediate, or high levels of liquid wealth, illiquid wealth, and debt, respectively; and four additional demographic characteristics (a college dummy, age, gender, and household size). Second, we consider dummy variables for the four quick-fixing types, which only depend on households’ responses to the \$50 shocks.

Echoing findings in the literature, the demographic and financial variables together explain only 13% of the cross-sectional variation in MPCs (see golden bars in Panel A of Figure 6 and Table B.1). In analogous regressions, we find that observable characteristics explain only 21% of variation in the household-level share of MPCs that equal 0 and—what is perhaps most striking—only 3% of the household-level share of MPCs that equal 1. Even though measurement error in our explanatory variables will somewhat attenuate the estimated  $R^2$ , this finding remains surprising since standard theories suggest that these characteristics should explain the majority of (or even all) heterogeneity in MPCs.

By contrast, the four quick-fixing types account for about a third of the variation in MPCs by themselves (35%, see red bars in Panel A of Figure 6 and Table B.1). The types also account for 44% and 41% of the variation in the share of extreme MPCs of 0 or 1, respectively. These findings are surprising because, although quick-fixing types are identified using data on MPCs, they are a very low-dimensional summary that uses information only about \$50 shocks (which we drop when we derive households’ average MPCs) and only uses information about whether these responses are extreme or interior. This suggests that households’ quick-fixing types are an important component of MPC heterogeneity and could go a long way toward opening the “black box” of MPC heterogeneity.

The near-rational model implies that quick-fixes predict MPCs best for smaller shocks,

**Figure 6:** Quick-fixing accounts for MPC heterogeneity but is hard to predict



*Notes:* Panel A displays the  $R^2$  from regressions of households’ average MPCs and their share of extreme MPCs on (i) their wealth, income, and demographics (golden bars) or (ii) their quick-fixing types (red bars). The average household-level MPC and the MPC shares are derived using all gains or losses ranging from \$100 to \$10,000. See Table B.1 for the complete list of variables and coefficients. Panel B displays the  $R^2$  for MPCs out of small, medium, and large losses or gains. Panel C displays the  $R^2$  from regressions of households’ quick-fixing types on economic and demographic characteristics. See Table B.2 for the complete list of variables and coefficients.

while traditional economic factors become more predictive for larger shocks. This is indeed what we find, as Panel B of Figure 6 shows. For example, quick-fixes strongly predict MPCs from small gains (\$100 and \$250) with an  $R^2$  of 41%, but less so ( $R^2 = 19\%$ ) for larger gains (\$5,000 and \$10,000). Observable characteristics of wealth, income, and demographics have negligible explanatory power ( $R^2 = 4\%$ ) for small gains (\$100 and \$250) but a higher explanatory power ( $R^2 = 19\%$ ) for large gains (\$5,000 and \$10,000).

**Quick-Fixes Are Hard to Predict.** The importance of quick-fixing in accounting for MPCs raises the question: are households’ quick-fixing behaviors themselves related to their economic and demographic characteristics? In particular, different economic circumstances could drive households to adopt different quick-fixes, or different quick-fixes could change households’ economic circumstances (*e.g.*, by affecting wealth accumulation).

To investigate this, we regress indicators for being categorized as one of our four types (consumption fixer, savings fixer, consumption prioritizer, or savings prioritizer) on the same characteristics describing wealth, income, and demographics as before. We find only a weak relationship between household observables and quick-fixing types, with  $R^2$  values ranging from 0.02 to 0.06, as displayed in Panel (C) of Figure 6 (see also Table B.2). These results are also consistent with our earlier observation that the distribution of MPCs looked similar conditioning on different levels of wealth and debt (Figures 3 and B.2). We observe a clearer relationship between households’ characteristics and whether we can classify them as

quick-fixer at all ( $R^2 = 0.17$ , Column 5). For example, households with high income risk, intermediate liquid wealth levels, low illiquid wealth, and high education are more likely to adopt interior MPCs even for the smallest shocks.

A related question is whether we can predict households’ switching points, *i.e.*, the smallest shocks for which they switch from an extreme to an interior MPC. We find that neither the quick-fixing types nor the economic characteristics are accurate predictors of switching points ( $R^2 = 0.02$  and  $R^2 = 0.06$ , respectively; Columns 6 and 7). Nonetheless, one consistent *qualitative* finding is that the wealthiest households have higher switching thresholds—that is, they are the most reluctant to give up their quick-fix (Figure B.4). In Appendix A.1, we observe that this is a natural consequence of our model if households have utility curvature that is declining in consumption levels (*prudence*).<sup>4</sup>

We summarize these findings below:

**Fact 3** (Quick-Fixes Account for MPC Heterogeneity But Quick-Fixes Are Hard to Predict): The four quick-fixing types account for a large share of variation in average MPCs across households. Yet, quick-fixing behavior is essentially unpredictable from households’ economic and demographic characteristics.

This fact presents an apparent tension. How can quick-fixing simultaneously generate differences in consumption responses without generating a correlation between quick-fixing behaviors and wealth? The ability of our model to resolve this tension will constitute an important “non-targeted moment” in our subsequent quantitative analysis.

### 3.5 Quick-Fixes Matter for Aggregate MPCs

So far, we have established that quick-fixing is prevalent at the household level and helps explain household heterogeneity in spending behavior. We next illustrate that this behavior matters for average, population-wide MPCs.

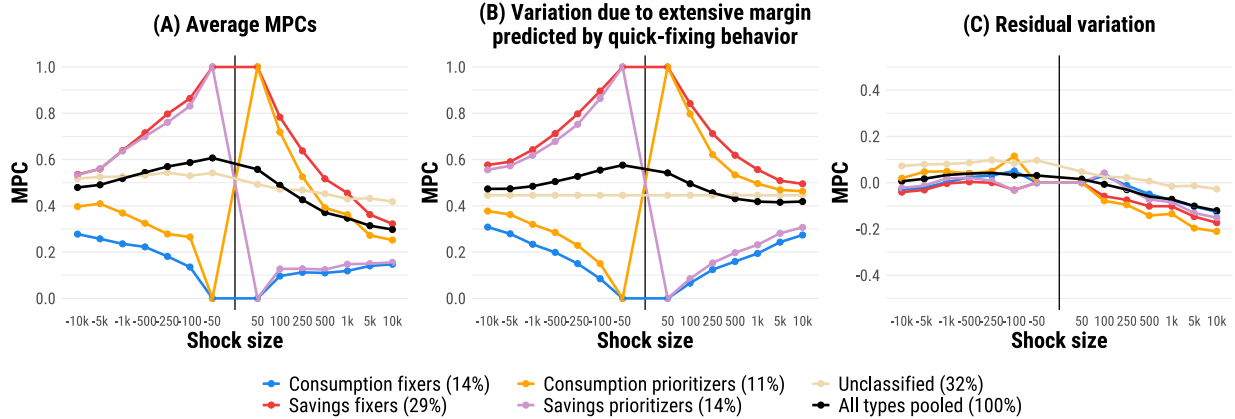
We first observe that the average MPC measured in our survey varies significantly across shock scenarios (Figure 7, Panel A). In particular, average MPCs (black dots) decline in the magnitude of shocks for both losses and gains, and they are higher for losses than for gains. Underlying this are sharply different patterns for each of the quick-fixing groups and for the unclassified respondents (colored dots).

We next illustrate how quick-fixing affects average MPCs by way of a simple calculation that is visualized in Panel B. Specifically, we construct a predicted average MPC based

---

<sup>4</sup>This also echoes Kueng’s (2018) finding that households exhibit higher MPCs out of shocks which are small *relative to their wealth*, which he explains using a near-rational model in which households’ default (in our language, quick-fix) is to be hand-to-mouth.

**Figure 7:** Quick-fixing types account for variation in aggregate MPCs



*Notes:* The left panel displays the average MPCs of four different quick-fixing types, which we define on page 12, and unclassified respondents for 14 different income shocks. The black line shows the aggregate MPC of the full household sample. The middle panel shows the same statistics after shutting down any variation in interior MPCs to isolate the effect of the extensive margin predicted by households’ quick-fixing types. All interior MPCs are fixed at 0.45. Households start with the MPC prescribed by their type. Once they change their MPC, they permanently switch to the interior. The right panel graphs the difference between the left panel and the middle panel.

on households’ switches from extreme to interior MPCs: households start with the MPC prescribed by their identified quick-fix type (zero or one). Once these households switch to a different MPC for the first time, they permanently switch to an MPC of 0.45, which is the average interior MPC in our data. Thus, this calculation shuts down any variation in interior MPCs: MPCs only vary if households stop quick-fixing and switch to an interior MPC. Finally, Panel C shows the residual variation in aggregate MPCs, *i.e.*, the difference between Panels A and B due to varying interior MPCs.

The decomposition reveals that a large share of the sign- and size-dependence of aggregate MPCs is driven by households using quick-fixes for small shocks and abandoning them for large shocks. For instance, the MPC difference across the smallest and largest gain in Panel B is 0.124, while the residual difference in Panel C is 0.136. Under this metric, quick-fixing increases the size-dependence in aggregate MPCs for gains by a factor of  $(0.124 + 0.136)/0.136 = 1.9$ . For losses, the factor is even larger at 3.5.

In theory, quick-fixes could lead to aggregate MPCs that are either decreasing or increasing in the absolute value of shock sizes. But our data reveals that quick-fixes reduce MPCs for larger shocks for two reasons. First, savings fixers—who transition from  $MPC = 1$  to the interior—are more common than consumption fixers—who transition from  $MPC = 0$  to the interior. Second, since the average interior MPC is below 0.5, abandoning an MPC of 1 has a larger impact than abandoning an MPC of 0. This result also illustrates why our quick-fixes are conceptually distinct from models of “inertial” behavior in which a household’s level

of consumption features adjustment frictions, as in Fuster et al. (2021). Such adjustment frictions generate an aggregate consumption function that is increasing in the absolute value of shock sizes, whereas our data generates the opposite pattern. More generally, our analysis underscores the need to flexibly identify consumption policy functions in the presence of near-rational alternatives and understand the “how” of near-rationality to discipline its emergent macroeconomic implications.

The residual variation driven by the intensive margin (Panel C) is much smaller and features MPCs that are generally declining in shock size. This feature can be rationalized via standard incomplete markets models, which give rise to a concave consumption function. While this is not the focus of our analysis, our quantification in Section 4 will show that our calibrated model can account for this residual variation.

**Fact 4** (Quick-Fixing Matters for Aggregate MPCs): Quick-fixes account for a large part of the variation in aggregate MPCs. Moreover, quick-fixing increases the size-dependence in aggregate MPCs.

### 3.6 Quick-Fixes Require Less Deliberation

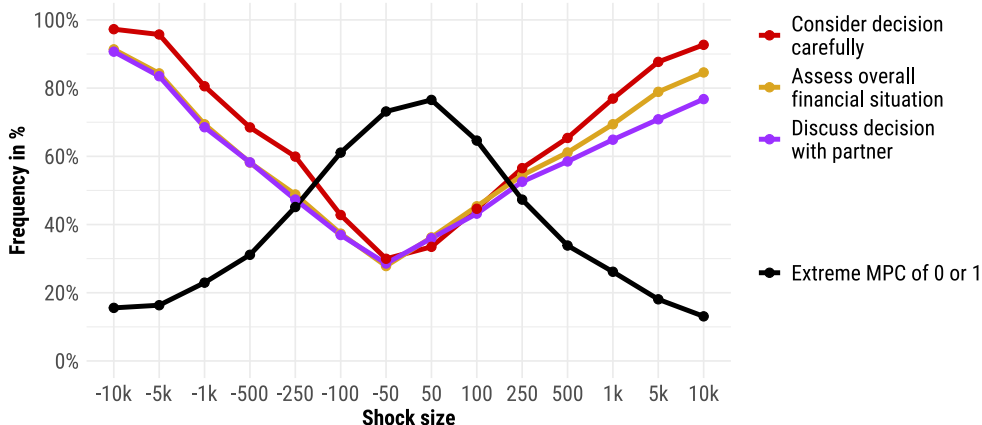
Our notion of quick-fixing is based on the idea that households prefer quick and simple solutions for small shocks but are willing to invest more resources to fine-tune their response to large shocks. To more directly test this mechanism, we designed a follow-up survey that digs deeper into *how* households make their spending and savings choices. This survey sampled 517 US households in August 2024. We first measure households’ consumption policy functions for gains or losses, following the same procedure as in the main study. In addition, we ask each respondent to rate for each shock (i) how carefully they would consider how to change their spending and saving (on a six-point scale), (ii) what is the percent chance that they would assess and consider their household’s overall financial situation prior to deciding how to respond, and (iii) what is the percent chance that they would discuss their response with other household members.<sup>5</sup> Our goal was to capture the multi-faceted ways in which making economic decisions might be costly: for example, through considering different plans, gathering relevant information, or generating agreement within the household. For brevity, we call all such behaviors “deliberation.”

We find that each of these three measures of deliberation strongly increases with shock size, consistent with our interpretation of the quick-fixing model (Figure 8). For example,

---

<sup>5</sup>We recruit participants with the survey company Prolific. The sample is not quota-based and does not represent the US population, but we obtain identical results if we correct for sampling imbalances with post-stratification weights (see Table B.3). Appendix C.2 contains the additional survey instructions.

**Figure 8:** Self-reported deliberation and extreme MPCs



*Notes:* Data from the additional deliberation study (517 US households). For fourteen different income shocks, the figure shows the average frequency with which households report to consider their response carefully (a score of at least four on a six-point scale, red line), assess their overall financial situation (yellow line), discuss the decision with other household members (purple line), or report extreme MPCs of 0 or 1 (black line). Households either face all seven positive or all seven negative income shocks.

the likelihood that households assess their overall financial situation when deciding how to respond to an income shock is on average 32% for the smallest shocks but 88% for the largest shocks (yellow line,  $p < 0.001$ ). Likewise, the likelihood that respondents consult other household members increases from 32% to 84% (purple line,  $p < 0.001$ ). These increases in deliberation mirror the *decreasing* likelihood that households choose an extreme MPC of 0 or 1 (black line). At the household level, a one-standard-deviation higher deliberation score (on any of three measures) predicts a 25 pp *lower* chance that a household adopts an extreme MPC of 0 or 1 (Table B.3).<sup>6</sup>

We summarize these findings below:

**Fact 5** (Quick-Fixing Requires Less Deliberation): Households more carefully consider choices, assess their finances, and discuss decisions amongst themselves when facing larger income shocks. Lower levels of deliberation come with a higher frequency of quick-fixing.

**Additional Qualitative Evidence.** We complement our evidence with a smaller qualitative survey to provide some first illustrative insights into why extreme MPCs may serve as convenient quick-fixes and require less deliberation. We ask approximately 500 households how they would respond to both a small shock (\$100) and a large shock (\$1,000). If they report a switch from an extreme to an interior MPC, we follow up by asking them to explain

<sup>6</sup>We find a similar relationship in the response-time data from our main survey. When respondents deliberate longer and spend more time responding to a shock scenario, they are less likely to report extreme MPCs (Table B.4).

their behavior in their own words. We find four broad patterns in these qualitative data, which we discuss in more detail in Appendix C.6.

First, 86% of households refer to the difference in shock size to explain the difference in their MPCs. For example, one response states matter-of-factly (MPC = 0 for \$100): “One hundred bucks is not that much. It’s great, don’t get me wrong, but it’s something you either spend on a dinner or put away. Where we’re at right now, it’s going right in the bank.”

Second, households commonly mention habits and routines such as a fixed spending budget, a fixed monthly transfer to savings, or the goal to maximize savings. For example, one household summarizes (MPC = 0 for \$100): “I have a budget for a reason and generally stick to it unless there are major changes.”

Third, for small shocks, extreme MPCs appear to be easier to imagine, evaluate, and appreciate. By contrast, interior MPCs lead to small changes that are not perceived to “make a dent” in households’ plans. Interestingly, some households make this case in terms of spending—“[\$100] is not really large enough to make an impact on our spending” (MPC = 0 for \$100)—while others make the same case in terms of saving—“the \$100 is not really enough to move the needle in saving” (MPC = 1 for \$100).

Fourth, many households recognize an income gain as a welcome opportunity to treat themselves or their families. As one respondent puts it, “Why not just use the unexpected \$100 to spend on something you can enjoy or something that can help you in the short-term?” Most balance consumption and saving for the large shock, but they approach the smaller \$100 gain differently. Some conclude that they can give in, “indulge”, and spend everything, while others choose to maintain “discipline” and save everything.

All in all, the qualitative evidence points to a multidimensional explanation of why households rely on quick-fixes. Extreme MPCs are convenient because they require little thought, preserve familiar routines, and discipline (or give in to) psychological urges. Our model of quick-fixing captures the convenience of extreme MPCs for small shocks and the transition pattern from extreme to interior MPCs, thus providing a theoretically plausible and quantitatively practical description of household behavior.

### 3.7 Can Existing Models Rationalize These Data?

In short, we argue the answer is no. In this section, we summarize why alternative theories of consumption-savings behavior do not satisfactorily rationalize our empirical findings. The list of models we discuss here exhausts the set of models studied in a recent review article by [Kaplan and Violante \(2022\)](#) and includes many additional models. Our intention is not to dismiss these models, which succeed in describing other dimensions of consumption-savings

decisions. Rather, our point is that these theories cannot account for the novel patterns that we observe.

**Incomplete Markets Models.** The standard incomplete markets model (Bewley, 1979; Carroll, 1997) cannot account for the “bowtie” pattern in Figure 1. For example, this model predicts that as shocks become more negative, the fraction of high MPC households should increase as more households become borrowing-constrained. This is the opposite of the pattern that we see in the data: as shocks become more negative, households become less likely to have high MPCs. A similar point applies to many extensions of the model that feature *ex ante* heterogeneity in preferences and investment technologies among agents: for example, among discount factors (Carroll et al., 2017; Aguiar et al., 2024), elasticities of intertemporal substitution (Aguiar et al., 2024), risk aversion (Kaplan and Violante, 2022), and investment opportunities (Kaplan and Violante, 2022). While heterogeneity along these margins could partially explain the *aggregate* bowtie pattern, they cannot explain the shape of the *within-household* policy function for consumption fixers, savings fixers, consumption prioritizers, and savings prioritizers.

**Multiple-Asset Models.** To account for the presence of wealthy agents with high MPCs, the wealthy hand-to-mouth, Kaplan and Violante (2014) introduce multiple accounts of varying liquidity to the standard incomplete markets model. A variant of this model forms the basis for the highly influential heterogeneous-agent New Keynesian (HANK) model of Kaplan et al. (2018). In this model, households are hand-to-mouth if they have low liquid wealth, even if they have high illiquid wealth. Nonetheless, households with high liquid wealth should be unconstrained and therefore not have extreme MPCs. In Figure 3, we document a pronounced “bowtie” pattern of adjustment for all levels of liquid wealth. Even households that have more than \$100,000 of liquid wealth or liquid wealth exceeding ten times their monthly income display the “bowtie” pattern. In Figure B.2, we corroborate this finding for other measures of wealth and debt.

**Models with Durables or Consumption Commitments.** To account for differences in household consumption-savings decisions between durables and non-durables, many models in the literature explicitly study the role of durable consumption (see, *e.g.*, Barsky et al., 2007). In a similar vein, Chetty and Szeidl (2016) study consumption-savings decisions when households may be pre-committed to certain spending patterns, *e.g.*, because of contracts that they have previously entered into to rent or lease a good. Such models predict that any given household will be increasingly likely to undergo a large increase in consumption and have high MPCs (as households purchase a lumpy durable good) as they experience progressively larger positive shocks. This is at odds with our finding that very few households

transition from an interior MPC to an extreme MPC of 1 (or even higher) as positive shocks get larger. These models are also inconsistent with the behavior of consumption prioritizers and savings fixers, which account for 40% of respondents.

**Models with Mistakes in Consumption.** To account for the high MPCs that we see in the data, many papers have introduced behavioral elements to consumption-savings problems. Some prominent such models are those with present bias (see, *e.g.*, Maxted et al., 2024), temptation (see, *e.g.*, Krusell et al., 2002), rational inattention (Sims, 2003), finite planning horizons (Boutros, 2025), sparsity (Gabaix, 2014), or misperceptions of wealth (Lian, 2023). While these models generate higher (or lower) MPCs than the incomplete markets model, they once again do not generate the “bowtie” pattern of responses as a function of shock size (Figure 1) or the stark and discrete heterogeneity in policy functions that we uncover.

**Models with Infrequent Optimization.** Fuster et al. (2021) generate infrequent optimization through “menu costs” of adjusting consumption. In this model, all households are consumption fixers who do not adjust their consumption for small shocks. However, in our data, 80% of quick-fixing households (savings fixers, consumption prioritizers, and savings prioritizers) adjust their consumption even in response to small shocks. In our model, households differ in whether they adjust along the margin of consumption or spending. Moreover, in the case of consumption and savings prioritizers, this margin also depends on the sign of the shock. In the related analysis of Gabaix and Laibson (2001), households similarly adjust consumption only every so many periods. This model is therefore similarly unable to account for the multiplicity of quick-fixing behavior.

**Our Model of Quick-Fixing.** We have argued that no existing model can capture the new empirical facts that we have documented. Instead, our empirical evidence on households’ policy functions is consistent with a model of quick-fixing. It suggests that extreme MPCs often constitute simple quick-fixes that reduce physical or mental transaction costs. Our model captures such a decision-maker who prefers quick-fixes as long as the opportunity cost of not implementing the optimal response is sufficiently small. In the remainder of the analysis, we will construct a quantitative model of quick-fixing that is consistent with our new findings as well as more familiar facts about MPCs, which have motivated many of the approaches described above.

## 4 A Quantitative Model with Quick-Fixing

Our simple framework of Section 2 lacked many empirically relevant features that influence household consumption dynamics. In this section, we enrich our framework with three

features present in benchmark quantitative macroeconomic models: more than two periods, uninsurable idiosyncratic income risk, and borrowing constraints. We then demonstrate that a parsimoniously calibrated, quantitative model of quick-fixing can account remarkably well for both the patterns of adjustments and marginal propensities to consume in the data.

## 4.1 Set-up

Time is discrete and indexed by  $t \in \mathbb{N}$ . There is a unit measure of households indexed by  $i \in [0, 1]$ . All households have expected discounted utility preferences with discount factor  $\beta \in [0, 1)$ . Their flow payoff is  $u(c) = \frac{c^{1-\gamma}-1}{1-\gamma}$ , where  $\gamma$  is the inverse elasticity of intertemporal substitution. In each period  $t$ , each household earns a stochastic income  $y_t$  which lies in a discrete set  $\mathcal{Y} \subset \mathbb{R}_{++}$ . Income follows a first-order Markov process with transition matrix  $P$ . Households can save in a risk-free bank account with gross interest rate  $R \in \mathbb{R}_+$ . We denote their savings as  $a_t$ . Due to incomplete markets, households cannot borrow:  $a_t \geq 0$ . Households differ in their quick-fix types, as we describe below.

**Frictionless Optimizers.** We first introduce dynamic optimization of households that frictionlessly optimize, type ‘‘R’’ for rational. We let  $V^R$  denote their value function:

$$\begin{aligned} V^R(a, y) &= \max_{a'} \{u(c) + \beta \mathbb{E} [V^R(a', y') \mid y]\} \\ \text{s.t. } a' &= Ra + y - c \\ a &\geq 0 \end{aligned} \tag{2}$$

where the expectation is taken over unknown income states  $y'$ . We let  $c^*$  denote the optimal consumption policy function for these households.

**Quick-Fixers.** We now introduce the problem of quick-fixers. As in Section 2, we associate each household  $i$  with a *quick-fix consumption function*  $c^{qi}$  and with a quick-fix-specific *utility cost*  $\kappa_{qi} \in \mathbb{R}_+$ .

We use the survey to discipline the quick-fixes that households use. To describe these behaviors dynamically, it is necessary to keep track of two additional household state variables: a reference consumption level  $\bar{c} \in \mathbb{R}_+$  and a reference income state  $\bar{y} \in \mathcal{Y}$ . The four quick-fixes are described by four functions, indexed by  $q \in \{\text{CF}, \text{SF}, \text{CP}, \text{SP}\}$ , that depend on reference consumption  $\bar{c}$  and an income deviation  $y - \bar{y}$ :

$$\begin{aligned} c^{\text{CF}}(\bar{c}, y - \bar{y}) &= \bar{c} & c^{\text{CP}}(\bar{c}, y - \bar{y}) &= \bar{c} + \max\{y - \bar{y}, 0\} \\ c^{\text{SF}}(\bar{c}, y - \bar{y}) &= \bar{c} + (y - \bar{y}) & c^{\text{SP}}(\bar{c}, y - \bar{y}) &= \bar{c} + \min\{y - \bar{y}, 0\} \end{aligned} \tag{3}$$

*Consumption fixers* (CF) consume a fixed amount. *Consumption prioritizers* (CP) consume a fixed amount plus the positive component of income shocks, while absorbing negative shocks with savings. *Savings fixers* (SF) save a fixed amount,  $s^{\text{SF}} = y - c^{\text{SF}}(\bar{c}, y - \bar{y}) = \bar{y} - \bar{c}$ . *Savings prioritizers* (SP) save a fixed amount plus the positive component of income shocks, absorbing negative shocks with consumption.<sup>7</sup>

In each period, quick-fixers decide whether to adopt their quick-fix, which we denote by  $D = 0$ , or to pay utility cost  $\kappa_{q_i}$  to adopt the unconstrained, optimal choice, in which case  $D = 1$ . If the household pays the utility cost, they also reset their reference consumption and income states to  $c^*(a, y)$  and  $y$ , respectively. For each type  $q$ , this behavior is described by the dynamic program

$$\begin{aligned}
 V^q(a, y, \bar{c}, \bar{y}) = \max_{D \in \{0,1\}} \{ & D (u(c^*(a, y)) + \beta \mathbb{E} [V^q(a', y', c^*(a, y), y) \mid y] - \kappa_q) \\
 & + (1 - D) (u(c^q(a, y, \bar{c}, \bar{y})) + \beta \mathbb{E} [V^q(a', y', \bar{c}, \bar{y}) \mid y]) \} \quad (4) \\
 \text{s.t. } & a' = Ra + y - (D(c^*(a, y)) + (1 - D)(c^q(a, y, \bar{c}, \bar{y}))) \\
 & a \geq 0
 \end{aligned}$$

Two modeling choices that are necessary in the dynamic model are the treatment of (i) the persistence of types and (ii) households' sophistication in understanding the future consequences of near-rationality. First, we treat the identity of an individual's quick-fixing function as a permanent characteristic. This is a conservative approach that makes it as hard as possible for us to match our empirical findings that household wealth and financial status are poor predictors of quick-fixing types (Fact 4). Second, when households abandon their quick-fix, they adopt the choice of frictionlessly optimizing households. We follow this approach because it is conservative in the sense that it will yield opportunity costs of near-rationality that exceed those that sophisticated households would suffer.

We also emphasize that this combination of assumptions makes the quick-fixing model highly numerically tractable, making it simple to integrate our framework in larger, general equilibrium models. This is because each step in the recursive algorithm requires solving only a binary optimization problem (to quick-fix or not) by comparing two numbers in each state. As such, it is computationally feasible to integrate a "quick-fixing consumption block" into a state-of-the-art heterogeneous agents equilibrium model. These points notwithstanding, it is of course straightforward to adapt the framework to accommodate transient type membership and sophistication.

---

<sup>7</sup>If  $c^q$  is negative, we adopt the convention that the household automatically abandons their quick-fix.

## 4.2 Calibration

We calibrate the model to match standard facts on US households’ behavior as well as our survey findings. We proceed in four steps.

First, we calibrate the flow utility and income process to match external estimates. We set  $\gamma = 1$  (logarithmic preferences) to match standard estimates of the EIS. We calibrate the earnings process to match the frequency and size of quarterly-frequency earnings shocks in US micro data. The process is a 5-state discretization of a Gaussian AR(1) process that targets a variance in log annual earnings of 0.70 and an expected state switch of once every five quarters.<sup>8</sup> We scale income such that one unit coincides with the median quarterly income reported in our survey, \$15,625. We set the quarterly interest rate to  $R = 1.01$ .

Second, we calibrate the discount rate to match the spending behavior of households that do *not* quick-fix in the survey (*i.e.*, are “unclassified”). For the frictionlessly optimizing types in the model, we calculate the average MPC out of transfer shocks of size  $x$  as  $\text{MPC}_x^R = \frac{1}{x} \int (c^*(a, y+x) - c^*(a, y)) d\Phi^R(a, y)$ , where  $\Phi^R(a, y)$  is the stationary distribution over assets and income for frictionless optimizers.<sup>9</sup> We choose the discount factor to minimize the sum of squared residuals between these predictions and the measured MPCs of “unclassified” survey respondents. This results in a calibrated value of  $\beta = 0.92$ . Insofar as this is a low value, we emphasize that this is what the standard incomplete markets model requires to match the data.

Third, we calibrate the fraction of agents of each quick-fixing type to match the categorization in Figure 4. The fraction of frictionless optimizers is matched to the share of unclassified households in the data. As any quick-fixers outside our four types are coded as unclassified in the data, this represents an upper bound on the fraction of frictionless optimizers and is therefore conservative for the near-rational theory.<sup>10</sup>

Fourth, we calibrate the four type-specific utility costs,  $(\kappa_{CF}, \kappa_{SF}, \kappa_{CP}, \kappa_{SP})$ , to match our main findings about quick-fixing behavior in the survey (Figure 4). Specifically, for each type and each shock size except the \$50 gain and loss (as these are used to empirically define the types), we calculate in the survey the fraction of respondents who abandon their quick-fix by reporting a propensity to consume that does not coincide with the quick-fix. In the model,

---

<sup>8</sup>This calibration matches the variance in log annual earnings estimated by Kaplan et al. (2018) using Social Security Administration data, as well as the variance in the 1-year change in log annual earnings (0.23). See Kaplan et al. (2018) (Table III) for further details.

<sup>9</sup>When computing MPCs, we treat the transfer shock as transitory (*i.e.*, as a shift in households’ liquid asset holdings), but write it as a shift in income to keep the notation consistent with our quick-fixes above.

<sup>10</sup>However, the behavior of frictionlessly optimizing households in the model does a good job of accounting for the average MPCs of unclassified households for *all* income shocks (see Figure B.5).

we calculate, for each shock  $x$ ,

$$\text{ReoptFraction}_x^q = \int D_x^{q*}(a, y, \bar{c}, \bar{y}) d\Phi^q(a, y, \bar{c}, \bar{y}) \quad (5)$$

where  $D_x^{q*} \in \{0, 1\}$  denotes the optimal reoptimization policy for type  $q$  in response to shock  $x$  and  $\Phi^q$  is the model-implied stationary distribution for those types. To make this experiment most consistent with the survey, we assume that the household contemplates a shock  $x \in \mathbb{R}$  in an interim period after initially choosing whether or not to reoptimize in a given period, but before observing income or making decisions of the next period (see Appendix A.2 for the formal details). The utility costs affect reoptimization behavior directly via the optimal policy and indirectly via the stationary distribution of observed and latent states. For each type, we choose the parameter  $\kappa_q$  to minimize the sum of squared residuals of the model versus the data:

$$\kappa_q^* = \arg \min_{\kappa_q > 0} \left\{ \sum_{i=1}^{12} \left( \text{ReoptFraction}_{x_i}^q - \widehat{\text{ReoptFraction}}_{x_i}^q \right)^2 \right\} \quad (6)$$

where the 12 shocks are those asked in the survey, excluding the \$50 gain and loss. We report the calibrated values of  $\kappa_q$  and provide an economic interpretation of their magnitude in Table 1 in Section 5.1.

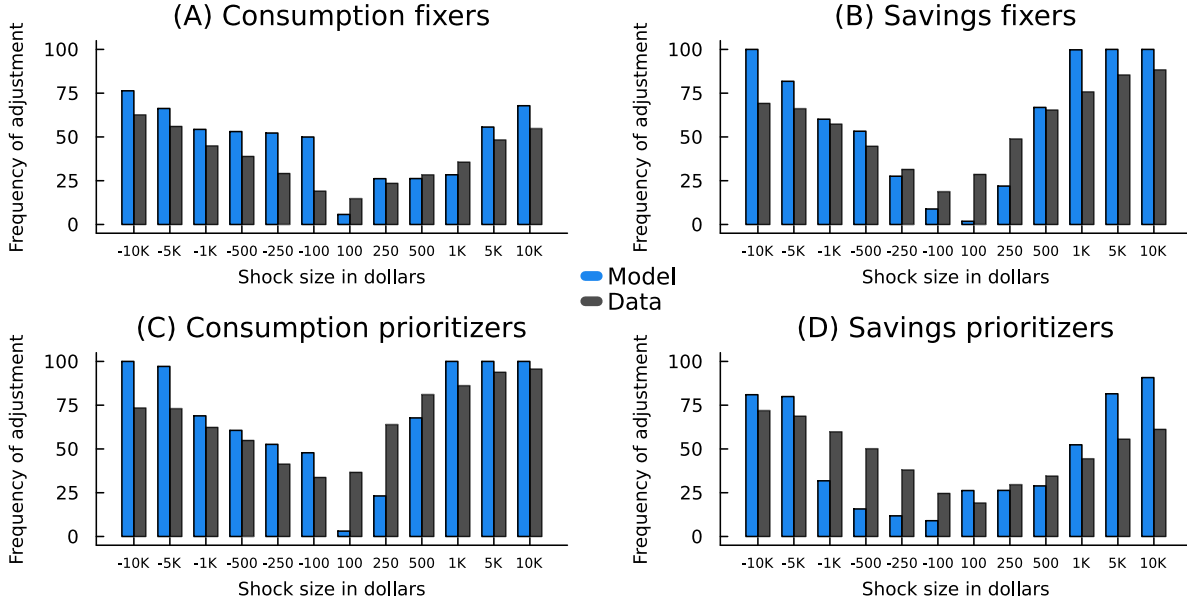
### 4.3 Model Fit

**Reoptimization.** Figure 9(i) compares the model prediction and data for the key moments that discipline the utility costs of not quick-fixing, namely the reoptimization thresholds for different shock sizes. The model fits the overall adjustment pattern quite well, in spite of using only four parameters (the utility costs) to match 48 moments (the adjustment thresholds). Naturally, because the model is heavily overidentified, we do not exactly match all of the measurements. The largest gap between the model’s fit with the data occurs in cases in which consumption adjusts and savings are fixed (all of Panel B, the positive shocks in Panel C, and the negative shocks in Panel D). In these cases, the model underestimates adjustment for low shock sizes and overestimates adjustments for large shock sizes.

**Average MPCs.** Figure 9(ii) shows how the model fits average MPCs and the decomposition of MPCs between the extensive and intensive margin. While the calibration directly targets reoptimization behavior (Figure 9(i)) and the MPC profiles of unclassified agents (see Figure B.5), it does not target the average MPCs of quick-fixing agents. The model deviation in average MPCs is small (Panel A). The maximum difference is attained for small

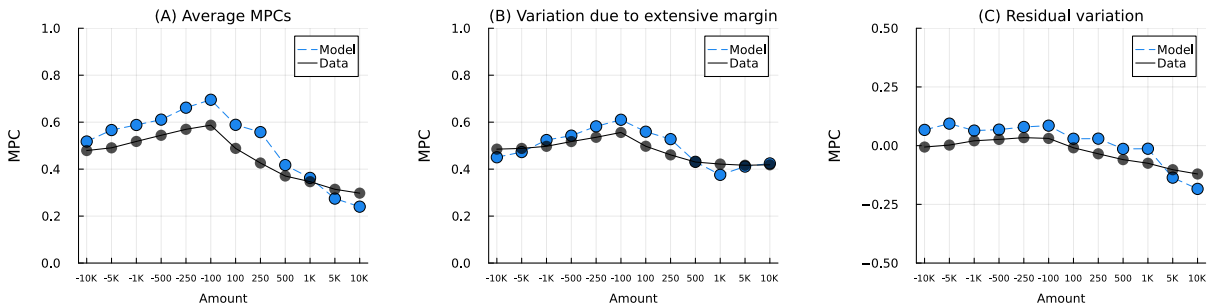
Figure 9: Quantitative model fit

(i) Reoptimization in response to shocks



Notes: The bar graphs compare empirical measurements and model predictions for the propensity of agents to abandon their quick-fix (reoptimize) following unanticipated shocks. Each panel corresponds to one of the four types and therefore to the calibration of the corresponding utility cost parameter. The blue bars denote model predictions, described in Equation 5, and the gray bars are empirical measurements, as reported in Figure 4.

(ii) Average marginal propensities to consume (MPCs)



Notes: The line graphs compare empirical measurements and model predictions for the average marginal propensity to consume out of shocks of different amounts. The decomposition corresponds exactly to that of Figure 7. Panel A shows average MPCs. Panel B shows variation due to the extensive margin, calculated by assuming that quick-fixers follow their associated quick-fix and non-quick-fixers use a reference interior MPC of 0.45. Panel C shows the residual between Panel A and Panel B. The blue dashed line and dots correspond to the model prediction, and the black solid line and dots correspond to the data (and exactly to the “All types pooled” lines of Figure 7).

negative shocks (less than or equal to \$250) and can be largely accounted for by a larger residual component (Panel C). This is driven by the high estimated fraction of agents at the binding borrowing constraint (see also Figure 10). This generates the correct qualitative pattern that residual variation is largest for small negative shocks, but slightly overstates the quantitative magnitude. Finally, as in the data, we note that a large part of the size-dependence in aggregate MPCs arises due to variation in the extensive margin (which is attributed to quick-fixes), as opposed to residual variation in the intensive margin (which is attributed to the concavity of the frictionlessly optimizing consumption function).

**The MPC Distribution.** We finally re-create our key empirical finding about the shape of the MPC distribution (Figure B.6). The model replicates the “bowtie” pattern, whereby extreme MPCs (0 or 1) are less likely in response to larger shocks.

**Summary.** Our quantitative model matches our novel empirical findings—that households adopt heterogeneous quick-fixes for small shocks before abruptly switching for large shocks—as well as established facts about the MPC distribution that are familiar from previous observational and survey studies (see Appendix C.4 for a full review of these findings). That is: (i) MPCs are high on average; (ii) MPCs decline in shock sizes; (iii) MPCs are higher for losses than for gains; (iv) MPCs vary widely across households; and (v) many households have an MPC of 0 or 1. Moreover, the model can account well for the observed frequency of reoptimizations and average MPCs observed in the data.

## 5 Microeconomic Consequences of Quick-Fixing

Having estimated the model, we next explore its microeconomic properties. First, we show quantitatively that our model predicts economically very small losses from quick-fixing, consistent with our hypothesis of near-rationality. Second, our model generates essentially no relationship between quick-fixing types and wealth accumulation (a non-targeted moment), consistent with our empirical results. Third, our model achieves this all while quick-fixing types generate considerable heterogeneity in MPCs. Thus, our model rationalizes the well-documented empirical fact that savers (low MPC households) are not rich and spenders (high MPC households) are not poor (Lewis et al., 2024).

### 5.1 The Losses from Near-Rationality Are Very Small

Our hypothesis of near-rationality relies on the assumption that the opportunity costs of quick-fixing are small. But how small is small? In Panel A of Table 1, we show that the

**Table 1:** The small costs of near-rationality

<b>Panel A: Utility costs <math>\kappa_q</math></b>		
Household type	% reduction in consumption	Average dollar cost
Consumption fixer	1.10	\$176.67
Savings fixer	0.007	\$1.47
Consumption prioritizer	0.006	\$1.44
Savings prioritizer	0.11	\$18.56

<b>Panel B: Value loss due to near rationality <math>V^R - V^q</math> (per quarter)</b>		
Household type	% reduction in consumption	Average dollar loss
Consumption fixer	0.45	\$71.67
Savings fixer	0.004	\$0.58
Consumption prioritizer	0.003	\$0.54
Savings prioritizer	0.06	\$8.68

*Notes:* Panel A reports the calibrated utility costs  $\kappa_q$ , in economically interpretable units. Panel B reports “costs of near rationality” defined as the change in value for frictionless optimizers were they to adopt quick-fixing, on average. See Section 5.1 for details.

calibrated utility costs that rationalize the quick-fixing uncovered by our survey are payoff-equivalent to at most a 1% reduction in consumption or \$175 one-time loss. The first column reports these losses for all types in payoff units ( $100 \times \kappa_q$ , which can be interpreted as percent consumption reduction due to logarithmic preferences), and the second column reports these as dollar equivalents.<sup>11</sup> The costs are, by some margin, highest for consumption fixers. The losses are on the order of *one hundredth of a percent* for savings fixers and consumption prioritizers, or about \$1.50.

We next compute the lifetime losses from near-rationality. Concretely, we compute the average lifetime loss in value for frictionless optimizers if they were to adopt the behaviors and bear the decision costs of quick-fixers:

$$\Delta V^q = \int (V^R(a, y) - V^q(a, y, c^*(a, y), y)) d\Phi^R(a, y) \quad (7)$$

We express this in units of an equivalent per-period reduction in consumption.<sup>12</sup>

The costs of near rationality in our calibrated model are all less than 0.5% of per-period

<sup>11</sup>For an agent in state  $z$ , the dollar-equivalent cost of abandoning their quick-fix when consuming  $c^q(z)$  solves  $\log(c^q(z) - \Delta^q(z)) - \log(c^q(z)) = \kappa$ , and is therefore  $\Delta^q(z) = c^q(z)(e^\kappa - 1)$ . We compute  $\mathbb{E}[\Delta^q(z)|D^*(z) = 1]$ , or the average dollar-equivalent cost conditional on reoptimizing.

<sup>12</sup>The equivalent percentage reduction in any consumption stream  $\{c_t\}_{t=0}^\infty$  solves  $\Delta V^q = \sum_{t=0}^\infty \beta^t \log(c_t) - \sum_{t=0}^\infty \beta^t \log((1 - \delta^d)c_t)$  and is therefore  $\delta^d := 1 - e^{-(1-\beta)\Delta V^q}$ .

**Figure 10: Wealth distributions by type**



*Notes:* This figure shows the wealth distribution for each type. Panel (a) shows the frequency of households at  $a = 0$ , the borrowing constraint. Panel (b) shows the distribution of wealth conditional on  $a > 0$ .

consumption or \$75 per quarter (Panel B of Table 1). On average, among quick-fixers, the loss is \$17 per quarter. Notably, the losses from quick-fixing for savings fixers are smaller than those of consumption fixers. This is because quick-fixes that fix savings allow consumption to respond (one-to-one) to shocks. For this reason, their response coincides with the optimal one when the borrowing constraint binds strictly. Moreover, away from the borrowing constraint, they can achieve close to the optimal response if the income shock is sufficiently persistent. A similar intuition underscores why consumption prioritizers face smaller losses than savings prioritizers.

Although we have shown that quick-fixes are unpredictable from standard economic and demographic characteristics in the data (Fact 4), this lower latent utility cost could explain why a much larger proportion of households are classified as savings-fixers (29%) relative to consumption fixers (14%). In all cases, however, the small loss from near-rationality helps explain why quick-fixing might persist even in a world of “selection pressure” against suboptimal strategies: the payoff cost of being a quick-fixer is extremely small.

## 5.2 Quick-Fixing Types Are Unrelated to Wealth

One moment that we did not use for calibration is the limited relationship between quick-fixing types and wealth (Fact 4). This is not a foregone conclusion in the quantitative model. Assets are an endogenous state variable and savings responses to shocks differ across types. It is a reasonable conjecture, for example, that households that default to increasing savings in response to shocks (savings prioritizers) accumulate more wealth than those that default to increasing consumption (consumption prioritizers).

We find instead that the wealth distributions of each of the four quick-fixing types and

**Table 2:** Variance in MPCs unexplained by assets and income

Model	Overall	Conditional on $a = 0$	Conditional on $a > 0$
Quick-fixing	28%	43%	70%
Frictionless optimization	0%	0%	0%

*Notes:* In each model, we calculate  $\text{Var}[\text{MPC}_i|a_i, y_i]$ , where  $i$  indexes households,  $\text{MPC}_i$  is the average MPC across the 14 scenarios considered in the survey,  $a_i$  is the household’s wealth, and  $y_i$  is the household’s income. In the first column, we report  $100 \cdot \mathbb{E}[\text{Var}[\text{MPC}_i|a_i, y_i]]/\text{Var}[\text{MPC}_i]$ , or the fraction of variance unexplained by wealth and income. In the second column, we report the same conditioning on  $a = 0$ :  $100 \cdot \mathbb{E}[\text{Var}[\text{MPC}_i|a_i, y_i] | a_i = 0]/\text{Var}[\text{MPC}_i | a_i = 0]$ . In the third, we report the same conditioning on  $a > 0$ :  $100 \cdot \mathbb{E}[\text{Var}[\text{MPC}_i|a_i, y_i] | a_i > 0]/\text{Var}[\text{MPC}_i | a_i > 0]$ . Since  $(a, y)$  is the state variable in the problem with frictionless optimization, the fraction unexplained is always 0 in that model.

frictionless optimizers are essentially identical (Figure 10). Most cross-sectional variation in wealth is driven by large and persistent shocks to income. Any short-run differences in savings responses to these shocks wash out in the long run, when households eventually reoptimize. As one concrete example: while savings fixers are as-if “hand to mouth” in response to small shocks, they are not actually “hand to mouth” in the long run. More succinctly, savers are not rich and spenders are not poor. This represents an important difference from two-agent New Keynesian (TANK) models, in which a fixed fraction of agents consumes their entire income in every period (*e.g.*, Campbell and Mankiw, 1989; Debortoli and Galí, 2024). These findings also contrast with those obtained in the class of models of agents with heterogeneous discount factors or present bias (Aguiar et al., 2024; Maxted et al., 2024), which naturally lead to very different long-run rates of saving.

### 5.3 Quick-Fixing Generates MPC Heterogeneity

We next explore how much quick-fixing contributes to heterogeneity in the marginal propensity to consume. Table 2 reports the percent of variance in this object that can be explained by assets and income in the quick-fixing model and the nested model in which all agents are frictionless optimizers. This is a stronger notion of “predictability” than what we can estimate via regression in the survey data because, in the model, we can calculate exact conditional expectation functions and have no measurement error; thus, this calculation gives a lower bound for what remains to be explained by other sources of heterogeneity. In the model with frictionless optimization, assets and income explain all variation by construction. This is strongly at odds with our findings as well as those from other studies in the literature (*e.g.*, Lewis et al., 2024). In our model, 28% of total MPC variance and 70% of variance conditional on  $a > 0$  is unexplained by assets and income, and therefore introduced

by quick-fixing behavior.<sup>13</sup> Thus, quick-fixing helps break the tight connection in incomplete markets models between financial observables and MPCs.

## 6 Macroeconomic Consequences of Quick-Fixing

Finally, we explore the consequences of quick-fixing behavior for macroeconomic questions. First, macroeconomic shocks of the same size but with different incidence induce markedly different aggregate responses. Second, targeting fiscal transfers by wealth can be much less effective than in standard models and can even backfire. Third, our model implies intertemporal MPCs (iMPCs) are substantially more front-loaded than standard models suggest, and this front-loading is larger for smaller shocks.

### 6.1 Incidence-Dependence of the Response to Shocks

Different macroeconomic shocks have different patterns of incidence on the population. For example, income losses during recessions are not experienced uniformly: the vast majority of people experience either no change in income or a small change in income, while some people lose their jobs and experience enormous income losses (Güvener et al., 2017; Schmidt, 2025). Conversely, fiscal transfer stimuli often affect incomes in ways that are closer to uniform. An important question is whether these different patterns of incidence have first-order effects on macroeconomic propagation.

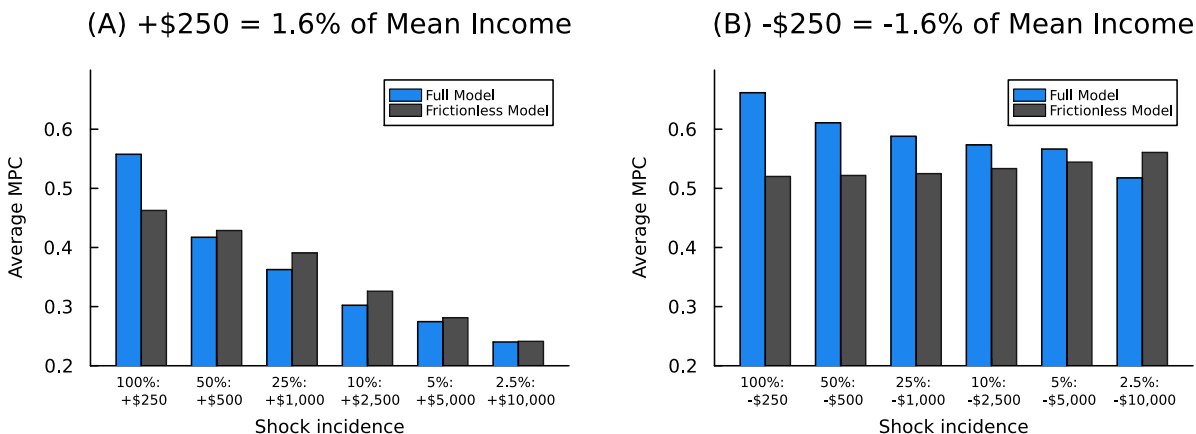
Existing work argues that, in representative agent theories and in standard calibrations of the incomplete markets model, average MPCs do *not* depend significantly on the exact size or incidence of shocks (Brinca et al., 2019; Auclert et al., 2025, 2024). Therefore, shock propagation depends primarily on a (nearly) context-independent average MPC.

By contrast, our near-rational model implies markedly different aggregate consumption responses to a shock depending on its distribution of incidence. We illustrate this in Figure 11, which shows the aggregate MPC for varying levels of incidence under quick-fixing (blue bars) and the frictionless benchmark (black bars). We fix the aggregate size of the shock at \$250, or a 1.6% change in quarterly income for the average household. Panel (A) shows that the aggregate MPC from a uniform \$250 shock is 0.55, while the aggregate MPC from a \$10,000 shock incident on 2.5% of the population is 0.24, more than half as small. This effect is smaller for the benchmark with frictionless optimization. Panel (B) shows that, for a loss, the near-rational model again features a declining MPC as incidence becomes more

---

<sup>13</sup>Note that the overall unexplained variance is lower than the unexplained variance conditional on either  $a = 0$  or  $a > 0$ . Intuitively, this is because the variance of MPCs conditional on assets is sizable, which reduces the overall unexplained variance once one conditions on assets (by the law of total variance).

**Figure 11:** Aggregate MPCs depend significantly on shock incidence



*Notes:* This figure plots aggregate MPCs out of transitory income shocks with different incidence distributions. Panel A is a \$250 gain per person and Panel B is a \$250 loss per person. These shocks are 1.6% of the mean income state in the model. In each figure, the horizontal axis varies the incidence scenario (*e.g.*,  $x\%$  of the population receives a shock of  $y$ , and  $(100 - x)\%$  of the population receives a shock of 0). The blue bars show average MPCs under the quick-fixing model and the gray bars show average MPCs under the nested model in which all agents are frictionless optimizers.

concentrated (0.66 vs. 0.52). By contrast, under the rational model, the MPC is very stable and even slightly *increases* as the incidence becomes more concentrated.

What drives these differences between the model predictions? In the calibrated quick-fixing model, MPCs are smaller under more concentrated incidence because of two interacting forces: (i) larger shocks trigger adjustments that shift MPCs from extremes to the interior and (ii) our estimated distribution of quick-fixing types is such that more reoptimizations *lower* average MPCs (see Section 3.5). In the frictionless model, all incidence dependence is driven by the strict concavity of the consumption function in liquid assets. Concentrated gains push households toward the flatter part of their consumption function, lowering average MPCs (but to a quantitatively smaller extent), while concentrated losses push households to the steeper part of their consumption function, raising average MPCs.

Our conclusions from the calibrated model critically hinge on the empirical distribution of quick-fixes in the population. For example, if we used the inertial model of Fuster et al. (2021)—in which all households are consumption-fixers—we would generate the opposite pattern. This again highlights the importance of our approach of empirically measuring the nature of households’ near-rationality for understanding the aggregate response of households to shocks.

**A Lucas Critique for HANK.** The incidence-dependence of the macroeconomic response to income shocks generates a new form of the Lucas (1976) critique, emphasizing the pitfalls of assuming a stable and (nearly) linear aggregate consumption function. This

approach has reemerged as a dominant paradigm in the HANK literature (see the review by Auclert et al., 2025). However, our analysis emphasizes that equally-sized aggregate shocks that close to uniformly affect the whole population (like a fiscal stimulus) may affect aggregate consumption very differently than shocks that are tightly concentrated on a subset of the population (like unemployment during a recession). Thus, the “right” aggregate MPC depends intimately on the counterfactual or shock scenario under consideration. A corollary of this point is that care should be taken in calibrating to MPCs estimated out of shocks of certain sizes when researchers are using such calibrated models to evaluate counterfactuals that feature differently-sized shocks.

## 6.2 MPCs Are High for the Liquid Wealthy

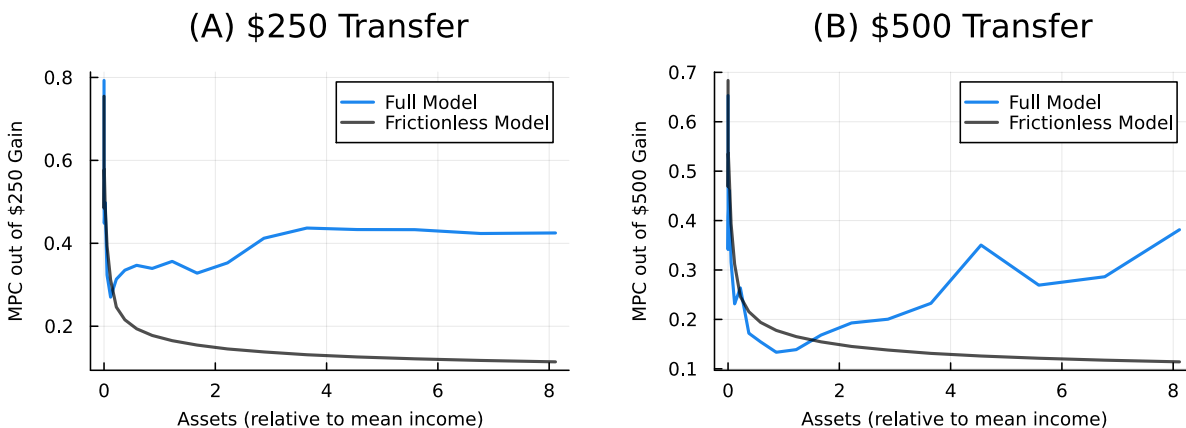
So far, we have emphasized how differentially concentrated shocks affect aggregate outcomes. A separate question is how differentially *targeted* shocks—shocks which are heterogeneously incident on the basis of observables—affect the macroeconomy. To study this question, Figure 12 shows how the aggregate MPC out of gains depends on wealth in the frictionlessly optimizing benchmark and in our quick-fixing model.

In the benchmark, high MPCs are exclusively concentrated among households with low levels of liquid wealth. This feature is driven by liquidity constraints and uninsurable income risk, which induce a consumption function that is concave in wealth. Nonetheless, this prediction of the frictionlessly optimizing benchmark is inconsistent with observational evidence showing that even households with relatively high liquid wealth have large MPCs (Boehm et al., 2025).

By contrast, in the quick-fixing model, MPCs are relatively high even for wealthy households. Strikingly, MPCs can even be *non-monotone* in wealth under our calibration. Intuitively, rich households can have high MPCs in our model because of quick-fixing. Richer households experience a lower utility loss from failing to perfectly smooth consumption, so many of them keep quick-fixing and maintain a high MPC in response to small income shocks. This mechanism is consistent with our empirical finding that richer households are indeed more likely to maintain quick-fixes for large shocks (Figure B.4).

A policy implication is that targeting fiscal stimulus by wealth—or even more precisely by *liquid* wealth—may be only marginally more effective than broad-based transfers. The appeal of targeting is even further diminished by incidence-dependence: as shown earlier, it may be more effective to spread transfers across households than to concentrate larger transfers within a small subset of the population.

**Figure 12:** Average MPCs out of gains as a function of assets



*Notes:* This figure plots aggregate MPCs out of a 250 dollar gain (Panel A) and a 500 dollar gain (Panel B) as a function of assets in the full model (blue line) and the frictionless model (gray line). In each case, we report MPCs conditional on wealth averaged over all other state variables, using the model’s stationary distribution.

### 6.3 Intertemporal MPCs are Highly Front-Loaded

We finally use the model to study how quick-fixing shapes the dynamic response of aggregate consumption to income shocks. To do this, we use the quantitative model to calculate a sequence of *intertemporal marginal propensities to consume* (“iMPCs”), or consumption responses at various future horizons in response to contemporaneous income shocks. Recent work has shown that the iMPCs govern—and are often sufficient statistics for—the response of the macroeconomy to an array of shocks (Auclert et al., 2024). Moreover, theoretical research has shown that the “front-loadedness” of iMPCs (how quickly households spend) is a critical determinant of how fiscal shocks affect the macroeconomy in a large family of HANK models (Angeletos et al., 2024). Understanding front-loadedness of iMPCs is especially important in light of recent experimental evidence, which has given rise to a front-loadedness puzzle: state-of-the-art incomplete markets models cannot account for how front-loaded iMPCs are in the data (Boehm et al., 2025).

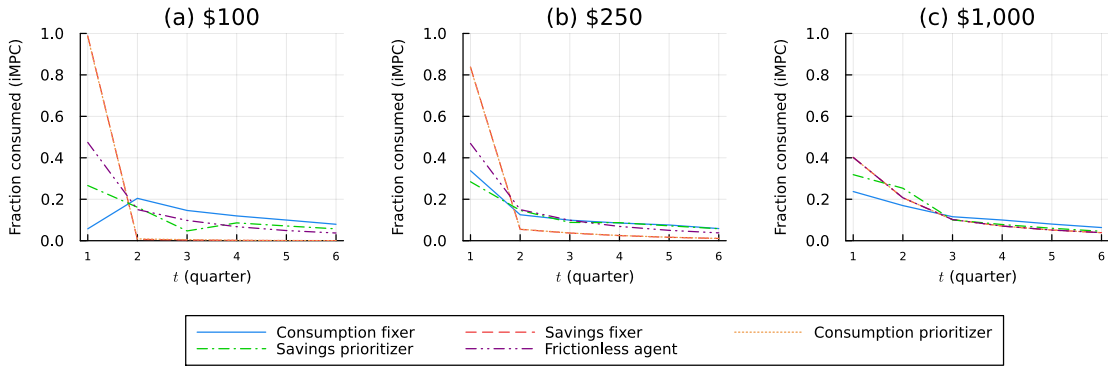
Concretely, for individual  $i$  with current idiosyncratic state  $z_i$ , we can define their iMPC at horizon  $h$  out of an income change  $x$  occurring at time  $t$ :

$$\text{MPC}_{h,x}(z_i) = \frac{\mathbb{E}[c_{i,t+h}|z_i, x] - \mathbb{E}[c_{i,t+h}|z_i, 0]}{x} \quad (8)$$

That is, averaged over realizations of income and expressed as a fraction of the shock, how much more does that household consume at  $t + h$  because of an unanticipated shock of size  $x$  at  $t$ ? Due to the budget constraint, intertemporal MPCs must add up to unity in

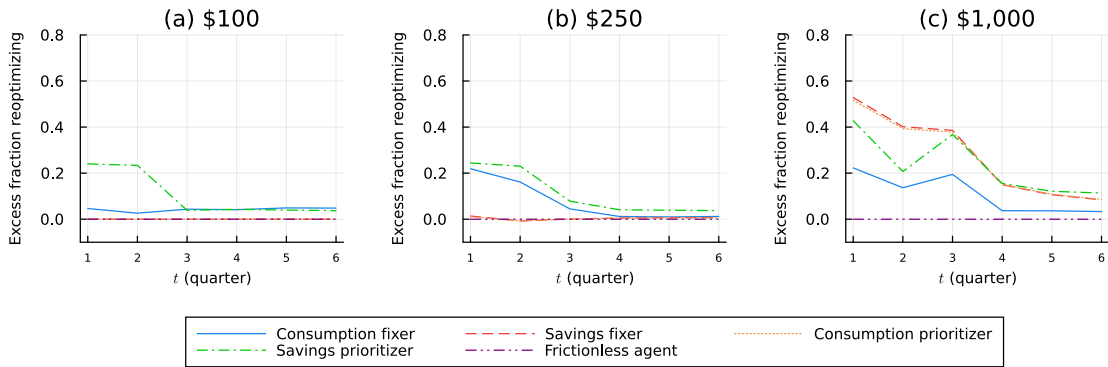
**Figure 13: Dynamic consumption responses to transfer shocks**

**(i) Profiles of intertemporal marginal propensities to consume**



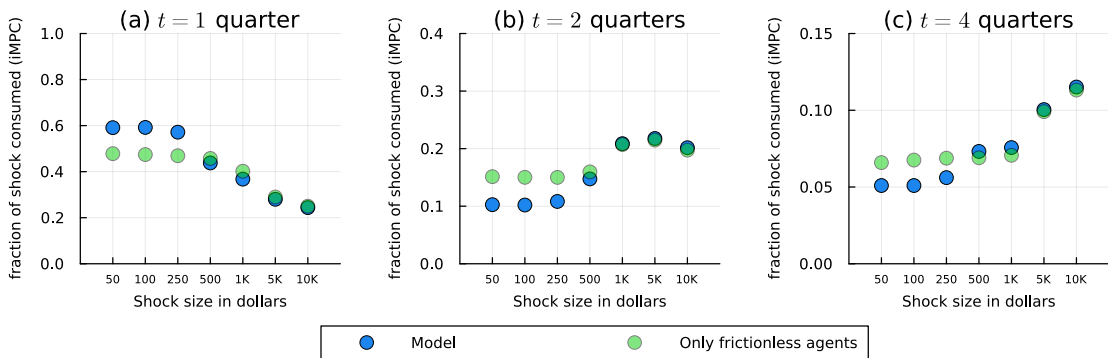
*Notes:* Each panel shows the intertemporal marginal propensity to consume (iMPC) in response to a different stimulus experiment. Each line corresponds to a different household type.

**(ii) Profiles of delayed reoptimization**



*Notes:* Each panel shows the fraction of additional households that reoptimize compared to the steady state in response to a different stimulus experiment. The fraction at  $t = 1$  is calculated based on whether households reoptimize in response to an income shock *or* the unanticipated stimulus. Each line corresponds to a different household type.

**(iii) Comparing iMPC profiles with the model with frictionless optimization**



*Notes:* Each panel shows the intertemporal marginal propensity to consume (iMPC) averaged over households of all types in response to the specified shocks (x-axis) at the specified horizon. Blue dots correspond to the model prediction. Green dots correspond to the subset of frictionlessly optimizing agents.

present value:  $\sum_{h=0}^{\infty} \frac{1}{(1+r)^h} \text{MPC}_{h,x}(z_i) = 1$  for all  $z_i$ . While our survey directly disciplines the contemporaneous MPC, the quantitative model is necessary to understand how quick-fixing households spend their savings over subsequent quarters.

iMPC profiles vary considerably across types, shock sizes, and horizons (Figure 13(i)). For a \$100 shock, savings fixers and consumption prioritizers consume almost everything in one quarter, consumption fixers and savings prioritizers keep about 40% saved after one year, and frictionless optimizers behave somewhere in between. For a \$1,000 shock, by contrast, iMPC profiles are more uniform across groups due to most types abandoning their quick-fixes—except for a significant fraction of consumption fixers who, consistent with our survey evidence, fully save even these large amounts.

To better illustrate how quick-fixing and adjustment drive these patterns, Figure 13(ii) shows the impulse response of the fraction of agents who abandon their quick-fix. In response to small shocks (Panel (a)), very few households adjust on impact, and essentially no one does after three quarters. As shocks get larger (Panels (b) and (c)), households abandon their quick-fix both on impact and after many quarters.

We directly illustrate front-loadedness in Figure 13(iii), which plots iMPCs at specific horizons (1, 2, 4, and 6 horizons) for each gain scenario under the calibrated quick-fixing model and the benchmark with frictionless optimization. For large shock sizes (\$1,000 and above), the models are much closer to one another at all horizons. This is natural because, as uncovered in the survey, most households do not quick-fix when confronted with large shocks. For small shocks, the quick-fixing model generates considerably higher spending at short horizons and lower spending at long horizons.

The heavy front-loadedness under quick-fixing is consistent with the experimental evidence of [Boehm et al. \(2025\)](#), who find that the majority of households’ consumption response tends to take place in the first 3-4 weeks after receiving an unanticipated transfer. Notably, [Boehm et al. \(2025\)](#) consider a transfer of 300 Euros, a size of transfer for which quick-fixing behavior is likely to be important.

## 7 Conclusion

This paper starts from the idea that, when changing consumption-savings behavior is costly, people may instead rely on simple near-rational policy functions: *quick-fixes* that avoid these costs. We develop a model of quick-fixing to capture this idea. By developing and fielding a novel survey, we can recover the consumption policy functions required to test the theory. We find that many households quick-fix by following simple rules of fully spending or fully saving in response to small income shocks, while pursuing a more moderate strategy for large

shocks. This behavior is consistent with near-rationality but inconsistent with alternative models.

Quick-fixing has implications for consumption and savings behavior at both microeconomic and macroeconomic levels. First, quick-fixing is near-rational. Even very small utility costs can account for the economically large deviations in behavior from the benchmark with frictionless optimization. Second, quick-fixing opens the “black box” of latent heterogeneity in the marginal propensity to consume. Quick-fixing types are essentially unpredictable by demographic and financial variables while accounting for a significant fraction of MPC variation. Third, by generating significant incidence-dependence, non-monotonicity in MPCs by wealth, and front-loading of iMPCs, quick-fixing has potentially important implications for business cycle dynamics and policy design. Indeed, given empirical quick-fixing behavior, care should be taken in extrapolating estimates of (i)MPCs into counterfactuals: if the distribution of income shocks under counterfactuals differs from that in the estimation sample, then these (i)MPCs may be substantially incorrect.

Finally, our findings convey a broader lesson about how to “put the near-rationality hypothesis to work.” The premise that changing behavioral rules is costly relative to the utility loss of a simple quick-fix plausibly holds for many other important economic decisions, like portfolio choice or labor supply. But this observation, by itself, does not suffice for making specific predictions for how people actually behave. We still need to understand *which* quick-fixes economic agents use in practice. In these cases, our approach of combining theory, a tailored empirical design to uncover which quick-fixes agents use, and quantitative modeling to study aggregate consequences could prove valuable.

## References

- Aguiar, Mark, Mark Bills, and Corina Boar**, “Who Are the Hand-to-Mouth?,” *The Review of Economic Studies*, 2024, *92* (3), 1293–1340.
- Akerlof, George A and Janet L Yellen**, “Can small deviations from rationality make significant differences to economic equilibria?,” *The American Economic Review*, 1985, *75* (4), 708–720.
- and —, “A near-rational model of the business cycle, with wage and price inertia,” *The Quarterly Journal of Economics*, 1985, *100* (Supplement), 823–838.
- Angeletos, George-Marios, Chen Lian, and Christian K Wolf**, “Can Deficits Finance Themselves?,” *Econometrica*, 2024, *92* (5), 1351–1390.
- Auclert, Adrien, Matthew Rognlie, and Ludwig Straub**, “The intertemporal keynesian cross,” *Journal of Political Economy*, 2024, *132* (12), 4068–4121.
- , —, and —, “Fiscal and Monetary Policy with Heterogeneous Agents,” *Annual Review of Economics*, 2025, *17*, 539–562.
- Barsky, Robert B, Christopher L House, and Miles S Kimball**, “Sticky-price models and durable goods,” *American Economic Review*, 2007, *97* (3), 984–998.
- Bewley, Truman**, “The optimum quantity of money,” Discussion Paper 383, Northwestern University 1979.
- Boehm, Johannes, Etienne Fize, and Xavier Jaravel**, “Five Facts about MPCs: Evidence from a Randomized Experiment,” *American Economic Review*, 2025, *115* (1), 1–42.
- Bordalo, Pedro, John Conlon, Nicola Gennaioli, Spencer Kwon, and Andrei Shleifer**, “How People Use Statistics,” *Review of Economic Studies*, 2025, *rdaf022*.
- Borusyak, Kirill, Xavier Jaravel, and Jann Spiess**, “Revisiting Event-Study Designs: Robust and Efficient Estimation,” *The Review of Economic Studies*, 2024, *91* (6), 3253–3285.
- Boutros, Michael**, “Windfall Income Shocks with Finite Planning Horizons,” *Working Paper*, 2025.
- Brinca, Pedro, Miguel Faria e Castro, Miguel H Ferreira, and Hans Holter**, “The nonlinear effects of fiscal policy,” in “Proceedings. Annual Conference on Taxation and Minutes of the Annual Meeting of the National Tax Association,” Vol. 112 2019, pp. 1–70.
- Broda, Christian and Jonathan A. Parker**, “The Economic Stimulus Payments of 2008 and the aggregate demand for consumption,” *Journal of Monetary Economics*, 2014, *68*, S20–S36.
- Browning, Martin and Thomas F Crossley**, “The Life-Cycle Model of Consumption and Saving,” *Journal of Economic Perspectives*, 2001, *15* (3), 3–22.
- Bunn, Philip, Jeanne Le Roux, Kate Reinold, and Paolo Surico**, “The consumption response to positive and negative income shocks,” *Journal of Monetary Economics*, 2018, *96*, 1–15.
- Campbell, John Y and N Gregory Mankiw**, “Consumption, income, and interest rates: Reinterpreting the time series evidence,” *NBER Macroeconomics Annual*, 1989, *4*, 185–216.
- Carroll, Christopher D**, “Buffer-stock saving and the life cycle/permanent income hypothesis,” *The Quarterly Journal of Economics*, 1997, *112* (1), 1–55.
- Carroll, Christopher, Jiri Slacalek, Kiichi Tokuoka, and Matthew N White**, “The distribution of wealth and the marginal propensity to consume,” *Quantitative Economics*, 2017, *8* (3), 977–1020.
- Chetty, Raj and Adam Szeidl**, “Consumption commitments and habit formation,” *Econometrica*, 2016, *84* (2), 855–890.
- Christelis, Dimitris, Dimitris Georgarakos, Tullio Jappelli, Luigi Pistaferri, and Maarten Van Rooij**, “Asymmetric Consumption Effects of Transitory Income Shocks,” *The Economic Journal*, 2019, *129* (622), 2322–2341.

- Cochrane, John H**, “The sensitivity of tests of the intertemporal allocation of consumption to near-rational alternatives,” *The American Economic Review*, 1989, 79, 319–337.
- Coibion, Olivier, Dimitris Georgarakos, Yuriy Gorodnichenko, Geoff Kenny, and Michael Weber**, “The effect of macroeconomic uncertainty on household spending,” *American Economic Review*, 2024, 114 (3), 645–677.
- , **Yuriy Gorodnichenko, and Michael Weber**, “How Did U.S. Consumers Use Their Stimulus Payments?,” Technical Report 27693, NBER 2020.
- Colarieti, Roberto, Pierfrancesco Mei, and Stefanie Stantcheva**, “The How and Why of Household Reactions to Income Shocks,” *Working Paper*, 2024.
- Debortoli, Davide and Jordi Galí**, “Heterogeneity and aggregate fluctuations: insights from TANK models,” Technical Report, National Bureau of Economic Research 2024.
- Fagereng, Andreas, Martin B. Holm, and Gisle J. Natvik**, “MPC Heterogeneity and Household Balance Sheets,” *American Economic Journal: Macroeconomics*, 2021, 13 (4), 1–54.
- Friedman, Milton**, *A Theory of the Consumption Function*, Princeton University Press, 1957.
- Fuster, Andreas, Greg Kaplan, and Basit Zafar**, “What Would You Do with \$500? Spending Responses to Gains, Losses, News and Loans,” *Review of Economic Studies*, 2021, 88 (4), 1760–1795.
- Gabaix, Xavier**, “A sparsity-based model of bounded rationality,” *The Quarterly Journal of Economics*, 2014, 129 (4), 1661–1710.
- and **David Laibson**, “The 6D bias and the equity-premium puzzle,” *NBER Macroeconomics Annual*, 2001, 16, 257–312.
- Ganong, Peter and Pascal Noel**, “Consumer Spending during Unemployment: Positive and Normative Implications,” *American Economic Review*, July 2019, 109 (7), 2383–2424.
- , **Damon Jones, Pascal Noel, and Daniel Sullivan**, “The Marginal Propensity to Consume out of Stimulus: Evidence From Administrative Bank Data,” *Working Paper*, 2025.
- Guvenen, Fatih, Sam Schulhofer-Wohl, Jae Song, and Motohiro Yogo**, “Worker betas: Five facts about systematic earnings risk,” *American Economic Review*, 2017, 107 (5), 398–403.
- Hall, Robert E**, “Stochastic implications of the life cycle-permanent income hypothesis: theory and evidence,” *Journal of Political Economy*, 1978, 86 (6), 971–987.
- Ilut, Cosmin and Rosen Valchev**, “Economic agents as imperfect problem solvers,” *The Quarterly Journal of Economics*, 2023, 138 (1), 313–362.
- and – , “Learning Optimal Behavior Through Reasoning and Experiences,” Paper 2024.
- Jappelli, Tullio and Luigi Pistaferri**, “Fiscal Policy and MPC Heterogeneity,” *American Economic Journal: Macroeconomics*, October 2014, 6 (4), 107–136.
- and – , “Reported MPC and Unobserved Heterogeneity,” *American Economic Journal: Economic Policy*, 2020, 12 (4), 275–297.
- Kaplan, Greg and Giovanni L Violante**, “A model of the consumption response to fiscal stimulus payments,” *Econometrica*, 2014, 82 (4), 1199–1239.
- and – , “The marginal propensity to consume in heterogeneous agent models,” *Annual Review of Economics*, 2022, 14 (1), 747–775.
- , **Benjamin Moll, and Giovanni L Violante**, “Monetary policy according to HANK,” *American Economic Review*, 2018, 108 (3), 697–743.
- Kőszegi, Botond and Filip Matějka**, “Choice simplification: A theory of mental budgeting and naive diversification,” *The Quarterly Journal of Economics*, 2020, 135 (2), 1153–1207.
- Kotsogiannis, Christos and Plutarchos Sakellaris**, “MPCs Estimated from Tax Lottery, Survey and Administrative Data,” *Working Paper*, 2025.

- Krusell, Per and Anthony A Smith**, “Rules of thumb in macroeconomic equilibrium a quantitative analysis,” *Journal of Economic Dynamics and Control*, 1996, 20 (4), 527–558.
- , **Burhanettin Kuruşçu, and Anthony A Smith**, “Time orientation and asset prices,” *Journal of Monetary Economics*, 2002, 49 (1), 107–135.
- Kueng, Lorenz**, “Excess Sensitivity of High-Income Consumers,” *The Quarterly Journal of Economics*, 2018, 133 (4), 1693–1751.
- Lewis, Daniel J., Davide Melcangi, and Laura Pilossoph**, “Latent Heterogeneity in the Marginal Propensity to Consume,” Working Paper 32523, NBER 2024.
- Lian, Chen**, “A theory of narrow thinking,” *The Review of Economic Studies*, 2021, 88 (5), 2344–2374.
- , “Mistakes in future consumption, high MPCs now,” *American Economic Review: Insights*, 2023, 5 (4), 563–581.
- Lucas, Robert E**, “Econometric policy evaluation: A critique,” in “Carnegie-Rochester Conference Series on Public Policy,” Vol. 1 North-Holland 1976, pp. 19–46.
- Malmendier, Ulrike and Stefan Nagel**, “Depression babies: do macroeconomic experiences affect risk taking?,” *The Quarterly Journal of Economics*, 2011, 126 (1), 373–416.
- Maxted, Peter, David Laibson, and Benjamin Moll**, “Present Bias Amplifies the Household Balance-Sheet Channels of Macroeconomic Policy\*,” *The Quarterly Journal of Economics*, 2024, 140 (1), 691–743.
- Misra, Kanishka and Paolo Surico**, “Consumption, Income Changes, and Heterogeneity: Evidence from Two Fiscal Stimulus Programs,” *American Economic Journal: Macroeconomics*, 2014, 6 (4), 84–106.
- Oprea, Ryan**, “Complexity and Its Measurement,” *Working Paper*, 2024.
- Orchard, Jacob D, Valerie A Ramey, and Johannes F Wieland**, “Micro MPCs and Macro Counterfactuals: The Case of the 2008 Rebates,” *The Quarterly Journal of Economics*, 2025, 140 (3), 2001–2052.
- Parker, Jonathan A and Nicholas S Souleles**, “Reported effects versus revealed-preference estimates: Evidence from the propensity to spend tax rebates,” *American Economic Review: Insights*, 2019, 1 (3), 273–290.
- , – , **David S Johnson, and Robert McClelland**, “Consumer Spending and the Economic Stimulus Payments of 2008,” *American Economic Review*, 2013, 103 (6), 2530–2553.
- Pfäuti, Oliver and Fabian Seyrich**, “A Behavioral Heterogeneous Agent New Keynesian Model,” Discussion Paper, DIW Berlin 2024.
- Schmidt, Lawrence DW**, “Climbing and falling off the ladder: Asset pricing implications of labor market event risk,” *Journal of Financial Economics*, 2025, 172, 104131.
- Sims, Christopher A**, “Implications of rational inattention,” *Journal of Monetary Economics*, 2003, 50 (3), 665–690.

## Online Appendix

### A Additional Technical Analysis

#### A.1 Full Analysis of the Simple Model

In this section, we fully analyze the simple model of Section 2. This supplements the simple example presented in the main text. For expositional simplicity, we impose (as in the example) that  $\beta R = 1$ .

##### A.1.1 The Opportunity Cost of Near-Rational Behavior

We first formalize that even very small utility costs can lead near-rational households to behave very differently from households who frictionlessly optimize. To do this, we define the *loss from quick-fixing* in state  $z = (y_1, y_2)$  as

$$\mathcal{L}^q(z) \equiv U^*(z) - U^q(z) \tag{9}$$

where we define the value  $U^*$  of optimal behavior and  $U^q$  of quick-fixing, respectively, via

$$\begin{aligned} U^*(z) &= u(c^*(z)) + \beta u(c^*(z)) \\ U^q(z) &= u(c^q(z)) + \beta u(R(y_1 - c^q(z)) + y_2) \end{aligned} \tag{10}$$

The following result describes the opportunity cost of quick-fixing in a given state compared to behaving optimally, up to a second-order approximation:

**Proposition 1** (Second-Order Losses from Near-Rational Behavior). *The loss from following a quick-fix consumption rule indexed by  $q$  when the asset-income state is  $z$  is:*

$$\mathcal{L}^q(z) = \frac{1}{2}(1 + R)|u''(c^*(z))|(c^q(z) - c^*(z))^2 + O(|c^*(z) - c^q(z)|^3) \tag{11}$$

*Proof.* We apply a state-dependent second-order approximation to  $\mathcal{L}^q(z)$ . Define:

$$U(c, z) = u(c) + \beta u(R(y_1 - c) + y_2) \tag{12}$$

and observe that  $U^q(z) = U(c^q(z), z)$ . We define the loss function on an enriched domain as

**Table 3:** Small utility costs can lead to large errors in consumption

Consumption mistake, $m$	1%	5%	10%	15%	20%
Consumption-equivalent cost, $\tau(m)$	0.01%	0.25%	1.00%	2.22%	3.92%

*Notes:* This table reports the maximum percentage of consumption that a household would be willing to give up to prefer quick-fixing to optimization when quick-fixing would lead to relative consumption mistake of size  $m$ :  $\tau(m) = 1 - e^{-m^2}$ .

$\mathcal{L}^U(c, z) = U^*(z) - U(c, z)$  and observe that  $\mathcal{L}^q(z) = \mathcal{L}^U(c^q(z), z)$ .

To approximate  $\mathcal{L}^q$ , we therefore approximate  $\mathcal{L}^U(\cdot, z)$  to second order around  $c^*(z)$  for every state  $z$ . This yields the following:

$$\begin{aligned} \mathcal{L}^q(z) &= \mathcal{L}^U(c^*(z), z) + \mathcal{L}_c^U(c^*(z), z)(c^q(z) - c^*(z)) \\ &\quad + \frac{1}{2}\mathcal{L}_{cc}^U(c^*(z), z)(c^q(z) - c^*(z))^2 + O(|c^*(z) - c^q(z)|^3) \end{aligned} \quad (13)$$

We observe however that  $\mathcal{L}^U(c^*(z), z) = 0$  by definition and that  $\mathcal{L}_c^U(c^*(z), z) = 0$  by the optimality of  $c^*$ . Thus,

$$\mathcal{L}^{qi}(z) = \frac{1}{2}\mathcal{L}_{cc}^U(c^*(z), z)(c^q(z) - c^*(z))^2 + O(|c^*(z) - c^q(z)|^3) \quad (14)$$

We can moreover compute  $\mathcal{L}_{cc}^U(c^*(z), z)$  as:

$$\mathcal{L}_{cc}^U(c, z) = -\beta R^2 u''(R(y_1 - c) + y_2) - u''(c) = -(1 + \beta R^2)u''(c^*(z)) \quad (15)$$

Completing the proof. □

This result formalizes that there is no first-order loss from deviations from frictionlessly optimizing behavior. This follows from the envelope theorem: when the household makes a small consumption mistake, their first-order condition implies that the slope of their lifetime utility is close to flat in the mistake. Thus, our model generates near-rational behavior by the [Akerlof and Yellen \(1985\)](#) criterion that there is no “first-order” opportunity cost from quick-fixing behavior.

Because of this basic envelope logic, even the presence of small utility costs may motivate a household to follow a quick-fix. To substantiate this, we provide a simple example of how the second-order losses from near-rationality implied by [Proposition 1](#) can lead small utility costs to generate large differences in behavior.

**Example 1** (Small Costs Allow for Large Mistakes). Suppose that  $u(c) = \log c$  and  $R = 1$ . [Proposition 1](#) implies that the payoff loss from following a suboptimal quick-fix is approx-

imately equal to  $m^2$ , where  $m = (c^{q_i}(z_i) - c^*(z_i))/c^*(z_i)$  is the household’s consumption “mistake” expressed as a proportional deviation from the optimal level. We now ask: what is the smallest utility cost in consumption-equivalent units (*i.e.*, equivalently costly to a  $\tau$  percent reduction in consumption in one period) that rationalizes making a consumption mistake of a size  $m$ ? The utility cost associated with the mistake, up to the second-order approximation of Proposition 1, is  $\kappa(m) = m^2$ . Putting this into units of a one-period proportionate loss in consumption  $\tau$ , we have that:

$$\begin{aligned} 2 \log(c^*(z_i)) - \kappa(m) &= \log((1 - \tau)c^*(z_i)) + \log(c^*(z_i)) \\ \implies \tau &= 1 - e^{-m^2} \end{aligned} \tag{16}$$

In Table 3, we report this utility cost for consumption mistakes ranging from 1% to 20%. Surprisingly, if a household were to have to pay anything less than 0.25% of consumption in order to optimize, then it would be unwilling to optimize even when making a 5% consumption mistake. Thus, even very small costs of optimization can lead to large differences in behavior from the model with frictionless optimization.  $\triangle$

### A.1.2 The Near-Rational Response to Income Shocks

Having established that quick-fixers may tolerate large deviations in consumption *levels*, we now study how quick-fixers respond to income *shocks*. Formally, we consider a household that is informed that its first-period income will be  $y_i(x) = y_i + x$  for some income shock  $x \in \mathbb{R}$ . What is the optimal near-rational response? By applying Proposition 1, defining the agent’s state after the income shock as  $z(x) = (y_1 + x, y_2)$ , we obtain that:

**Corollary 1** (When to Quick-Fix). *Up to a second-order approximation, a household with asset-income state  $z$  with quick-fixing type given by  $q$  follows the consumption policy function*

$$c(x) = \begin{cases} c^q(z(x)) & \text{if } |c^q(z(x)) - c^*(z(x))| \leq \sqrt{\frac{\kappa_q}{\frac{1}{2}(1+R)|u''(c^*(z(x)))|}}, \\ c^*(z(x)) & \text{otherwise.} \end{cases} \tag{17}$$

Intuitively, households abandon their quick-fix only after a sufficiently large change in circumstances causes the quick-fix to induce a large “mistake.” Under natural conditions, this is to say that households quick-fix for small shocks and behave optimally for large shocks. To benchmark the economic significance of this result, we briefly return to the setting of Example 1.

**Example 1** (continued). Consider a household whose quick-fix sets consumption to a level appropriate for some “default” state  $z_0$ : that is,  $\bar{c} = c^*(z_0)$  and  $c^{q_i}(z) \equiv \bar{c}$ . Since optimal

consumption  $c^*$  is proportional to permanent income, an  $x\%$  shock to permanent income starting from  $z_0$  corresponds to an  $\frac{x}{1+x}\%$  consumption mistake, which is increasing in the size of the income shock. Moreover, for such a “consumption-fixing” household, Table 3 can be reinterpreted as giving the maximum shocks to permanent income in response to which a household would persist with the quick-fix. For example, a household with a utility cost that equals 0.25% of their consumption would persist in consuming  $\bar{c}$  after a shock to permanent income of less than approximately 5%. Since “large” *transitory* shocks, like generous government stimulus, are potentially “small” shocks to permanent income, quick-fixing can critically shape households’ responses to changes in their economic situation.  $\triangle$

What determines whether households quick-fix if and only if shocks are small enough? Income shocks affect households’ decision whether to quick-fix via two mechanisms: the size of the mistake  $|c^q(z(x)) - c^*(z(x))|$  and the curvature of utility  $|u''(c^*(z(x)))|$ . As highlighted before, we restrict our attention to quick-fixes that make mistakes that are increasing in the magnitude of shocks. Under the conventional case with prudence ( $u''' \geq 0$ ), households will quick-fix out of small negative shocks and act optimally for large negative shocks. For large positive shocks, a high degree of prudence can theoretically fight the prediction that households reoptimize: intuitively, they may not care much about mistakes after a large positive shock makes them rich. However, we do not find empirical evidence for these “switch backs,” where households quick-fix for both small and very large positive shocks, and we do not detect this non-convexity of inaction bands in our empirically calibrated model.

## A.2 One-Time Shocks in the Quantitative Model

In this section, we describe one-time, unanticipated shocks in the quantitative model. We use this experiment to compute reoptimization fractions in the model and match these moments to the data (see Section 4.2 and Equation 5).

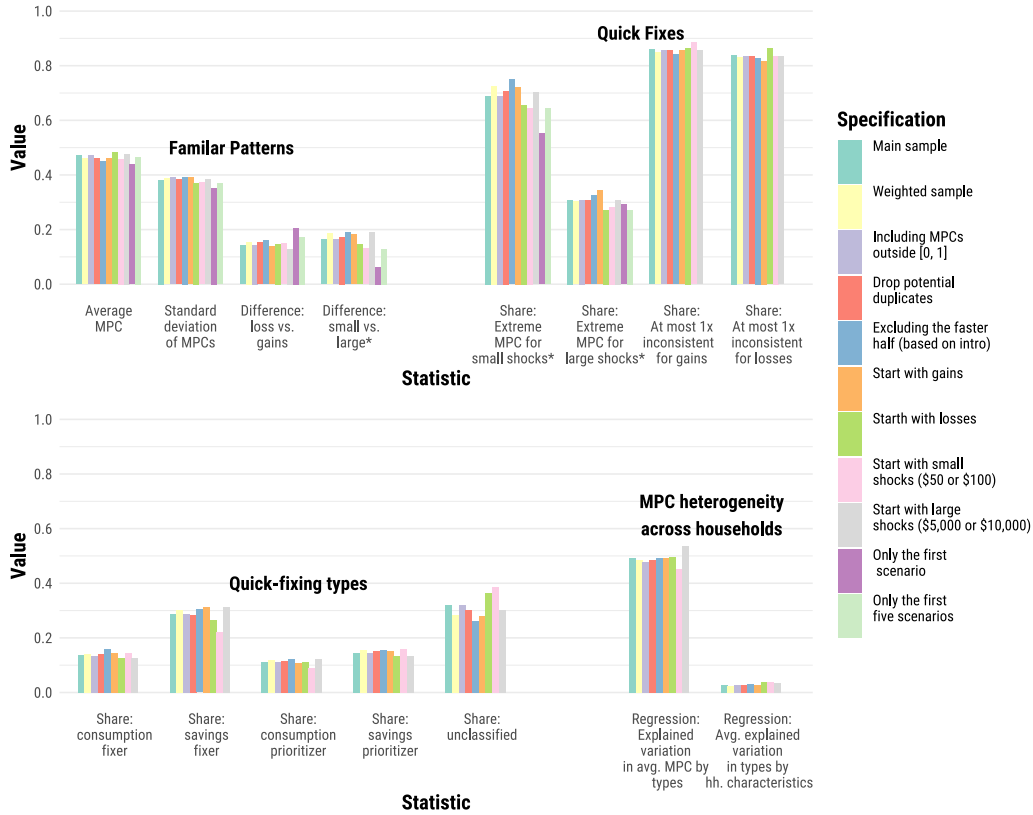
The household contemplates a shock  $x \in \mathbb{R}$  in an interim period after initially choosing whether to quick-fix or reoptimize in a given period, but before observing income or making decisions for the next period. With some abuse of notation, we write  $c^{q*}$ ,  $\bar{c}^*$ , and  $\bar{y}^*$  as the optimal choices in the original decision period, suppressing dependence on the household’s state. The household’s problem, when faced with a shock  $x$ , is

$$\begin{aligned}
& \max_{D_x \in \{0,1\}} \{ D_x (u(c^*(a, y + x)) + \beta \mathbb{E} [V^q(a', y', c^*(a, y + x), y) | y] - \kappa_q) \\
& \quad + (1 - D_x) (u(c^q(c^{q*}, x)) + \beta \mathbb{E} [V^q(a', y', \bar{c}^*, \bar{y}^*) | y]) \} \\
& \text{s.t. } a' = Ra + y - (D_x(c^*(a, y + x)) + (1 - D_x)(c^q(c^{q*}, x))) \\
& \quad a \geq 0
\end{aligned} \tag{18}$$

If a household reoptimizes, then it follows the optimal, forward-looking behavior embodied in  $c^*$ . This entails a utility cost. If the household quick-fixes, then it treats  $c^{q*}$  as its reference consumption and the shock  $x$  as the income shock (Equation 3).

## B Additional Figures and Tables

Figure B.1: Results are robust in a variety of sensitivity tests

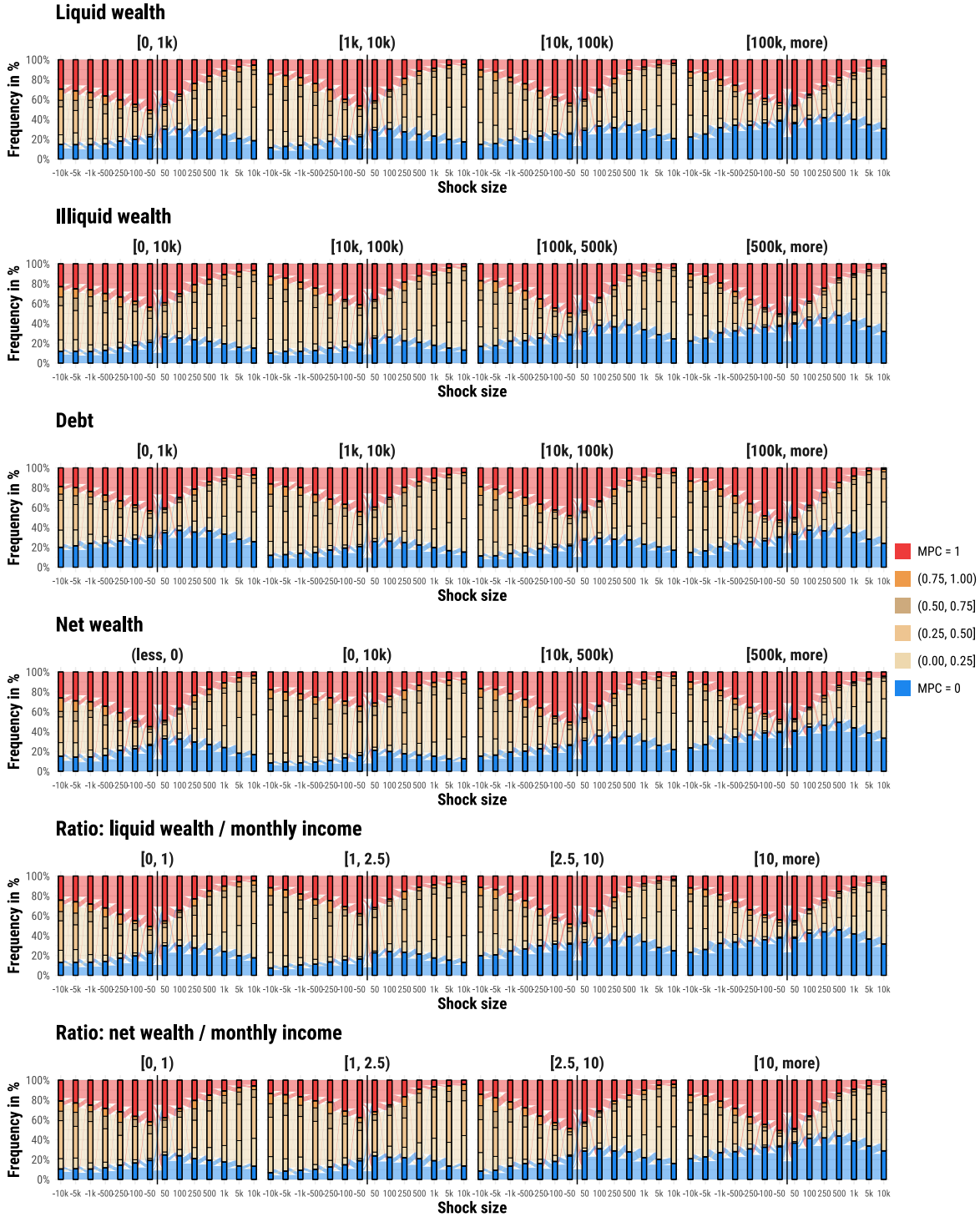


Notes: We recalculate key statistics from Section 3 for a variety of robustness specifications:

- *Main sample*: We reproduce the statistics from the main text.
- *Weighted sample*: We use post-stratification weights that correct for possible imbalances across the variables reported in Table C.1.
- *Including MPCs outside  $[0, 1]$* : We add 51 additional respondents whom we drop from the main analysis because they report MPCs outside  $[0, 1]$  (see Appendix C.1).
- *Drop potential duplicates*: We drop potential duplicate respondents who submitted similar data on the same day (see Appendix C.1).
- *Excluding the faster half (based on intro)*: We exclude the 50% fastest respondents who “speed through” the introductory instructions of the survey.
- *Start with gains*: We restrict the sample to respondents who first respond to gains.
- *Start with losses*: We restrict the sample to respondents who first respond to losses.
- *Start with small shocks (\$50 or \$100)*: We restrict the sample to respondents who first respond to a small income shock of \$50, \$100,  $-\$50$ , or  $-\$100$ .
- *Start with large shocks (\$5,000 or \$10,000)*: We restrict the sample to respondents who first respond to a large income shock of \$5,000, \$10,000,  $-\$5,000$ , or  $-\$10,000$ .
- *Only the first scenario*: We restrict the sample to the first MPC that respondents report.
- *Only the first five scenarios*: We restrict the sample to the first five MPCs that respondents report.

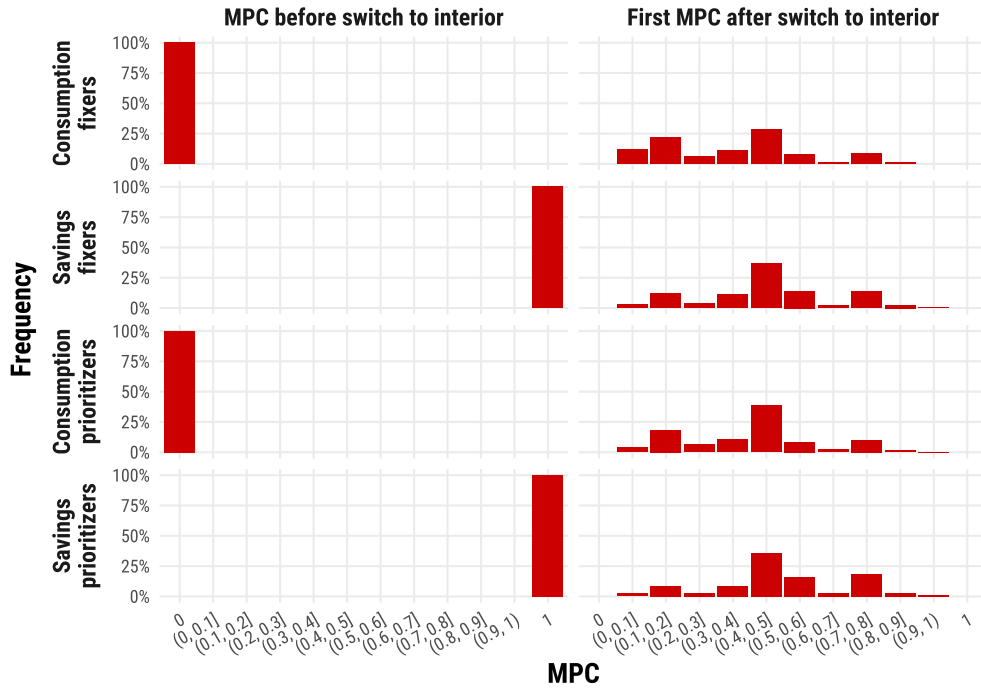
\*Small shocks: \$50 and \$100. Large shocks: \$5,000 and \$10,000.

Figure B.2: MPC profiles across the wealth distribution



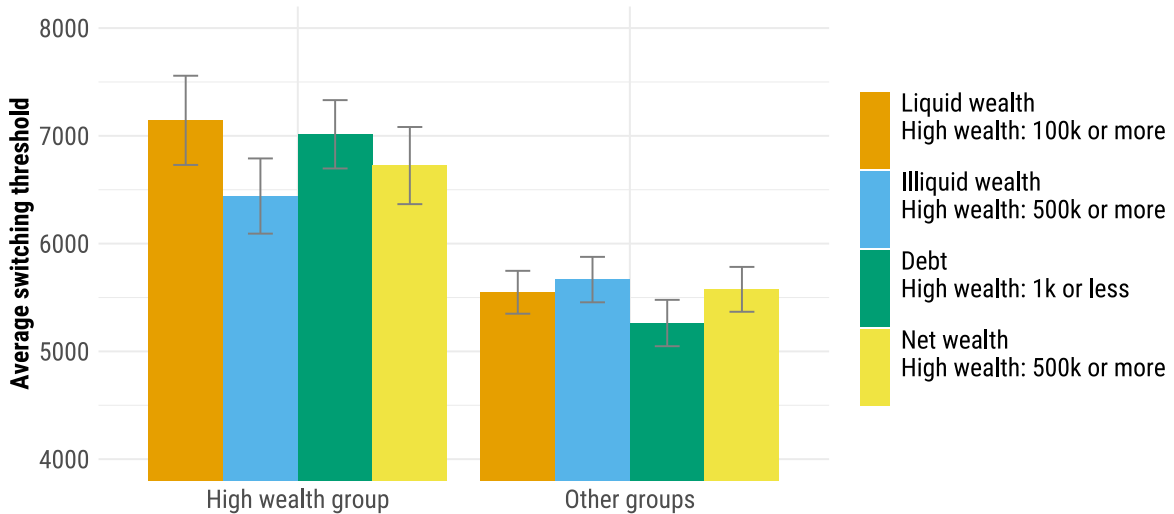
Notes: The alluvial graphs summarize the MPC profiles of households with varying (i) liquid wealth, (ii) illiquid wealth, and (iii) debt, (iv) net wealth, (v) liquid wealth to income ratio (Kaplan et al., 2014), and (vi) net wealth to income ratio (see Appendix C.3 for variable definitions). In each panel, each of the 14 columns displays the distribution of MPCs for one particular shock size, and the streams between bars indicate how households' MPCs transition between two neighboring shocks.

**Figure B.3:** Distribution of MPCs for losses before and after switching to interior



*Notes:* These histograms show conditional distributions of MPCs for losses. The rows correspond to the four quick-fixing types. The first column shows the distribution of MPCs before households switch to an interior MPC, which by construction puts all mass at either MPC = 0 or MPC = 1. The second column shows the conditional distribution of MPCs (given type and shock size) for the first shock for which the respondent reports an interior value. An analogous analysis for gains is reported in Figure 5.

**Figure B.4:** Switching thresholds are higher among high-wealth households



*Notes:* The figure displays the average switching threshold—*i.e.*, the smallest shock for which quick-fixing households switch from an extreme to an interior MPC, averaged across gains and losses—for high-wealth households and other households.

**Table B.1:** Exploring the variation in MPCs across households

<b>Households' average MPCs (across shocks +/- \$100 to 10,000)</b>						
	<b>Average MPC</b>		<b>Share with MPC = 0</b>		<b>Share with MPC = 1</b>	
	(1)	(2)	(3)	(4)	(5)	(6)
<b>Consumption and income</b>						
Monthly spending (log.)		0.012*** (0.003)		-0.015*** (0.004)		0.007** (0.003)
Annual income (log.)		0.008 (0.005)		0.005 (0.006)		0.001 (0.006)
Income risk (std.)		0.023*** (0.004)		-0.037*** (0.004)		-0.003 (0.004)
<b>Liquid wealth: dummies with reference group: [0k, 1k)</b>						
[1k, 10k)		-0.054*** (0.009)		0.001 (0.010)		-0.078*** (0.010)
[10k, 100k)		-0.071*** (0.010)		0.014 (0.012)		-0.096*** (0.012)
[100k, more)		-0.071*** (0.013)		0.057*** (0.015)		-0.064*** (0.014)
<b>Illiquid wealth: dummies with reference group: [0k, 10k)</b>						
[10k, 100k)		-0.019* (0.010)		0.009 (0.011)		-0.028** (0.012)
[100k, 500k)		-0.033*** (0.010)		0.050*** (0.011)		-0.000 (0.011)
[500k, more)		-0.078*** (0.012)		0.105*** (0.014)		0.004 (0.014)
<b>Debt: dummies with reference group: [0k, 1k)</b>						
[1k, 10k)		0.028*** (0.008)		-0.060*** (0.010)		-0.008 (0.010)
[10k, 100k)		0.036*** (0.009)		-0.055*** (0.010)		0.022** (0.010)
[100k, more)		0.014 (0.010)		-0.028** (0.012)		0.007 (0.011)
<b>Other characteristics</b>						
College		0.004 (0.007)		-0.022*** (0.008)		-0.013 (0.008)
Age (in 10y)		-0.017*** (0.002)		0.036*** (0.002)		0.004* (0.002)
Female respondent		0.008 (0.006)		-0.013* (0.007)		0.004 (0.007)
Household size		0.010*** (0.003)		-0.012*** (0.003)		0.002 (0.003)
<b>Quick-fixing types</b>						
Consumption fixer	-0.318*** (0.008)		0.573*** (0.012)		0.009 (0.006)	
Savings fixer	0.112*** (0.007)		0.119*** (0.007)		0.383*** (0.008)	
Consumption prioritizer	-0.107*** (0.009)		0.302*** (0.012)		0.147*** (0.009)	
Savings prioritizer	-0.083*** (0.007)		0.313*** (0.009)		0.208*** (0.008)	
Constant	0.487*** (0.003)	0.381*** (0.050)	0.047*** (0.003)	0.147** (0.058)	0.051*** (0.003)	0.177*** (0.058)
Obs.	4,981	4,981	4,981	4,981	4,981	4,981
R <sup>2</sup>	0.350	0.129	0.444	0.212	0.406	0.026

*Notes:* This table reports regressions that explore the heterogeneity of households' MPCs. Columns 1–2 analyze households' mean MPC (averaged across the 12 losses or gains ranging from \$100 to \$10,000), Columns 3–4 analyze households' share of MPCs that equal 0 (among the same 12 shocks), and Columns 5–6 analyze households' share of MPCs that equal 1 (among the same 12 shocks). Appendix C.3 describes how we measure the economic background variables. Robust standard errors are in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

**Table B.2:** Exploring the variation in quick-fixing types across households

	Type membership				Switching point		
	Consumption fixer	Savings fixer	Consumption prioritizer	Savings prioritizer	Unclassified	Average log(shock size)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<b>Consumption and income</b>							
Monthly spending (log.)	-0.012*** (0.004)	0.011** (0.005)	0.005 (0.003)	0.000 (0.004)	-0.005 (0.005)		-0.091*** (0.023)
Annual income (log.)	0.005 (0.007)	0.000 (0.010)	-0.009 (0.007)	-0.018** (0.008)	0.022** (0.010)		0.126*** (0.044)
Income risk (std.)	-0.022*** (0.005)	-0.021*** (0.007)	-0.020*** (0.004)	-0.022*** (0.005)	0.084*** (0.007)		0.057* (0.031)
<b>Liquid wealth:</b> dummies with reference group: [0k, 1k)							
[1k, 10k)	0.012 (0.013)	-0.051*** (0.019)	0.003 (0.013)	-0.010 (0.015)	0.046** (0.018)		-0.438*** (0.082)
[10k, 100k)	0.020 (0.015)	-0.086*** (0.020)	0.005 (0.015)	-0.026 (0.017)	0.088*** (0.019)		-0.205** (0.087)
[100k, more)	0.070*** (0.020)	-0.088*** (0.026)	0.037** (0.019)	-0.045** (0.020)	0.026 (0.023)		0.052 (0.104)
<b>Illiquid wealth:</b> dummies with reference group: [0k, 10k)							
[10k, 100k)	-0.002 (0.015)	-0.000 (0.022)	-0.003 (0.014)	0.015 (0.017)	-0.009 (0.021)		-0.053 (0.100)
[100k, 500k)	0.001 (0.014)	0.050** (0.020)	0.016 (0.014)	0.020 (0.016)	-0.086*** (0.019)		0.111 (0.084)
[500k, more)	0.061*** (0.019)	0.082*** (0.024)	0.003 (0.017)	0.043** (0.019)	-0.189*** (0.021)		0.141 (0.098)
<b>Debt:</b> dummies with reference group: [0k, 1k)							
[1k, 10k)	-0.057*** (0.013)	-0.002 (0.018)	0.008 (0.012)	0.009 (0.014)	0.042** (0.018)		-0.364*** (0.083)
[10k, 100k)	-0.051*** (0.013)	0.046** (0.018)	-0.000 (0.012)	0.017 (0.014)	-0.012 (0.017)		-0.269*** (0.076)
[100k, more)	-0.039** (0.016)	0.045** (0.020)	0.034** (0.015)	0.038** (0.016)	-0.077*** (0.018)		-0.363*** (0.080)
<b>Other characteristics</b>							
College	-0.011 (0.011)	-0.021 (0.015)	0.007 (0.010)	-0.019* (0.011)	0.045*** (0.013)		-0.005 (0.061)
Age (in 10y)	0.016*** (0.003)	0.010** (0.004)	0.011*** (0.003)	0.007** (0.003)	-0.045*** (0.004)		0.100*** (0.018)
Female respondent	-0.009 (0.010)	0.015 (0.013)	-0.017* (0.009)	-0.003 (0.010)	0.014 (0.012)		-0.011 (0.056)
Household size	-0.013*** (0.004)	0.005 (0.005)	0.003 (0.004)	-0.007 (0.004)	0.012** (0.005)		-0.029 (0.025)
<b>Quick-fixing types</b>							
Consumption fixer						0.236*** (0.085)	
Savings fixer						-0.218*** (0.073)	
Consumption prioritizer						-0.555*** (0.093)	
Constant	0.138* (0.077)	0.134 (0.099)	0.089 (0.067)	0.324*** (0.082)	0.315*** (0.103)	7.960*** (0.059)	6.987*** (0.459)
Obs.	4,981	4,981	4,981	4,981	4,981	3,381	3,381
R <sup>2</sup>	0.057	0.017	0.020	0.020	0.174	0.024	0.058

*Notes:* This table reports regressions that explore the heterogeneity of households' quick-fixing types. Columns 1–5 analyze households' type (binary indicators), and Columns 6–7 analyze households' mean log switching threshold (the smallest shock for which they switch from an extreme to an interior MPC, averaged across gains and losses). Columns 6–7 restrict the sample to the four quick-fixing types. Appendix C.3 describes how we measure the economic background variables. Robust standard errors are in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

**Table B.3:** Deliberation negatively predicts extreme MPCs (deliberation ratings study)

	Extreme MPC of 0 or 1 (binary indicator)					
	(1)	(2)	(3)	(4)	(5)	(6)
Deliberation (std.)	-0.263*** (0.011)	-0.274*** (0.020)	-0.248*** (0.011)	-0.254*** (0.020)	-0.257*** (0.013)	-0.276*** (0.020)
Respondent FE	✓	✓	✓	✓	✓	✓
Weights	-	✓	-	✓	-	✓
<b>Measure</b>	<b>Carefully consider how to change spending</b>		<b>Assess overall financial situation</b>		<b>Discuss with household members</b>	
Observations	3,619	3,619	3,619	3,619	3,080	3,080
R <sup>2</sup>	0.740	0.761	0.723	0.744	0.711	0.719

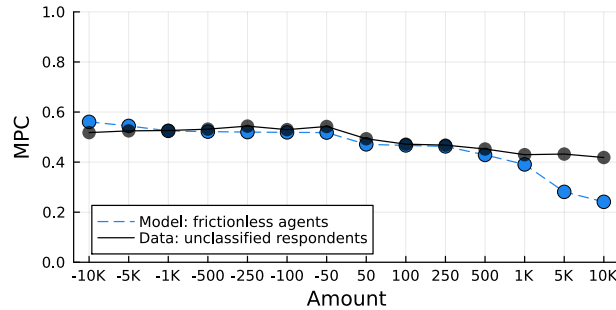
*Notes:* This table reports regression results and uses data from the deliberation ratings study. We regress a binary indicator for whether a household adopts an extreme MPC of 0 or 1 on different standardized deliberation measures (see row “Measure”). Columns 1, 3, and 5 display unweighted results. Columns 2, 4, and 6 use post-stratification weights that correct for imbalances in the distribution of demographic characteristics (see Table C.2). All regressions use household-level fixed effects. The standard errors in parentheses are robust and clustered at the household level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

**Table B.4:** Response time negatively predicts extreme MPCs (main study)

	Extreme MPC of 0 or 1 (binary indicator)			
	(1)	(2)	(3)	(4)
Response time (in 10s)	-0.060*** (0.001)	-0.078*** (0.002)	-0.068*** (0.002)	-0.094*** (0.002)
Respondent FE	✓	✓	✓	✓
Order FE			✓	✓
Sample	Full	Only quick-fixers	Full	Only quick-fixers
Observations	69,734	47,334	69,734	47,334

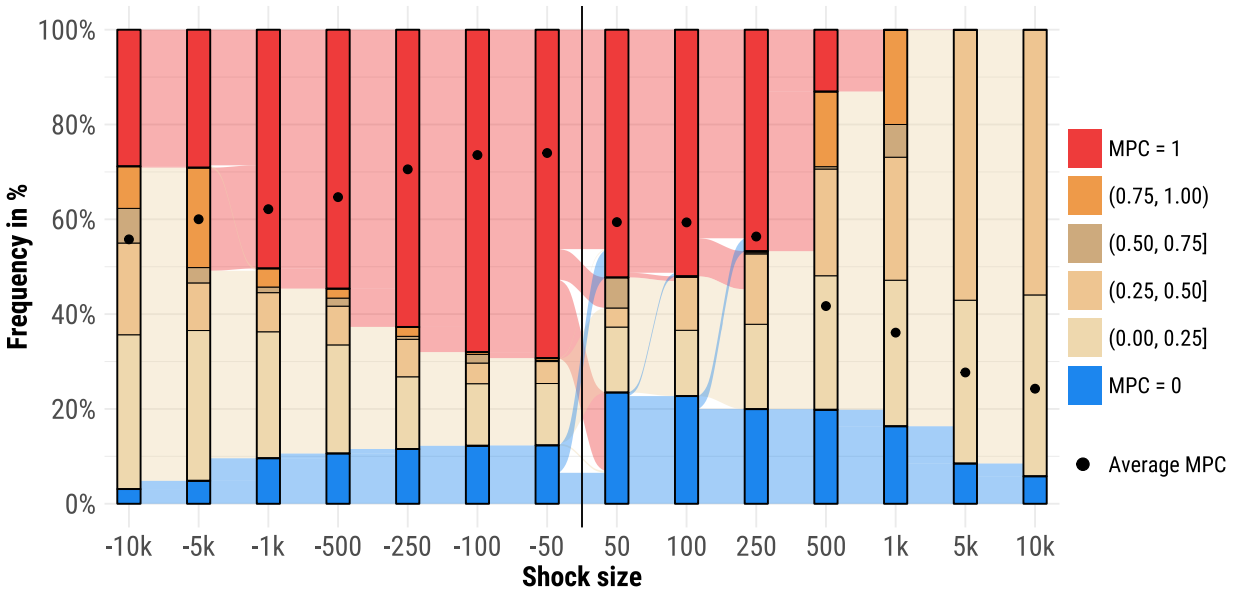
*Notes:* This table reports regression results and uses data from the main study. We regress a binary indicator for whether a household adopts an extreme MPC of 0 or 1 on the time respondents spent on the shock scenario. We winsorize response time at its 95% quantile. Columns 1 and 3 use the full data, while Columns 2 and 4 report results for quick-fixing households only. Columns 3 and 4 include fixed effects for order, i.e. fourteen dummies that indicate whether respondents make their first, second, third, ... decision. All regressions use household-level fixed effects. The robust standard errors are robust and clustered at the household level. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

**Figure B.5:** Quantitative model fit for frictionlessly optimizing agents' MPCs



*Notes:* The line graphs compare empirical measurements and model predictions for the average marginal propensity to consume out of shocks of different amounts, among Unclassified respondents in the survey (black line and dots) and frictionlessly optimizing agents in the model (blue line and dots). We calibrate the discount factor  $\beta$  to minimize the sum of squared differences between these model predictions and measurements.

**Figure B.6:** Quantitative model fit for the MPC distribution



*Notes:* The alluvial graph summarizes the MPC predictions generated by the quantitative model, following the format of Figure 1. Each of the 14 columns displays the distribution of MPCs for one particular shock size, with colors indicating the size of the MPC. The streams between bars indicate how households' MPCs transition between two neighboring shocks. Black dots depict the average MPCs for each shock. We exclude a few respondents with MPCs outside  $[0, 1]$  to facilitate the visual presentation.

## C Additional Material for the Empirical Studies

### C.1 Sample

**Sampling.** We recruited respondents in October and November 2023 collaborating with the survey company Bilendi. We recruited respondents from different parts of the Bilendi respondent pool in order to approximate the general US population in terms of gender, age, income, education, and region.

**Final Sample Characteristics.** Table C.1 presents demographic summary statistics for our final sample and compares them to the demographic characteristics of the US adult population.

**Exclusion Criteria.** Three exclusion criteria are preregistered. The sample does not contain the following responses: (i) incomplete responses, (ii) responses at both extreme 1% tails in the response duration, and (iii) responses with duplicate IDs (very rare cases).

In addition, we exclude 51 respondents who have at least one MPCs outside the interval  $[0, 1]$ . Many of these respondents report just one or a few MPCs outside  $[0, 1]$ , which could simply reflect response error. Excluding them simplifies the visual presentation of the results and ensures that outliers do not distort our analyses of averages. Unsurprisingly, the robustness check in Figure B.1 confirms that we obtain virtually the same results with the full sample.

**Attention Screener.** Only participants who pass an attention screener at the beginning of the survey can proceed to the main part of the survey.

**Potential duplicate responses.** Even though we included a captcha and an attention screener, we observe a couple of very similar respondents who start the survey at a similar time. About 75 respondents have identical answers to 23 different demographic questions and start the survey at a similar time. Fortunately, our results are robust to excluding them. Figure B.1 takes an even more conservative approach and drops roughly 7% of responses with the most similar demographic data within each day. Again, the results are virtually identical.

**Table C.1:** Demographic characteristics of the sample

Variable	ACS (2022)	Sample
<b>Gender</b>		
Female	50%	50%
<b>Age</b>		
18-34	29%	27%
35-54	32%	33%
55+	38%	40%
<b>Household income</b>		
Below 50k	34%	34%
50k-100k	29%	28%
Above 100k	37%	37%
<b>Education</b>		
Bachelor's degree or more	33%	40%
<b>Region</b>		
Northeast	17%	17%
Midwest	21%	21%
South	39%	39%
West	24%	23%
Sample size	1,980,550	4,981
Variable	SCF (2022)	Sample
<b>Liquid assets</b>		
Below 1k	20%	29%
1k-10k	31%	25%
10k-100k	31%	28%
Above 100k	19%	18%
<b>Illiquid assets</b>		
Below 10k	26%	38%
10k-100k	11%	14%
100k-500k	34%	26%
Above 500k	29%	22%
<b>Debt</b>		
Below 1k	27%	35%
1k-10k	10%	21%
10k-100k	27%	25%
Above 100k	36%	19%
Sample size	4,602	4,981

*Notes:* This table presents summary statistics for the sample of US households and compares them to benchmark characteristics for the US adult population based on data from the American Community Survey 2022 and the Survey of Consumer Finances 2022. Appendix C.3 describes how we measure the economic background variables.

**Table C.2:** Demographic characteristics in the additional studies

Variable	ACS (2022)	Deliberation ratings study	Qualitative study
<b>Gender</b>			
Female	50%	50%	49%
<b>Age</b>			
18-34	29%	43%	47%
35-54	32%	48%	46%
55+	38%	10%	7%
<b>Household income</b>			
Below 50k	34%	25%	32%
50k-100k	29%	40%	35%
Above 100k	37%	35%	33%
<b>Education</b>			
Bachelor's degree or more	33%	63%	64%
<b>Region</b>			
Northeast	17%	19%	18%
Midwest	21%	15%	17%
South	39%	49%	41%
West	24%	18%	25%
Sample size	1,980,550	517	502
Variable	SCF (2022)	Deliberation ratings study	Qualitative study
<b>Liquid assets</b>			
Below 1k	20%	26%	26%
1k-10k	31%	32%	27%
10k-100k	31%	32%	37%
Above 100k	19%	10%	10%
<b>Illiquid assets</b>			
Below 10k	26%	38%	38%
10k-100k	11%	21%	19%
100k-500k	34%	24%	25%
Above 500k	29%	18%	19%
<b>Debt</b>			
Below 1k	27%	20%	22%
1k-10k	10%	23%	18%
10k-100k	27%	26%	30%
Above 100k	36%	30%	30%
Sample size	4,602	517	502

*Notes:* This table presents summary statistics for the sample of US households in the additional studies and compares them to benchmark characteristics for the US adult population based on data from the American Community Survey 2022 and the Survey of Consumer Finances 2022. Appendix C.3 describes how we measure the economic background variables.

## C.2 Instructions

The complete instructions are available online at <https://osf.io/2s7cf>. The survey begins with a participation information and informed consent form. Respondents who participate on a mobile device are screened out. Next, respondents have to pass an attention check. Subsequently, respondents fill out a block of demographic questions. Then, the main part of the survey begins (see below). The survey ends with additional questions on households' economic situation.

### Introduction

In this survey, we are seeking to understand how your household reacts to unanticipated changes in income. By “household”, we mean everyone who usually lives with you in your primary residence including yourself (but excluding roommates and renters).

**You will be presented with various hypothetical scenarios that involve shifts in your income, and we will ask you how such changes would impact your household's spending and saving.** Below, we provide a short description of what we mean by “spending” and “saving”. Please read them carefully.

**Spending: Spending includes all money spent on goods and services, including rent.** Goods include durable goods (such as electronics, furniture, or car maintenance) and nondurable goods (such as groceries, vacations, or gasoline).

**Saving: Saving means that, instead of using money today, you reserve it for future use.** Examples of savings include cash reserves, money in bank accounts, retirement accounts, financial assets, or real estate. **Repaying debt is also an important form of saving.** By repaying debt today, you owe less money in the future, which means that more money is available for future use.

---

On the next pages, you will consider hypothetical situations where your household unexpectedly receives a

**one-time payment today.**

That is, your household's income will be higher for one month due to a one-time payment. The one-time payment comes unexpectedly.

*Comment: We randomize whether income losses or gains are displayed first.*

## A one-time payment

### Situation 1

Consider a hypothetical situation where your household unexpectedly receives a

**one-time payment of \$250 today.**

How would this one-time extra income cause your household to change its spending and saving over the next three months?

**Note:** Your responses need to add up to \$250.

Enter \$0 if your household's spending/saving would not change.

Enter negative numbers for *decreases* in your household's spending/saving.

**Increase in spending**

(By how much) would your household increase its monthly spending over the next three months?

\$

**Increase in saving**

(By how much) would your household increase its monthly saving (which includes increases in debt repayment or decreases in debt-taking) over the next three months?

\$

Total

\$

*Comment: Situation 2–7 are analogous. We randomize the order of shock sizes. Each respondent faces seven shocks: \$50, \$100, \$250, \$500, \$1,000, \$5,000, and \$10,000.*

On the next pages, you will consider hypothetical situations where your household unexpectedly incurs a

**one-time income loss today.**

That is, your household's income will be lower for one month due to a one-time income loss. The one-time income loss comes unexpectedly.

---

**A one-time income loss**

**Situation 1**

Consider a hypothetical situation where your household unexpectedly incurs a

**one-time income loss of \$100 today.**

How would this one-time income loss cause your household to change its spending and saving over the next three months?

**Note:** Your responses need to add up to \$100.

Enter \$0 if your household's spending/saving would not change.

Enter negative numbers for *increases* in your household's spending/saving.

**Decrease in spending**

(By how much) would your household decrease its monthly spending over the next three months?

\$

**Decrease in saving**

(By how much) would your household decrease its monthly saving (which includes decreases in debt repayment or increases in debt-taking) over the next three months?

\$

Total

\$

*Comment: Situation 2–7 are analogous. We randomize the order of shock sizes. Each respondent faces seven shocks: \$50, \$100, \$250, \$500, \$1,000, \$5,000, and \$10,000.*

## Deliberation study

The complete instructions are available online at <https://osf.io/2s7cf>. Below, we show the example screen for a \$50 income gain.

### A one-time payment

#### Situation 1

Consider a hypothetical situation where your household unexpectedly receives a

**one-time payment of \$50 today.**

How would this one-time extra income cause your household to change its spending and saving over the next three months?

**Note:** Your responses need to add up to \$50.

Enter \$0 if your household's spending/saving would not change.

Enter negative numbers for *decreases* in your household's spending/saving.

#### Increase in spending

(By how much) would your household increase its monthly spending over the next three months?

\$

#### Increase in saving

(By how much) would your household increase its monthly saving (which includes increases in debt repayment or decreases in debt-taking) over the next three months?

\$

Total \$

In response to the unexpected one-time payment of \$50 ...

On a scale from 1 (not at all) to 6 (very carefully), how carefully would your household consider how to change its spending and saving?

Would not consider it at all. 1	2	3	4	5	Would consider it very carefully. 6
------------------------------------	---	---	---	---	--

In response to the unexpected one-time payment of \$50 ...

What is the percent chance that you would discuss with other household members (like your partner) how your household should change its spending and saving?

0%	1-20%	21-40%	41-60%	61-80%	81-99%	100%
----	-------	--------	--------	--------	--------	------

In response to the unexpected one-time payment of \$50 ...

What is the percent chance that you would assess and consider your household's overall financial situation prior to deciding how to change your household's spending and saving?

0%	1-20%	21-40%	41-60%	61-80%	81-99%	100%
----	-------	--------	--------	--------	--------	------

### C.3 Definition of Additional Variables

**Age:** Age of the respondent.

**Education:** Highest education level of the respondent.

**Gender:** Gender of the respondent.

**Household size:** Size of the respondent’s household, capped at 10 to account for outliers.

**Income, annual:** Household income in 2022 before taxes and transfers.

**Income risk:** Households indicate whether their monthly household income varies by less than 5% (1), between 5% and 10% (2), between 10% and 25% (3), or by more than 25% (4). We derive a standardized index based on ordinal response (1–4).

**Monthly spending:** Household spending (in contrast to saving and debt repayment) in a typical month, capped at the 95% quantile to account for outliers.

**Region:** Census region.

**Wealth, Liquid:** The total value of a household’s financial savings and investments, such as cash holdings, checking and savings accounts, money market funds, government/municipal bonds or treasury bills, stocks and bonds in publicly held corporations, stock and bond mutual funds.

**Wealth, Illiquid:** The sum of (i) the total value of the land and real estate a household owns, including primary residence, second homes and other real estate, and (ii) the total value of a household’s currently non-withdrawable financial savings and investments, such as the value of your retirement accounts (401(k)s, IRAs, thrift accounts, and future pensions), the cash value of life insurance policies, certificates of deposit, and saving bonds.

**Wealth, Debt:** Total household debt including credit card debt, mortgages, and other debt, such as student loans, auto loans, and personal loans.

**Wealth, Net:** Liquid wealth + illiquid wealth – debt.

### C.4 Comparison of Cross-Sectional Results to Previous Work

In the cross-section of MPCs, our data replicate many patterns that are familiar to the literature. This appendix section compares our cross-sectional results to related work. It is important to keep in mind that we estimate households’ marginal propensity to consume over a three-month horizon in response to unexpected one-time income shocks and that our survey-based consumption measure includes both nondurable and durable consumption. This means we do not measure *notional consumption* as defined by [Laibson et al. \(2022\)](#) but *consumption expenditures*, which is common in the literature.

**High average MPCs.** The average MPC in our data is 0.47, but the comparison to other estimates becomes easier if we focus on the MPC to a larger income gain, *e.g.*, the \$1,000 shock, for which we estimate an MPC of 0.35 (Figure 1).

This estimate is within the range of typical estimates in the literature. Using a survey-based approach, Jappelli and Pistaferri (2014) estimate an MPC of 0.48 in Italy, Christelis et al. (2019) estimate an MPC of 0.39 in the Netherlands, Drescher et al. (2020) find MPCs ranging from 0.33 to 0.57 in 17 European countries over the first twelve months, and Colarieti et al. (2024) estimate an MPC of 0.16 over the first quarter, which continues to increase over subsequent months. An exception is Fuster et al. (2021) who observe an MPC of 0.07 for \$500, mainly because 74% of respondents report an MPC of 0. Studying consumption responses to the 2008 US tax rebate, Borusyak et al. (2024) and Orchard et al. (2025) estimate an MPC of 0.30, correcting earlier higher estimates by Parker et al. (2013) and Broda and Parker (2014). Estimates for the consumption response to the 2020 Economic Stimulus Payment in the US range from 0.08–0.028 (Parker et al., 2022), to 0.25–0.30 (Baker et al., 2023), or 40% (Coibion et al., 2020). In a randomized experiment, Boehm et al. (2025) observe a one-month MPC of 0.23 in response to an unanticipated 300 Euro transfer. Ganong et al. (2023) study responses to typical income shocks and find an MPC of 0.21 for nondurable consumption on a monthly basis and 0.29 on a quarterly basis. Fagereng et al. (2021) estimate a within-year MPC of around 0.50 out of lottery winnings. Kotsogiannis and Sakellaris (2025) estimate an MPC of 0.43 out of \$1,000 tax lottery winnings in Greece.

**MPCs decline for larger shocks.** MPCs decline with larger shock size. This has been observed, *e.g.*, by Kueng (2018), Fagereng et al. (2021), Colarieti et al. (2024), and Ganong et al. (2025). An exception are Fuster et al. (2021) who find that MPCs increase with shock size, though they also find a negative relationship on the intensive margin.

**MPCs are larger for losses.** An asymmetry between equally-sized gains and losses has been observed, *e.g.*, by Bunn et al. (2018), Christelis et al. (2019), Fuster et al. (2021), and Colarieti et al. (2024).

**Heterogeneity in MPCs.** MPCs vary widely in the cross-section of households (see, *e.g.*, Jappelli and Pistaferri, 2014; Lewis et al., 2024; Misra and Surico, 2014; Boehm et al., 2025; Fuster et al., 2021).

**Extreme MPCs of 0 or 1 are common.** Identifying extreme MPCs requires identifying MPCs on the household level. Due to the inherent noise in households' consumption processes, most studies only estimate and report average MPCs or average MPCs in a subgroup of the population. Here, survey-based methods are at an advantage because they can directly

elicit household-level MPCs from each respondent. These studies typically find many households who report an MPC of either 0 or 1, *e.g.*, [Drescher et al. \(2020\)](#) who use HFCS data from 17 European countries, [Andreou et al. \(2024\)](#) who work with the NielsenIQ Consumer Panel 2008 tax rebate survey in the US, [Coibion et al. \(2020\)](#) who study consumer responses to the 2020 Economic Stimulus Payment in the US, or [Jappelli and Pistaferri \(2020\)](#) who use survey data from Italy.

An exception among survey work is [Fuster et al. \(2021\)](#) who observe a large share of MPCs of 0 (74% for a \$500 gain) but few MPCs close to 1. If we had to speculate, we would attribute this difference to their two-stage response format and its sensitivity to response noise. Respondents are first asked whether they would increase, not change, or decrease their spending, debt repayment, or savings. Only then can households specify their precise responses. This means response noise strongly favors MPCs of 0 and below 0 (8% of respondents select an MPC below 0 for a \$500 gain), while it is hard to indicate an MPC of 1. This hypothesis is consistent with findings from an experiment by [Crossley et al. \(2023\)](#) that, within a common survey, randomized whether respondents were asked a two-part question (as in [Fuster et al., 2021](#)) or a one-part question (as in our study and others) to measure MPCs. The authors find that the two-part elicitation significantly decreases the probability of reported MPC greater than zero. For example, for the scenario of responding to a £2,500 gain over three months, 85% of respondents indicate an MPC  $> 0$  in response to the one-part question compared to only 25% in response to the two-part question.

Recent research has started to attempt inferring the distribution of MPCs in field settings. [Misra and Surico \(2014\)](#) estimate quantile consumption effects of the 2001 and 2008 US tax rebates and find that many households have an MPC close to 0, while a smaller group of households has MPCs close to 1. [Karger and Rajan \(2020\)](#) estimate consumption responses to the Covid stimulus payments. Their estimated individual-level distribution is noisy but exhibits spikes at 0 and 1 (Figure A11 and A12 in their paper). [Lewis et al. \(2024\)](#) estimate the latent distribution of MPCs in response to the 2008 US tax rebate, taking a parametric approach and assuming that there are three latent MPC types, which they estimate to have MPCs of 0.04, 0.23, and 1.33, respectively. However, their clustering approach is not designed to detect spikes in the distribution. [Boehm et al. \(2025\)](#) use a non-parametric approach to recover the distribution of MPC profiles from receiving an additional 300 Euro debit card in a randomized field. Although they do not detect exact mass points of zero and one, some smoothing of the density is inevitable due to measurement error and the kernel density deconvolution method that the authors employ to non-parametrically recover the CDF.

## C.5 Discussion of Response Noise

**Can noise falsely generate quick-fixing in our data?** To find out, we test whether quick-fixing can arise from chance. We focus on the prediction that quick-fixing households should transition to an interior MPC at most once and remain interior thereafter, while unclassified households should never report extreme MPCs.

In our data, 52% of households behave fully consistent with this hypothesis, 71% deviate from the one-switch pattern at most once, and 83% deviate at most twice.

In a randomization test, we derive the distribution of consistency levels under the null hypothesis that there is no link between MPCs, shock sizes, and valence. Specifically, we break any systematic quick-fixing transition in our data by reshuffling MPCs within households (separately for gains and losses, 10,000 permutations). On average, we find much lower consistency rates (29% full consistency, 39% deviate at most once, 53% deviate at most twice). Moreover, these low consistency levels vary little across permuted datasets due to the large sample size. Hence, the frequency of quick-fixing observed in our data is extremely unlikely to arise from chance.

**Can noise conceal quick-fixing in our data?** To find out, we test whether noise can obscure true quick-fixing in a simulation. We assume that households are truly quick-fixing but have “trembling hands” and erroneously report a wrong extensive margin response with probability  $\alpha=0.1$ . We set the share of each type in our simulation equal to the estimated type shares in our data and the distribution of switching points equal to the actually observed distribution of switching points in our data. This simulation, based on a modest amount of response noise, yields consistency rates that are comparable to our data: 67% deviate at most once; though only 29% behave fully consistently, much less than in our data, and 90% deviate at most twice, a bit more than in our data. The key takeaway of this analysis is that even modest levels of response error can explain inconsistencies in households’ responses across our 14 different MPC questions.

**Test-retest study.** We conduct an empirical “test-retest” study to gauge the likely response error directly. For 138 households recruited on Prolific, we elicit our full MPC module twice—a few hours apart. Unlike for other data collections reported in the paper, we missed the chance to preregister the test-retest study, though its objective should be clear nonetheless. Households report an identical extensive margin response to identical shocks in 82% of cases (random benchmark: 39%), and we assign the same household to the same type in 69% of cases (random benchmark: 24%). The random benchmark indicates which consistency level could be expected if we had randomly matched wave 1 and wave 2 responses.

Averaged across the three extensive margin responses (MPC of 0 or 1 or in interior),

weighted by their respective frequency, we find a test-retest correlation of 0.71. Averaged across the five types, weighted by their respective frequency, we find a test-retest correlation of 0.61. Hence, the test-retest correlations are close the test-retest correlations ranging from 0.71–0.86 reported for economic references like risk or time preferences in Falk et al. (2023).

## C.6 Additional Qualitative Evidence

**Sample and Design.** How do households explain their extreme MPCs for small shocks and their transition to interior MPCs for large shocks? We survey 502 additional US households and ask them for their consumption-savings responses to \$100 and \$1,000 income shocks. We recruit households with the survey company Prolific. The demographic characteristics of the sample are summarized in Table C.2.

57% of households adopt an extreme MPC for the \$100 shocks, but only 23% do so for the \$1,000 shocks. We ask respondents who switch from an extreme MPC to an interior MPC to explain why they do so. This qualitative approach complements our quantitative evidence and sheds light on why households prefer extreme MPCs for small shocks.

For example, households who report an MPC of 1 for a small income gain of \$100 but an MPC below 1 for an income gain of \$1,000 are asked:

[Q1] You responded that your household would not increase its saving in response to a \$100 one-time payment. You would spend everything.

Please explain why your household would spend everything and would not increase its saving.

[Q2] However, you responded that your household would increase its saving in response to a \$1,000 one-time payment.

Please explain why your household would respond differently in these two situations.

We ask analogous questions for households who report an MPC of 0 for the small income gain but an MPC above 0 for a large income gain. We also ask these questions for losses.

**Results.** We manually identify common themes in households' responses, develop a coding scheme, and assign each response to the themes it contains. Table C.3 provides an overview of the resulting coding scheme. We discuss the results below.

We focus on gains first. For gains, the coded text data reveal that almost all households (86%) explicitly refer to the contrast in shock size (\$100 versus \$1,000) when explaining their extreme MPC for the small shock or their transition from extreme MPCs to an interior MPC.

Many households view small shocks as insignificant and conveniently addressed with an extreme MPC of 0 or 1. The following respondent expresses it directly:

“One hundred bucks is not that much. It’s great, don’t get me wrong, but it’s something you either spend on a dinner or put away. Where we’re at right now, it’s going right in the bank.” (*MPC=0 for \$100, MPC=0.2 for \$1,000*)

By contrast, the large shock of \$1,000 is often described as a significant change to their household finances, and households realize that this requires a more balanced approach.

“Since the amount of \$1000 is fairly significant, and we are increasing our savings by a good amount, I think taking \$100 dollars out and saving the other \$900 is fair and feels rewarding from both a long-term and short-term perspective.” (*MPC=0 for \$100, MPC=0.1 for \$1,000*)

Why do households adopt extreme MPCs for small shocks? The reasons for this behavior can be multifaceted. For example, some households (16%) refer to **habits** or rules such as fixed spending budgets (leading to an MPC of 0) or saving targets (leading to an MPC of 1) that they do not want to overturn for small shocks.

“I have a budget for a reason and generally stick to it unless there are major changes.” (*MPC=0 for \$100, MPC=0.2 for \$1,000*)

“\$100 is not such a big amount that it will make me change my spending habits.” (*MPC=0 for \$100, MPC=0.1 for \$1,000*)

“My wife and I already contribute regularly to our savings.” (*MPC=1 for \$100, MPC=0.25 for \$1,000*)

Many households recognize the unexpected income as a welcome opportunity to **treat themselves**. Some households are ready to immediately spend the \$100 for themselves or their families, but they view it as “irresponsible” to not save a good part of the larger windfall.

“\$100 is not all that much when it comes down to it. It will cover one or maybe 2 utility bills. Why not just use the unexpected \$100 to spend on something you can enjoy or something that can help you in the short-term?” (*MPC=1 for \$100, MPC=0.75 for \$1,000*)

“The \$1,000 is a larger amount so I would be overindulging if I did spend it all and increased my spending instead of saving. I could have done a percentage and saved the \$100 but I felt like \$100 was an appropriate gift for myself. When the dollar amounts get much larger, the impact is much bigger if I don’t save anything.” (*MPC=1 for \$100, MPC=0.2 for \$1,000*)

Other households instead choose to maintain their **household discipline**. They seek to avoid “frivolous consumption” for small shocks, but, in case of a larger payment, they “feel comfortable” to treat themselves and spend a part of the larger income shock.

“\$100 is not a lot of money and since it came unexpectedly, I would put it in savings. That way I could use it later. I think if I spent the money now, it would be spent frivolously.” (*MPC=0 for \$100, MPC=0.5 for \$1,000*)

“An extra \$1000 feels like it is a lot more extra than an extra \$100. While I would still want to save the majority of it, it feels more comfortable to be able to use some of the larger sum of money for extra spending right now versus saving it.” (*MPC=0 for \$100, MPC=0.2 for \$1,000*)

We code 41% of households as talking about the desire to treat themselves and 17% as referring to household discipline. As illustrated above, the two arguments often occur together.

Another prominent argument that 25% of households express is that the \$100 would **not have any meaningful impact** if split between spending and saving. To avoid such a “drop in the bucket”, they choose an extreme, one-sided response. For example, the following household cannot think of a meaningful way to spend a small amount of money and hence opts to save the entire amount ( $MPC = 0$ ).

“Our bills are mostly covered and we do not have significant debt. This amount of money is not really large enough to make an impact on our spending. It would be put into our savings as we typically save extra money.” (*MPC=0 for \$100, MPC=0.3 for \$1,000*)

Other households make the opposite case, arguing that it “would not make a dent” in their savings if they save part of the \$100 ( $MPC = 1$ ), hence preferring to spend everything.

“It [\$100] is not enough money to make a real dent in any debt payments. We would use this money like a “treat” to go out to dinner or the movies.” (*MPC=1 for \$100, MPC=0.4 for \$1,000*)

“The \$100 is not really enough to move the needle in saving. It is a very small amount that spending it would actually provide more joy and benefit from it then it would to save.” (*MPC=1 for \$100, MPC=0.1 for \$1,000*)

The results are similar for losses where households who absorb the small \$100 loss with their savings do not view the loss as substantial enough to disrupt their regular spending habits. This strategy becomes infeasible or undesirable for the larger \$1,000 loss. Households who absorb the \$100 loss with their spending provide a mirror image. They do not want

**Table C.3:** Overview of the coding scheme

<b>Theme</b> (and detected freq.)	<b>Description</b>
<b>Gains</b>	
<i>199 cases where respondents choose an extreme MPC for small shock but not for large shock.</i>	
Shock size (86%)	Respondent mentions the difference in the shock sizes, e.g. contrasts the two shocks or says that \$100 is little or \$1000 a lot.
Habit (16%)	Respondent mentions that they generally try to save/spend in situations with small income gains.
Does not make a difference (25%)	Respondent mentions that spending/saving the money would not make a meaningful difference to their spending or savings.
Household discipline (17%)	<ul style="list-style-type: none"> <li>• MPC of 0 for \$100: Only in case of a larger amount, respondent feels comfortable to spend part of the amount, but they avoid “frivolous” spending for the small amount.</li> <li>• MPC of 1 for \$100: Respondent is fine with spending the small amount, but they argue it would be “irresponsible” to fully spend the larger amount.</li> </ul>
Treat oneself (41%)	<ul style="list-style-type: none"> <li>• MPC of 0 for \$100: Only in case of a larger amount, respondent wants to use a part to treat themselves.</li> <li>• MPC of 1 for \$100: Respondent wants to use the \$100 to treat themselves.</li> </ul>
Need (15%)	<ul style="list-style-type: none"> <li>• MPC of 0 for \$100: Respondent argues that they do not need additional purchases.</li> <li>• MPC of 1 for \$100: Respondent immediately needs the money for essential purchases.</li> </ul>
Lumpy consumption plans (6%)	Respondent has a specific spending plan or need, but \$100 is not yet enough to realize it.
<b>Losses</b>	
<i>184 cases where respondents choose an extreme MPC for small shock but not for large shock.</i>	
Shock size (84%)	Respondent mentions the difference in the shock sizes, e.g. contrasts the two shocks or says that \$100 is little or \$1000 a lot.
Habit (13%)	Respondent mentions that they generally try to cut saving/spending in situations with small income losses.
Buffer (49%)	<ul style="list-style-type: none"> <li>• MPC of 0 for 100: Respondent can easily draw on a buffer of savings.</li> <li>• MPC of 1 for 100: Respondent can easily cut discretionary, non-essential consumption.</li> </ul>
Balance required (34%)	Interior MPC for large loss because respondents do not want to or simply cannot afford to reduce their spending/savings by the full \$1000.
Budget already tight (8%)	Respondent reports having such a tight spending budget they prefer to not reduce spending any further in response to a \$100 loss.

to disrupt their savings routines and find it easy to cut back on discretionary expenses like dining out, entertainment, or non-essential purchases. But for the \$1,000 loss, households want or need to draw on their savings or even increase debt (through loans or credit) to manage this larger loss without cutting important expenditures.

Multiple factors appear to make extreme MPCs convenient solutions. First, households refer to habits and routines, *e.g.*, a fixed spending budget, a fixed monthly transfer to savings, or the goal to maximize savings, and deviating from such default rules could come at a cost. Second, for small shocks, extreme MPCs appear to be easier to imagine, evaluate, and appreciate. By contrast, interior MPCs lead to two small, seemingly imperceptible changes that are not perceived to “make a dent” in households’ savings or spending. Third, many households recognize an income gain as a welcome opportunity to treat themselves or their families. Most balance consumption and saving for the large shock, but they approach the smaller \$100 gain differently. Some conclude that they should “indulge” and spend everything, while others choose to maintain “discipline” and save everything. Of course, it seems plausible that further psychological forces are at work, which are harder for households to explicitly articulate. For example, finding a compromise between consumption and saving could require more computational effort.

Our model of quick-fixing captures the convenience of extreme MPCs for small shocks and the transition pattern from extreme to interior MPCs, thus providing a plausible representation of households’ introspection.

## References

- Akerlof, George A and Janet L Yellen**, “A near-rational model of the business cycle, with wage and price inertia,” *The Quarterly Journal of Economics*, 1985, 100 (Supplement), 823–838.
- Andreou, Elena, Maria Demetriadou, and Andreas Tryphonides**, “The Cross Section of Household Preferences: MPCs and the HtM Status,” *Working Paper*, 2024.
- Baker, Scott R., Robert A Farrokhnia, Steffen Meyer, Michaela Pagel, and Constantine Yannelis**, “Income, Liquidity, and the Consumption Response to the 2020 Economic Stimulus Payments,” *Review of Finance*, 2023, 27 (6), 2271–2304.
- Boehm, Johannes, Etienne Fize, and Xavier Jaravel**, “Five Facts about MPCs: Evidence from a Randomized Experiment,” *American Economic Review*, 2025, 115 (1), 1–42.
- Borusyak, Kirill, Xavier Jaravel, and Jann Spiess**, “Revisiting Event-Study Designs: Robust and Efficient Estimation,” *The Review of Economic Studies*, 2024, 91 (6), 3253–3285.
- Broda, Christian and Jonathan A. Parker**, “The Economic Stimulus Payments of 2008 and the aggregate demand for consumption,” *Journal of Monetary Economics*, 2014, 68, S20–S36.
- Bunn, Philip, Jeanne Le Roux, Kate Reinold, and Paolo Surico**, “The consumption response to positive and negative income shocks,” *Journal of Monetary Economics*, 2018, 96, 1–15.
- Christelis, Dimitris, Dimitris Georgarakos, Tullio Jappelli, Luigi Pistaferri, and Maarten Van Rooij**, “Asymmetric Consumption Effects of Transitory Income Shocks,” *The Economic Journal*, 2019, 129 (622), 2322–2341.

- Coibion, Olivier, Yuriy Gorodnichenko, and Michael Weber**, “How Did U.S. Consumers Use Their Stimulus Payments?,” Technical Report 27693, NBER 2020.
- Colarieti, Roberto, Pierfrancesco Mei, and Stefanie Stantcheva**, “The How and Why of Household Reactions to Income Shocks,” *Working Paper*, 2024.
- Crossley, Thomas F, Peter Levell, Hamish Low, and Paul Fisher**, “Eliciting the marginal propensity to consume in surveys,” *Understanding Society Innovation Panel Wave 15: Results from Methodological Experiments*, 2023.
- Drescher, Katharina, Pirmin Fessler, and Peter Lindner**, “Helicopter money in Europe: New evidence on the marginal propensity to consume across European households,” *Economics Letters*, 2020, 195, 109416.
- Fagereng, Andreas, Martin B. Holm, and Gisle J. Natvik**, “MPC Heterogeneity and Household Balance Sheets,” *American Economic Journal: Macroeconomics*, 2021, 13 (4), 1–54.
- Falk, Armin, Anke Becker, Thomas Dohmen, David Huffman, and Uwe Sunde**, “The Preference Survey Module: A Validated Instrument for Measuring Risk, Time, and Social Preferences,” *Management Science*, 2023, 69 (4), 1935–1950.
- Fuster, Andreas, Greg Kaplan, and Basit Zafar**, “What Would You Do with \$500? Spending Responses to Gains, Losses, News and Loans,” *Review of Economic Studies*, 2021, 88 (4), 1760–1795.
- Ganong, Peter, Damon Jones, Pascal Noel, and Daniel Sullivan**, “The Marginal Propensity to Consume out of Stimulus: Evidence From Administrative Bank Data,” *Working Paper*, 2025.
- , —, —, **Diana Farrell, Fiona Greig, and Chris Wheat**, “Wealth, Race, and Consumption Smoothing of Typical Labor Income Shocks,” *Working Paper*, 2023.
- Jappelli, Tullio and Luigi Pistaferri**, “Fiscal Policy and MPC Heterogeneity,” *American Economic Journal: Macroeconomics*, October 2014, 6 (4), 107–136.
- and —, “Reported MPC and Unobserved Heterogeneity,” *American Economic Journal: Economic Policy*, 2020, 12 (4), 275–297.
- Kaplan, Greg, Giovanni L Violante, and Justin Weidner**, “The wealthy hand-to-mouth,” *Brookings Papers on Economic Activity*, 2014, pp. 77–153.
- Karger, Ezra and Aastha Rajan**, “Heterogeneity in the Marginal Propensity to Consume: Evidence from Covid-19 Stimulus Pay,” Working Paper, FRB of Chicago 2020.
- Kotsogiannis, Christos and Plutarchos Sakellaris**, “MPCs Estimated from Tax Lottery, Survey and Administrative Data,” *Working Paper*, 2025.
- Kueng, Lorenz**, “Excess Sensitivity of High-Income Consumers,” *The Quarterly Journal of Economics*, 2018, 133 (4), 1693–1751.
- Laibson, David, Peter Maxted, and Benjamin Moll**, “A Simple Mapping from MPCs to MPXs,” Technical Report, Harvard University 2022.
- Lewis, Daniel J., Davide Melcangi, and Laura Pilossoph**, “Latent Heterogeneity in the Marginal Propensity to Consume,” Working Paper 32523, NBER 2024.
- Misra, Kanishka and Paolo Surico**, “Consumption, Income Changes, and Heterogeneity: Evidence from Two Fiscal Stimulus Programs,” *American Economic Journal: Macroeconomics*, 2014, 6 (4), 84–106.
- Orchard, Jacob D, Valerie A Ramey, and Johannes F Wieland**, “Micro MPCs and Macro Counterfactuals: The Case of the 2008 Rebates,” *The Quarterly Journal of Economics*, 2025, 140 (3), 2001–2052.
- Parker, Jonathan A, Nicholas S Souleles, David S Johnson, and Robert McClelland**, “Consumer Spending and the Economic Stimulus Payments of 2008,” *American Economic Review*, 2013, 103 (6), 2530–2553.

**Parker, Jonathan, Jake Schild, Laura Erhard, and David Johnson**, “Household Spending Responses to the Economic Impact Payments of 2020: Evidence from the Consumer Expenditure Survey,” *Working Paper*, 2022.