Linearity Bias in the Marginal Propensity To Spend and Macro Models*

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Abstract

This paper identifies a "linearity bias" in standard methods for estimating the average Marginal Propensity to Spend (MPX) from natural experiments. These methods typically yield a weighted average of individual spending responses, giving greater weight to households that receive larger income shocks. If the propensity to spend declines with the size of the shock, this weighting scheme can lead to a biased measure of the true unweighted average MPX and its systematic underestimation. The nature of this bias depends on the economic setting. For quasi-random shocks, such as lottery winnings, the bias is negative. The net effect is ambiguous if the shocks are correlated with household characteristics, as is the case with stimulus payments. By re-examining the 2008 U.S. Tax Rebate, I find evidence of a negative bias even in this context, with the spending response concentrated primarily on durable goods. I explain this large spending response using a heterogeneous-agent model featuring durable goods and endogenous financing. I show how targeted transfers enable liquidity-constrained households to finance large durable purchases with debt, generating a high contemporaneous MPX. This framework highlights a crucial intertemporal trade-off: the initial spending surge may come at the cost of future demand, a key consideration for the design of targeted fiscal stimulus.

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1 Introduction

How much do households spend out of an unexpected and transitory income shock? This question is central to designing effective fiscal stimulus, calibrating macroeconomic models, and understanding household behavior (Kaplan and Violante, 2022). A complete answer would require knowing the full distribution of the Marginal Propensity to Spend (MPX) —the fraction of an unexpected windfall that is spent— across both households and shock sizes.[1] This, however, is infeasible, as the unit-level MPX is fundamentally unobservable: it depends on the counterfactual of what a household would have spent absent the shock. Consequently, a vast empirical literature leverages natural experiments, in which households or individuals receive positive income shocks of varying sizes, to estimate the average MPX using reduced-form linear estimators.[2]

Under the appropriate identifying assumptions, these estimators recover a weighted average of the heterogeneous unit-level MPXs, assigning greater weight to households receiving larger income shocks. If households receiving a larger income shock tend to spend a smaller fraction of it—a prediction of canonical heterogeneous-agent models (Carroll and Kimball, 1996) and a feature of the data (Fagereng et al., 2021; Andre et al., 2025)—then these estimators can produce misleading results, often leading to an *underestimation* of the true average MPX.

This paper studies the direction and magnitude of the "linearity bias" that arises when the output of these estimators is interpreted as if all households had the same, constant MPX. I analyze the effect of this bias on two key quantities relevant for policymaking, for calibrating macroeconomic models, and for analyzing structural features of household expenditure : (i) the average unweighted MPX across households receiving heterogeneous transitory income shocks, and (ii) the counterfactual average MPX in response to a uniform shock equal to the average shock size. I combine simple econometric theory, empirical evidence, and quantitative modeling to argue that we may underestimate both quantities, depending on the type of one-time income shocks. After accounting for this bias, I find evidence of surprisingly large spending responses, which I then rationalize using a heterogeneous-agent model with durable goods and endogenous financing.

[1] In this paper, the marginal propensity to spend (MPX) is the unit-level change in total expenditure (i.e., the sum of the expenditure on services, durable goods, and nondurable goods) from a one-time, unanticipated positive income shock, divided by the shock size. The marginal propensity to consume (MPC) is defined similarly but excludes expenditure on durable goods. The average MPX or MPC refers to the population mean of these unit-level measures.

[2] These methods include direct estimation via Ordinary Least Squares (OLS) and Instrumental Variables (IV), as well as the construction of a Wald-type estimator by dividing the average change in expenditure by the average size of the shock, where the numerator is often estimated using a method like Difference-in-Differences (DiD).

My first contribution is to formally characterize and quantify this linearity bias in settings where income shocks are non-negative and can be assumed to be randomly assigned. The logic is as follows: widely used linear estimators, such as OLS, mechanically place greater weight on households that receive larger shocks. If the expenditure response is (on average) concave, meaning the MPX declines with shock size, this weighting scheme systematically pulls the estimate below the two key policy-relevant quantities defined above.

To assess the economic significance of this bias, I use the setting of Fagereng et al. (2021), who study the change in household total expenditure after a lottery win using Norwegian administrative data. They estimate an average annual MPX using a linear regression. Their preferred OLS estimate for the average MPX using the entire distribution of lottery winnings is 0.52. However, the authors also provide evidence that the MPX declines with the prize amount, particularly by showing that estimates are higher when the sample is restricted to smaller winnings. This suggests that full-sample linear estimate is a downward-biased measure of the true average propensity to spend.

To align the annual frequency of the lottery data with the quarterly frequency of my models, I simulate the distribution of winnings within two heterogeneous-agent models (one with and one without durables) by dividing each prize by four. Both models are calibrated at a quarterly frequency to match a low-liquidity economy with a target aggregate liquid asset-to-income ratio of 26% (Kaplan and Violante, 2022; Beraja and Zorzi, 2025).

In a standard model with only nondurable goods, the average MPX is 0.22. In contrast, the OLS estimate is just 0.09, underestimating the true average MPX by 59% and the MPX at the average shock (0.16) by 44%. This large negative bias stems from the concavity of the expenditure function, in which the MPX decreases sharply with shock size. The bias remains large even in a richer model that incorporates durable goods, despite the expenditure function being less concave (Beraja and Zorzi, 2025). In this second model, the average MPX is 0.36, whereas the OLS estimate is 0.21, underestimating the average MPX by 42% and the MPX at the average shock (0.32) by 34%. These findings demonstrate that linear estimators can severely understate the partial-equilibrium effects of fiscal transfers.

The analysis of the linearity bias is more complex when temporary income shocks are correlated with household characteristics. In this case, for the estimators I study, the bias for the average MPX is proportional to the covariance between unit-level MPX and the income shock. This single covariance term reflects the combined effect of underlying heterogeneity in MPXs and the specific assignment of shocks across households. Since the covariance is a function of which households receive a shock and the shock magnitude, the bias is no longer guaranteed to be negative and may even be positive. Consider, for example, government transfers such as stimulus checks. If policymakers successfully target households with a high propensity to spend (e.g., low-income households), this covariance term could be positive, potentially causing linear estimators to *overestimate* the average MPX.

A second contribution of this paper is to analyze this more complex case and to provide partial identification bounds for both the average MPX and the average MPX out of the average shock by combining the biased linear estimators and assuming that (on average) the expenditure or consumption function of the household facing the temporary shock is non-decreasing, and an *upper bound* on the average MPX conditional on the shock.[3]

I apply this framework to re-examine one of the most studied fiscal interventions: the 2008 U.S. Tax Rebate (e.g., Parker et al., 2013; Misra and Surico, 2014; Kaplan and Violante, 2014; Orchard et al., 2025) using the Consumer Expenditure Survey (CEX) data. My empirical analysis focuses on households receiving the rebate by mail, for whom the timing of the rebate was effectively randomized. This randomization allows for a clean comparison between "treatment" (households that have received the check) and a "control" group (households that have not yet received the check) within the same period (Parker et al., 2013). As a benchmark, I first estimate the weighted-average MPX, a central parameter identified in the analyses of Parker et al. (2013) and Orchard et al. (2025).[4] Similar to Parker et al. (2013), I find a large spending response concentrated in durable goods. Next, I use the biased linear estimates as an input to compute bounds. Under the plausible assumption that the average MPX conditional on the shock is bounded above by 2, the estimated bounds for these households imply a large average quarterly MPX, ranging from 0.33 to 1.26, with the majority of the response concentrated in durable goods. Moreover, the estimated upper bound for the average MPX out of the average shock is 0.91.

Furthermore, I find evidence that the covariance between the household-level MPX and the transfer amount is negative, implying that even in this setting, linear methods may underestimate the true average spending response. Exploiting the independence of the rebate size from the random treatment status, I directly estimate the average quarterly unweighted MPX with an imputation estimator. By moving away from standard linear estimators, the point estimates increase substantially, rising from 0.20 to 0.39 for nondurables, from 0.53 to 0.66 for durables, and from 0.73 to 1.05 for total expenditure in

[3]Manski and Pepper (2018) use a similar bounded-variation assumption to study the impact of right-to-carry gun laws in the United States.

[4]Orchard, Ramey, and Wieland (2025) refer to this parameter as the implied MPX.

my preferred specification.

To rationalize these empirical findings —specifically the large contemporaneous MPX, mostly concentrated in durables— I use a rich heterogeneous-agent model with durable goods. The model features a lumpy durable adjustment as in Beraja and Zorzi (2025), but it allows for a continuous choice of borrowing to finance durable purchases as in Murphy (2024) and Sciacovelli (2024).

Unlike the model by Beraja and Zorzi (2025), the borrowing amount for the purchase of a durable good is a continuous variable to better capture the fact that liquidity-constrained and low-income households will be more likely to finance a new durable good with debt, helping to generate a high durable MPX for this group. In fact, the model shows that poorer households would adjust their durable goods stock mostly through debt, while richer households would prefer to use their disposable liquidity. This could therefore lead to "excessive" borrowing by poorer households, which substantially increases *contemporaneous* expenditure at the expense of *future* expenditure, as their budget constraints become burdened by debt repayment.[5]

A partial equilibrium experiment illustrates this trade-off. A transfer, calibrated to the average 2008 U.S. Tax Rebate from my empirical sample, is targeted to the bottom third of the income distribution. For this targeted group, the yearly MPX surges to nearly 1.2 in the first year before falling below zero in the second and remains negative for 4 years. This translates to a very large surge in expenditure relative to the steady state in the first year, followed by a significant decline in the second. In contrast, a universal transfer of the same per-person amount given to all households, while spent largely in the first year (i.e. with an MPX of about 0.7), is either very close to zero or positive in subsequent years.

This dynamic provides a rationale for the large initial spending response observed in the 2008 data and offers a cautionary lesson for policymakers: stimulus policies that encourage debt-financed durable purchases may boost current spending at the cost of future demand.

Related literature This paper contributes to three strands of literature. First, it adds to the large empirical literature on the estimation of the average MPC and average MPX with non-structural models (e.g., Johnson et al., 2006; Parker et al., 2013; Kaplan and Violante, 2014; Fagereng et al., 2021; Golosov et al., 2023; Orchard et al., 2025; Boehm et al., 2025). It does so by identifying a specific, quantitatively important bias in standard linear

[5] McKay and Wieland (2021) identify a similar intertemporal trade-off in aggregate demand following an expansionary monetary policy in a model with durables that nonetheless generates a low average marginal propensity to spend. Mian et al. (2021) investigates a similar mechanism after an accommodating monetary policy in a model with *borrowers* and *savers*, without a durable good.

methods and characterizing its properties by applying plausible assumptions on the shape of the expenditure response function, drawing on insights from the partial identification literature (Manski, 1997; Manski and Pepper, 2018). Moreover, by revisiting the 2008 U.S. Tax Rebate, I provide evidence of a large average MPX for households receiving the payment by mail, especially for durable goods. This result contributes to the debate on the effects of this stimulus program by diverging from the recent findings of Borusyak et al. (2024); Orchard et al. (2025) while corroborating the original analysis by Parker et al. (2013).

Second, it connects to the econometrics literature on the limitations of reduced-form estimators under treatment effect heterogeneity for estimating causal or structural parameters (see, e.g. Heckman and Vytlacil (2005); Mogstad, Santos, and Torgovitsky (2018); Słoczyński (2022); Callaway, Goodman-Bacon, and Sant'Anna (2024)). However, its specific focus on marginal propensity to spend allows for a better characterization of the bias using assumptions rooted in modern macroeconomic models.

This paper contributes to a recent literature on heterogeneous-agent models with durable goods. In this setting, Berger and Vavra (2015) study the differential response of aggregate durable expenditures across the business cycle. McKay and Wieland (2021) focus on the intertemporal trade-off driven by durable goods of the aggregate demand after an expansionary monetary policy. However, a key distinction is that their mechanism operates in an environment with a low average MPX. Gavazza and Lanteri (2021) focus on the effect of aggregate credit shocks on durables. In the paper by Sciacovelli (2024), the durable good represents the housing stock. In this case, the analysis is centered on the effect of monetary policy when households have adjustable-rate mortgages. Murphy (2024) highlights how liquidity constraints influence consumers' loan length decisions for car purchases, as liquidity-constrained consumers prefer longer loan terms. Following Murphy (2024); Sciacovelli (2024), my model expands the work of Beraja and Zorzi (2025) by treating the borrowing amount as a continuous variable. This approach more accurately captures the interplay between liquidity constraints and financing decisions for durable goods purchases, thereby generating a high MPX among liquidity-constrained households and revealing significant intertemporal trade-offs of targeted stimulus policies. Moreover, it contributes to this literature by incorporating insights from heterogeneous-agent macroeconomic models into reduced-form econometric analysis to sharpen the interpretation of empirical results about the average marginal propensity to spend.

2 The Linearity Bias

Heterogeneity in the MPX and MPC is a central feature of modern macroeconomics, appearing as both a robust empirical finding and a core feature of heterogeneous-agent models. In the data, significant heterogeneity is documented across various settings, including survey responses (Jappelli and Pistaferri, 2014; Fuster et al., 2020; Andre et al., 2025), randomized controlled trials (Boehm et al., 2025), and quasi-experimental evidence (Misra and Surico, 2014; Lewis et al., 2024; Ganong et al., 2025).[6]

In heterogeneous-agent models, this heterogeneity arises from multiple sources. For a baseline model with only a nondurable good, these include differences in discount factors, the presence of borrowing constraints, idiosyncratic income processes, and holdings of illiquid assets. The introduction of a durable good further amplifies this heterogeneity because of the additional state variables of the stock of durables, debt position, and the presence of adjustment costs.

An addition source of heterogeneity lies in the nature of the one-time income shock affecting consumers. A standard result from heterogeneous agent models with nondurables, is that the consumption function is concave in liquid wealth (Carroll and Kimball, 1996). Therefore, in the model, the average consumption response will also be concave with respect to the size of the shock, provided the shock is independent of all other variables affecting consumption. This implies that the marginal propensity to consume (MPC) decreases as the size of the shock increases. This is not necessarily the case if the shock depends on other characteristics of the consumers that also affect consumption. For example, a targeted shock, such as a government transfer that targets low-income consumers, can produce MPCs that are not necessarily decreasing with the size of the shock. Moreover, the recent work by Beraja and Zorzi (2025) show that even a model with durables can feature a concave (average) expenditure function when the probability of adjusting the durable good is influenced by additive taste shocks à la McFadden (1972).

These multiple sources of heterogeneity pose a fundamental challenge for estimating the average MPX, interpreting estimates from reduced-form estimators, and using such estimates to calibrate macroeconomic models. In practice, while the MPX may be treated as a fixed parameter, its value —both in natural experiments and in heterogeneous-agent models— depends critically on two dimensions: *who* receives the shock and the *size* of the shock. A direct consequence is that assuming a model in which all households have the same linear response to one-time income shocks is inappropriate for estimating the MPX.

[6]For example, the reported MPC in the survey by Jappelli and Pistaferri (2014) is by construction between 0 and 1, with a sample average of 0.476. The in-sample standard deviation of the reported MPC (0.357) is approximately 70% of the *maximum* possible in-sample standard deviation, $\sqrt{(1-0.476)(0.476)} \approx 0.499$.

To analyze this challenge formally, let the *expenditure response function* to a one-time income shock be defined (either for households or individuals) in general terms as

$$c = g(x, u). \tag{1}$$

Focusing on a household response, x denotes the size of the income shock and u captures the household's characteristics, including preferences, income, net-worth, financial constraints, and other relevant attributes. Such expenditure can be defined for nondurables and services, durables, or the sum of those (total expenditure). The function $g(\cdot)$ is left unspecified, allowing for arbitrary heterogeneity in expenditure responses.[7]

The object of primary interest is the marginal propensity to spend (MPX), which measures the share of the shock that translates into increased expenditure. This is defined as:

$$MPX = \frac{\Delta c}{\Delta x} = \frac{g(x,u) - g(0,u)}{x}. \tag{2}$$

This definition emphasizes that the MPX represents the household's expenditure response relative to the counterfactual of receiving no shock.[8]

A critical feature of this framework is that it permits the MPX to vary freely across both households and shock amounts. Different households may respond differently to identical shocks due to heterogeneity in their characteristics u. Moreover, a given household may exhibit non-constant MPX, responding differently to small versus large shocks. This flexibility is essential for capturing the rich heterogeneity observed in empirical consumption data.

This section analyzes the estimation of the average MPX, E[MPX], in the common empirical setting where reduced-form estimators are applied to natural experiments involving non-negative, heterogeneous income shocks ($X \geq 0$).[9] The core challenge in this setting arises when the true expenditure response, $g(\cdot)$, is non-linear and heterogeneous. In fact, a substantial econometric literature has demonstrated that reduced-form estimators—including ordinary least squares (OLS), instrumental variables (IV), and difference-in-differences—can be represented as weighted averages of heterogeneous marginal responses under the right assumptions (e.g., Yitzhaki, 1996; Angrist et al., 2000;

[7] In the canonical one-account heterogenous-agent models with non-durable consumption, $g(\cdot)$ would be the consumption *policy function*: a function of current income y and liquid assets a, i.e. $c = g(a, y)$. The one-time income shock x enters additively as $c = g(a + x, y)$.

[8] This definition treats the vector of household characteristics u as fixed with respect to the shock x. An alternative would allow u to vary with the shock, yielding $MPX' = (g(x, u(x)) - g(0, u(x)))/x$. The former definition is chosen because it facilitates a more direct comparison with standard heterogeneous-agent models and provides a clearer interpretation of the MPX.

[9] Throughout the paper, variables in capital letters denote random variables.

Table 1: Weighting Properties of Reduced-Form Estimators for the MPX

Estimator	**Formula**	**Weight** w
Panel A: Independent Shocks ($X \perp\!\!\!\perp U$)		
OLS	$\dfrac{\mathrm{Cov}(C, X)}{\mathrm{Var}(X)}$	$\dfrac{X(X - \mathrm{E}[X])}{\mathrm{Var}(X)}$
IV / DiD	$\dfrac{\mathrm{E}[\Delta C \mid X > 0]}{\mathrm{E}[X \mid X > 0]}$	$\dfrac{X}{\mathrm{E}[X \mid X > 0]}$
Panel B: Dependent Shocks ($X \not\perp\!\!\!\perp U$)		
IV / DiD	$\dfrac{\mathrm{E}[\Delta C \mid X > 0]}{\mathrm{E}[X \mid X > 0]}$	$\dfrac{X}{\mathrm{E}[X \mid X > 0]}$

Notes: This table presents the weighting schemes implied by common reduced-form estimators when estimating the average MPX for a non-negative income shock ($X \geq 0$). Panel A shows results for shocks independent of household characteristics (e.g., lottery winnings), while Panel B shows results for shocks correlated with characteristics (e.g., tax rebates). OLS is omitted from Panel B because it does not produce a weighted average of unit-level MPXs under dependent shocks.

Heckman and Vytlacil, 2005; Mogstad et al., 2018; Słoczyński, 2022; Callaway et al., 2024).

Applying these insights to the estimation of the average MPX, I focus on a class of estimators that, under the appropriate identifying assumptions, produce a weighted average of the individual MPX, $\mathrm{E}[\mathrm{MPX} \cdot W]$, where the weights W are a function of the shock X. While the specific form of the weights depends on the estimator, a common pattern emerges: households receiving larger shocks are systematically given greater weight in the estimation.

This weighting scheme is problematic for interpretation. The resulting estimate can be interpreted as the true average MPX, $\mathrm{E}[\mathrm{MPX}]$, only under the strong and counterfactual assumption that the MPX is constant across all households and shock sizes. I term the resulting gap between the estimator's output and the true average MPX the *linearity bias*. This bias is not a failure of the estimator itself, but an error of interpretation. This distinction is critical, as such a misinterpretation can lead to significant errors when these estimates are used for fiscal policy counterfactuals or to calibrate macroeconomic models.

Table 1 summarizes the estimators that I study in this paper having this representation, depending on the type of the shock: if the shocks can be assumed as-if they randomly assigned ($X \perp\!\!\!\perp U$) or if the shocks depend on the affect unit characteristics ($X \not\perp\!\!\!\perp U$). The first type consists of shocks that are independent of household characteristics, such as lottery winnings, (e.g., Fagereng et al., 2021). The second type comprises shocks that depend on household characteristics like income, exemplified by tax rebates (e.g., Parker et al., 2013).

In the case of independent shocks ($X \perp\!\!\!\perp U$), both OLS and Wald-type estimators $\left(\frac{\mathrm{E}[\Delta C|X>0]}{\mathrm{E}[X|X>0]}\right)$ recover a weighted average of the MPX. While both estimators place greater emphasis on larger shocks, their weighting schemes differ. The OLS weights are complex and not guaranteed to be positive, whereas the weights for the Wald-type estimator are non-negative and increasing in the shock size. This latter form is particularly common and can be constructed in two steps (e.g., in a DiD design by separately identifying the numerator and denominator) or recovered in a single step through an IV regression using a discrete instrument Z independent of U and that satisfies $Z = 1 \implies X > 0$ and $Z = 0 \implies X = 0$ (e.g., $Z = 1_{X>0}$).[10]

For dependent shocks ($X \not\perp\!\!\!\perp U$), the set of applicable estimators narrows. The Wald-type estimator remains central to the analysis. It can still be constructed via a two-step DiD specification or a one-step IV design, provided one has an instrument Z that is independent of U and again satisfies the conditions $Z = 1 \implies X > 0$ and $Z = 0 \implies X = 0$. Critically, it retains its key property: it recovers a weighted average of the MPX where the weights are increasing in the shock size. The OLS estimator, in contrast, is omitted from this case, as it is no longer yields a weighted average of unit-level MPXs.

To gain intuition about the linearity bias, consider the following separable model of the household response to shocks using the notation of equation (1)

$$c = g(x, u) = m(x) + h(u), \tag{3}$$

where $m(x)$ is a *nonlinear* function that governs the response to the shock, with $m(0) = 0$. The MPX is then defined, using equation (2) as

$$MPX = \frac{m(x)}{x}, \tag{4}$$

thus all the properties about the unit-level MPX depend on the shape of the function $m(\cdot)$. In this model, the expenditure response to the shocks is the same for every household except for the *shifter* function $h(u)$. Since $g(0, u) = h(u)$, represents expenditure absent any shock, the shifter$h(u)$ captures baseline consumption differences across households

[10]For example, the two-step approach would be necessary if, due to data limitations, the researcher knows only whether a unit was treated or not ($X_i > 0$ versus $X_i = 0$), but does not observe the individual shock amount X_i. In such a scenario, the average shock size for the treated group, $\mathrm{E}[X|X > 0]$, must be obtained from other sources, such as the policy design or administrative records.

driven for example, by income, liquid assets, and preferences, all contained in the vector u.

Suppose we observe a shock $X \geq 0$ that is independent of all household characteristics U (i.e., $X \perp\!\!\!\perp U$) and is continuously distributed. The OLS estimator in this case is simply

$$\begin{aligned} \beta^{OLS} &= \frac{\text{Cov}(m(X) + h(U), X)}{\text{Var}(X)} = \frac{\text{Cov}(m(X) + h(U), X)}{\text{Var}(X)} \\ &= \text{E}\left[\frac{m(X)}{X} \frac{X - E[X]}{Var(X)} | X > 0\right] = \text{E}\left[MPX\left(\frac{X - E[X]}{Var(X)}\right) | X > 0\right], \end{aligned} \tag{5}$$

where $\text{Cov}(h(U), X) = 0$ by the independence assumption.

The complex weighting scheme of this estimator complicates its application to policy analysis. Given the assumption of an independent shock ($X \perp\!\!\!\perp U$), the relevant partial-equilibrium counterfactual is a policy that is also untargeted, namely, a uniform transfer. Such a policy could involve direct payments of the same amount to every household or a fiscal expansion that raises all household incomes by a fixed amount.[11] This fundamental mismatch between the estimator and the uniform counterfactual means the OLS estimate may not be directly informative about the average response to shocks and may be uninformative for policy counterfactuals and model calibration.[12]

The magnitude of this bias depends on the interaction between the curvature of the response function, $m(\cdot)$, and the distribution of the shocks, X. Intuitively, the problem is most severe when the function $m(\cdot)$ is highly nonlinear and the shocks are highly heterogeneous, as this creates substantial variation in the MPXs that OLS will disproportionately weight.

2.1 The Shock X is independent of U

Figure 1 illustrates this example. Moreover, because of the concavity, we have that equation (**??**) can severely underestimate the average derivative of consumption.

Suppose we observe for each unit the following vector (c_i, d_i, w_i), where the vector of covariates w can be a subset of U, although $w \neq u$ as by assumption we do not know all the variables affecting consumption and some of them are unobservable. In this section, we assume that the shock $X \perp\!\!\!\perp (U, W)$. Moreover, as it is more common in practice, the

[11] We could also be interested in some weighted averages of the MPXs to run policy counterfactuals, such as income-weighted average as in Auclert et al. (2024), that however are not immediately recovered by linear regressions.

[12] Formally, the policy estimand of interest is the average causal effect of a uniform transfer of size $\bar{x}$ across the population, $\mathbb{E}[g(\bar{x}, U) - g(0, U)]$, which for equation (3) simplifies to $m(\bar{x})$. In contrast, the OLS-based prediction for the average effect is $\beta^{OLS} \times \bar{x}$. The resulting bias, $(\beta^{OLS} \times \bar{x}) - m(\bar{x})$, arises precisely because β^{OLS} is a weighted average of the MPX over the observed shock distribution and is not in general equal to the specific MPX relevant for the uniform policy, $m(\bar{x})/\bar{x}$.

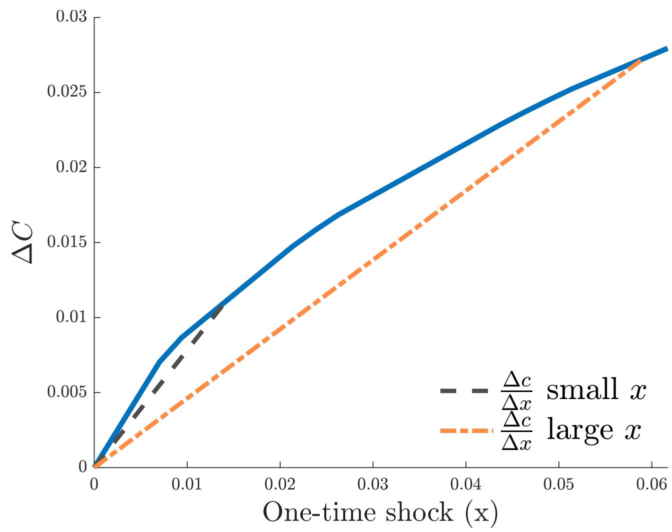

Figure 1: Differences in MPX for a concave (average) consumption function.

Notes: Difference in the MPX as a function of a random shock x.

shock has non-negative support, $X \geq 0$, and its distribution has a mass point at zero, that for some units $x_i = 0$. Therefore, I will focus on this case.

Suppose further we regress c_i over a constant and (d_i, w_i). At the population level, the coefficient of the variable X, β^{OLS}, will be a weighted average of MPC, with weights proportional to X.[13]

Proposition 1. *Let* $\frac{\Delta C}{\Delta X} = \frac{g(X,U)-g(0,U)}{X}$. *The OLS regression coefficient is*

$$\beta^{OLS} = \frac{\mathrm{E}\left[\frac{\Delta C}{\Delta X} X(X - \mathrm{E}[X])p | X > 0\right]}{\mathrm{Var}(X)}$$

if $X \perp\!\!\!\perp (U, W)$, $X \geq 0$ *and* X *has a mass point in zero such that* $p = Pr(X > 0) \in (0, 1)$.

Proof. See appendix B.1 □

If we let $\omega(X) = \frac{pX(X-\mathrm{E}[X])}{\mathrm{Var}(X)}$, we can see that the OLS coefficient is a weighted average of the individual MPC, with weights $\omega(X)$ such that $\mathrm{E}[\omega(X)] = 1$ that are negative for those that have $X \leq \mathrm{E}[X]$.[14] Thus, this weighted average is not particularly attractive, unless the MPX is (the same) linear function of the shock. In that case, the OLS regression recovers the average MPX.

[13] Fagereng, Holm, and Natvik (2021) have a similar decomposition in their appendix in terms of expenditure of unit i receiving lottery winning i.

[14] This representation of the OLS coefficient holds even when we have a panel and use in the regression of the first difference of consumption $(c_{it} - c_{it-1})$ instead of the level, since given $X_t \perp\!\!\!\perp (W_{t-1}, U_{t-1})$ we have $\mathrm{E}[C_{t-1}(X_t - \mathrm{E}[X_t])] = 0$.

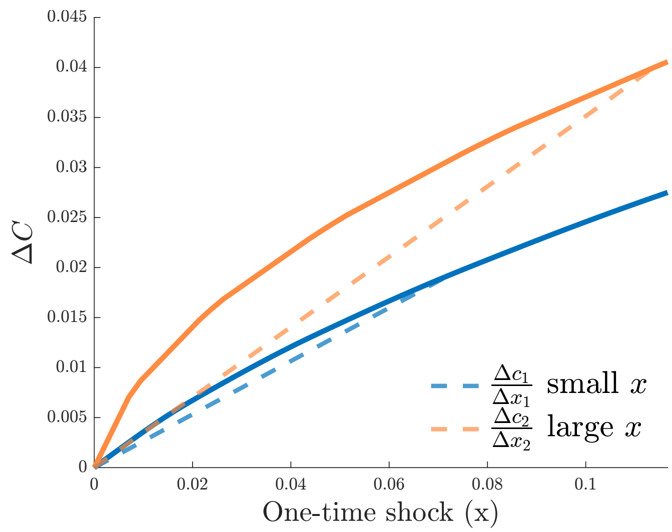

Figure 2: Differences in MPX after a non-random shock.

Notes: Difference in the MPX as a function of a (non-random)shock x, resulting in a positive covariance between the shock and the individual MPX.

The OLS coefficient produces this average because the distribution of the shock X skews the average. We do not have this problem if we run a regression when X can only have two values, that is $X \in \{0, x\}$.[15]

It is easily seen that the bias of the OLS estimator for the average MPX is the following

Corollary 1. *The OLS regression coefficient is a weighted average of the unit-level MPX(X)*

$$\beta^{OLS} = \mathrm{E}[MPX\omega(X)|X > 0] = \mathrm{E}[MPC(X)|X > 0] + \mathrm{Cov}(MPX(X), \omega(X)|X > 0),$$

where $\omega(X) = \frac{pX(X-\mathrm{E}[X])}{\mathrm{Var}(X)}$*, and* $\mathrm{Cov}(MPX(X), \omega(X)|X > 0)$ *is the bias.*

The bias thus depends on the relationship between the MPC and the weight $\omega(X)$.[16]

2.2 The Shock X is not independent of U

Figure 2 illustrates this example.

If the shock X is not independent of U, it is the case that units are *selected* into the "treatment" X. For example, suppose X is a targeted government transfer. Thus, in case

[15]This would be the case of a randomized controlled trial, in which only the randomly treated units receive the same amount x. In this case, the OLS coefficient effectively recovers $\mathrm{E}\left[\frac{\Delta C}{\Delta x}\right] = \mathrm{E}\left[\frac{g(x,u)-g(0,u)}{x}\right]$. Boehm et al. (2025) is an example of this case.

[16]Yitzhaki (1996) presents a representation of the coefficient β^{OLS} as a weighted average of the *derivative* of the expectation function, with positive weights that integrate to one.

we may be able to estimate an average MPC, this would be of the following form

$$\mathrm{E}\left[\tilde{MPX}(X)\right] = \mathrm{E}\left[\frac{g(X,U)-g(0,U)}{X}\right] = \int_0^\infty \mathrm{E}\left[\frac{g(d,U)-g(0,U)}{d}|X=d\right] \d{F}(d). \tag{6}$$

Compared to the expectation in (**??**), since $X \not\perp\!\!\!\perp U$, we have that $\mathrm{E}\left[\frac{g(x,U)-g(0,U)}{x}|X=x\right] \neq \mathrm{E}\left[\frac{g(x,U)-g(0,U)}{x}\right]$.

Depending on the context and the assumptions we make, Diff-in-Diff methods (Callaway et al., 2024; Borusyak et al., 2024; Orchard et al., 2025) or IV methods have been used to estimate the average MPX in this setting.

For instance, $\mathrm{E}\left[\tilde{MPC}(X)\right]$ has been approximated with the so-called implied MPC (τ) (Orchard et al., 2025), the (sample equivalent) of the ratio of the ATT and the average size of the (positive) shock

$$\tau = \frac{\mathrm{E}\left[g(X,U)-g(0,U)|X>0\right]}{\mathrm{E}[X|X>0]}. \tag{7}$$

Using an IV estimator, under the assumption that we have an instrument Z such that $Z \perp\!\!\!\perp U$ and $\mathrm{Cov}(Z,X) \neq 0$[17]

$$\beta^{IV} = \frac{\mathrm{Cov}(C,Z)}{\mathrm{Cov}(X,Z)} = \frac{\mathrm{E}[g(X,U)(Z-\mathrm{E}[Z])]}{\mathrm{E}[X(Z-\mathrm{E}[Z])]}. \tag{8}$$

Similarly to the previous section, I will decompose these estimators, highlighting how they are weighted averages of MPX(X), and the correspondent bias term.[18]

Starting from the implied MPX, we have the following result.

Proposition 2 (Implied MPX)**.** *Let* $\frac{\Delta C}{\Delta X} = \frac{g(X,U)-g(0,U)}{X}$. *Suppose* $X \geq 0$. *Then, the implied MPC* τ *can be decomposed as*

$$\tau = \frac{\mathrm{E}\left[\frac{\Delta C}{\Delta X}X|X>0\right]}{\mathrm{E}[X|X>0]} = \mathrm{E}\left[\frac{\Delta C}{\Delta X}\omega^{\tau}(X)\right]$$

with weights $\omega^{\tau}(X) = \frac{X}{\mathrm{E}[X|X>0]} > 0$ *and* $\mathrm{E}[\omega^{\tau}(X)] = 1$. *Moreover,*

$$\tau = \mathrm{E}\left[\frac{\Delta C}{\Delta X}|X>0\right] + \frac{\mathrm{Cov}(\frac{\Delta C}{\Delta X}, X|X>0)}{\mathrm{E}[X|X>0]},$$

[17]The first condition is a stronger form of the *exogeneity* condition neded whenever we assume a linear model for the dependent variable, while the second is the standard *relevance* condition.

[18]Similar decompositions can be found in Angrist et al. (2000).

where $\frac{\mathrm{Cov}(\frac{\Delta C}{\Delta X}, X|X>0)}{\mathrm{E}[X|X>0]}$ *is the bias.*

Proof. It follows immediately after dividing and multiplying the numerator of τ by X. $\square$

Similarly, we can also rewrite the IV estimator as a weighted average of MPC(X). However, without restrictions on the instrument Z and the relationship between X and Z, the weights can be negative.

Proposition 3 (IV Estimator)**.** *Let* $\frac{\Delta C}{\Delta X} = \frac{g(X,U)-g(0,U)}{X}$. *Suppose* $X \geq 0$. *Moreover, suppose the instrument* Z *is such that* $Z \perp\!\!\!\perp U$. *Then, the IV estimator* β^{IV} *can be decomposed as*

$$\beta^{IV} = \frac{\mathrm{E}\left[\frac{\Delta C}{\Delta X} X(Z - \mathrm{E}[Z])\right]}{\mathrm{E}[X(Z - \mathrm{E}[Z])]} = \mathrm{E}\left[\frac{\Delta C}{\Delta X}\omega^{IV}(X)\right]$$

with weights $\omega^{IV}(X) = \frac{X(Z-\mathrm{E}[Z])}{\mathrm{E}[X(Z-\mathrm{E}[Z])}$ *and* $\mathrm{E}[\omega^{IV}(X)] = 1$. *Moreover,*

$$\beta^{IV} = \mathrm{E}\left[\frac{\Delta C}{\Delta X}\right] + \frac{\mathrm{Cov}(\frac{\Delta C}{\Delta X}, XZ)}{\mathrm{E}[X(Z - \mathrm{E}[Z])]},$$

where $\frac{\mathrm{Cov}(\frac{\Delta C}{\Delta X}, XZ)}{\mathrm{E}[X(Z-\mathrm{E}[Z])]}$ *is the bias.*

Proof. It follows from the fact that $Z \perp\!\!\!\perp U$ implies $\mathrm{E}[g(0,U)(Z - \mathrm{E}[Z]]) = 0$, and by dividing and multiplying the numerator of β^{IV} by X. $\square$

Finally, we can use a standard result about the LATE estimator (Angrist and Imbens, 1995) Assuming the instrument is binary, $Z \in \{0,1\}$, and that $z = 0 \Rightarrow d = 0$, the IV estimator is equivalent to the implied MPC. This is particularly the case of the extensively studied 2008 US tax rebate.

2.3 Concavity and Monotonicity Assumptions

A standard result of the canonical HA macro model is that the (non-durable) consumption function is concave and increasing (Carroll and Kimball, 1996). In this section and the following section, it seems natural to use these two assumptions whenever we try to estimate the average MPC from data for two reasons: first, to have consistency between theory and empirical work; second, because we can sharpen some empirical results while maintaining these two assumptions.

Using the notation from before, let $C = g(X,U)$ be the spending function of a unit. I will now list the assumptions.

Assumption 1 (Average Monotonicity)**.** *Let $x', x^{''} \geq 0$, then for all x*

$$E[g(x'', U)|X = x] \geq E[g(x', U)|X = x]$$

whenever $x'' \geq x'$.

Assumption 1 means that, on average, units affected by a shock $X = x$ would have spent more if they were to receive a larger shock, and less if they were to receive a smaller shock. Moreover, Assumption 1 implies that the MPX is a non-negative random variable. That is, assuming $X > 0$,

$$\mathrm{E}\left[\frac{\Delta C}{\Delta X}|X = x\right] = \mathrm{E}\left[\frac{g(x, U) - g(0, U)}{x}|X = x\right] \geq 0.$$

Assumption 2 (Average Concavity)**.** *Let $x' \in \mathbb{R}$ and U be a random vector.*

$$\mathrm{E}[g(x', U)|X = x]$$

is concave in x' for all x.

A sufficient, but not necessary, condition for Assumption 2 is that any given unit has a concave spending function, that is $g(x, u)$ is concave in x for every $u \in U$. This is the case in the canonical HA model with a nondurable good. When we add a durable good, this is not necessarily the case if units adjust the durable good infrequently. Thus, I use a weaker assumption that holds on average. The concavity and monotonicity assumptions on the individual response function to a treatment were used by Manski (1997) to partially identify quantities related to the distribution of responses, such as the average treatment effect. I will be using the above assumptions to derive bounds on the asymptotic limits of some commonly used estimators for the average MPX.

Under these assumptions, in the case of an independent shock the OLS estimator is a lower bound for the average unweighted MPX.

Proposition 4. *Suppose $X \perp\!\!\!\perp (U, W)$, $X \geq 0$ and $Pr(X > 0) = p < 1$. Under assumptions 1 and 2, the OLS regression coefficient β^{OLS} is a lower bound for the average MPX. In particular*

$$\beta^{OLS} \leq \mathrm{E}\left[\frac{\Delta C}{\Delta X}|X > 0\right]$$

Proof. See appendix B.2. □

Proposition 5. *Suppose* $X \perp\!\!\!\perp (U, W)$*,* $X \geq 0$ *and* $Pr(X > 0) = p < 1$*. Let* $\bar{X} = \mathrm{E}[X|X > 0]$*. Under assumptions 1 and 2, the OLS regression coefficient* β^{OLS} *is a lower bound for the counterfactual MPX if everybody received a shock equal to* $\bar{X}$*:*

$$\beta^{OLS} \leq \mathrm{E}\left[\frac{g(\bar{X}, U) - g(0, U)}{\bar{X}}\right]$$

Proof. See appendix B.3. □

Similarly, since by assumption 2 the covariance between the MPC and X is negative, the implied MPC estimator is downward biased too.

If the shock is D not independent of U, we cannot conclude that the β^{IV} or τ estimators are lower bounds for the average MPC using assumptions 1 and 2. Unlike the previous case, where the covariance term was necessarily negative due to concavity, could be either positive or negative. This means that researchers would need to make specific assumptions about the sign of the covariance term to draw definitive conclusions.

Given the form of the bias term, is it still possible to use those estimators to compute bounds for average MPX by making the assumption that the MPX is bounded, other than non-negative:

Assumption 3 (Boundedness)**.** *Let* $x \geq 0$*. For every* $u \in \mathrm{U}$*, the MPC is bounded on average*

$$\mathrm{E}\left[\frac{\Delta c}{\Delta x}|X = x\right] = \mathrm{E}\left[\frac{g(x, U) - g(0, U)}{x}|X = x\right] \leq U.$$

This assumption applies to the average spending response for the group implicitly defined by the income shock, that is the group receiving an income shock $X = x$. A stronger alternative would be to impose an upper bound on the *individual* spending function, $g(x, u)$. For nondurable goods, a natural upper bound is $U = 1$. However, this upper bound becomes restrictive once durable goods are included, as their lumpy nature and the ability to finance purchases with debt mean that an individual's expenditure can exceed their income shock. For this reason, I use the weaker assumption on the average response.

By using assumption 3, we can bound the covariance term using the Cauchy-Schwarz inequality. In fact, although this requires the standard deviation of the MPX, which is unobserved, we can circumvent this issue by bounding its variance. For any random variable X bounded in an interval $[a, b]$, its variance is bounded by $\mathrm{Var}(X) \leq (b - \mathrm{E}[X])(\mathrm{E}[X] - a)$.

Applying this property yields a bound on the numerator of the bias:

$$|\operatorname{Cov}(MPX, \omega^i)| \leq \sqrt{(B - \operatorname{E}[MPC])\operatorname{E}[MPC]}\sqrt{\operatorname{Var}(\omega^i)},$$

where ω^i is the "weight" associated with estimator i that is observable, and therefore we can compute its standard deviation. In the case of the implied MPC, applying this framework yields the following solution for the parameter of interest $\operatorname{E}[MPX|X > 0]$ is

$$\operatorname{E}[MPX|X > 0] \in \left[\frac{2\tau + k^2 - k\sqrt{k^2 + 4\tau(1-\tau)}}{2(1+k^2)}, \frac{2\tau + k^2 + k\sqrt{k^2 + 4\tau(1-\tau)}}{2(1+k^2)}\right],$$

where τ is the implied MPX/MPC and $k = \frac{sd(X)}{\operatorname{E[X]}}$. To have real solution, we need $\frac{1-\sqrt{1+k^2}}{2} \leq \hat{\tau} \leq \frac{1+\sqrt{1+k^2}}{2}$.

3 The 2008 US Tax Rebate Case

The 2008 US Tax rebate is a clear example for the case in which the one-time income shock, being a government transfer aiming to stimulate the economy, depends on household characteristics. For the empirical exercise, I will use the Consumer Expenditure Surveys (CEX) dataset. The CEX provides quarterly expenditure data across a wide range of goods and services for the previous three months in each interview. Crucially for this analysis, the survey records the month of receipt and the precise amount of each household's stimulus payment, alongside a set of demographic and economic characteristics.

The stimulus payment amount was a deterministic function of household characteristics reported on their 2007 tax returns, making it endogenous to the very factors that likely influence consumption. Eligibility required a minimum qualifying income of $3,000 dollars. For eligible households, the base payment was determined by their 2007 tax liability, ranging from a minimum of $300 ($600 for joint filers) to a maximum of $600 ($1,200 for joint filers). Households also received an additional $300 for each qualifying child. The total payment was then subject to a phase-out, reduced by 5% for adjusted gross incomes exceeding $75,000 for single filers and $150,000 for joint filers.

While the amount of the rebate is endogenous, the estimation strategy for the average MPC and MPX used by Parker et al. (2013) leverages the random timing of rebate disbursements, which was determined by the last two digits of the recipient's Social Security Number (SSN), conditional on the chosen payment method. Payments were delivered either by mail or electronic funds transfer (EFT), depending on what was selected by taxpayers on their 2007 tax returns. For those that filed their 2007 Tax return on time, EFT payments were disbursed from late April to mid-May 2008, while paper checks were mailed from early May through early July 2008 (Parker et al., 2013).[19]

The randomization of the rebate's timing allows for the construction of a valid "treatment" group (households that recently received the rebate) and a "control" group (households that will receive it in the future), with both groups observed within the same CEX wave. The difference in average expenditure between these two groups identifies the average treatment effect on the treated (ATT). This is the average causal effect of the rebate on expenditure for the population of eligible households.

Let Z_t be an indicator for whether a household received a rebate X_t in the three-month period prior to its interview at time t:

$$Z_t = \begin{cases} 1 & \text{if } X_t > 0, \\ 0 & \text{otherwise.} \end{cases} \tag{9}$$

Further, let X denote the total rebate amount a household eventually receives. For households that receive the rebate in only one period (i.e., for which $\sum_{t>0} Z_t = 1$), this amount is given by

$$X = \max_{t>0} X_t = \sum_{t>0} Z_t X_t. \tag{10}$$

The key randomization assumption—that the timing of the rebate is as-good-as-random—implies that for any period t, the indicator for rebate receipt is independent of the total rebate amount and other determinants of expenditure, U_t: $Z_t \perp\!\!\!\perp (X, U_t)$ This condition **holds**, however, only for the sample of households that meet three criteria: they (i) eventually received the rebate, (ii) received it during the on-time distribution period, and (iii) did not report rebates in multiple periods.[20] The analysis is therefore restricted to this subsample. Households that did not report any rebate, such as those ineligible due to high income, are excluded as they do not constitute a comparable control group.

By making use of the randomization of the rebate timing, the average treatment effect

[19]Parker et al. (2013) used a difference-in-differences (DID) methodology to estimate the average MPC and average MPX. This method is biased under treatment effect heterogeneity, since the estimand is a weighted average of causal effects with potentially negative weights (see, for example, Borusyak et al. (2024) and Orchard et al. (2025)).

[20]That is, the sample includes only observations for households that eventually received the rebate ($X > 0$), received it between May and July 2008, and did not report it in multiple three-month CEX periods.

on the treated (ATT) can be identified:

$$\begin{aligned} ATT_t &= \mathrm{E}[\Delta C_t | X_t > 0] \\ &= \mathrm{E}[g(X_t, U_t) - g(0, U_t) \mid X_t > 0] \\ &= \mathrm{E}[C_t \mid Z_t = 1] - \mathrm{E}[C_t \mid Z_t = 0]. \end{aligned} \tag{11}$$

This estimand is equivalent to that identified by the difference-in-differences estimator used in Borusyak et al. (2024) and Orchard et al. (2023).

A linear estimator for the average MPX, by Orchard et al. (2023, 2025), can then be constructed as the ratio of the ATT to the average rebate amount:

$$\tau_t = \frac{\mathrm{E}[\Delta C_t | X_t > 0]}{\mathrm{E}[X_t | X_t > 0]}. \tag{12}$$

As established previously, this estimator, τ_t, is biased for the true average MPX.

3.1 Estimating the Implied MPX in the CEX

Given the discussion of the previous section, the analysis relies on a sample of CEX households receiving a rebate, with identification leveraging the quasi-random disbursement timing for on-time 2007 tax filers. However, the validity of this approach diminishes significantly after July 2008 due to two confounding factors. First, the control group of not-yet-treated households becomes composed almost exclusively of late filers, for whom the timing of receipt is endogenous. This compositional shift makes the control and treatment groups non-comparable, violating the core randomization assumption. Second, this remaining control group is also numerically insignificant, as over 93% of recipient households were treated by the end of July 2008. This severely limits statistical power and the reliability of estimates. Therefore, to ensure the validity of the research design by addressing both selection bias and small sample concerns, the primary analysis is restricted to households receiving payments between April and July.[21]

Another issue is the presence of two distinct groups: households that received the rebate electronically and those that received it by mail.[22] Given the structure of the CEX, a valid control group cannot be constructed for those who received the rebate electronically. The reason is as follows: each CEX interview covers expenditures over the preceding

[21] Only 0.24% of the household reporting a rebate, received it in 2009

[22] Among CEX households that reported their rebate disbursement method, approximately 39% received it only by electronic transfer, 1% by both electronic transfer and mail, and the remaining 60% received it only by mail.

Table 2: T-tests for Differences in Means by Rebate Disbursement Method

	Difference	**t-statistic**	**p-value**
Expenditure	1423.04	6.599	0.000
Nondurables	689.38	7.146	0.000
Durables	733.66	4.284	0.000
Income (after tax)	13123.62	8.789	0.000
Age Ref.	-6.37	-12.205	0.000
N. Adults	0.01	0.256	0.798
N. Kids	0.19	5.006	0.000
Rebate	186.58	10.872	0.000
Observations	4221		

Notes: T-tests for differences in means between the Electronic Transfer and Mailed Check groups, assuming unequal variances. Expenditure is the variable used in the text to denote total expenditure (e.g., it does not include mortgage payments, etc.). Income (after tax) is the total amount of family income before taxes. Age Ref. is the age of the reference person in the survey.

three months. For households receiving the rebate via electronic deposit in May, their interview timing determines their treatment status. Specifically, households interviewed in May report expenditures from a pre-treatment period (e.g., February-April), whereas those interviewed in June or later report expenditures from a post-treatment period (e.g., March-May and later). Consequently, it is not possible to construct a valid control group for this cohort within the same calendar period.

For households that received the rebate by mail, a valid control group exists for those surveyed in June and July 2008. However, the ETF and mail groups are not comparable, as shown by t-tests of the difference in means in Table 2 for a selection of variables. For example, those who received the rebate electronically have on average higher expenditure across all categories, have higher after-tax total income (by about $13,000), are younger (the survey reference person is about six years younger), and receive a larger rebate (by about $185). Including households from the ETF group in the regressions could therefore create an upward bias in the numerator of the implied MPX. The reason is that, for the month of June, we would be comparing the expenditure of the richer households of the ETF group with that of poorer households. For this reason, my results pertain only to households receiving the rebate exclusively by mail; this cohort constitutes the treatment group implicitly defined in equations (11) and (12). On the other hand, given the information from Table 2, we can expect the true average MPX of the mail group to be larger than the true average MPX of the ETF group.

I analyze three primary expenditure categories: nondurables, durables, and total expenditure (the sum of the two). All spending is measured in gross terms. The expenditures measure follow almost exactly the classification from Parker et al. (2013). Nondurable expenditure include expenditure CEX categories of: food, alcohol, tobacco, personal care, apparel, misc, reading materials, public transportation and gas and motor oil, health care (net of insurance payments). Durable expenditure includes instead housing expenditure such as home furnishings (but I exclude mortgage payments, rent payments, property taxes and expenditure on other lodging), transportation purchases and entertainment expenditure. I removed major housing payments since they are likely not affected by the stimulus checks, and because, following Beraja and Zorzi (2025) housing will be excluded from my model. Moreover, vehicles sales are removed from the transportation expenditure.

The CEX survey is highly susceptible to large outliers (Misra and Surico, 2014; Kaplan and Violante, 2014). For this reason, I will present the result also after dropping the top and bottom 0.5% and 1% of the total expenditure.

Since $Z_t = 1 \iff X_t > 0$ and $Z_t = 1 \iff X_t = 0$ the implied MPX (12) can be estimated in one step using an 2SLS regressions, with Z_t the timing of the rebate as an instrument for the size of the rebate X_t. In the sample I use, I can observe the households expenditure in June and July (two periods) where there is an overlap of treated and never-treated.[23] I run the following regression

$$Y_{it} = \alpha + \tau X_{it} + \gamma \cdot 1_{\text{July}} + \delta \cdot \text{Controls}_{it} + u_{it}, \tag{13}$$

where Z_{it} is an instrument for X_{it} and Y_{it} denotes the expenditure for one of each category (nondurables,durables,total). Because the probability of having received the rebate by June is different from the probability of getting the rebate by July, adding the time fixed effect γ allow me to compute in one step an average of the implied MPX τ in the two periods.[24] Other control variables, while not needed for identification, are added to improve precision, although the coefficient of interest seems sensitive to the inclusion of the variables.[25] An attractive feature of this IV estimator is that standard errors for the implied MPX are

[23] I drop the observation that are already treated (i.e. they received the rebate in some period before the three-months period) from the sample, so only the treated and not-yet-treated groups are compared.

[24] The weights increase with the per-period sample size and are maximized when the per-period fraction of treated units is $1/2$. In the sample used for this analysis, however, the weights are very close to 0.5.

[25] As controls I use: number of adults, number of kids, total family income after taxes, reference person age, and a dummy variable for each of the family type category as defined in the CEX (Married Couple only; Married Couple, own children only, oldest child < 6; Married Couple, own children only, oldest child $\geq 6 \leq 17$; Married Couple, own children only, oldest child > 17; All other husband and wife families; One parent, male, own children, at least one age < 18; One parent, female, own children, at least one age < 18; Single consumers; Other families)

computed directly.

Table 3 reports the results for the three expenditure categories and for the untrimmed and trimmed samples. A key finding is that while the coefficients for both durables and nondurables lack statistical precision, the point estimate for the former is always larger, being even more than twice the magnitude of the latter in the regressions on the trimmed samples. The coefficient on total expenditure, however, is estimated with greater precision, presumably due to the increased statistical power inherent in detecting an effect in the broader category. All these coefficients are large. These results are in line with the IV regressions findings from Parker et al. (2013), who originally estimated a coefficient of 0.252 for nondurables and a coefficient of 0.866 for total expenditure on the sample of households receiving the rebate, and coefficients of 0.308 and 0.911 after removing households that received a late rebate, with this latter sample being the most similar to my results.

On the other hand, these results are much larger compared to the preferred estimation of Orchard et al. (2025). Their preferred estimate for the MPX considering total expenditure is 0.30. However, when they use only the observations that recived at any point the rebate, the estimate largely increases to 0.82, in their "Homogeneous treatment" specification and to 0.64 in their "Heterogeneous treatment" specification.[26]

3.2 Partial identification using the Implied MPX for the 2008 US Tax Rebate

In this section I will apply the results of section **[Valerio: Add reference section]** to partial identify the two quantities. Since the coefficient estimated in part 3.1 referes to the (weighted) average for two different periods, I will assume that we are interested in the same quantities takan as the same average across time.

I compute the coefficient of variation of the rebate size, $\kappa = \frac{\sqrt{\mathrm{Var}(X_t)}}{\mathrm{E}[X_t]}$, using the full sample of households that received a rebate. The resulting estimate is $\hat{\kappa} = 0.544$.[27] Applying the result.

3.3 Estimating the average MPX for the 2008 US Tax Rebate

A key feature of the 2008 U.S. Tax Rebate is that the transfer size was predetermined, as it was based on households' 2007 tax returns. Crucially, this predetermined rebate amount

[26] However, Orchard et al. (2025) use a different methodology and a more inclusive sample. Specifically, their sample combines households that received the rebate electronically with those that received it by mail and includes data from households reporting a rebate after July.

[27] The estimate remains practically constant when computed over the different trimmed samples.

Table 3: Estimates of Implied Marginal Propensity to Spend (τ) for different categories.

	(1) Total Expenditure	(2) Nondurables	(3) Durables
Panel A: Full Sample			
τ	0.741	0.338	0.403
	(0.415)	(0.226)	(0.328)
Observations	1,383	1,383	1,383
Panel B: Excluding top & bottom 0.5%			
τ	0.804*	0.247	0.557*
	(0.355)	(0.189)	(0.283)
Observations	1,371	1,371	1,371
Panel C: Excluding top & bottom 1%			
τ	0.733*	0.206	0.528*
	(0.317)	(0.188)	(0.237)
Observations	1,357	1,357	1,357

Notes: Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ All regressions include the following control variables: number of adults, number of kids, total family income after taxes, reference person age, family type category dummies, and a time fixed effect.

was also independent of the randomized timing of the payment. The combination of these two features, a known transfer size and random timing, makes it possible to construct an imputation estimator for the average MPX, since the future rebate amount (X) is observable in the CEX for the not-yet-treated control group. The following lemma formalizes this estimator.

Lemma 1. *Given that $Z_t \perp\!\!\!\perp (U_t, X)$, $Z_t = 1 \iff X_t > 0$ and $Z_t = 0 \iff X_t = 0$ the average MPX, $\mathrm{E}\left[\frac{\Delta C_t}{X} | X > 0\right]$, is identified as*

$$\mathrm{E}\left[\frac{C_t}{X_t} \mid Z_t = 1, X > 0\right] - \mathrm{E}\left[\frac{C_t}{X} \mid Z_t = 0, X > 0\right].$$

Proof. Since $X > 0$, we have from the definition of the average MPX

$$\begin{aligned}
\mathrm{E}\left[\frac{\Delta C_t}{X}|X>0\right] &= \mathrm{E}\left[\frac{g(X,U_t)-g(0,U_t)}{X}|X>0\right] \\
&= \mathrm{E}\left[\frac{g(X,U_t)}{X}|X>0\right] - \mathrm{E}\left[\frac{g(0,U_t)}{X}|X>0\right] \\
&= \mathrm{E}\left[\frac{g(X,U_t)}{X}|Z_t=1,X>0\right] - \mathrm{E}\left[\frac{g(0,U_t)}{X}|Z_t=0,X>0\right] \\
&= \mathrm{E}\left[\frac{g(X_t,U_t)}{X_t}|Z_t=1,X>0\right] - \mathrm{E}\left[\frac{g(0,U_t)}{X}|Z_t=0,X>0\right] \\
&= \mathrm{E}\left[\frac{C_t}{X_t} \mid Z_t=1,X>0\right] - \mathrm{E}\left[\frac{C_t}{X} \mid Z_t=0,X>0\right],
\end{aligned}$$

where the third equality follows from $Z_t \perp\!\!\!\perp (U_t, X)$, the fourth equality from the fact that $X = X_t$ if $Z_t = 1$. Finally, both elements from the last line can be computed from the data. □

Note that this estimator can be interpreted as the average MPX only if the households that received multiple rebates, received all of them in the same period. Otherwise, the total rebate amount used to normalize expenditure for those in the control group becomes ambiguous, making the estimator ill-defined.

Because the estimator from Lemma 1 is a difference-in-means estimator, it is numerically equivalent to the β coefficient from the following OLS regression:

$$Y_{it} = \alpha + \beta Z_{it} + \gamma 1_{\text{July}} + \gamma \text{Controls}_{it} + u_{it}, \tag{14}$$

where the dependent variable, Y_{it}, is the ratio of expenditure (on nondurables, durables, or total expenditure) to the rebate amount. For treated households in period t ($Z_{it} = 1$), this is defined as $Y_{it} = C_{it}/X_{it}$. For the control group of not-yet-treated households ($Z_{it} = 0$), the variable is constructed as $Y_{it} = C_{it}/X_i$, where X_i is the pre-determined rebate amount that household i will receive in a future period.

Table 4 shows the results for the biased estimate (the implied MPX, τ) and the unbiased MPX, as well as the size of the bias using the sample that excludes the top 1% of observations.[28] All estimates are computed on the same sample for all three expenditure categories.

The results show that the implied MPX underestimates the true average MPX for all three categories, providing evidence of a *negative* selection effect. The average MPX for

[28] See table 7 in appendix C for the estimates in the three samples. Across samples and spending categories, there is evidence of a negative bias.

Table 4: Biased (τ) and Unbiased Average MPX for different categories.

	(1) Biased Average	(2) Unbiased Average	(3) Bias
Nondurables	0.206	0.388	0.182
	(0.188)	(0.373)	(0.315)
Durables	0.528	0.660	0.132
	(0.237)	(0.282)	(0.176)
Total Expenditure	0.733*	1.048*	0.315
	(0.317)	(0.534)	(0.450)

Notes: Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. The estimates are after excluding and top & bottom 1%. All regressions include the following control variables: number of adults, number of kids, total family income after taxes, reference person age, family type category dummies, and a time fixed effect. The standard errors for the bias are computed using 3000 bootstraps simulations.

total expenditure increases to a high and statistically significant 1.091 from the linear estimator's already high estimate of 0.706. While not statistically significant, the point estimates for nondurables and durables show large increases of approximately 59% and 51%, respectively.

How does the bias computed compare with the bounds of Section 3.2? **[Valerio: Add comparison]**

The bias depends on the overall covariance between the individual MPX and the size of rebate. The size of the rebate for each household was in fact a function of the family status (i.e. married and filing jointly), the previous year tax liability, the number of components and children of the households, and the adjusted gross income. A feature that emerges from the data, however is that this rebate formula did not necessarily target lower-income households with larger transfer.

[Valerio: Add interpretation of the bias.]

4 Model

This section presents a model with a durable and a nondurable good (consumption) to rationalize a large expenditure response to a one-time income shock, particularly in durable goods.

The model follows Murphy (2024) and Sciacovelli (2024) and adds an endogenous borrowing decision for financing the durable good. This model also builds on Beraja and Zorzi (2025), who shows that introducing additive taste shocks to model the probability of durable adjustment can better match empirical evidence compared to other models of durable goods.

In this model, households face idiosyncratic uncertainty in their productivity process. They can hold a risk-free liquid asset subject to a borrowing constraint, and they can finance the purchase of the durable good by choosing how much debt to take on. The decision to adjust the durable good stock is lumpy, given the presence of taste shocks and non-convex adjustment costs.

4.1 Household decision problem

A continuum of infinitely-lived households populates the economy. Households discount future utility at a rate β, and their momentary utility function over consumption c and the stock of the durable good d is given by:

$$u(c,d) = \frac{c^{1-\gamma}}{1-\gamma} + \nu \frac{d^{1-\eta}}{1-\eta}, \tag{15}$$

where $\gamma > 0$ and $\eta > 0$, and ν is the relative weight on utility from durables.[29]

Households can freely adjust their nondurable consumption c, but the decision to adjust their durable stock d is a discrete choice. This implies that in a stationary equilibrium, only a fraction of households adjusts their durable stock in any given period. Households can save in a one-period liquid asset a' at the risk-free rate r. Following the literature, markets are incomplete, and households are subject to a borrowing constraint, $a' \geq 0$.

The durable stock depreciates at a rate δ. For a household that does not adjust, its stock evolves according to the law of motion $d' = (1-\delta)d$.

For households that choose to adjust, the process involves several steps. They first sell their depreciated stock of durables, receiving revenue equal to $p(1-\delta)d$, where p is the relative price of the durable good. They then purchase a new durable stock, d', at price p. To finance this purchase, households can take on new debt, b', which is subject to a borrowing limit: $b' \in [0, \phi p d']$, where ϕ is the loan-to-value ratio. Simultaneously, they must repay their outstanding debt from the previous period, $(1+r_b)b$.

Debt, b, is therefore a state variable in the household's problem. The law of motion for debt depends on the durable adjustment decision. For households that do not adjust their durable stock, their debt amortizes at a constant rate μ:

$$b' = (1-\mu)b. \tag{16}$$

[29]The parameter ν can be interpreted as incorporating a service flow parameter, s. A utility term of the form $\tilde{\nu}\frac{(sd)^{1-\eta}}{1-\eta}$ is equivalent to the one used in the text, with the re-parameterization $\nu = \tilde{\nu} s^{1-\eta}$.

The interest rate r_b paid on all outstanding debt is equal to the risk-free rate plus a constant positive spread:

$$r_b = r + \Delta, \tag{17}$$

where $\Delta > 0$.

The income process of the household is modeled as in Auclert et al. (2024), so that for each household the after-tax income process is

$$y_{it} = (Y_t - T_t) \cdot \frac{e_{it}^{1-\theta}}{\mathrm{E}[e_{it}^{1-\theta}]}, \tag{18}$$

where θ is a constant progressivity tax parameter (Heathcote, Storesletten, and Violante, 2017), Y_t and T_t are the aggregate output and aggregate taxes respectively, and e_{it} is the productivity of the households.[30] Moreover, the log of productivity follows an AR(1) process with normal innovations u_{it} with standard deviation σ_u:

$$\log(e_{it}) = \rho e_{it-1} + u_{it}. \tag{19}$$

The household's decision to adjust its stock of durables is modeled as a discrete choice. Each period, given its state vector $s = (e, d, a, b)$, the household chooses between adjusting its durable stock, which yields value $V^{\mathrm{a}}(s)$, or maintaining its current stock, which yields value $V^{\mathrm{n}}(s)$.

The choice is subject to additive, idiosyncratic taste shocks, ϵ^{a} and ϵ^{n}, which are assumed to be independently drawn from a Type-I Extreme Value (Gumbel) distribution. The ex-ante value function, before the shocks are realized, is therefore:

$$V(s) = \mathrm{E}_{\epsilon}\left[\max\{V^{\mathrm{a}}(s) - \kappa + \sigma_{\epsilon}\epsilon^{\mathrm{a}}, V^{\mathrm{n}}(s) + \sigma_{\epsilon}\epsilon^{\mathrm{n}}\}\right], \tag{20}$$

where κ is a fixed utility cost of adjustment as in Beraja and Zorzi (2025) and σ_{ϵ} is the scale parameter of the taste shocks.

The introduction of taste shocks offers several key advantages. Computationally, by smoothing the choice problem, taste shocks permit the use of efficient algorithms like a modified Endogenous Gridpoint Method (Carroll, 2006), which significantly reduces solution time (Iskhakov et al., 2017). Moreover, Beraja and Zorzi (2025) shows that in models with durables, taste shocks better capture the empirical patterns of household

[30] This after tax income process when flexible prices and sticky wages are assumed and output is equal to effective labor $Y_t = N_t = \int e_i n_{it}\, di$, assuming everybody supply the same quantify of labor $n_{it} = N_t$. See Auclert, Rognlie, and Straub (2024) for more details.

expenditure on durable goods.

The specific assumption of a Gumbel distribution is what underpins this tractability, as the expectation in (20) admits a closed-form solution given by the *log-sum* formula (McFadden, 1972):

$$V(s) = \sigma_\epsilon \log \left(\exp \left(\frac{V^{\mathrm{a}}(s) - \kappa}{\sigma_\epsilon} \right) + \exp \left(\frac{V^{\mathrm{n}}(s)}{\sigma_\epsilon} \right) \right). \tag{21}$$

Consequently, the probability that the household chooses to adjust its durable stock is then given by a logit probability, which depends on the relative values of the two choices:

$$\Pr(\text{adjust}|s) = \frac{\exp\left(\frac{V^{\mathrm{a}}(s)-\kappa}{\sigma_\epsilon}\right)}{\exp\left(\frac{V^{\mathrm{a}}(s)-\kappa}{\sigma_\epsilon}\right) + \exp\left(\frac{V^{\mathrm{n}}(s)}{\sigma_\epsilon}\right)}. \tag{22}$$

For completeness, the bellman equation *conditional* on the non adjusting choice is then

$$\begin{aligned} V^{\mathrm{n}}(e,d,a,b) = \max_{c,a'} \ & \frac{c^{1-\gamma}}{1-\gamma} + \nu \frac{d'^{1-\eta}}{1-\eta} + \beta \, \mathrm{E}[V(e',d',a',b')|e] \\ \text{s.t. } & a' + c = y(e) + (1+r)a - (r_b + \mu)b \\ & a' \geq 0 \\ & d' = (1-\delta)d \\ & b' = (1-\mu)b, \end{aligned} \tag{23}$$

while for those that adjust, it is

$$\begin{aligned} V^{\mathrm{a}}(e,d,a,b) = \max_{c,a',d',b'} & \frac{c^{1-\gamma}}{1-\gamma} + \nu \frac{d'^{1-\eta}}{1-\eta} + \beta \mathbb{E}[V(e',d',a',b')|e] \\ \text{s.t. } a' + c + (pd' - b') = \ & y(e) + (1+r)a - (1+r_b)b + (1-\delta)pd \\ & a' \geq 0 \\ & b' \in [0, \phi p d'], \end{aligned} \tag{24}$$

where $y(e)$ denotes in both problems the post-tax income.

4.2 Calibration

The model is calibrated at a quarterly frequency. The definition of the durable good is chosen to align with the empirical MPX literature: it includes vehicles and household appliances but, crucially, excludes housing. This approach is similar to that of Beraja and

Table 5: Model Calibration

Parameter	Description	Value	Target/Source
Calibrated Parameters			
β	Discount factor	0.983	Liq Asset share = 0.26
ν	Relative durable weight	0.236	D spending/C = 0.21
κ	Adj. cost	0.035	Adj. probability = 29.8%
σ_ϵ	Adj. taste shock scale	0.025	See text
Set Parameters			
η, γ	1/EIS	2	See text
δ	Annual depreciation rate	0.166	BEA Durable Goods
ρ	Income persistence	0.976	Floden and Lindé (2001)
σ_u	Income st. dev.	0.92	Auclert et al. (2024)
r	Risk-free real rate	0.25%	Real Fed. Funds Rate
$r_b - r$	Real borrowing rate	0.38%	Beraja and Zorzi (2025)
μ	Exogenous repayment share	6.88%	Beraja and Zorzi (2025)
ϕ	Borrowing limit	0.5856	Beraja and Zorzi (2025)

Notes: Table with the calibrated parameters of the model. See Section 4.2 for details.

Zorzi (2025). It differs from studies that focus primarily on cars (Attanasio et al., 2022; Murphy, 2024), those that focus primarily on housing (Wong, 2020; Eichenbaum et al., 2022; Berger et al., 2024; Sciacovelli, 2024), and those that use a broader definition of durables that includes housing (Berger and Vavra, 2015; McKay and Wieland, 2021).

Housing is excluded for two primary reasons. First, this narrower definition is more consistent with the consumption data used in seminal empirical MPX studies (Parker et al., 2013; Fagereng et al., 2021). Second, it is reasonable to assume that households do not adjust their primary residence in response to a typical one-time government transfer.

The calibrated parameter in the model are the discount factor β, the relative weight on the durable good in the utility function ν, the adjustment cost parameter κ and the taste shock scale parameter σ_ϵ. The discount factor is calibrated to match the ratio of the average stock of net-liquid asset holdings to average annual income of 26%. This is the calibration target from Beraja and Zorzi (2025), representing the low-liquidity benchmark for HA models (Kaplan et al., 2018). The utility weight ν is set to match the ratio of durable to nondurable expenditure of 0.21, which I calculate from BEA data for the years 1970-2019. To compute this ratio, the numerator is total expenditure on durables. The denominator is expenditure on non-durables and services, from which I exclude spending on housing, utilities, financial services, and insurance. This exclusion ensures greater consistency with empirical MPX measures and reflects that the model does not feature a housing choice.

The taste shock scale parameter σ_ϵ is calibrated such that a (targeted) transfer equal to the average transfer in the sample I study for the 2008 US Tax Rebate ($858) to the bottom third of the distribution is equal to the observed change in expenditure of 0.733.[31] The resulting value is $\sigma_\epsilon = 0.17$

A crucial element in the model is the steady-state (unconditional) probability of durable adjustment. Given the definition of durable goods in the model, I calibrated this parameter to the average adjustment frequency in the CEX using the years 1996-2019. To compute the frequency of adjustment, I identified households that adjusted their cars, household furnishing, and equipment, categorizing an adjustment as any expenditure over a minimum of $250 (in 2017 U.S. dollars) in one of these durable categories.[32] This procedure yields a quarterly adjustment frequency of 29.8%, which is consistent with the definition of durables in my model and is higher compared to other papers (McKay and Wieland, 2021; Beraja and Zorzi, 2025).

For the utility function in equation (15), the parameters η and γ are both set to the standard value of 2. This choice implies an elasticity of intertemporal substitution of 0.5 in a frictionless benchmark where households can freely adjust both durables and nondurables. The annual depreciation rate is set to 16.6%, a value computed from BEA data on current-cost depreciation and the net stock of consumer durable goods. The remaining financial parameters follow Beraja and Zorzi (2025): the borrowing limit is $\phi = 0.5856$, the quarterly real interest rate on liquid assets is $r = 0.25\%$, the interest rate spread is $\Delta = r - r_b = 0.38\%$, and the debt repayment speed is $\mu = 6.88\%$.

The calibration of the income process (equations (18) and (19)) follows Auclert et al. (2024). This, the income process ρ is set to match the persistence of the US wage process of 0.91 yearly (Floden and Lindé, 2001) and the variance of innovations is set to match a the standard deviation of the log (gross) earnings in the US of 0.92. The progressivity tax parameter θ is set to 0.181 (Heathcote et al., 2017). Finally aggregate and aggregate output enters equation (18). Aggregate output is normalized to 1, while aggregate taxes T_t are set equal to the interest rate $r \cdot NA$, where NA is the net aggregate assets $NA = A - B$.[33]

[31] To solve the model, the income process is approximated by a discrete Markov chain using the Rouwenhorst method (Kopecky and Suen, 2010). Within this framework, a targeted policy must be implemented as a transfer to a specific set of states. Accordingly, the policy is modeled as a transfer to the lowest income states that collectively constitute the bottom 0.34% of the stationary income distribution.

[32] See Appendix A.1 for more details.

[33] This rule for is adopted for simplicity. If the government supplies all assets in the economy (liquid asset a and debt b), the tax rule becomes $T = r(A - B) - r\Delta$ and government spending is $G = r\Delta$.

4.3 Uniform vs Targeted Transfers

The model generates a high overall MPX, given the possibility to finance the durable good. Moreover, the MPX stays elevated even for larger transfer.

In the previous parts of the paper I showed how we may potentially underestimate how much household spent out of a temporary income shock. I focused on two types of income shock, one that is uncorrelated with households characteristics (a lottery winning), and one that depends on household characteristics (a tax rebate). In both cases, I find evidence of a downward bias. What is the consequence of a large MPX that is concentrated on durable goods?

I compare two fiscal transfer schemes in partial equilibrium. The first is a uniform, lump-sum transfer distributed to all households. The second is a transfer of the same per-household size, but targeted exclusively to households in the bottom 30% of the income distribution.[34]

Households exhibit substantial heterogeneity in how they finance new durable good purchases. Figure 3 illustrates this by plotting the fraction of durable expenditure financed with debt across the income distribution. This debt-financed share declines sharply with income. Households in the bottom third of the income distribution, which corresponds to the first three income states in the calibration, finance their purchases by borrowing nearly up to the limit ($\phi = 0.5856$) when they adjust their durable stock. On the other hand, for higher-income households, the debt-financed share of new durable purchases is much smaller, reaching almost zero for the last income group.

This financing pattern is driven by differences in households' financial resources. Lacking sufficient liquid assets, lower-income households must rely on leverage to fund durable adjustments. In contrast, higher-income households can typically self-finance these expenditures with internal funds, minimizing their use of debt. This differential reliance on external financing is a key mechanism that generates larger spending responses to income shocks among lower-income groups.

Figure 4 illustrates the intertemporal average MPX under two distinct policy experiments: a universal transfer and a targeted transfer. Under a universal transfer, the average MPX is high initially at approximately 0.7, decaying rapidly in subsequent years but remaining positive over the five-year horizon. The targeted transfer generates a much stronger immediate response: the first-year MPX exceeds 1.2. However, this debt-fueled surge in expenditure is followed by a prolonged period of spending below the steady-state level for the next four years.

[34] By construction, the per-recipient transfer amount is identical across both scenarios. Consequently, the aggregate fiscal outlay of the uniform policy is larger, as it covers the entire population.

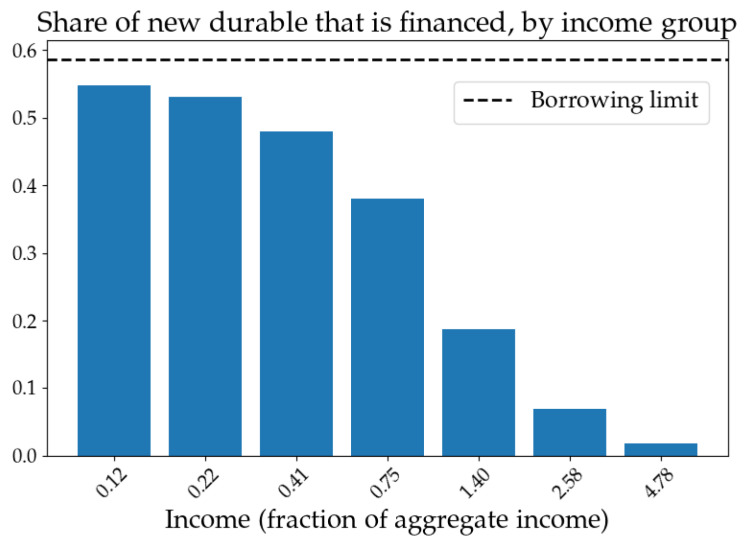

Figure 3: Share of the new durable good that is financed

Notes: Share of the new durable good that is financed across income groups. The borrowing limit is the parameter ϕ, equal to 0.5856.

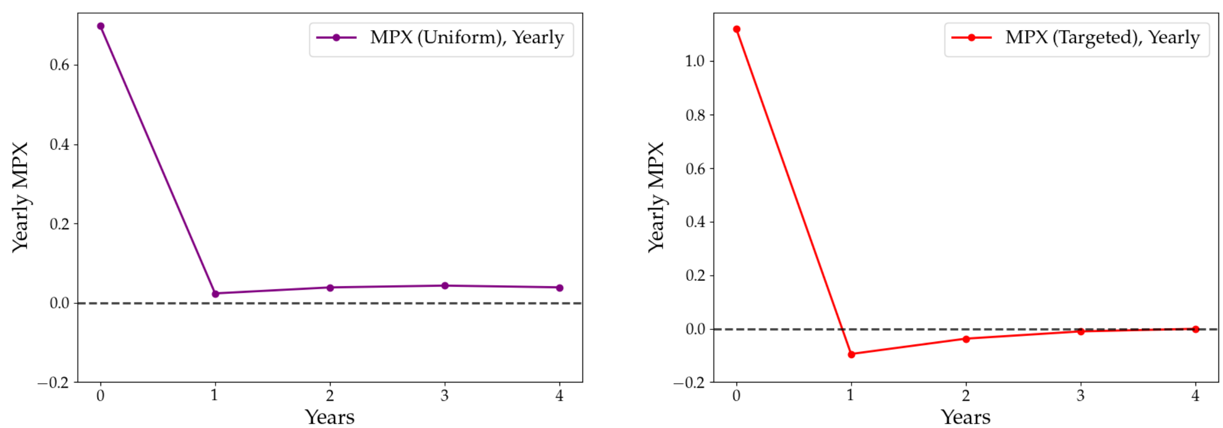

Figure 4: Intertemporal average MPX after a uniform transfer (left) or a targeted transfer (right)

The mechanism behind these dynamics is tied to households' durable good adjustments and use of credit. The targeted transfer primarily reaches low-income households, for whom the transfer raises both the likelihood of purchasing a durable good (the extensive margin) and the amount spent (the intensive margin). The ability to finance these purchases with debt allows the immediate expenditure to exceed the transfer amount. This initial consumption is pulled forward from the future, however. The subsequent spending reversal is explained by two forces: a lower probability of a repeat durable purchase and the burden of debt service, which constrains future resources.

The universal transfer dilutes this effect by including higher-income households. These households are less liquidity-constrained and have a higher existing stock of durables, making them less responsive to the transfer. Furthermore, their lower propensity to borrow for durable purchases dampens the aggregate MPX, leading to a smaller overall response compared to the targeted policy.

5 Conclusion

Lorem ipsum

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Appendix

A Data

A.1 Calibration

The (implied) *yearly depreciation rate* for durable goods is computed as the average of the "Current-Cost Depreciation of Consumer Durable Goods" series (NIPA Table 1.3. Current-Cost Depreciation of Fixed Assets and Consumer Durable Goods) divided by the sum of "Current-Cost Net Stock of Consumer Durable Goods" series (NIPA Table 1.1. Current-Cost Net Stock of Fixed Assets and Consumer Durable Goods) and the mentioned depreciation series.

The *probability of adjustment* of the stock of durable goods in a three-month period is estimated using the Consumer Expenditure Survey (CEX) dataset from the years 1996-2019.

To compute this probability, I used the aggregate expenditure variable for "House furnishings and equipment" and all expenditure for new and used vehicles. The "House furnishings and equipment" variable is the sum of expenditures for household textiles, furniture, floor coverings, major appliances, small appliances, miscellaneous housewares, and miscellaneous household equipment.

Similarly to McKay and Wieland (2021), expenditure for new and used vehicles includes spending on new and used cars (UCC 450110, 460110), new and used motorcycles (UCC 450220, 460902), and new and used trucks (UCC 450210, 460901).

Any household that has purchased any new or used vehicles or has spent at least $250 (in 2017 US Dollars) in the "House furnishings and equipment" category is regarded as having adjusted their stock of durables. The *probability of durable adjustment* is the three-month average of the fraction of households that have adjusted their stock of durables.

Lowering the threshold to $100 would greatly increase this probability to a quarterly value of about 40%.

B The linearity bias

B.1 Proof of Proposition 1

By independence, we can disregard the control variables w. Let $p = Pr(X > 0)$ and $\frac{\Delta C}{\Delta X} = \frac{g(X,U)-g(0,U)}{X}$ We have that

$$\begin{aligned}
\beta^{OLS} &= \frac{\mathrm{E}\left[C(X-\mathrm{E}[X])\right]}{\mathrm{Var}(X)} = \frac{p\,\mathrm{E}\left[C(X-\mathrm{E}[X])|X>0\right]-(1-p)\,\mathrm{E}[C|X=0]\,\mathrm{E}[X]}{\mathrm{Var}(X)}\\
&= \frac{p\,\mathrm{E}[g(0,U)(X-\mathrm{E}[X])+\frac{\Delta C}{\Delta X}X(X-\mathrm{E}[X])|X>0]-(1-p)\,\mathrm{E}[g(0,U)]\,\mathrm{E}[X]}{\mathrm{Var}(X)}\\
&= p\frac{\mathrm{E}[\frac{\Delta C}{\Delta X}X(X-\mathrm{E}[X])|X>0]}{\mathrm{Var}(X)}+\frac{pE[g(0,U)]\,(\mathrm{E}[X|X>0]-\mathrm{E}[X])}{\mathrm{Var}(X)}\\
&+\frac{(1-p)E[g(0,U)]\,\mathrm{E}[X]}{\mathrm{Var}(X)}\\
&= p\frac{\mathrm{E}[\frac{\Delta C}{\Delta X}X(X-\mathrm{E}[X])|X>0]}{\mathrm{Var}(X)}+\frac{E[g(0,U)]\,\overbrace{(p\,\mathrm{E}[X|X>0]-E[X])}^{0}}{\mathrm{Var}(X)}\\
&= p\frac{\mathrm{E}[\frac{\Delta C}{\Delta X}X(X-\mathrm{E}[X])|X>0]}{\mathrm{Var}(X)}
\end{aligned}$$

B.2 Proof of Proposition 4

Let $h(X) = \mathrm{E}[\frac{\Delta C}{\Delta X}|X]$. By concavity, it is decreasing in X. By the law of iterated expectation, we have

$$\beta^{OLS} = \frac{\mathrm{E}[\frac{\Delta C}{\Delta X}pX(X-\mathrm{E}[X])|D>0]}{\mathrm{Var}(X)} = \frac{\mathrm{E}[h(X)pD(X-\mathrm{E}[X])|D>0]}{\mathrm{Var}(X)}.$$

By the Chebyshev integral inequality we have in probabilistic terms that

$$\mathrm{E}[f(X)g(X)] \geq \mathrm{E}[f(X)]\,\mathrm{E}[g(X)]$$

or conditioning on an event A,

$$\mathrm{E}[f(X)g(X)|A] \geq \mathrm{E}[f(X)|A]\,\mathrm{E}[g(X)|A]$$

if $f(X), g(X)$ are both increasing or decreasing and the opposite inequality if one is increasing and the other is decreasing over the support of X conditional on A (Mitrinović et al., 1993). Note that $X(X-\mathrm{E}[X])$ is increasing whenever $X \geq \mathrm{E}[X]/2$. We have then

$$
\begin{aligned}
\mathrm{E}[h(X)pX(X-\mathrm{E}[X])|X>0] &= \int_0^{\mathrm{E}[X]/2} h(X)p\underbrace{X(X-\mathrm{E}[X])}_{<0}dF(x) \\
&+ \int_{\mathrm{E}[X]/2}^{\infty} h(X)pX(X-\mathrm{E}[X])dF(x) \\
&\leq \mathrm{E}[h(X)|X>0, X>\mathrm{E}[X]/2]\int_0^{\mathrm{E}[X]/2} pX(X-\mathrm{E}[X])dF(x) \\
&+ \mathrm{E}[h(X)|X>0, X>\mathrm{E}[X]/2]\int_{\mathrm{E}[X]/2}^{\infty} pX(X-\mathrm{E}[X])dF(x) \\
&= E[h(X)|X>0, X>\mathrm{E}[X]/2]\,\mathrm{E}[pX(X-\mathrm{E}[X])|X>0],
\end{aligned}
$$

where for the first inequality I used the fact that by concavity, $\mathrm{E}[\frac{\Delta C}{\Delta X}|X>0] \leq \mathrm{E}[\frac{\Delta C}{\Delta X}|X>0, X>\mathrm{E}[X]/2]$ and I applied the Chebyshev integral inequality to the second integral. Finally, note that

$$
\mathrm{Var}\,(X) = p\,\mathrm{E}[X^2|X>0] - p^2\,\mathrm{E}[X|X>0]
$$

so that

$$
\begin{aligned}
\mathrm{E}[pX(X-\mathrm{E}[X])|X>0] &= p\,\mathrm{E}[X^2|X>0] + p\,\mathrm{E}[X|X>0]\,\mathrm{E}[X] \\
&= p\,\mathrm{E}[X^2|X>0] - p^2\,\mathrm{E}[X|X>0] \\
&= \mathrm{Var}\,(X).
\end{aligned}
$$

Thus,

$$
\beta^{OLS} \leq E\left[\frac{\Delta C}{\Delta X}|X>0, X>\frac{\mathrm{E}[X]}{2}\right] \leq \mathrm{E}\left[\frac{\Delta C}{\Delta X}|X>0\right]
$$

B.3 Proof of Proposition 5

Let $\bar{X} = \mathrm{E}[X|X>0]$. By Jensen's inequality, $\mathrm{E}[C|X>0] = \mathrm{E}[g(X,U)|X>0] \leq \mathrm{E}[g(\bar{X},U)]$. Thus, for the ratio estimator (arising, for example, from an IV regression with the instrument $Z = 1_{X>0}$), the following inequality holds:

$$
\frac{\mathrm{E}[\Delta C|X>0]}{\mathrm{E}[X|X>0]} \leq \mathrm{E}\left[\frac{g(\bar{X},U)-g(0,U)}{\bar{X}}\right].
$$

The inequality in Proposition 5 then follows because the OLS weights, are larger than those of the ratio estimator for large X and smaller for small X. Let $p = Pr(X>0)$. Since $w^{IV} = \frac{X}{E[X|X>0]}$ be the weights of the ratio estimator (denoted by β^{IV}) and $w^{OLS} = \frac{pX(X-\mathrm{E}[X])}{\mathrm{Var}\,X}$ be the weights for the OLS estimator.

Then $w^{OLS} \leq w^{IV}$ X whenever $X \leq \frac{\operatorname{Var} X}{\bar{X}} \frac{1}{p} + \frac{\bar{X}}{p}$. Moreover, the difference $w^{OLS} - w^{IV}$ is increasing whenever $X \leq \frac{1}{2}\left(\frac{\operatorname{Var} X}{\bar{X}} \frac{1}{p} + \frac{\bar{X}}{p}\right)$. Let A be the event $\{X \leq \frac{1}{2}\left(\frac{\operatorname{Var} X}{\bar{X}} \frac{1}{p} + \frac{\bar{X}}{p}\right)\}$. We then have,

$$\begin{aligned}
\beta^{OLS} - \beta^{IV} &= E[MPX(w^{OLS} - w^{IV})] \\
&= E[MPX(w^{OLS} - w^{IV})1_{A^c}] + [MPX(w^{OLS} - w^{IV})1_A] \\
&\leq \mathrm{E}[MPX|A]\,\mathrm{E}[(w^{OLS} - w^{IV})1_{A^c}] + \mathrm{E}[MPX|A]\,\mathrm{E}[(w^{OLS} - w^{IV})1_A] = 0,
\end{aligned}$$

where the first inequality follows from the fact that the MPX is decreasing in X and that $(w^{OLS} - w^{IV})1_{A^c} \leq 0$, and from the Chebyshev integral inequality on the part in which the difference $w^{OLS} - w^{IV}$ is increasing.

B.4 Estimation Results

Proposition 6. *OLS regression coefficient* $\beta^{OLS} = \mathrm{E}[g'(\theta, U)]$ *with* $\theta \in (0, a)$ *(not a r.v) if* $A \perp\!\!\!\perp U$ *and* $A \in \{0, a\}$ *(it can take only two values).*

Proof. Let $p = Pr(A = a)$ We have that

$$\begin{aligned}
\beta^{OLS} &= \frac{\mathrm{E}\left[C(A - \mathrm{E}[A])\right]}{\mathrm{E}[A^2] - \mathrm{E}[A]^2} = \frac{p\left(\mathrm{E}[g(a, U)]a - \mathrm{E}[c]a\right)}{a^2(p(1-p))} = \frac{\mathrm{E}[g(a, U)] - \mathrm{E}[c]}{a(1-p)} \\
&= \frac{\mathrm{E}[g(a, U)] - p\,\mathrm{E}[g(a, U)] - (1-p)\,\mathrm{E}[g(0, U)]}{a(1-p)} \\
&= \frac{\mathrm{E}[g(a, U) - g(0, U)]}{a} = \mathrm{E}[g'(\theta, U)]
\end{aligned}$$

□

Proposition 7. *IV regression coefficient*

$$\beta_{IV} = \frac{\mathrm{E}[g'(\theta, U)A|A > 0]}{\mathrm{E}[A|A > 0]}$$

with $\theta \in (0, A)$ *(i.e. a r.v) if* $Z = 1_{A>0} \perp\!\!\!\perp U$ *and* $A \geq 0$*.*

Proof. Let $\mathrm{E}[Z] = Pr(A > 0) = p$. By definition, $\beta_{IV} = \frac{\mathrm{Cov}(C,Z)}{\mathrm{Cov}(A,Z)}$. We have that

$$\begin{aligned}
\mathrm{Cov}(A,Z) &= \mathrm{E}[A(Z-p)] \\
&= \mathrm{E}[A|A>0]p - \mathrm{E}[A]p \\
&= p(1-p)\left(\mathrm{E}[A|A>0] - \mathrm{E}[A|A=0]\right) \\
&= \mathrm{E}[A|A>0],
\end{aligned}$$

and

$$\begin{aligned}
\mathrm{Cov}(C,Z) &= \mathrm{E}[g(A,U)|A>0]p - \mathrm{E}[g(A,U)]\,\mathrm{E}[A] \\
&= \mathrm{E}[g(A,U)|A>0]p - \mathrm{E}[g(A,U)]p \\
&= p(\mathrm{E}[g(A,U)|A>0] - \mathrm{E}[g(A,U)|A>0]p + \mathrm{E}[g(0,U)](1-p)) \\
&= p(1-p)\left(\mathrm{E}[g(A,U) - g(0,U)|A>0]\right).
\end{aligned}$$

Therefore,

$$\beta_{IV} = \frac{\mathrm{Cov}(C,Z)}{\mathrm{Cov}(A,Z)} = \frac{\mathrm{E}[g(A,U) - g(0,U)|A>0]}{\mathrm{E}[A|A>0]}$$

Moreover, using Taylor theorem we have

$$\beta_{IV} = \frac{\mathrm{E}[g'(\theta,U)A|A>0]}{\mathrm{E}[A|A>0]}$$

□

C The 2008 US Tax Rebate Case

Table 6: Summary Statistics by Rebate Disbursement Method

	Mean	Std. Dev.	Min	Max	Count
Electronic Transfer					
Expenditure	8691.41	6586.56	-22580	54975	1324
Nondurables	5429.14	2829.53	508	24638	1324
Durables	3262.27	5336.30	-30969	44949	1324
Income (after tax)	69677.28	45898.15	-36146	341079	1324
Age Ref.	45.80	14.84	18	87	1324
N. Adults	1.92	0.74	1	6	1324
N. Kids	0.83	1.15	0	8	1324
Rebate	1070.65	529.34	7	3000	1324
Mailed Check					
Expenditure	7268.37	6307.09	-14698	71704	2897
Nondurables	4739.76	3072.95	-3479	49700	2897
Durables	2528.61	4757.90	-16703	62683	2897
Income (after tax)	56561.43	49318.34	-26954	526051	2897
Age Ref.	52.18	17.54	17	87	2897
N. Adults	1.92	0.86	1	8	2897
N. Kids	0.64	1.11	0	8	2897
Rebate	884.08	490.02	6	3660	2897
Observations	4221				

Notes: Unweighted summary statistics. Expenditure is the variable used in the text to denote total expenditure (i.e., it does not include mortgages payments, etc.). Income is the total amount of family income after taxes.

Table 7: Estimates of the Treatment Effect on Expenditure

	(1) Total Expenditure	(2) Nondurables	(3) Durables
Panel A: Full Sample			
Treated	0.897 (0.674)	0.449 (0.382)	0.448 (0.460)
Observations	1,383	1,383	1,383
Panel B: Excluding top & bottom 0.5%			
Treated	1.238* (0.573)	0.449 (0.371)	0.788* (0.343)
Observations	1,371	1,371	1,371
Panel C: Excluding top & bottom 1%			
Treated	1.048* (0.534)	0.388 (0.373)	0.660* (0.282)
Observations	1,357	1,357	1,357

Notes: Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ All regressions include the following control variables: number of adults, number of kids, total family income after taxes, reference person age, family type category dummies, and a time fixed effect.