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 MPXs, assigning 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 to 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 when 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, transitory income shock? This question is central to the design of 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 weights to households that receive larger income shocks. If households receiving larger income shocks spend a smaller fraction of them —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 *underestimating* 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, calibrating macroeconomic models, and 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 econometric theory, empirical evidence, and quantitative modeling to argue that we may underestimate both quantities, depending on the type of one-time income shock. 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 size of the shock. 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 derive theoretical results that formally establish this linearity bias in settings where income shocks are non-negative and can be assumed to be randomly assigned. I prove that widely used linear estimators, such as OLS, which mechanically place greater weight on households that receive larger shocks, produce downward-biased estimates when 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 examine 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 the models used in this paper, the distribution of winnings is simulated 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 0.18. In contrast, the OLS estimate is just 0.09, underestimating the true average MPX by 50% and the MPX at the average shock (0.15) by 40%. This large negative bias stems from the concavity of the expenditure function, as 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 the durable goods 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 provide partial identification bounds for both the average MPX and the average MPX out of the average shock. These bounds are derived by combining the biased linear estimators with two assumptions: first, that the expenditure or consumption function of the household facing the temporary shock is, on average, non-decreasing, and second, that there is a known *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; Lewis et al., 2024; Orchard et al., 2025) using the Consumer Expenditure Survey (CEX) data. My empirical analysis focuses on households that received the rebate by mail, for whom the timing of the rebate was effectively randomized (Parker et al., 2013). This randomization allows for a clean comparison between a "treatment" group (households that have received the check) and a "control" group (households that have not yet received the check) within the same period. 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. The framework also provides bounds for the counterfactual MPX from a uniform transfer equal to the average shock; under the same assumption, its estimated upper bound is 1.03.

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 0.21 to 0.39 for

[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 et al. (2025) refer to this parameter as the implied MPX.

nondurables, from 0.53 to 0.66 for durables, and from 0.73 to 1.05 for total expenditure in my preferred specification.

To explain these empirical findings —specifically the large contemporaneous MPX, concentrated mostly 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).

This endogenous financing margin is introduced specifically to rationalize the empirical findings, as it captures how liquidity-constrained and low-income households are more likely to finance new durable goods with debt, a channel that generates a high durable MPX for this group. In fact, the model shows that poorer households adjust their durable goods stock mostly through debt, while richer households prefer to use their disposable liquidity. This behavior can 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 (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 two 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, the paper 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 et al. (2018); Słoczyński (2022); Callaway et al. (2025)). However, its specific focus on the marginal propensity to spend allows for a more precise characterization of the bias by using assumptions drawn from modern macroeconomic models.

Third, this paper contributes to the recent literature on heterogeneous-agent models with durable goods. Within this literature, Berger and Vavra (2015) study the differential response of aggregate durable expenditure across the business cycle. McKay and Wieland (2021) focus on the intertemporal trade-off in aggregate demand driven by durable goods 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) analyze the effect of aggregate credit shocks on durables, with a focus on cars. In their model, the durable good is a discrete variable, whereas the model in this paper features a continuous choice for the durable stock, along with an endogenous financing margin.

The modeling of this financing margin is informed by Murphy (2024) and Sciacovelli (2024). In the analysis of Sciacovelli (2024), the durable good represents the housing stock, and the focus is on the effect of monetary policy when households have adjustable-rate mortgages. Murphy (2024) highlights how liquidity constraints influence loan duration decisions for car purchases, showing that constrained consumers prefer longer loan terms. Following these approaches, the model expands the framework of Beraja and Zorzi (2025) by treating the borrowing amount as a continuous variable. This design is intended to capture the interplay between liquidity constraints and financing decisions for durable goods purchases. This provides a channel to generate a high MPX among liquidity-constrained households and reveals significant intertemporal trade-offs of targeted stimulus policies.

Moreover, the paper contributes to this literature by incorporating insights from heterogeneous-agent macroeconomic models into reduced-form econometric analysis, sharpening the interpretation of empirical results on the average marginal propensity to spend.

Structure of the paper. Section 2 describes the linearity bias in commonly used estimators for the average MPX. Section 3 quantifies this bias for independent shocks using calibrated heterogeneous-agent models. Section 4 applies the framework to the 2008 U.S. Tax Rebate. Section 5 presents the heterogeneous-agent model with durable goods and endogenous financing. Section 6 concludes.

2 The linearity bias

Heterogeneity in the MPX and MPC represents a central feature of modern macroeconomics, emerging both as a robust empirical finding and as a core element of heterogeneous-agent models. Empirical studies document significant heterogeneity 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; Fagereng et al., 2021; 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 through additional state variables such as of the stock of durables and the presence of adjustment costs.

An additional 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 decreases as the size of the shock increases. This pattern need not hold if the shock depends on other consumer characteristics that also affect consumption. A targeted shock, such as a government transfer aimed at low-income consumers, can produce MPCs that do not necessarily decrease with shock size. Moreover, recent work by Beraja and Zorzi (2025) shows that even a model with durables can feature a concave average expenditure function

[6]By construction, the reported MPC in the survey of Jappelli and Pistaferri (2014) lies 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, $\sqrt{(1-0.476)(0.476)} \approx 0.499$.

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 may be inappropriate for estimating the MPX.

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 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 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

[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.

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, instrumental variables, and Diff-in-Diff— can be represented as weighted averages of heterogeneous marginal responses under the appropriate assumptions (e.g., Yitzhaki, 1996; Angrist et al., 2000; Heckman and Vytlacil, 2005; Mogstad et al., 2018; Callaway et al., 2025).

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, $\mathrm{E}[MPX \cdot W | X > 0]$, 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, $\mathrm{E}[MPX | X > 0]$, 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 output and the true average MPX the *linearity bias*. This bias is not a failure of the estimator itself, but rather a challenge of interpretation. Recognizing this distinction is relevant when using these estimates for fiscal policy counterfactuals or to calibrate macroeconomic models, where the implicit weighting may not align with the policy or modeling objective.

Table 1 summarizes the estimators studied in this paper that have this representation, depending on the type of shock: if the shocks can be assumed as-if they randomly assigned ($X \perp\!\!\!\perp U$) or if the shocks depend on the affected 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 $\tau \left(\frac{\mathrm{E}[\Delta C | X > 0]}{\mathrm{E}[X | X > 0]} \right)$ recover a weighted average of the MPX. While both estimators place greater emphasis on larger shocks, their weighting schemes differ. The OLS are not guaranteed to be positive, whereas the weights for the Wald-type estimator are non-negative and always increasing in the shock size. This latter form is particularly common and can be

[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	$\frac{\text{Cov}(C,X)}{\text{Var}(X)}$	$\frac{X(X-\text{E}[X])}{\text{Var}(X)}$
IV / DiD	$\frac{\text{E}[\Delta C \mid X>0]}{\text{E}[X \mid X>0]}$	$\frac{X}{\text{E}[X \mid X>0]}$
Panel B: Dependent Shocks ($X \not\perp\!\!\!\perp U$)		
IV / DiD	$\frac{\text{E}[\Delta C \mid X>0]}{\text{E}[X \mid X>0]}$	$\frac{X}{\text{E}[X \mid X>0]}$

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.

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 a discrete instrument Z that is independent of U and 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 shock size. The OLS estimator, in contrast, is omitted from this case, as it no longer yields a weighted average of unit-level MPXs.

To gain intuition about the linearity bias, consider the following separable model of household responses 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 $m(0) = 0$.

[10]The two-step approach would be necessary if, due to data limitations, the researcher observes only whether a unit was treated ($X_i > 0$ 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, $\text{E}[X|X > 0]$, must be obtained from other sources, such as the policy design or administrative records.

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 the baseline consumption differences across households 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

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

where $\mathrm{Cov}(h(U), X) = 0$ 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

[11]One could also be interested in weighted averages of the MPXs to run policy counterfactuals, such as income-weighted averages as in Auclert et al. (2024), though these 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, $\mathrm{E}[g(\bar{x}, U) - g(0, U)]$, 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, $m(\bar{x})/\bar{x}$.

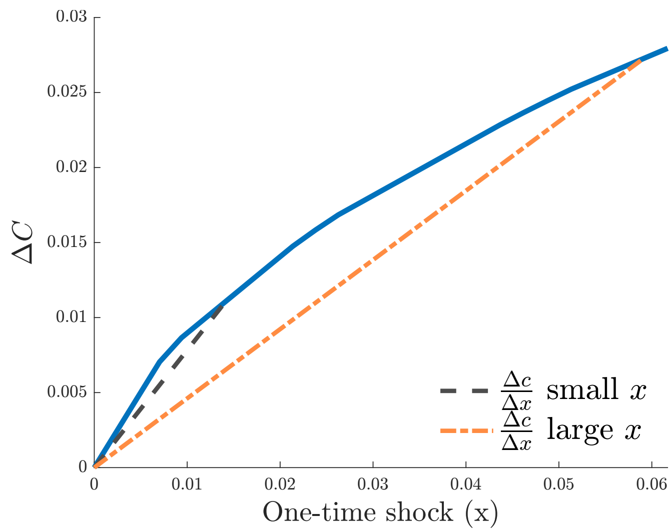


Figure 1: Differences in MPX for a concave (average) expenditure function.

Notes: This figure illustrates differences in the marginal propensity to spend (MPX) as a function of a random shock x. The concave average expenditure function implies that the MPX decreases with shock size, leading to a wedge between the unweighted average MPX and estimates that overweight larger shocks.

weight.

Even without imposing functional form assumptions on the response function in (1), the unweighted average MPX, $\mathrm{E}[MPX]$, provides a more transparent measure of the underlying structural response to an income shock. This estimand quantifies, on average, how much households spend out of a windfall, giving equal importance to the response of each household. A large estimated value for $\mathrm{E}[MPX]$ is consistent with several distinct patterns of underlying microeconomic behavior. It could reflect a homogeneous and widespread MPX across the population. Alternatively, it could be generated by a distribution characterized by substantial heterogeneity, where the high average is driven by households with large spending responses. This includes, for instance, households that use the shock to partially finance a larger durable good purchase, a behavior that can result in an individual MPX exceeding one.

2.1 The Shock X is independent of U

For the analysis when shock X is independent of U, we can simply analyze the conditional expectation functions, since $\mathrm{E}[C|X = x] = \mathrm{E}[g(x, U)]$ and $\mathrm{E}[MPX|X = x] = \mathrm{E}[\frac{g(x,U)-g(0,U)}{x}]$. Figure 1 illustrates this case when the average expenditure is concave as a function of the income shock x and therefore the MPX out of smaller shocks is larger than the MPX out of larger shocks.

Moreover, as is more common in practice, I consider the case where the shock has non-negative support, $X \geq 0$, and its distribution has a mass point at zero, meaning we can observe a proper "treatment" group (units receiving the income shock) and a "control" group (units receiving no income shock). Therefore, I focus on this case.[13]

Suppose we regress C on X. At the population level, the coefficient on X, β^{OLS}, will be a weighted average of the MPXs, with weights depending on X.[14] The Wald-like estimator $\tau = \frac{\mathrm{E}[\Delta C|X>0]}{\mathrm{E}[X|X>0]}$ (the "implied MPX") will also be a weighted average of MPXs, with weights depending on X.[15]

Proposition 1. *Suppose $X \geq 0$, with $p = \Pr(X > 0) \in (0,1)$, and that $X \perp\!\!\!\perp U$. Define the unit-level MPX for $X > 0$ as $\frac{\Delta C}{\Delta X} = \frac{g(X,U)-g(0,U)}{X}$.*

Then the OLS coefficient from a regression of C on X, β^{OLS}, and the implied MPX, τ, can both be expressed as weighted averages of the unit-level MPX, conditional on a positive shock:

$$\beta^{OLS} = \mathrm{E}\left[\frac{\Delta C}{\Delta X} W^{OLS} \middle| X > 0\right], \quad \textit{with weights } W^{OLS} = \frac{pX(X - \mathrm{E}[X])}{\mathrm{Var}(X)}$$
$$\tau = \mathrm{E}\left[\frac{\Delta C}{\Delta X} W^{\tau} \middle| X > 0\right], \quad \textit{with weights } W^{\tau} = \frac{X}{\mathrm{E}[X|X > 0]}$$

where the weights for both estimators have a conditional expectation of one: $\mathrm{E}[W^{OLS}|X > 0] = 1$ *and* $\mathrm{E}[W^{\tau}|X > 0] = 1$.

Proof. See appendix B.1 □

Proposition 1 reveals that these standard estimators do not recover the simple unweighted average MPX. Instead, the fact that the implicit weights are functions of the shock size, X, directly implies that the estimators are susceptible to bias and that the estimates may not be particularly informative. This result is formalized in the following corollary.

Corollary 1. *The OLS coefficient, β^{OLS}, and the implied MPX, τ, can be decomposed into the unweighted average MPX and a potential bias term. This bias term is given by the covariance*

[13]These two assumptions are not needed for the decomposition of the estimators as weighted averages of the MPXs.

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

[15]These representations hold even when we use the first difference of C, i.e., $C_t - C_{t-1}$, assuming $X_t \perp\!\!\!\perp U_{t-1}$.

between the unit-level MPX and the implicit weights of the estimator:

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

Corollary 1 shows that the bias is given by a covariance term. Under the independence assumption ($X \perp\!\!\!\perp U$), the sign and magnitude of this covariance are determined entirely by the shape of the conditional expectation function, $\mathrm{E}[\mathrm{MPX}|X = x]$, and the distribution of the shocks. It follows that the bias term vanishes under two sufficient conditions: (i) the MPX is invariant to the shock size and homogeneous across the population, or (ii) all positive shocks are of a uniform magnitude.[16]

Finally, the independence assumption would, in principle, permit the nonparametric estimation of the full conditional expectation function, $\mathrm{E}[C|X = x] = \mathrm{E}[g(x, U)]$. This would allow for the evaluation of the average response to a uniform transfer of any size x, as the conditional MPX could be recovered from this function. In practice, however, data limitations and small sample sizes may preclude the reliable estimation of this function.

2.2 The Shock X is not independent of U

Consider now the case where the shock X is not independent of household characteristics U, that is there is an endogenous selection into the shock. In this scenario, the conditional expectation function fails to identify the average causal effect of the income gain, as the observed average consumption for a given shock, $\mathrm{E}[C|X = x]$, no longer coincides with the true structural response, $\mathrm{E}[g(x, U)]$. A prime example is a targeted government transfer, where the transfer amount X is an explicit function of characteristics contained in U, such as income level or the number of children.

Regardless of the specific way in which the shock is correlated with household characteristics, an estimated average MPX in this setting may lack external validity. The estimand is no longer the average MPX for the general population, but rather the average MPX for the specific subpopulation receiving the shock. An average MPX identified from a transfer targeted to low-income households would be expected to overestimate the average response of the overall population to transfers of a similar magnitude.

Difference-in-differences or IV methods have been used to estimate an average MPX in this setting. By identifying and computing separately the average treatment on the treated

[16]The latter condition —a shock of uniform magnitude— is met in randomized controlled trials that assign a fixed transfer to all treated units, as in the 300 euros experiment of Boehm et al. (2025).

(ATT), $\mathrm{E}\left[g(X,U) - g(0,U)|X > 0\right]$, using difference-in-differences methods (e.g., Parker et al., 2013; Orchard et al., 2023; Borusyak et al., 2024, in the context of tax rebates), and the average size of the positive shock, $\mathrm{E}[X|X > 0]$, one can compute the Wald-type implied MPX by taking the ratio of these two quantities:

$$\tau = \frac{\mathrm{E}\left[g(X,U) - g(0,U)|X > 0\right]}{\mathrm{E}[X|X > 0]}. \tag{6}$$

The same parameter can be recovered using an IV estimator, provided one has a discrete instrument Z that is independent of U and satisfies the conditions $Z = 1 \implies X > 0$ and $Z = 0 \implies X = 0$.[17]

Similarly to the previous section, the same result applies here: the implied MPX is a weighted average of individual MPXs, and it is biased for the unweighted MPX, assuming heterogeneity in the MPXs and the shocks.[18]

Proposition 2. *Suppose $X \geq 0$ and $X \not\perp\!\!\!\perp U$. Define the unit-level MPX for $X > 0$ as $\frac{\Delta C}{\Delta X} = \frac{g(X,U) - g(0,U)}{X}$.*

Then the implied MPX, τ, can be expressed as weighted averages of the unit-level MPX, conditional on a positive shock:

$$\tau = \mathrm{E}\left[\frac{\Delta C}{\Delta X} W^{\tau} \middle| X > 0\right], \quad \textit{with weights } W^{\tau} = \frac{X}{\mathrm{E}[X|X > 0]}$$

where the weights have a conditional expectation of one: $\mathrm{E}[W^{\tau}|X > 0] = 1$.

Proof. Same as for Proposition 1. □

Given the weighted average representation, Corollary 1 applies directly here. However, because the shock is not randomly assigned in this case, the bias term must be interpreted as a *selection bias*. This bias reflects both the heterogeneity in MPXs and the specific assignment of shocks across households. Consequently, the bias is determined not only by how the MPX varies with the *size* of the shock, but also by *who* receives the shock.

Next, I explore what assumptions can be made to better characterize these estimators that yield a weighted average of the individual MPX.

[17] This means everybody is a *complier* in the language of the LATE framework (Imbens and Angrist, 1994; Angrist and Imbens, 1995).

[18] Similar decompositions can be found in Callaway et al. (2025) for the analysis of difference-in-differences with a continuous treatment.

2.3 Bias under assumptions of monotonicity, concavity, and boundedness

While precisely measuring the average MPX across different income shock sizes is challenging, empirical evidence suggests that the share of a shock that households spend declines with the magnitude of the shock.

The analysis of Norwegian lottery winnings by Fagereng et al. (2021) provides a clear illustration of this pattern. While their linear OLS specification on the full sample of prizes (up to \$150,000) yields an average yearly MPX of 0.52, the estimates are substantially higher when the regression is restricted to subsamples of smaller prizes. Specifically, the average MPX is 1.30 for the first quartile of winnings (\$1,100–\$2,070), falls to 0.97 for the second quartile (\$2,070–\$5,200), and to 0.69 for the third quartile (\$5,200–\$8,300). For the largest prizes (above \$8,300), the average MPX is 0.51, similar to the full-sample estimate.

Similarly, using survey data, Andre et al. (2025) find that the average MPC declines with the size of the shock. They estimate, for instance, an average MPC of 0.49 for a \$100 shock, which falls to 0.35 for a \$1,000 shock, and further to 0.30 for a \$10,000 shock.

This empirical pattern is consistent with theoretical predictions from heterogeneous-agent models. In canonical heterogeneous-agent models with only nondurable goods, the consumption function for each household is concave and increasing (Carroll and Kimball, 1996). While this property may not hold at the individual level in models with durable goods, Beraja and Zorzi (2025) demonstrate that the introduction of taste shocks restores concavity to the *average* expenditure function.

Motivated by this theoretical and empirical evidence, the following analysis relies on monotonicity and concavity of the average expenditure function. These assumptions serve two purposes: they maintain consistency between the empirical framework and standard economic theory, and they provide sufficient structure to formally characterize the bias of the estimators.

Using the notation from before, let $C = g(X, U)$ be the spending function of a unit. I now list the assumptions about *counterfactual objects*. For consistency with the setting introduced in Section 2, I maintain the assumption of non-negative shocks.

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. If X is independent of U, the assumption means that the conditional expectation function $\mathrm{E}[C|X=x]$ is increasing in x. Moreover, Assumption 1 implies that the MPX is a non-negative random variable. That is, assuming $X>0$, letting $x''=x$ and $x'=0$, we have

$$\mathrm{E}\left[\frac{\Delta C}{\Delta X}\middle|X=x\right]=\mathrm{E}\left[\frac{g(x,U)-g(0,U)}{x}\middle|X=x\right]\geq 0. \tag{7}$$

Assumption 2 (Average Concavity)**.** *Let $x'\geq 0$ and U be a random vector. Then*

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

is concave in x' for all x.

Again, when X is independent of U, the assumption means that the conditional expectation function $\mathrm{E}[C|X=x]$ is concave in x and that $\mathrm{E}[MPX|X=x]$ is decreasing in x. More generally, an implication of Assumption 2 is that the counterfactual MPX of units affected by a shock $X=x$ would be smaller if they were to receive a larger shock.

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 heterogeneous-agent model with a nondurable good (Carroll and Kimball, 1996). When a durable good is added, this is not necessarily the case if units adjust the durable good infrequently. Thus, I use a weaker assumption that holds on average as in Beraja and Zorzi (2025). The concavity and monotonicity assumptions on the individual response function to a treatment are 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.

Independent shocks Under these assumptions, in the case of an independent shock, the OLS estimator and the implied MPX τ are a lower bound for the average unweighted MPX and the counterfactual MPX if everybody received a shock equal to the average positive shock $\bar{X}=\mathrm{E}[X|X>0]$.

Proposition 3. *Suppose $X\perp\!\!\!\perp U$, $X\geq 0$, and $\Pr(X>0)=p\in(0,1)$. Under Assumptions 1 and 2, the OLS regression coefficient β^{OLS} and the implied MPX τ are both lower bounds for the average MPX. In particular,*

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

Proof. See Appendix B.2. □

The result follows naturally from the maintained assumptions. Assumptions 1 and 2 imply that the average expenditure response to shocks is weakly increasing and concave. This, in turn, implies that the MPX, while positive, is a decreasing function of the shock size. Because these estimators are weighted averages of individual MPXs with weights that increase in the magnitude of the shock, they systematically overweight the smaller MPXs associated with larger shocks. As a result, the weighted average produced by these estimators will necessarily be smaller than the unweighted average MPX.

The difference in the implicit weighting schemes explains the ordering between τ and β^{OLS}. The OLS estimator places even greater relative weight on larger shocks compared to the weighting scheme for τ. Since concavity implies that these larger shocks are associated with the lowest MPXs, the downward bias from the OLS estimator is more severe, resulting in the inequality $\beta^{OLS} \leq \tau$.

Moreover, we can also relate the estimates to the counterfactual MPX out of the average shock $\bar{X} = \mathrm{E}[X|X > 0]$.

Proposition 4. *Suppose $X \perp\!\!\!\perp U$, $X \geq 0$ and $Pr(X > 0) = p \in (0, 1)$. Let $\bar{X} = \mathrm{E}[X|X > 0]$. Under assumptions 1 and 2, the OLS regression coefficient β^{OLS} and the implied MPX τ are both lower bounds for the counterfactual average MPX if everybody received a shock equal to $\bar{X}$:*

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

Proof. See appendix B.3. □

This result follows again from the concavity of the average expenditure, and the ordering between the two estimators.

Dependent Shock If the shock X is not independent of U, we cannot conclude whether the implied MPX τ estimator is a lower bound for the average MPC using Assumptions 1 and 2. Unlike the previous case, where the covariance term was necessarily negative due to concavity, in this case it could be either positive or negative. Figure 2 illustrates an example with a *positive* bias where the shock is positively correlated with the MPX, even if the expenditure responses are concave. This means that researchers would need to make specific assumptions about the sign of the covariance term to draw definitive conclusions.

However, it is still possible to use the implied MPX to compute bounds for the average MPX and the counterfactual MPX if everybody in the treated population (i.e., those

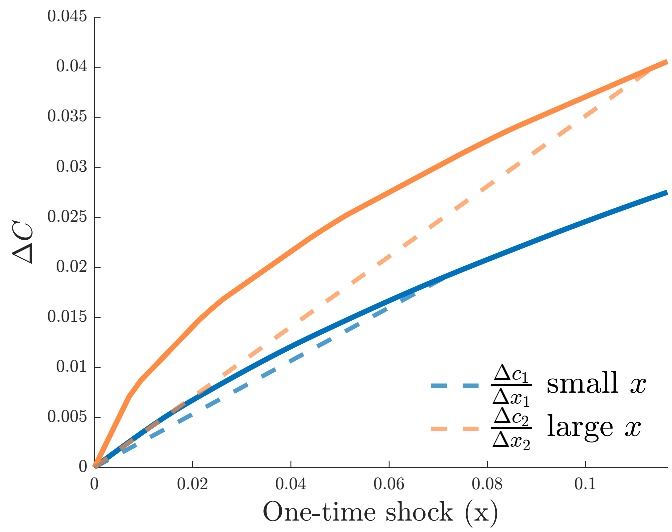


Figure 2: Differences in MPX after a non-random shock.

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.

for which $X > 0$) received a shock equal to the average positive shock, by making the assumption that the MPX is bounded in addition to being non-negative:

Assumption 3 (Boundedness)**.** *For every $x \geq 0$, the MPX is bounded on average:*

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

Proof. See appendix B.3. □

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 $\bar{U} = 1$. 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 the individual expenditure response can exceed the income shock. For this reason, I use the weaker assumption on the average response.

By using Assumption 3, the covariance term composing the bias in Corollary 1 can be bounded. The covariance term depends on the standard deviation of the MPX, which is unobserved. However, the Cauchy-Schwarz inequality circumvents this issue by bounding its variance. For any random variable X bounded in an interval $[a,b]$, its variance satisfies $\mathrm{Var}(X) \leq (b - \mathrm{E}[X])(\mathrm{E}[X] - a)$. Therefore, using this fact and Assumptions 1 and 3, we have the following:

Proposition 5. *Let* $X \geq 0$ *and* τ *denote the implied MPX. Let* $\kappa = \frac{\sqrt{\mathrm{Var}(X|X>0)}}{\mathrm{E}[X|X>0]}$. *Under Assumptions 1 and 3, we have*

$$|\tau - \mathrm{E}\left[MPX|X>0\right]| \leq \kappa\sqrt{\left(U - \mathrm{E}\left[MPX|X>0\right]\right)\mathrm{E}\left[MPX|X>0\right]}.$$

Thus, assuming a real solution,

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

Proof. See appendix B.4. □

Since τ and κ can be estimated from the data, this inequality applies to the estimators $\hat{\tau}$ and $\hat{\kappa}$. However, sample variability may preclude a real solution.

Using the same assumptions, we can also bound the counterfactual average MPX out of any shock of size x across the treated population.[19] Focusing on $x = \mathrm{E}[X|X>0]$, we have

Proposition 6. *Let* $X \geq 0$ *and* τ *denote the implied MPX. Let* $\bar{X} = \mathrm{E}[X|X>0]$. *Let* A *denote the event* $\{0 < X \leq \bar{X}\}$. *Let* B *denote the event* $\{X \geq \bar{X}\}$ *Then the the counterfactual MPX out of the shock* $\mathrm{E}\left[\frac{g(\bar{X},U)-g(0,U)}{\bar{X}}\right]$ *satisfies the following, with all expectation referring to the treated population:*

$$\mathrm{E}\left[\frac{g(\bar{X},U)-g(0,U)}{\bar{X}}\right] \leq \max\left\{U, \frac{(\bar{K}_0+\bar{K}_1-\mathrm{E}[g(0,U)])}{\bar{X}}\right\}$$

where $\bar{K}_0 = \mathrm{E}[g(X,U)+U(X-\bar{X})|A]Pr(A)$ *and* $\bar{K}_1 = \mathrm{E}[g(X,U)|B]Pr(B)$, *and*

$$\mathrm{E}\left[\frac{g(\bar{X},U)-g(0,U)}{\bar{X}}\right] \geq \min\left\{0, \frac{(\underline{K}_0+\underline{K}_1-\mathrm{E}[g(0,U)])}{\bar{X}}\right\},$$

where $\underline{K}_0 = \mathrm{E}[g(X,U)|A]Pr(A)$ *and* $\underline{K}_1 = \mathrm{E}[g(X,U)-U(X-\bar{X})|B]Pr(B)$.

Proof. See appendix B.5. □

Again, all the quantities can be estimated from the data in both the difference-in-differences and IV settings described above.[20]

[19] One could also use a concavity assumption as in Manski (1997) to bound this quantity. However, the bounds may not be very informative if the size of the shock is small compared to the average expenditure conditional on the shock.

[20] See, for instance, Borusyak et al. (2024); Callaway et al. (2025) for the "parallel trend" assumptions needed to estimate the counterfactual $\mathrm{E}[g(0,U)|X>0]$ in difference-in-differences settings.

A natural question is how large the bias of these estimators is for those quantities. I analyze the economic significance of the bias in two settings, depending on whether the shock is independent of household characteristics. For the first case, I use the setting of Fagereng et al. (2021), who study lottery winnings in Norway. For the second, I revisit the 2008 U.S. tax rebate case, which has been extensively studied in the literature (e.g., Parker et al., 2013; Misra and Surico, 2014; Kaplan and Violante, 2014; Lewis et al., 2024; Orchard et al., 2025).

3 Quantifying the bias for independent shocks

To quantify the potential bias inherent in the OLS and implied MPX estimators under the key assumptions of monotonicity (1) and concavity (2), this section simulates consumption responses to a quasi-random shock within two heterogeneous-agent models. The shock distribution is calibrated to replicate the Norwegian lottery winnings data from Fagereng et al. (2021).

The first model is a canonical one-asset HA framework with only nondurable consumption, while the second incorporates both nondurable and durable goods. The model with durable goods is fully introduced in Section 5. The model with nondurables is the canonical one-asset heterogeneous-agent model.[21]

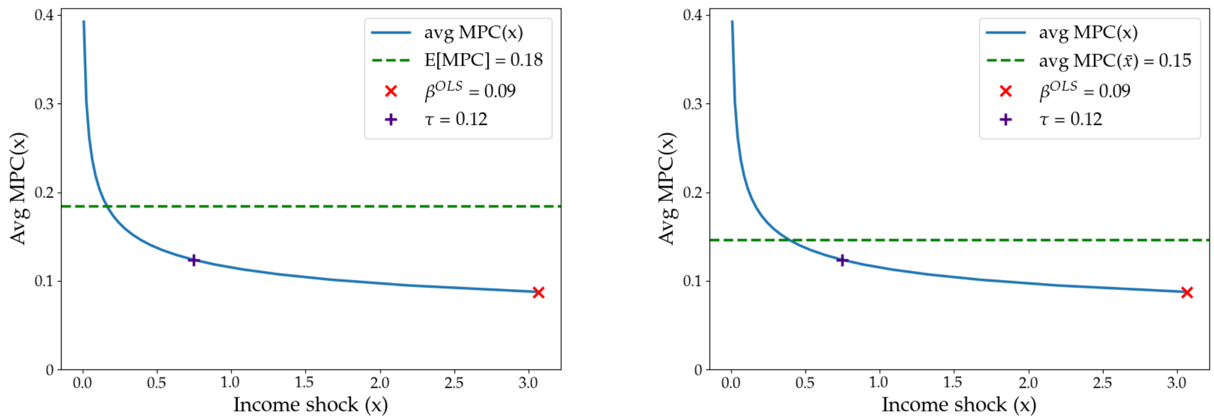
The income shock process in the model is designed to replicate the empirical distribution of lottery prizes. The prize distribution is modeled using a truncated log-normal distribution, calibrated to match the support (prizes between \$1,100 and \$150,000), mean (\$9,240), and standard deviation (\$16,130) of the Norwegian data. To reconcile the annual frequency of the data with the quarterly model, all prize amounts are divided by four, and all quantities are normalized by aggregate quarterly income. The estimates are computed from a simulation of one million households drawn from the steady-state distribution, with a win probability of 0.25 to match the winner-to-nonwinner ratio in Fagereng et al. (2021).

Figure 3 plots the bias of the estimators for both models. The bias is calculated relative to two distinct estimands of interest: the unweighted average MPX and the MPX evaluated at the average positive shock size.

The simulation results from the nondurable consumption model (Panel A) reveal a

[21]The model with nondurable consumption is a special case of the model from Section 5, corresponding to a specification with zero utility from durables, no taste shocks, and a no-borrowing constraint. However, for numerical accuracy, the model is solved separately. The common parameters across both models are γ, ρ, σ_u, and r, as detailed in Table 7; in the nondurable model, a discount factor of $\beta = 0.94$ is required to match the asset-to-income target.

Panel A: Model with nondurables



Panel B: Model with durables

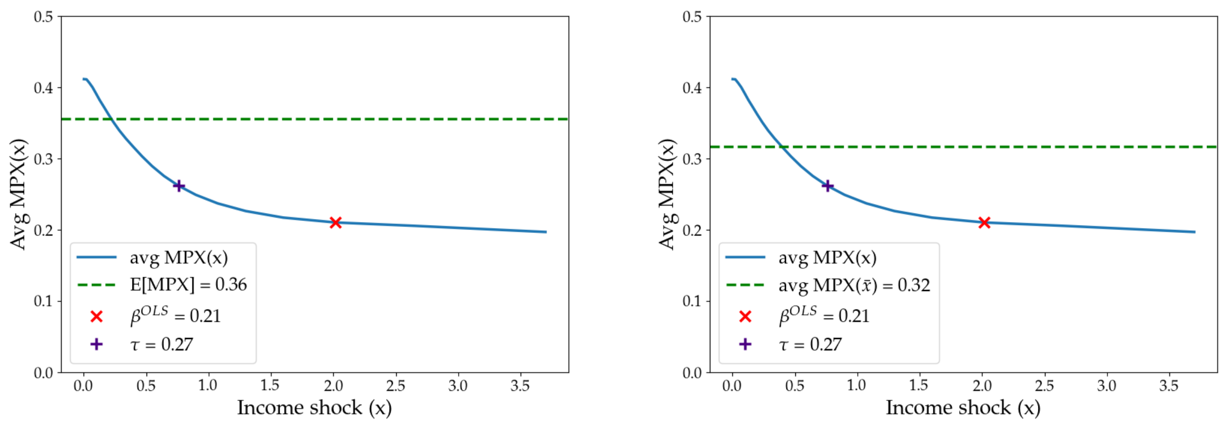


Figure 3: Estimator bias in HA Models with and without durable goods

Notes: Panel A displays results for the nondurables consumption model. The left and right plots show the bias of the OLS (β^{OLS}) and implied MPX (τ) estimators, relative to the unweighted average MPC and the MPC at the average shock size, respectively. Panel B displays the analogous results for the durable goods model, showing the bias of the estimators relative to the unweighted average MPX and the MPX at the average shock size. $\bar{x}$ is the average (positive) income shock.

substantial downward bias. The OLS estimate of the marginal propensity to consume is $\beta^{OLS} = 0.09$, which is only half of the true unweighted average MPC of 0.18, a 50% downward bias. The bias remains economically significant when compared to the MPC at the average prize size (0.15), with OLS underestimating this value by 0.06, or 40%. Reflecting the theoretical ordering between the two estimators, the bias for the implied MPC ($\tau = 0.12$) is smaller but still substantial: it underestimates the unweighted MPC by 33% and the MPC at the average shock by 20%. A key implication of this concavity-driven bias is that both estimators severely underestimate the MPC for shocks smaller than the average prize.

In the model with durable goods (Panel B), the percentage bias is smaller for both estimators. The OLS estimate is $\beta^{OLS} = 0.21$ and the implied MPX is $\tau = 0.27$, while the unweighted average MPX is 0.36 and the MPX at the average shock size is 0.32. The OLS estimate is therefore biased downwards by 0.15 (42%) relative to the unweighted MPX and by 0.11 (34%) relative to the MPX at the average shock. The bias of the implied MPX is again smaller, underestimating the two targets by 0.09 (25%) and 0.05 (16%), respectively. The smaller bias in the durable goods model is explained by a less concave average expenditure function, meaning the average MPX declines less steeply as a function of the shock size. Nonetheless, even in this case, both estimators remain poor approximations for the average response to smaller-than-average income shocks.

4 The 2008 U.S. tax rebate case

The 2008 U.S. 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 analysis, I 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 amount of each household 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. 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 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 who 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).[22]

The randomization of the rebate 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:

$$Z_t = \begin{cases} 1 & \text{if } X_t > 0, \\ 0 & \text{otherwise.} \end{cases} \tag{8}$$

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

$$X = \max_{t>0} X_t = \sum_{t>0} Z_t X_t. \tag{9}$$

The key randomization assumption —that the timing of the rebate is quasi-random— requires that the distribution of positive rebates is stable over time ($F_{X|X>0,t}(c)$ is constant across the relevant periods t) and that the timing is independent of other determinants of expenditure, U_t. These two conditions jointly imply the formal independence assumption: $Z_t \perp\!\!\!\perp (X, U_t)$.

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. My analysis is therefore restricted to this subsample.[23] 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.

Leveraging the randomization of the rebate timing allows for the identification of the

[22] 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)).

[23] That is, the sample includes only households that received the rebate between May and July 2008 and did not report multiple rebates across different periods.

average treatment effect on the treated (ATT) in period t:

$$\begin{aligned} ATT_t &= \mathrm{E}[\Delta C_t | X_t > 0] \\ &= \mathrm{E}[g(X_t, U_t) - g(0, U_t) \mid X_t > 0] \\ &= \mathrm{E}[C_t \mid Z_t = 1] - \mathrm{E}[C_t \mid Z_t = 0]. \end{aligned} \tag{10}$$

This ATT serves as the numerator for the implied MPX, τ_t, which is constructed by normalizing the average expenditure change by the average rebate amount among recipients:

$$\tau_t = \frac{\mathrm{E}[\Delta C_t | X_t > 0]}{\mathrm{E}[X_t | X_t > 0]}. \tag{11}$$

As established in Section 2, this Wald-type estimator, τ_t, is biased for the true unweighted average MPX in period t. The ATT estimand is equivalent to that identified by the Difference-in-Differences estimators in Borusyak et al. (2024) and Orchard et al. (2023).

4.1 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 small, 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.[24]

Another issue is the presence of two distinct groups: households that received the rebate electronically and those that received it by mail.[25] 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

[24] Only 0.24% of the households reporting a rebate, received it in 2009

[25] 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	1353.25	6.177	0.000
Nondurables	648.91	6.680	0.000
Durables	704.34	4.028	0.000
Income (after tax)	12703.73	8.370	0.000
Age Ref.	-5.35	-10.804	0.000
N. Adults	-0.02	-0.677	0.499
N. Kids	0.17	4.495	0.000
Rebate	178.80	10.305	0.000
Observations	4067		

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 EFT 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 $12,700), are younger (the survey reference person is about five years younger), and receive a larger rebate (by about $180). Including households from the EFT 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 EFT 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 (10) and (11). 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 EFT group.[26]

[26]See Table 8 in appendix C for summary statistics by disbursement method.

The analysis considers three primary expenditure categories: nondurables, durables, and total expenditure (the sum of the two). The expenditure categories closely follow the classification in Parker et al. (2013). Nondurable expenditure includes the CEX categories of food, alcohol, tobacco, personal care, apparel, miscellaneous items, reading materials, public transportation, gas and motor oil, and health care (net of insurance payments).

Durable expenditure includes home furnishings, transportation purchases, and entertainment, but excludes major housing payments (such as mortgages, rent, and property taxes). These housing payments are excluded as they are unlikely to be affected by the stimulus checks and, following Beraja and Zorzi (2025), are not featured in the model. Additionally, vehicle sales are removed from transportation expenditure. Following Parker et al. (2013), the sample excludes households where the respondent is older than 85; the sample also excludes those where the respondent is younger than 18.

The CEX survey is highly susceptible to large outliers (Misra and Surico, 2014; Kaplan and Violante, 2014). To mitigate the influence of outliers, results are presented for both the full sample and for samples excluding the top and bottom 0.5% and 1% of total expenditure. All regressions are weighted using CEX sampling weights. The average rebate amount in the full sample is \$881.

Since $Z_t = 1 \iff X_t > 0$ and $Z_t = 0 \iff X_t = 0$ the implied MPX (11) can be estimated in one step using a 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.[27] The following regression is estimated:

$$Y_{it} = \alpha + \tau X_{it} + \gamma \cdot 1_{\text{July}} + \delta \cdot \text{Controls}_{it} + u_{it}, \tag{12}$$

where Z_{it} is an instrument for X_{it} and Y_{it} denotes expenditure for each category (nondurables, durables, and total). Because the probability of treatment differs between June and July, the inclusion of a time fixed effect for July (γ) allows τ to be interpreted as a weighted average of the period-specific implied MPXs (τ_t).[28] Other control variables, while not needed for identification, are added to improve precision.[29] An attractive feature of

[27] I drop the observations 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.

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

[29] 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 categories 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;

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.

this IV estimator is that standard errors for the implied MPX are 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) (Table 4), who estimated a coefficient of 0.308 for nondurables and a coefficient of 0.868 for total expenditure on the sample of all households receiving the rebate by check, after removing households that received a late rebate.

On the other hand, these results are much larger than the preferred estimate of 0.30 for total expenditure from Orchard et al. (2025). When using only households that ever received the rebate (Table 3, Panel B), they report an estimate of 0.82 in their "Homogeneous

Single consumers; Other families)

Table 4: Estimated bounds for the average MPX for different expenditure categories

	(1) Total Expenditure	(2) Nondurables	(3) Durables
Panel A: U = 1			
Average MPX	$[0.462, 0.898]$	$[0.069, 0.477]$	$[0.283, 0.760]$
Panel B: U = 2			
Average MPX	$[0.330, 1.258]$	$[0.045, 0.729]$	$[0.201, 1.070]$
Panel C: U = 3			
Average MPX	$[0.268, 1.547]$	$[0.034, 0.967]$	$[0.161, 1.338]$

Notes: Bounds for the average MPX. The bounds are computed using the estimate for $\hat{\tau}$ from Table 3 (Panel C) and an estimated coefficient of variation of the rebate size, $\hat{\kappa} = 0.543$. Each panel corresponds to a different assumption for the upper bound, U, on the conditional average MPX.

treatment" specification and 0.64 in their "Heterogeneous treatment" specification, which then decreases to 0.34 with the inclusion of additional controls. However, Orchard et al. (2025) use a different methodology and a more inclusive sample. Specifically, their sample combines households that received the rebate electronically and by mail and includes data from households reporting a rebate after July.

4.2 Partial identification of the MPX for the 2008 U.S. tax rebate

This section applies the results from Section 2.3 to partially identify two quantities of interest. Because the coefficient estimated in Section 4.1 is a weighted average across two periods, the quantities of interest are also defined as averages over this same time frame.

To compute the bounds, the analysis uses the coefficients from Table 3 (Panel C), estimated on the sample that excludes the top and bottom 1% of total expenditure. The estimated coefficient of variation of the rebate size is $\hat{\kappa} = \frac{\sqrt{\mathrm{Var}(X_t)}}{\mathrm{E}[X_t]} = 0.543$. Using Proposition 5, Table 4 reports the resulting bounds on the average MPX for each expenditure category under different assumptions for the upper bound, U, on the conditional average MPX.

The estimated bounds reveal that a wide range of average MPX values is consistent with the point estimate for the implied MPX. For example, for nondurables, assuming an upper bound of $U = 1$, the average MPX is bounded between 0.069 and 0.477. For total expenditure, however, even a higher upper bound of $U = 2$ yields a lower bound of 0.330, which remains a not-negligible estimate for a quarterly MPX.

The second object of interest is the counterfactual average MPX under a policy where

Table 5: Estimated bounds for the counterfactual average MPX out of the average rebate

	(1) Total Expenditure	(2) Nondurables	(3) Durables
Panel A: U = 1			
Average MPX (uniform rebate)	$[0.351, 0.802]$	$[0, 0.438]$	$[0.139, 0.590]$
Panel B: U = 2			
Average MPX (uniform rebate)	$[0.126, 1.027]$	$[0, 0.663]$	$[0, 0.815]$
Panel C: U = 3			
Average MPX (uniform rebate)	$[0, 1.253]$	$[0, 0.888]$	$[0, 1.041]$

Notes: Bounds for the counterfactual average MPX if all households (in the treated population) received a transfer equal to the average rebate size. The bounds are computed for different values of the upper bound, U, on the conditional average MPX. A lower bound of 0 indicates that the data-driven component of the bound was negative.

every household receives a rebate equal to the average size, $\bar{X}$ (i.e., $\mathrm{E}\left[\frac{g(\bar{X},U)-g(0,U)}{\bar{X}}\right]$). To compute the bounds for this quantity, Proposition 6 is applied using the same trimmed sample as before. The bounds are calculated separately for each of the two CEX interview months (June and July), and the simple average of the two is taken.[30] Table 5 reports the resulting bounds.

The resulting identification intervals are wide. The upper bound indicates the maximum potential effect of a uniform transfer, given the monotonicity and boundedness assumptions. For example, under the assumption that $U = 2$, the upper bound for the counterfactual MPX on total expenditure is 1.03, compared to the estimated implied MPX of 0.73. This suggests that a uniform transfer of this size could have generated a substantially larger partial-equilibrium expenditure response.

4.3 Estimating the average MPX for the 2008 U.S. Tax Rebate

The design of the 2008 U.S. Tax Rebate allows for the estimation of the average MPX using an imputation estimator. This is possible because the transfer size was not only predetermined based on 2007 tax returns but was also, crucially, independent of the randomized payment timing. This independence ensures that the rebate amount (X) is known ex-ante for the not-yet-treated control group, providing the information required for imputation. 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*

[30] The implicit weights across the two periods of equation 12 are very close to 0.5.

average MPX, $\mathrm{E}\left[\frac{\Delta C_t}{X} | X > 0\right]$, *is identified by*

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

Proof. See Appendix C.1 □

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, similarly to Equation (12), the average across the two periods is given by the β coefficient from the following OLS regression:

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

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 6 presents a direct comparison between the biased implied MPX (τ) and the unbiased average MPX, along with the resulting bias, for the sample that excludes the top and bottom 1% of the total expenditure distribution.[31] All estimates are computed on the same sample for each of the three expenditure categories.

The results show that the implied MPX underestimates the true average MPX across all three categories, providing evidence of a *negative* selection effect. For total expenditure, the unbiased average MPX is a high and statistically significant 1.048, an increase of 0.315 points (43%) from the already large implied MPX of 0.733. The point estimates for nondurables and durables also show large increases: nondurables rise by 0.182 points (88%) and durables by 0.132 points (25%), though these individual estimates are not statistically significant. Reflecting the imprecision in these underlying estimates, the measured bias (the difference between the two estimators) is itself not statistically distinguishable from zero for any category.

[31] See Table 9 in Appendix C for estimates on all three samples. Across all specifications, there is consistent evidence of a negative bias.

Table 6: Implied MPX, Average MPX, and the Linearity Bias

	(1) Implied MPX (τ)	(2) Average MPX	(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 computed excluding and top & bottom 1% of total expenditure. 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. Standard errors for the bias are computed using 3,000 bootstrap replications.

These unbiased estimates are consistent with the partial identification bounds presented in Table 4. The estimates for both nondurables (0.388) and durables (0.660) fall within the bounds identified under a tight upper bound of $U = 1$. However, the point estimate for total expenditure (1.048) is only consistent with the bounds if the upper bound on the conditional MPX is greater than one ($U > 1$). This suggests that, for at least some households, the spending response exceeded the rebate amount—a behavior consistent with debt-financed durable purchases.

As established, the sign of the bias between the implied MPX and the average MPX depends on the covariance between the individual MPX and the rebate amount. The rebate formula did not systematically target lower-income households, as the size of the rebate for each household was a function of family status (i.e., married and filing jointly), prior-year tax liability, the number of adults and children, and adjusted gross income. On the contrary, the binned scatterplot in Figure 4 provides evidence against such targeting. It shows an almost monotonic relationship between the rebate amount and the total after-tax income (which includes not only salary but also income from financial assets), after controlling linearly for the number of children and adults, with the rebate increasing until a very high level of income. Therefore, if one assumes that the MPX is negatively correlated with total income, the positive correlation between the rebate size and total income could explain the overall negative bias.[32]

[32] By the Law of Total Covariance, $\mathrm{Cov}(MPX, X) = \mathrm{Cov}(\mathrm{E}[\mathrm{MPX}|Z], \mathrm{E}[X|Z]) + \mathrm{E}\left[\mathrm{Cov}(\mathrm{MPX}, X|Z)\right]$, where Z is total after-tax income. The first term captures the covariance across different income groups, while the second term captures the average covariance within income groups. The first term is negative if the MPX falls with income while the rebate rises with it. Therefore, the overall covariance is negative if this first term dominates the second.

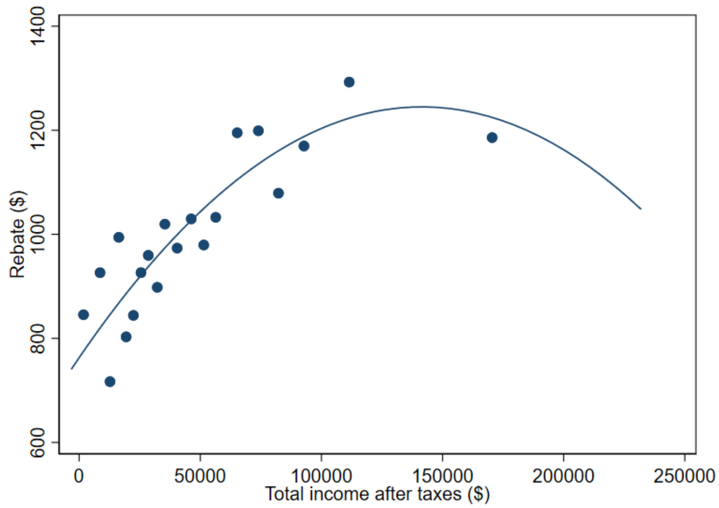


Figure 4: Rebate size and total income

Notes: Binned scatter plot with a quadratic fit showing the relationship between the size of the rebate and total family income after taxes, after controlling linearly for the number of children and adults in the household and for family type dummies, following the method of Cattaneo et al. (2024).

Additional insight can be gained by imposing structural assumptions on the response function in (1). Consider the additively separable model from Equation (3), $g(x, u) = m(x) + h(u)$. In this framework, the bias is determined entirely by the functional form of the common response component, $m(\cdot)$. If $m(\cdot)$ is concave, the MPX is a decreasing function of the rebate size for all households, which again guarantees a negative bias. However, this separable model has a strong and potentially unrealistic implication: the negative bias arises *solely* from the curvature of $m(\cdot)$. This means that the MPX declines with the rebate size at the same rate for each households, regardless of any other interaction between household characteristics and the shock.

Finally, an alternative explanation centers on household heterogeneity and lumpy durables adjustment. This mechanism reconciles the negative bias with the finding of a large unweighted average MPX, particularly for durable goods. Specifically, the high average MPX may not be a widespread phenomenon but could instead be driven by a subset of households that use the rebate as a down payment for a durable good purchase, such as a vehicle or a large appliance. Such behavior would result in individual MPXs far greater than one. While the previously discussed channels are plausible, the model developed in this paper is designed to explore this latter mechanism.

5 Model

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

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

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. Taste shocks and a non-convex adjustment cost induce lumpy adjustment of the durable good stock.

5.1 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:

$$u(c,d) = \frac{c^{1-\gamma}}{1-\gamma} + \nu \frac{d^{1-\eta}}{1-\eta}, \tag{14}$$

where $\gamma > 0$ and $\eta > 0$, and ν is the relative weight on utility from durables.[33]

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

[33]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 $\nu = \tilde{\nu}s^{1-\eta}$.

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 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:

$$b' = (1 - \mu)b. \tag{15}$$

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

$$r_b = r + \Delta, \tag{16}$$

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

$$y_{it} = (Y_t - T_t) \cdot \frac{e_{it}^{1-\theta}}{\mathrm{E}[e_{it}^{1-\theta}]}, \tag{17}$$

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.[34] Moreover, the log of productivity follows an AR(1) process with normal innovations u_{it} with standard deviation σ_u:

$$log(e_{it}) = \rho \log(e_{it-1}) + u_{it}. \tag{18}$$

The household 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.

[34] 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 supplies the same quantify of labor $n_{it} = N_t$. See Auclert, Rognlie, and Straub (2024) for more details.

The ex-ante value function, before the shocks are realized, is therefore:

$$V(s) = \mathrm{E}_\epsilon \left[\max\{ V^{\mathrm{a}}(s) - \kappa + \sigma_\epsilon \epsilon^{\mathrm{a}}, V^{\mathrm{n}}(s) + \sigma_\epsilon \epsilon^{\mathrm{n}} \} \right], \tag{19}$$

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 expenditure on durable goods.

The specific assumption of a Gumbel distribution is what underpins this tractability, as the expectation in (19) admits a closed-form solution given by the *log-sum* formula (McFadden, 1972):

$$V(s) = \sigma_\epsilon \log \left(\exp\left(\frac{V^{\mathrm{a}}(s) - \kappa}{\sigma_\epsilon} \right) + \exp\left(\frac{V^{\mathrm{n}}(s)}{\sigma_\epsilon} \right) \right). \tag{20}$$

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:

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

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

$$\begin{aligned} V^{\mathrm{n}}(e,d,a,b) &= \max_{c,a'} \frac{c^{1-\gamma}}{1-\gamma} + \nu \frac{d'^{1-\eta}}{1-\eta} + \beta\, \mathrm{E}[V(e',d',a',b')|e] \\ \text{s.t. } a' + c &= y(e) + (1+r)a - (r_b + \mu)b \\ a' &\geq 0 \\ d' &= (1-\delta)d \\ b' &= (1-\mu)b, \end{aligned} \tag{22}$$

while for those who adjust, it is

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

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

5.2 Calibration

The model is calibrated at a quarterly frequency. The definition of durable goods is chosen to align with the empirical MPX literature: it includes vehicles and household appliances but excludes housing. Following Beraja and Zorzi (2025), this approach differs from studies focused primarily on cars (Attanasio et al., 2022; Murphy, 2024), housing (Wong, 2020; Eichenbaum et al., 2022; Berger et al., 2024; Sciacovelli, 2024), or those that use a broader definition of durables inclusive of housing (Berger and Vavra, 2015; McKay and Wieland, 2021).

The exclusion of housing is motivated by two considerations. First, this narrower definition ensures consistency with the consumption data used to estimate the average MPX. Second, households are unlikely to adjust their housing stock in response to a typical one-time income shock, making its exclusion empirically appropriate. Table 7 summarizes all parameter values.

Calibrated parameters The calibrated parameters 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 average net liquid asset holdings to average annual income of 26%. This calibration target, taken from Beraja and Zorzi (2025), represents a low-liquidity benchmark for heterogeneous-agent models (Kaplan et al., 2018).

The utility weight ν is set to match the ratio of durable to nondurable expenditure of 0.21, calculated from BEA data for the years 1970–2019. To compute this ratio, the numerator is total expenditure on durables. The denominator is expenditure on nondurables and services, excluding spending on housing, utilities, financial services, and insurance. This

Table 7: Model parameters

Parameter	Description	Value	Target/Source
Calibrated parameters			
β	Discount factor	0.963	Liq Asset share = 0.26
ν	Relative durable weight	1.153	D spending/C = 0.21
κ	Adj. cost	0.202	Adj. probability = 29.8%
σ_ϵ	Adj. taste shock scale	0.165	See text
Externally set parameters			
η, γ	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)
θ	Tax progressivity	0.181	Heathcote et al. (2017)
r	Risk-free real rate	0.25%	Beraja and Zorzi (2025)
$r_b - r$	Real borrowing rate spread	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 and the externally set parameters of the model. See Section 5.2 for details.

exclusion ensures greater consistency with empirical MPX measures and reflects that the model abstracts from housing decisions.

A crucial element in the model is the steady-state (unconditional) probability of durable adjustment. The adjustment cost parameter κ is calibrated to match an average adjustment frequency of 29.8% per quarter, computed from the CEX for the years 1996–2019. The adjustment frequency is computed by identifying households that adjusted their cars or household furnishings and equipment, categorizing an adjustment as any expenditure over $250 (in 2017 U.S. dollars) in one of these categories.[35] This procedure yields a quarterly adjustment frequency consistent with the definition of durables used in the model and is higher than in other studies that use a broader or different definition (e.g., Berger and Vavra, 2015; McKay and Wieland, 2021; Murphy, 2024; Beraja and Zorzi, 2025).

The taste shock scale parameter σ_ϵ is calibrated to match the estimated expenditure response to the 2008 U.S. tax rebate. Specifically, the parameter is set such that a transfer of $881 —equal to the average rebate in the full sample— targeted to the bottom third of the income distribution generates a total average MPX of 0.733, consistent with the empirical estimate in Table 3, Panel C.[36] The resulting calibrated value is $\sigma_\epsilon = 0.165$.

[35]See Appendix A.1 for details.

[36]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 is implemented as a

This calibration strategy addresses two key challenges in mapping the model to the empirical setting. First, the estimates in Table 3 are based on households receiving rebates by check, a subgroup that differs significantly from those receiving rebates by electronic transfer (Table 2). The model does not explicitly distinguish between these payment methods, making direct mapping infeasible. Second, the actual rebate distribution depends heavily on household characteristics outside the model, such as marital status, family size, and number of children, making precise replication of the empirical distribution challenging. By targeting the average MPX for the bottom third of the income distribution, the calibration is designed to capture the large, debt-financed spending response that represents the key margin of interest, allowing for heterogeneous financing choices across the household distribution, while abstracting from these complications.

Drawing from the empirical results of Section 4.2, the model is also calibrated targeting the estimated upper bound of 1.027 (Table 5, Panel B); see Appendix D.1 for details.

Externally set parameters For the utility function in equation (14), 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 are adopted from Beraja and Zorzi (2025), as this paper shares their definition of durable goods: the borrowing limit is $\phi = 0.5856$, the quarterly real interest rate on liquid assets is $r = 0.25\%$, the interest rate spread is $\Delta = r_b - r = 0.38\%$, and the debt repayment speed is $\mu = 6.88\%$.

The calibration of the income process (equations (17) and (18)) follows Auclert et al. (2024). Thus, the income process persistence 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 the standard deviation of the log (gross) earnings in the US of 0.92. The tax progressivity parameter θ is set to 0.181 (Heathcote et al., 2017). Finally, aggregate output is normalized to one, while aggregate taxes T_t are set to equal the interest payments on net aggregate assets, $r(A - B)$.

5.3 Uniform vs targeted transfers

Consistent with Beraja and Zorzi (2025), the model generates a high overall MPX which remains elevated even for larger transfers (Figure 5). This result stems from two key

transfer to a specific set of income states. The policy is modeled as a transfer to the lowest income states that collectively constitute the bottom third (34%) of the stationary income distribution.

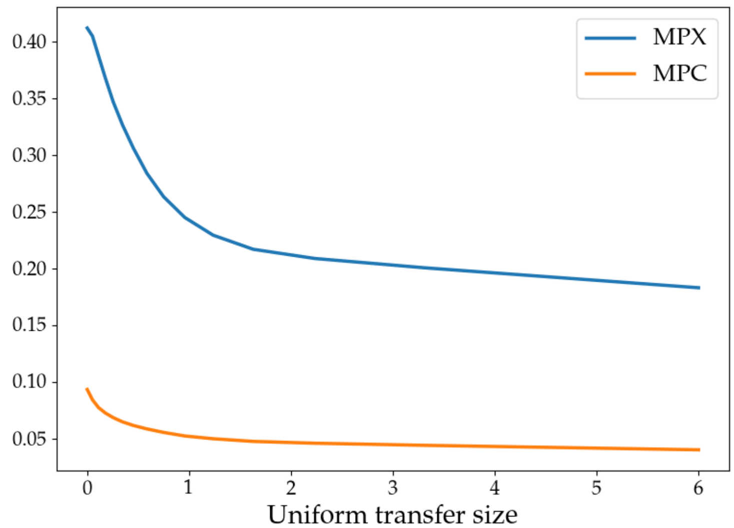


Figure 5: Average MPX, MPC

Notes: Average MPX and MPC after a uniform transfer.

features: the ability of households to finance durable goods purchases and a low-liquidity calibration. Furthermore, both the average MPX and the average MPC are decreasing functions of the uniform shock size, implying that the average total expenditure and average consumption functions are concave.

Having established the linearity bias inherent in standard estimators for both uncorrelated and correlated shocks, the analysis now turns to the intertemporal consequences of a large, durable-led spending response.

To illustrate these dynamics, 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. The transfer amount replicates the average rebate ($881) from Section 4.1.[37][38]

Understanding the intertemporal dynamics requires examining how households finance their durable purchases. Households exhibit substantial heterogeneity in their financing choices, which drives the differential spending patterns across the income distribution.

Figure 6 illustrates this by plotting the fraction of durable expenditure financed with

[37] 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.

[38] See Appendix D.1 for the results under the alternative calibration, matching the upper bound of the average MPX.

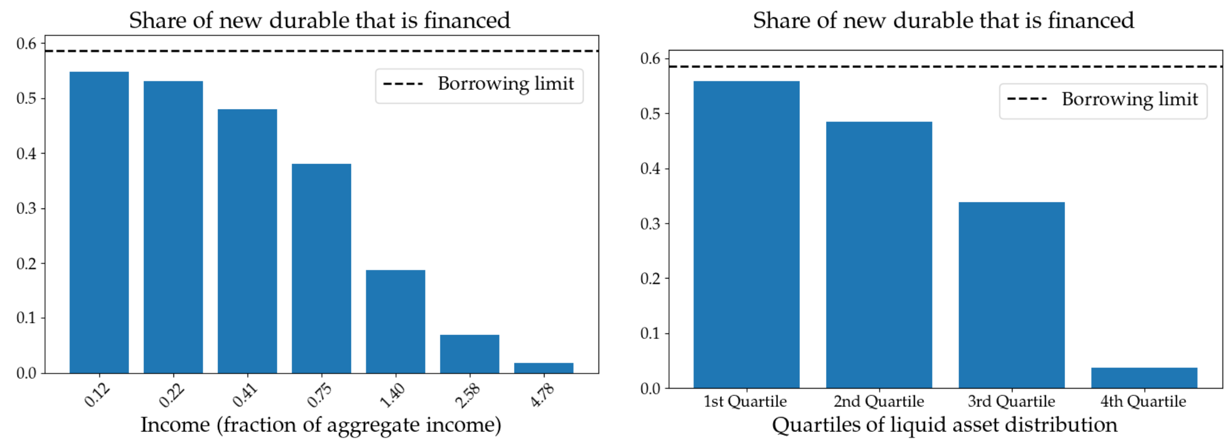


Figure 6: Share of the new durable good that is financed

Notes: Share of the new durable good that is financed across income groups (left) and quartiles of the liquid asset distribution (right), when households adjust their durable stock. The borrowing limit is the parameter ϕ, equal to 0.5856.

debt across both the income distribution (left) and the liquid asset distribution (right) at the moment of durable adjustment. This debt-financed share declines sharply and monotonically with income. For instance, households in the bottom third of the income distribution (the first three income states) finance their purchases almost entirely with debt, borrowing nearly to their limit ($\phi = 0.5856$). Conversely, the reliance on debt is minimal for higher-income households, with the debt-financed share approaching zero at the top of the income distribution.

A similar pattern emerges when households are sorted by their liquid asset holdings; more liquidity-constrained households finance a larger share of their durable purchases with debt. This financing pattern is a direct consequence of available 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 from internal resources, 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 8 illustrates the intertemporal average MPX under the two distinct policy experiments. 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. 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.

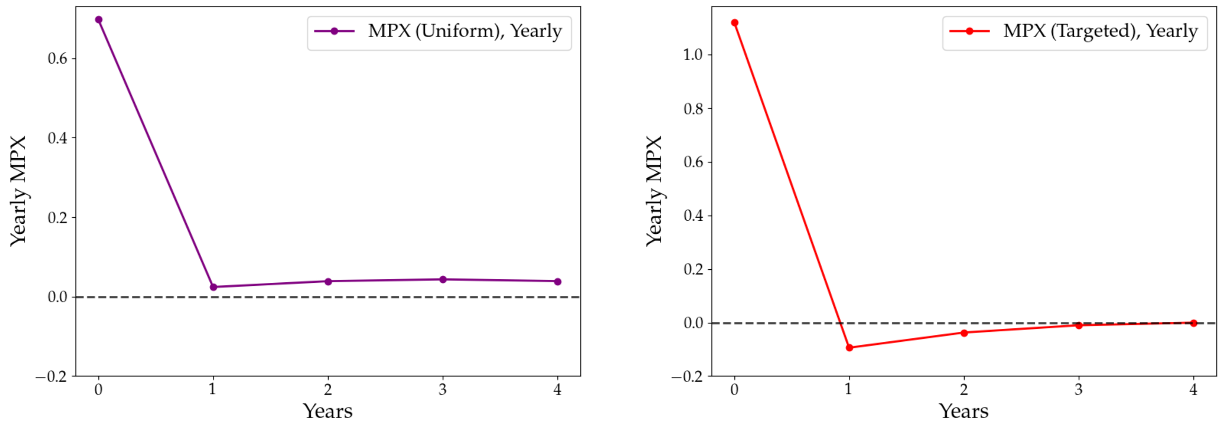


Figure 7: Intertemporal average MPX after a uniform transfer (left) or a targeted transfer (right)

Notes: The targeted transfer (right) is a transfer of $881 to the bottom third of the income distribution. The uniform transfer is a transfer of the same size across all households.

The mechanism behind these dynamics is tied to household durable good adjustments and use of credit. The targeted transfer 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 expenditure for some households to exceed the transfer amount. This initial consumption, however, is pulled forward from the future. The subsequent spending reversal reflects two forces: a reduced probability of repeat durable purchases and the burden of debt service, which constrains future resources.

In contrast, the universal transfer includes higher-income households who are less liquidity-constrained and hold larger existing durable stocks. Their lower responsiveness to the transfer and reduced reliance on debt financing dampens the aggregate MPX, yielding a smaller but more persistent response.

This comparison reveals a crucial trade-off in fiscal transfer design. When low-income consumers exhibit a high propensity to spend that is concentrated on durable goods and can finance purchases with debt, policymakers face an intertemporal choice: targeted transfers can generate large immediate stimulus at the cost of depressed future demand. Depending on policymakers time preferences and economic conditions, this front-loading may or may not be desirable.[39]

[39] This reasoning abstracts from general equilibrium effects. Furthermore, a front-loaded spending response can have desirable macroeconomic consequences, for instance by helping to self-finance deficit-financed fiscal shocks (Angeletos et al., 2024).

6 Conclusion

This paper analyzed the linearity bias in standard methods for estimating the average Marginal Propensity to Spend (MPX) from natural experiments. These methods yield a weighted average of individual MPXs, assigning greater weight to households that receive larger income shocks. This weighting scheme can produce misleading estimates for fiscal policy design and for understanding the structural features of household spending responses.

Combining theoretical insights from heterogeneous-agent models with empirical evidence, the analysis demonstrates that this bias can be economically significant. In settings with quasi-random shocks, the bias is shown to be negative. For shocks correlated with household characteristics, such as government transfers, the bias depends on the covariance between the shock and the unit-level MPX. A re-examination of the 2008 U.S. Tax Rebate finds evidence of a negative bias even in this setting, revealing a large average MPX concentrated primarily in durable goods.

To explain the large spending responses, I use a heterogeneous-agent model with durable goods and an endogenous financing margin. The model generates large MPXs because the financing margin allows households to use transfers as down payments for debt-financed durable purchases. Since households exhibit substantial heterogeneity in their financing choices, targeted transfers can have different intertemporal consequences than uniform transfers. Lower-income households, in particular, front-load expenditure by taking on debt, which reduces future demand. This trade-off is an important consideration for the design of targeted fiscal stimulus.

These findings have immediate implications for fiscal policy. First, policymakers using estimates from standard linear methods may substantially underestimate the effect of fiscal policy. Second, the concentration of spending on durable goods means that targeted transfers involve a trade-off between current and future demand. Third, heterogeneity in financing patterns across the income distribution suggests that the composition of transfer recipients is a first-order concern.

Future research could explore several dimensions. First, incorporating the endogenous financing margin into a general equilibrium framework would allow for a comparison of the aggregate effects of targeted versus universal transfer policies and a quantification of how the intertemporal trade-offs identified here affect aggregate demand dynamics. Second, exploring household-level heterogeneity in credit conditions (including not only access and interest rates but also borrowing limits) could refine predictions about which households respond most strongly to transfers. Finally, combining micro-level evidence on how the MPX varies with observable characteristics with macroeconomic models could enable a more precise design of targeted stimulus policies.

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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

Let $p = Pr(X > 0)$ and $MPX = \frac{\Delta C}{\Delta X} = \frac{g(X,U)-g(0,U)}{X}$ We have that

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

where $W^{OLS} = \frac{pX(X-\mathrm{E}[X])}{\mathrm{Var}(X)}$. Moreover, $\mathrm{E}[W^{OLS}|X>0] = 1$ as

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

For the implied MPX τ, we can divide and multiply the numerator by X

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

where $W^{\tau} = \frac{X}{\mathrm{E}[X|X>0]}$ and $\mathrm{E}[W^{\tau}|X>0] = 1$.

B.2 Proof of Proposition 3

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

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

By the Chebyshev integral inequality we have in probabilistic terms that

$$\mathrm{E}[f(X)g(X)] \geq \mathrm{E}[f(X)]\,\mathrm{E}[g(X)] \tag{24}$$

or conditioning on an event A,

$$\mathrm{E}[f(X)g(X)|A] \geq \mathrm{E}[f(X)|A]\,\mathrm{E}[g(X)|A] \tag{25}$$

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

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

where for the first inequality I used the fact that by concavity, $\mathrm{E}[\frac{\Delta C}{\Delta X}|X > 0] \geq \mathrm{E}[\frac{\Delta C}{\Delta X}|X > \mathrm{E}[X]/2]$ and I applied the Chebyshev integral inequality to the second integral and the fact that $\mathrm{Var}\,(X) = p\,\mathrm{E}[X^2|X>0] - p^2\,\mathrm{E}[X|X>0]$. Thus,

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

Since the implied MPX has the form $\tau = \mathrm{E}\left[h(X)\frac{X}{\mathrm{E[X|X>0]}}|X>0\right]$, direct application of (25) yields the inequality.

To establish the ordering between the two estimators, let $w^{\tau} = \frac{X}{\mathrm{E[X|X>0]}}$ be the weights for the implied MPX and $w^{OLS} = \frac{pX(X-\mathrm{E}[X])}{\mathrm{Var}\,X}$ be the weights for the OLS estimator.

Then $w^{OLS} \leq w^{\tau}$ if $X \leq \frac{\mathrm{Var}\,X}{\bar{X}}\frac{1}{p} + \frac{\bar{X}}{p}$. Moreover, the difference $w^{OLS} - w^{\tau}$ is increasing for $X \geq \frac{1}{2}\left(\frac{\operatorname{Var} X}{\bar{X}}\frac{1}{p}+\frac{\bar{X}}{p}\right)$. Let A be the event that X falls in this latter region. We then have:

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

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

B.3 Proof of Proposition 4

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 implied MPX τ we have

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

The inequality in Proposition 4 then follows from ordering between the two estimators.

B.4 Proof of Proposition 5

By corollary 1, $\tau = \mathrm{E}[MPX|X>0] + \frac{\operatorname{Cov}(MPX,X|X>0)}{\mathrm{E}[X|X>0]}$. Note also that the variance of any random variable X with support on a closed interval $[a,b]$ is bounded by the inequality: $\operatorname{Var}(X) \leq (b-\mathrm{E}[X])(\mathrm{E}[X]-a)$. By Assumptions 2 and 3, $\mathrm{E}[MPX|X] \in [0,U]$. Therefore by Cauchy-Schwarz inequality we have

$$\begin{aligned}
|\operatorname{Cov}(MPX,X|X>0)| &= |\operatorname{Cov}(E[MPX|X],X|X>0)| \\
&\leq \sqrt{\operatorname{Var}(E[MPX|X>0])\operatorname{Var}(X|X>0)} \\
&\leq \sqrt{(U-\mathrm{E}[MPX|X>0])(\mathrm{E}[MPX|X>0])}\sqrt{\operatorname{Var}(X|X>0)}.
\end{aligned}$$

Then we arrive at the solution by solving for $\mathrm{E}[MPX|X>0]$ the inequality given by

$$|\tau-\mathrm{E}[MPX|X>0]| \leq \frac{\sqrt{\operatorname{Var}(X|X>0)}}{\mathrm{E}[X|X>0]}\sqrt{(U-\mathrm{E}[MPX|X>0])\,\mathrm{E}[MPX|X>0]}$$

B.5 Proof of Proposition 6

First, by Assumptions 3 and 1 we have $\mathrm{E}\left[\frac{g(\bar{X},U)-g(0,U)}{\bar{X}}\right] \in [0, U]$. Consider the upper bound. The argument for the lower bound is symmetric. By Assumptions 3 we have

$$1_{\{0<X<\bar{X}\}}(\mathrm{E}[g(X,U)+U(\bar{X}-X)|X]-\mathrm{E}[g(\bar{X},U)|X]) \geq 0.$$

By Assumptions 1 we have

$$1_{\{X>\bar{X}\}}(\mathrm{E}[g(X,U)|X]-\mathrm{E}[g(\bar{X},U)|X]) \geq 0.$$

Taking expectations, subtracting $\mathrm{E}[g(0,U)]$ and dividing by $\bar{X}$ yields the result.

C The 2008 U.S. Tax Rebate Case

C.1 Proof of Lemma 1

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

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

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 quantities from the last line can be computed from the data.

C.2 Additional tables

Table 8: Summary Statistics by Rebate Disbursement Method

	Mean	Std. Dev.	Min	Max	Count
Electronic Transfer					
Expenditure	8763.54	6606.91	-22580	54975	1304
Nondurables	5463.28	2831.45	508	24638	1304
Durables	3300.26	5367.33	-30969	44949	1304
Income (after tax)	70296.93	45893.49	-36146	341079	1304
Age Ref.	45.17	14.04	18	81	1304
N. Adults	1.93	0.74	1	6	1304
N. Kids	0.85	1.15	0	8	1304
Rebate	1078.81	528.18	7	3000	1304
Mailed Check					
Expenditure	7410.29	6333.76	-14698	71704	2763
Nondurables	4814.37	3014.59	-3479	49700	2763
Durables	2595.92	4840.98	-16703	62683	2763
Income (after tax)	57866.94	49792.34	-26954	526051	2763
Age Ref.	50.53	16.15	18	82	2763
N. Adults	1.95	0.86	1	8	2763
N. Kids	0.67	1.12	0	8	2763
Rebate	900.00	490.70	6	3660	2763
Observations	4067				

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

Table 9: Estimates of the Average MPX

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

D Model

D.1 Alternative calibration: matching the upper bound

The partial equilibrium experiment from Section 5.3 is repeated under an alternative calibration. The model is recalibrated to generate a quarterly MPX of 1.027 for the targeted group. This target value corresponds to the empirical upper bound for the average MPX from a counterfactual in which all households receive the average rebate (Table 5, Panel B). The new parameter values are: $\beta = 0.963$, $\kappa = 0.045$, $\nu = 1.129$, and $\sigma_\epsilon = 0.02$.

Figure 8 plots the intertemporal MPX path for this calibration. The dynamic response is qualitatively similar to the baseline simulation. The key difference arises from the initial (quarterly) MPX for the targeted group being greater than one. This implies that these households front-load their expenditure, causing the spending reversal to begin within the first year. Consequently, the subsequent decline in expenditure below the steady state for this group is less pronounced than in the baseline case.

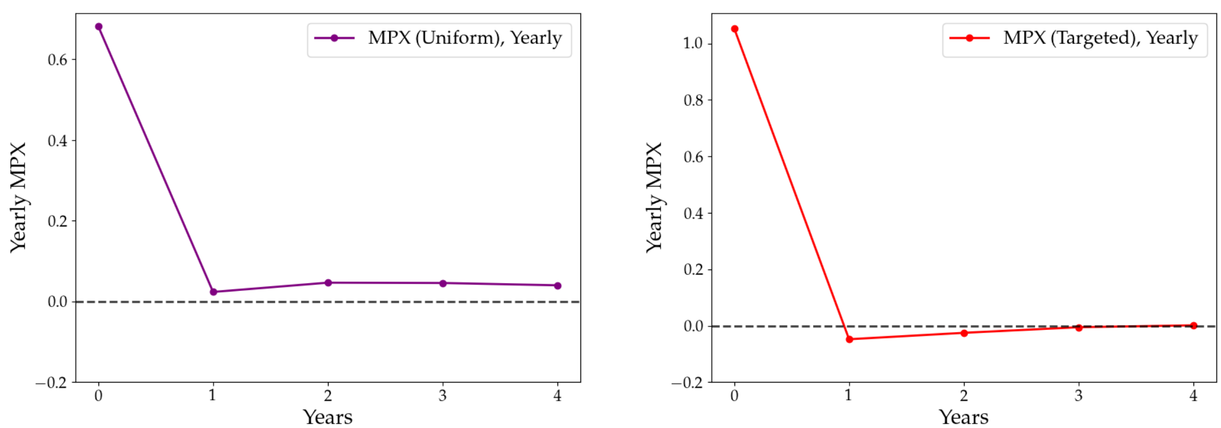

Figure 8: Intertemporal average MPX after a uniform transfer (left) or a targeted transfer (right) under the alternative calibration.