CONSUMPTION HETEROGENEITY: MICRO DRIVERS AND MACRO IMPLICATIONS

August 10, 2020

Edmund Crawley[1]

Federal Reserve Board

Andreas Kuchler[2]

Danmarks Nationalbank

Abstract

This paper explores the microfoundations of consumption models and quantifies the macro implications of consumption heterogeneity. We propose a new empirical method to estimate the response of consumption to permanent and transitory income shocks for different groups of households. We then apply this method to administrative data from Denmark. The large sample size, along with detailed household balance sheet information, allows us to finely divide the population along relevant dimensions. We find that households that stand to lose from an interest rate hike are significantly more responsive to income shocks than those that stand to gain. Following a 1-percentage-point interest rate increase, we estimate that consumption growth decreases by a 1/4 percentage point through this interest rate exposure channel alone, making this channel substantially larger than the intertemporal substitution channel that is at the core of representative agent New Keynesian models.

Keywords	Uncertainty, Consumption Dynamics, MPC
JEL codes	D12, D31, D91, E21

1 Introduction

How do differences in household consumption behavior affect the business cycle? Recent heterogeneous agent models suggest that wealth redistribution between households with high and low marginal propensity to consume (MPC) may play a dominant role in propagating macroeconomic shocks, particularly for monetary policy.[1] Testing the microfoundations of these models empirically, and quantifying the macroeconomic importance of redistribution, often boils down to measuring how MPCs vary systematically over dimensions such as wealth and exposure to interest rate movements. However, shortcomings both in the empirical methods used to measure MPCs and in the available data have limited the literature's ability to do this reliably.

In this paper we overcome some of these shortcomings. We present a new method to measure MPCs from income and consumption panel data, building upon that of Blundell, Pistaferri, and Preston (2008) (henceforth BPP). We then apply our method to different groups of households in administrative data from Denmark. We estimate the average MPC in the economy to be 0.5, close to estimates obtained from natural experiments. By contrast, using the BPP method on our data yields a much lower estimate of 0.12.

We follow BPP by imposing identifying restrictions on household income and consumption dynamics.[2] We make two improvements to the method. First, we explicitly model the time aggregated nature of income and consumption in our data. Time aggregation, overlooked in much of the household finance literature, can result in significant estimation bias if it is not explicitly modeled.[3] Second, we allow complete freedom in how consumption responds to a transitory income shock for up to two years, after which we assume no further response. This short-term response contrasts with the assumption in BPP that consumption follows a random walk. The random walk assumption, incompatible with high MPCs, was previously thought to be benign for estimates of consumption responses to transitory shocks. However, this is not true for time aggregated data.[4]

Our data consist of a panel of income and expenditure for the entire Danish population,

---

[1]See, for example, Kaplan, Moll, and Violante (2018); Garriga, Kydland, and Sustek (2017); and Greenwald (2018).

[2]The BPP method, and those closely related to it, has become a standard tool in the consumption literature. See, for example, Violante, Kaplan, and Weidner (2014); Auclert (2019); and Manovskii and Hryshko (2017)

[3]Crawley (2020) shows that estimates of the elasticity of consumption to transitory income shocks using the BPP method depend strongly on the nature of time aggregation in the data. He shows the estimate of the elasticity of consumption to transitory income increases from to 0.05—the estimate in BPP—to 0.25 when the time aggregated nature of the PSID data is modeled.

[4]Kaplan and Violante (2010) show that the estimate of the period one consumption response to transitory shocks in BPP is unaffected by assumptions on the path of consumption in following periods. This result does not hold for time aggregated data—in our data we would greatly overestimate MPCs if we assumed a random walk, see online appendix E.

along with details of the interest rate sensitivity of households' financial assets and liabilities that we require to estimate the redistribution effects of monetary policy. Income and wealth data are largely third-party reported to tax authorities and correspondingly accurate. Accordingly, we use the household budget constraint to back out expenditure from income and wealth.

Speaking to the microfoundations of consumption behavior, we uncover a clear negative monotonic relation between MPC and liquid wealth. We show that the sign of this relationship is in line with standard buffer-stock models, although the magnitude of MPCs, especially for households with the most liquid assets, is difficult to reconcile with theory. When we include illiquid wealth, such as housing, this monotonic relationship between wealth and MPC no longer holds: Those with close to zero net worth have higher MPCs than both those with negative and positive net worth. That liquid wealth is more predictive of MPCs than net worth is consistent with the wealthy hand-to-mouth model of Violante, Kaplan, and Weidner (2014).

Broadly speaking, in our data, we see three groups with distinct MPCs and exposure to interest rate movements: the "poor hand-to-mouth," with MPCs around 0.8, who own few assets, liquid or otherwise and are not directly exposed to interest rates; the "wealthy hand-to-mouth," with MPCs around 0.5, who typically own houses and have mortgages and other debts whose payments rise with interest rates; and the "wealthy," a smaller group with MPCs around 0.25, who typically own houses and also have large liquid bank balances, and whose income rises with interest rates.[5]

The strength of our method and data over previous studies can be seen when we quantify the size of monetary policy redistribution channels. We follow the decomposition of Auclert (2019) who reports sufficient statistics that determine the effect of monetary policy redistribution on aggregate consumption. However, being limited by the econometric methods he has at hand as well as by publicly available data sources, he finds it challenging to get a clear picture of how MPCs vary over the dimensions he identifies as relevant, such as unhedged interest rate exposure.

We estimate that a 1-percentage-point rise in the real interest rate, which redistributes wealth from the wealthy hand-to-mouth debtors who pay interest to the wealthy creditors who receive it, reduces aggregate consumption growth by 26 basis points through this redistribution channel alone. This channel is absent in representative agent models in which the intertemporal substitution channel instead dominates.

---

[5]These groups loosely line up with those of the same name in Violante, Kaplan, and Weidner (2014), who define wealthy hand-to-mouth as households with significant illiquid assets but little or no liquid assets. We observe that these three groups are naturally separated along the dimension of unhedged interest rate exposure. See figure VIII in section 6.

We believe the rich detail we are able to provide on the relationships between MPC, home ownership, liquidity, and interest rate exposure could be used to discipline microfounded macroeconomic models going forward. Furthermore, a growing number of large, high-quality panel datasets on income and consumption are becoming available to economists, which increases the value of robust econometric methods that can uncover household behavior.[6] Beyond the applications in this paper, our method has a wide variety of potential applications in the consumption, household finance, and labor literatures.

2 Background

The need for better methods and data to measure consumption behavior at the household level has grown with the increasing recognition that household heterogeneity may play a key role in macroeconomic dynamics. Models of household heterogeneity, with incomplete markets and idiosyncratic risk, began with Bewley (1983), followed by Imrohoroğlu (1989), Huggett (1993), and Aiyagari (1994) studying the steady-state properties of these models. However, computational and methodological limitations, along with early work by Krusell and Smith (1998) showing that the aggregate dynamics of a TFP shock were not much changed in a heterogeneous agent model, have resulted in a slow start for the literature combining heterogeneity with business cycle dynamics.[7] Recent methodological advances, particularly Reiter (2009), have overcome some of these computational limitations and a new generation of models have demonstrated that heterogeneity can play a large role in the business cycle. Heterogeneous Agent New Keynesian (HANK) models, combining elements from both the heterogeneous agent and New Keynesian literature, not only match the growing evidence on micro level consumption behavior, but also imply very different aggregate dynamics and/or propagation mechanisms following macroeconomic shocks, compared to their representative agent equivalents. In particular, the transmission mechanism of monetary policy can look very different in a HANK model.[8]

While these heterogeneous-agent models make clear the potential importance of heterogeneity in economic fluctuations, particularly for monetary and fiscal policy, their

---

[6]Panel data from financial aggregation platforms have been highly informative about consumption behavior, although access to these data by researchers is very restricted. See Gelman, Kariv, Shapiro, Silverman, and Tadelis (2014), Ganong and Noel (2019), and Baker (2018) for examples using U.S. data. Olafsson and Pagel (2018) use data from Iceland.

[7]Kaplan and Violante (2018) provide an overview of the theoretical literature incorporating household heterogeneity into models of economic fluctuations.

[8]For example, in the model of Kaplan, Moll, and Violante (2018), the intertemporal substitution channel is dwarfed by indirect general equilibrium effects, in stark contrast to a representative agent model.

quantitative results hinge on assumptions, such as the tenure of debt instruments and the timing of taxes, that were unimportant in representative agent models. Thus far the ability of these models to help us distinguish transmission channels *empirically* has been limited. Auclert (2019), in contrast to the fully structural HANK models, takes a simplified approach to aggregate dynamics, and one that we will follow in this paper.[9] He derives a set of sufficient statistics, directly measurable from a suitable dataset, that is highly informative about the relative size of different monetary policy transmission channels. His methodology benefits from being transparent and closely tied to the data, reducing the problem to that of measuring the distribution of MPCs across relevant dimensions of redistribution. However, as we will see in the following section, evidence on how MPCs vary across the population has been hard to come by.

2.1 Empirical Evidence on Heterogeneity in Consumption Behavior

Most micro empirical evidence on consumption behavior comes in the form of an estimate of the marginal propensity to consume out of a one-time source of income over the following three months to one year. Table I shows a selection of the population average estimates from the literature. Most of these studies do not have the power to say much, if anything, about heterogeneity within the population.

Three methods are used to empirically determine the marginal propensity to consume. The first is to identify a natural experiment and measure the consumption response to it. For example, Johnson, Parker, and Souleles (2006) use randomly assigned timing of 2001 tax rebates and questions in the U.S. Consumer Expenditure Survey to identify a three-month aggregate marginal propensity to consume of 0.2 to 0.4. Of the three methods, natural experiments likely have the strongest identification, but estimates vary, and there is no strong consensus. Identification issues arise as to when exactly households learn about the payment versus when it is received, and the extent to which external validity extends from these natural experiments to the kinds of transitory shocks found in heterogeneous agent models is unclear.[10] As most of these studies rely on consumer survey data, they tend to lack power due to high measurement error and low sample sizes. As a result, they have produced very little evidence of how the MPC varies among

---

[9]Wong (2016) also takes an empirical approach by identifying how the consumption response to monetary policy shocks varies with age.

[10]Many studies find a smaller MPC for positive shocks than negative shocks—for example, Bunn, Le Roux, Reinold, and Surico (2018). In this paper we implicitly assume that the response is symmetric. In reality, our estimates represent an average of positive and negative shock reactions.

different groups in the economy. A recent paper by Fagereng, Holm, and Natvik (2016) overcomes some of these problems. By using lottery data, the shock to income is truly random.[11] They use administrative data from Norway similar to the data we use from Denmark and have a sample of over 30,000 lottery winners over 10 years. As a result, they can identify the MPC for households with differing liquid wealth, as well as by the size of the lottery win. They find that households in the lowest quartile of liquid wealth have an MPC of approximately 0.61 over a six-month period, as opposed to 0.45 for households in the highest quartile of liquid wealth. In another study using data from a financial aggregator, Gelman (2016) identifies large differences in the impulse response to a tax rebate at a monthly frequency across household quintiles of cash-on-hand.

The second method is simply to ask individuals how much of a transitory income change they would consume. Jappelli and Pistaferri (2014) find an aggregate MPC of 0.48 using the Italian Survey of Household Income and Wealth and are able to identify clear differences across levels of liquid wealth. Fuster, Kaplan, and Zafar (2018) find a lower aggregate MPC in the NY Fed's Survey of Consumer Expectations, but they find heterogeneity by both size and sign of the shock. Of course the reliability of these studies is limited by the accuracy of households' own response to the question.

The third method, which we will follow, is to impose covariance restrictions on panel data of income and consumption and use these to identify the consumption response to income shocks of differing persistence. This method has the advantage that it can be used in a panel dataset with no natural experiment, such as the Danish administrative data we use or the PSID. The most well-known paper to use this method is by Blundell, Pistaferri, and Preston (2008), who use imputed non-durable consumption data based on food expenditure reported in PSID data. They estimate a consumption elasticity (closely related to an MPC if households' consumption level is close to their income) and find almost no consumption response to transitory shocks; however, as we will discuss in section 3.3, their estimates are strongly downward biased.

Our paper also adds to the limited literature on consumption responses to permanent shocks to income. Natural experiments for permanent shocks are rare. Gelman, Gorodnichenko, Kariv, Koustas, Shapiro, Silverman, and Tadelis (2016) use shocks to gasoline prices as a proxy for a permanent shock to income and find an MPC close to 1 across the population, with little heterogeneity. BPP find a consumption elasticity to permanent shocks to income around 0.65 (the permanent shock elasticity is less affected by the time

---

[11]We should note that even lottery winnings have some problems. First, the results hold for winners of the lottery who may not be representative of the wider population. Second the consumption response to a lottery win may be very different than other income shocks.

Table I Estimates of the Marginal Propensity to Consume from Income Shocks

	Consumption Measure		Horizon	Method	Event/Sample
	Nondurables	Total PCE			
Permanent Shocks					
Blundell, Pistaferri, and Preston (2008)*	0.65		~	3	Estimation Sample: 1980–92
Gelman, Gorodnichenko, Kariv, Koustas, Shapiro, Silverman, and Tadelis (2016)		1.0	~	1	Gasoline Price Shock
Transitory Shocks					
Agarwal and Qian (2014)		0.90	10m	1	Singapore Growth Dividend 2011
Blundell, Pistaferri, and Preston (2008)*	0.05			3	Estimation Sample: 1980–92
Browning and Collado (2001)		~ 0		1	Spanish ECPF Data, 1985–95
Coronado, Lupton, and Sheiner (2005)		0.36	1y	1	2003 Tax Cut
Fuster, Kaplan, and Zafar (2018)		0.08–0.31	3m	2	NY Fed Survey Cons. Expectations
Hausman (2012)		0.6–0.75	1y	1	1936 Veterans' Bonus
Hsieh (2003)*	~ 0	0.6–0.75		1	CEX, 1980–2001
Jappelli and Pistaferri (2014)	0.48			2	Italy, 2010
Johnson, Parker, and Souleles (2009)	~ 0.25		3m	1	2003 Child Tax Credit
Lusardi (1996)*	0.2–0.5			3	Estimation Sample: 1980–87
Parker (1999)	0.2		3m	1	Estimation Sample: 1980–93
Parker, Souleles, Johnson, and McClelland (2013)	0.12–0.30	0.50–0.90	3m	1	2008 Economic Stimulus
Sahm, Shapiro, and Slemrod (2010)		~ 1/3	1y	1	2008 Economic Stimulus
Shapiro and Slemrod (2009)		~ 1/3	1y	1	2008 Economic Stimulus
Souleles (1999)	0.045–0.09	0.34–0.64	3m	1	Estimation Sample: 1980–91
Souleles (2002)	0.6–0.9		1y	1	The Reagan Tax Cuts of the Early 1980s

* Elasticity of consumption to income.
Methods: 1) Natural experiment 2) Survey question 3) Covariance restrictions
This table is adapted from Carroll, Slacalek, Tokuoka, and White (2017).

aggregation problem). For a more complete overview of the literature on consumption responses to income changes, see Jappelli and Pistaferri (2010).

3 Empirical Strategy

We take a reduced form approach to estimate four parameters: the variance of permanent and transitory income shocks and the marginal propensity to consume out of permanent and transitory income shocks. To do so, we will make identifying restrictions on income and consumption dynamics. Specifically, we will assume that income is made up of a permanent component that moves as a random walk and a transitory component with persistence of less than two years. For consumption, we assume it responds permanently to a permanent income shock but has a short-lived response of no more than two years to a transitory income shock. Our model will be in continuous time in order to correctly account for the time aggregated nature of our data. These restrictions allow us to calculate a set of observable moments with which we can estimate the four parameters of interest using GMM.

3.1 Identifying Restrictions

Here, we describe the restrictions we impose on income and consumption dynamics that allow us to identify the variance of permanent and transitory income shocks, as well as how households respond to them.

3.1.1 Income Dynamics

Our identification of permanent and transitory income variance follows the methodology of Carroll and Samwick (1997) closely. As in their approach, we assume idiosyncratic income is composed of permanent and transitory components, where the permanent component follows a random walk and the transitory component persists for no more than two years. Our main innovation is to account for time aggregation by setting their discrete time model in continuous time and aggregating income over each year appropriately. We choose to model the level income process, rather than the log income process as in Carroll and Samwick (1997), because this allows more direct estimates of marginal propensities to consume.[12]

---

[12]In online appendix J we get qualitatively similar results using a model of log income and expenditure.

Generic Transitory Impulse Response, f(t)
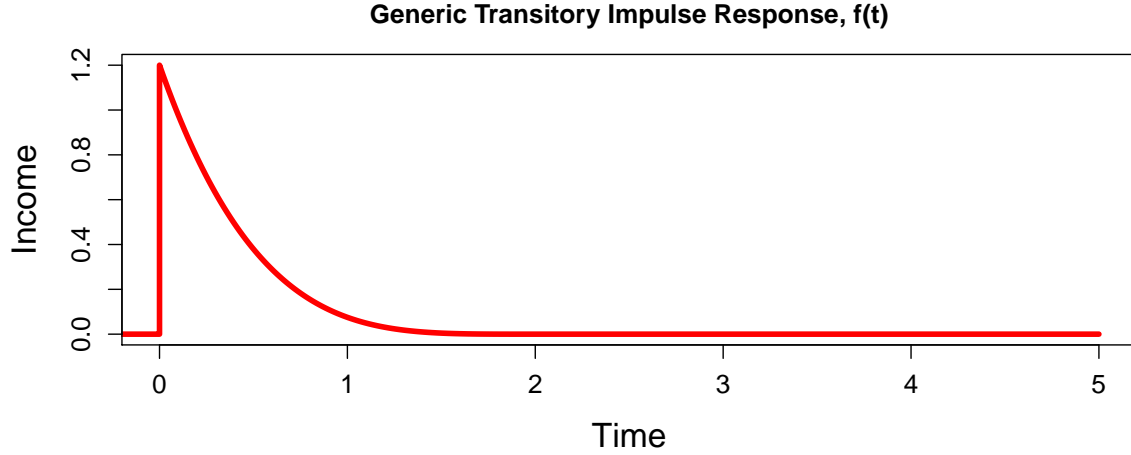
Figure I Generic Transitory Shock Impulse Response

Our model is set in continuous time where each time period represents one year. We define two independent martingale processes (possibly with jumps), P_t and Q_t, where P_t will represent the *flow* of permanent income at time t and the change in Q_t provides the transitory *impulses* that generate the transitory income. We assume that for all $s_1 > s_2 > s_3 > s_4 > 0$:

$$\text{Var}(P_{s_1} - P_{s_2}) = (s_1 - s_2)\sigma_P^2$$
$$\text{Cov}(P_{s_1} - P_{s_2}, P_{s_3} - P_{s_4}) = 0$$
$$P_s = 0 \qquad \text{if } s < 0$$

and similarly for Q_t. That is, these martingales have independent increments. As a useful benchmark, two Brownian motions satisfy these criteria.

The natural generalization of the MA(2) transitory income process from Carroll and Samwick (1997) is to allow for a generically shaped transitory income shock that decays to zero in under two years.[13] Figure I shows an example of such a transitory income shape $f(t)$, but the model also allows for completely transitory shocks in which case $f(t)$ would be a delta function with all the income from the transitory shocks arriving as a mass at the time of the shock. In this model the *flow* of income arriving at time t is given by the flow of permanent income and the sum of income arising from any transitory shocks to income that have occurred in the previous two years:

---

[13]Previous studies have found little evidence of transitory dynamics lasting more than one year, but to be conservative and in line with BPP we allow transitory income to persist for up to two years.

$$y_t = P_t + \int_{t-2}^{t} f(t-s)dQ_s$$

We do not observe y_t directly but instead $\bar{y}_T$, the time aggregated income over each one year period:

$$\bar{y}_T = \int_{T-1}^{T} y_t dt \text{ for } T \in \{1, 2, 3...\} \tag{1}$$

Taking the Nth difference for $N \geq 3$ we get:

$$\begin{aligned}
\Delta^N \bar{y}_T &= \int_{T-1}^{T} y_t dt - \int_{T-N-1}^{T-N} y_t dt \\
&= \int_{T-1}^{T} (T-s)dP_s + (P_{T-1} - P_{T-N}) + \int_{T-N-1}^{T-N} (s-(T-2))dP_s \\
&\quad + \left(\int_{T-1}^{T} \int_{t-2}^{t} f(t-s)dQ_t dt - \int_{T-N-1}^{T-N} \int_{t-2}^{t} f(t-s)dQ_t dt \right)
\end{aligned} \tag{2}$$

The variance of time aggregated income of an N year period is therefore:[14]

$$\text{Var}(\Delta^N \bar{y}_T) = (N - \frac{1}{3})\sigma_P^2 + 2\text{Var}(\tilde{y}) \text{ for } N \geq 3 \tag{3}$$

This is similar to the discrete time model in Carroll and Samwick (1997) except that the coefficient on permanent variance is $N - \frac{1}{3}$ in place of N. The transitory variance identified is the variance of "total" transitory income received in the year, $\tilde{y}$, where this is defined as[15]

$$\tilde{y}_T = \int_{T-1}^{T} \int_{t-2}^{t} f(t-s)dQ_s dt \tag{4}$$

Equation 3 shows that the variance of income growth grows linearly with the number of years of growth beyond three years. This result comes from the fact that the transitory component adds variance at the beginning and end of the growth period, but any transitory shock to income that occurs in the middle of the period does not affect income growth, as it will have decayed by the end of the measured period.

3.1.2 Consumption Dynamics

Our approach will be to extend the identification of income variance by using growth over three, four and five years to also identify the covariance of income and consumption.

---

[14] See online appendix A for full details of this derivation.

[15] In the discrete time MA(2) model, $y_t = p_t + \varepsilon_t + \theta_1 \varepsilon_{t-1} + \theta_2 \varepsilon_{t-2}$, different definitions of transitory variance are used. Carroll and Samwick (1997) estimate $(1 + \theta_1^2 + \theta_2^2)\sigma_\varepsilon$ while Blundell, Pistaferri, and Preston (2008) estimate σ_ε. Our definition of $\text{Var}(\tilde{y})$ is the continuous time analog of Carroll and Samwick (1997). It is not clear what the analog of σ_ε would be in continuous time.
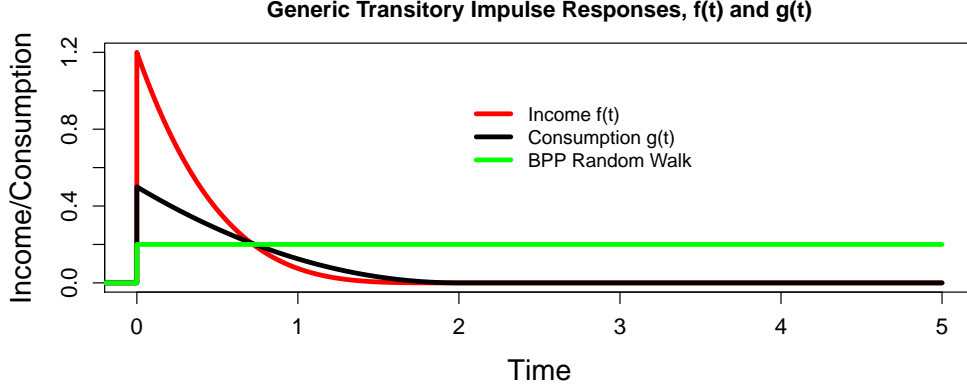
Figure II Generic Transitory Shock Impulse Response

In contrast to Blundell, Pistaferri, and Preston (2008), who assume that consumption follows a random walk, we will instead assume that the impulse response to a transitory shock follows a generic path, $g(t)$, that, like the transitory income shock, has fallen to zero two years after the news of the shock. Figure II shows possible paths for both income and consumption, along with the alternative random walk impulse response of BPP. The best evidence for the speed at which the consumption response decays comes from Gelman (2016) and Fagereng, Holm, and Natvik (2016), both of which show that the response has entirely (or almost entirely) decayed two years after the shock. [16] We will maintain the assumption from BPP that the consumption response to a permanent shock to income follows a random walk proportional to the permanent shock. Under these assumptions the instantaneous flow of consumption is given by:

$$c_t = \phi P_s + \int_{t-2}^{t} g(t-s)dQ_s$$

and the covariance of time aggregated income and consumption growth over $N \geq 3$ years is given by

$$\text{Cov}(\Delta^N \bar{c}_T, \Delta^N \bar{y}_T) = \phi(N - \frac{1}{3})\sigma_p^2 + 2\text{Cov}(\tilde{c}, \tilde{y}) \text{ for } N \geq 3 \tag{5}$$

where total transitory income, $\tilde{y}$, is given by equation 4 and total transitory consumption, $\tilde{c}$, is defined by

$$\tilde{c}_T = \int_{T-1}^{T} \int_{t-2}^{t} g(t-s)dQ_s dt \tag{6}$$

---

[16] In section F, we will show how this assumption may potentially bias the transitory consumption response down, but that this bias is small, especially for all but the most liquid households.

10

3.2 Minimum Distance Estimation

Using the equations for variance (3) and covariance (5) of observed income and consumption growth over N years for at least two different values of N, we are able to estimate the following four unknowns in which we are interested:[17]

1. σ_p^2 Variance of permanent shocks

2. $\sigma_{\tilde{q}}^2 = \text{Var}(\tilde{y})$ Variance of transitory income received in a year

3. ϕ Marginal Propensity to eXpend (MPX) w.r.t. permanent income

4. $\psi = \frac{\text{Cov}(\tilde{c},\tilde{y})}{\text{Var}(\tilde{y})}$ Regression coefficient of transitory consumption w.r.t. transitory income over a year (MPX out of transitory income).

To identify heterogeneity, we estimate these four unknowns separately for different subpopulations in our data. Our panel data cover 13 years and we choose to use growth over three, four and five years to balance greater identification (longer growth periods give more power) with three identification problems that grow with N. First, many households drop out of the sample if we demand they have reliable data for too many consecutive years. Second, if the permanent shock in fact decays slowly over time (e.g. is in fact AR(1)), the bias this introduces will be larger for large N. Third, the validity of running the regressions in levels (rather than logs) is reduced over large N when the potential for the variance of income to change significantly from the start to the end of the sample is high. In section 8 and online appendix J we test the importance of these issues.

We follow Blundell, Pistaferri, and Preston (2008) and use diagonally weighted minimum distance estimation, although our results are not significantly changed by using other popular weighting methods.[18]

As the main part of our analysis will focus on the parameter ψ, it is worth describing exactly what this is and why we have labeled it the marginal propensity to expend out of transitory income. If we were able to exactly observe transitory income and consumption resulting from transitory income, then ψ would be the regression coefficient

---

[17]We have a total of 96 moments (we have eight consecutive five-year periods, each of which has three three-year growth periods, two four-year growth periods, and one five-year growth period. $8 \times (3+2+1) = 48$. Each of these growth periods has both a variance and a covariance moment, $48 \times 2 = 96$). With only four parameters to estimate, the system is over identified. We strongly reject the null of the Sargen-Hansen J-test when run on our data, but this is not surprising given the sample size of our data.

[18]As our sample size is large, the motivation for using diagonally weighted minimum distance (DWMD) over optimal minimum distance (OMD) is small; see Altonji and Segal (1996). We get very similar results using OMD. In general, our results may be subject to misspecification problems, but the sample size of our data means that standard errors are small.

of this transitory consumption on transitory income. If transitory income shocks have no persistence, this is approximately a six-month MPX (on average, the shock will happen six months into the year so that the regression will pick up the change in consumption in the following six months). If transitory income shocks have a little persistence (online appendix B shows evidence of a small amount of transitory income persistence), ψ can only loosely be interpreted as the MPX to an income shock, and the reader should bear in mind that the true interpretation is, "if income is higher by one unit this year due to transitory factors, then consumption this year will be expected to be higher by ψ units."

## 3.3   A Brief Introduction to the Time Aggregation Problem

An obvious question is why we have chosen not to use the well-known methodology of Blundell, Pistaferri, and Preston (2008), who achieve identification of transitory shocks from the facts that: (i) transitory income shocks in period t will mean-revert in period $t + 1$; and (ii) permanent shocks in period t are uncorrelated with income changes in period $t + 1$.[19] Unfortunately, as noted in Working (1960), in time aggregated data (ii) does not hold—a time aggregated random walk is positively autocorrelated. While macroeconomists have long been aware of the importance of time aggregation in time series regressions (see Campbell and Mankiw (1989) for a well-known example), the problem has been overlooked by the household finance and labor economics literature.[20] We will therefore briefly describe the problem here. For a more detailed account with particular attention to BPP, see Crawley (2020).

Time aggregation transforms an underlying, high-frequency time series, into a lower frequency time series. For example, we observe Danish tax returns at an annual frequency, while income in fact consists of paychecks arriving at a monthly, biweekly or irregular frequency. The observable income is the sum of all the income that was received by a household during the year. The key insight of Working (1960) is that even if there is no correlation between changes in income at the underlying frequency (it is a random walk), changes in the resulting time aggregated series will show positive autocorrelation. Figure III shows how this autocorrelation is generated. The solid line shows the flow of income for a household that receives a permanent pay rise from $50,000 to $100,000 mid-

---

[19]Kaplan and Violante (2010) show in discrete time simulations that the methodology works reasonably well for standard calibrations of buffer-stock models and end up concluding, "The BPP insurance coefficients should become central in quantitative macroeconomics." However, some recent papers such as Commault (2017) and Hryshko and Manovskii (2018) have pointed to other potential problems of the methodology.

[20]For examples, see Moffitt and Gottschalk (2012); Meghir and Pistaferri (2004); Nielsen and Vissing-jorgensen (2004); Heathcote, Perri, and Violante (2010); and more recent quantile regression approaches such as Arellano, Blundell, and Bonhomme (2017).
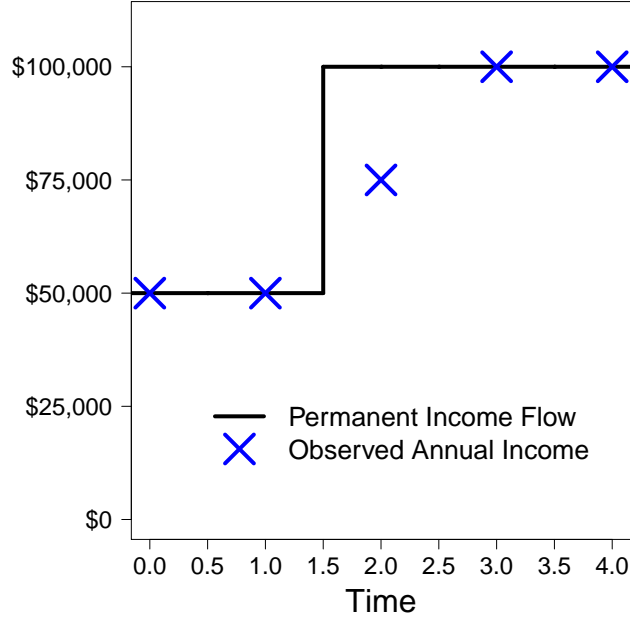
Figure III The Time Aggregation Problem

way through the second year. The crosses show the income we would observe in annual tax data. During the second year, the household receives an annual $50,000 salary for six months, followed by $100,000 in the second six months, resulting in a reported income of $75,000 for the entire year. The single shock to income therefore appears in the observed data as two increases. In this way, if the underlying income process follows a random walk, shocks in one year result in observed income changes in that *and* the following year. The resulting autocorrelation—if the underlying process is in continuous time— is 0.25. Continuous time is a good approximation for quarterly or monthly underlying processes. The autocorrelation of an annual-time-aggregated quarterly random walk is 0.23. At a monthly or higher frequency the autocorrelation is almost indistinguishable from 0.25.[21]

While it would be possible to stick very closely to the original BPP model and adjust the covariance restrictions to take account of the time aggregation problem,[22] we have found that in practice the underlying assumptions made by BPP (in particular that consumption follows a random walk) do not fit with the data. The random walk assumption was previously thought to be benign. Not only were the estimates of

---

[21]If all permanent shocks to income occurred on January 1 each year, then this would not hold. Low, Meghir, and Pistaferri (2010) show that a significant portion of permanent income variance is explained by job mobility, which can occur at any point in the year.

[22]Crawley (2020) takes this more straightforward approach using the same PSID data as used in BPP.

the consumption response to transitory shocks in BPP small and consistent with such an assumption, Kaplan and Violante (2010) show that without time aggregation, the BPP method correctly identifies the transitory consumption response in the period of the income shock regardless of the consumption dynamics going forward. This fact is again not robust to the time aggregation problem. With time aggregation taken into account, the estimates are highly sensitive to assumptions about short-term consumption dynamics—online appendix E shows how the random walk assumption affects MPX estimates. Therefore we have chosen to attain identification in a manner similar to Carroll and Samwick (1997), which allows us to be agnostic about the exact short-term dynamics of income and consumption.

4   Data

Our panel data on income and expenditure comes from Danish registry data from 2003-2015. These data have a number of advantages over survey-based measures. First, the sample contains millions of households rather than thousands. Second, households are required by law to report their data, so there is much less risk of selection bias through drop outs. Third, measurement error in income data is largely eradicated, as employees' income data is third party reported by their employer, compared to survey data where self-reported income has been shown to be particularly unreliable for irregular income.[23]

4.1   Income

We are interested in income and consumption decisions at the household level. We define a household as having either one or two adult members. Two adults are considered to be in the same household if they are living together and (i) are married to each other or have entered into a registered partnership, (ii) have at least one common child registered in the Civil Registration System or, (iii) are of opposite sex and have an age difference of 15 years or less, are not closely related and live in a household with no other adults.[24] In the panel data, an individual's household will change if he or she gets married or divorced, which leads to some selection bias given that we

---

[23]See David, Marquis, Moore, Stinson, and Welniak (1997) for a survey of income measurement error issues in survey data.

[24]Adults living at the same address but not meeting one of the three criteria are regarded as separate families. Children living with their parents are regarded as members of their parents' family if they are under 25 years old, have never been married or entered into a registered partnership, and do not themselves have children. A family meeting these criteria can consist of only two generations. If three or more generations live at the same address, the two younger generations are considered one family, while the members of the eldest generation constitute a separate family.

require households to survive for at least five years. Following the literature on income dynamics, our baseline results will be reported using the labor income of the head of household.[25] Note that expenditure is measured at a household level, so our results should be interpreted as how household expenditure responds to head-of-household labor income—appendix J shows using household income yields virtually identical results. We will use after tax and transfer income, as we are interested in the consumption response to these changes in income, although the method could be used to measure the extent of consumption insurance provided by the tax and transfer system. Our data come from the administrative records from the tax authority. The tax reporting system in Denmark is highly automated and individuals bear little of the reporting burden. For employees, income is reported by their employer and is thought to be highly accurate. The underground economy in Denmark is small. We remove business owners from the sample, as their income may be less accurately reported but, more importantly, the expenditure imputation method does not work well for them (see section 4.2).

We work with the residual of income after controlling for observable characteristics of households that may affect their income and consumption. To start, we remove households in the top and bottom 1% of the income distribution. We then normalize by average household income over the observed period and regress income on dummies for age, year, highest level of education, marital status, homeowner status, and number of children along with interaction of age with education, marital status, and homeowner status. We take the change in the residuals of this regression to be the unexpected income change for a household from one year to the next and remove households in the top and bottom 1% of the unexpected income *change* distribution.

4.2 Imputed Expenditure

We use data on expenditure, and will therefore refer to the Marginal Propensity to eXpend (MPX), for the rest of the paper to reflect the fact that our measure includes durables.[26] Our expenditure data come from imputing expenditure from income and wealth. Along with other Scandinavian countries, Denmark is unusual in that tax reporting includes information about wealth along with income, a legacy from the wealth tax that was phased out between 1989 and 1997. Following the methodology from Browning and Leth-Petersen (2003) and Fagereng and Halvorsen (2017), we impute

---

[25]See Moffitt and Zhang (2018) for an overview of the literature on income volatility in the PSID. In contrast to the PSID literature, we define the head of household as the highest earner over the 13-year period in our sample. We believe this definition better fits the social structure in Denmark.

[26]See section 8.2 for how durables affect our estimates.

expenditure using the identity

$$\bar{C}_t \equiv \bar{Y}_t - \bar{S}_t = \bar{Y}_t - P_t - \Delta NW_t$$

where $\bar{C}_t$, $\bar{Y}_t$, and $\bar{S}_t$ are the sum of expenditure, income, and savings over the year t, respectively. P_t is contributions to privately administered pension schemes, for which we have very accurate data due to tax deductibility, ΔNW_t is the change in (non-pension, non-housing) net worth measured at the end of years t and $t-1$. Banks and brokers are required to report the value of their clients' accounts on December 31 each year, and the tax reporting year runs from January 1 to December 31, so the data for income and wealth reported in the tax returns match that required to use this identity to impute consumption.

The method works well for households with simple financial lives. One of the biggest problems with the method is its inability to handle capital gains well. The income used in the imputation includes all labor income and capital income; however, it excludes capital gains. The value of assets will vary due to savings from reported income but, also due to capital gains and losses, which we handle in a number of ways. First, we completely exclude housing wealth, treating housing as an off-balance-sheet asset for this calculation.[27] The problem with treating housing in this way is that we must exclude households in years in which they are involved in a housing transaction. For the self-employed, it is also difficult to distinguish between expenditure and investment in their business, so we exclude all households that receive more than a trivial amount of their income from business ventures. Finally, households that hold significant equity investments are likely to see sizable capital gains and losses. We make a naive adjustment by assuming that they hold a diversified index of stocks. While this assumption will likely lead to significant measurement error for these individuals, the concern is mitigated first by the fact that stock holding is much more unusual in Denmark than in the United States, for example. Only around 10% of households hold any stocks, and for many of those households stocks make up only a small proportion of their total wealth. Furthermore, as we will explain in section 8.1, measurement error in consumption is not a concern unless it is correlated with changes in income. However, Baker, Kueng, Pagel, and Meyer (2018) show, in a German dataset, that the relation between income and imputation error is economically small.

Another concern with the imputation method is transfers of wealth—say, between

---

[27]We exclude housing wealth from net worth only for the imputation procedure, which correctly identifies expenditure for years in which there is no housing transaction—knowing house prices with precision in these years would not help impute expenditure. Our measure of net wealth where wealth itself is of interest, for example table II, includes housing.
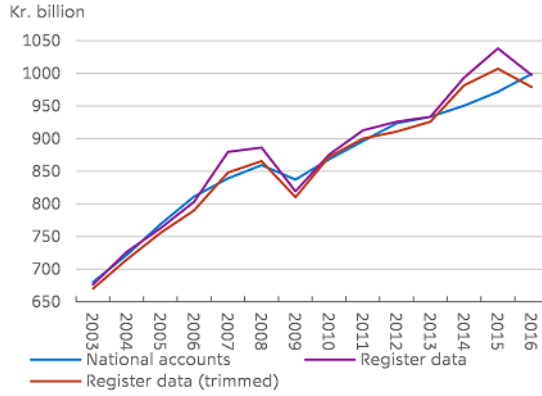
Figure IV Imputed Register Measure and National Account Measure of Expenditure (from Abildgren, Kuchler, Rasmussen, and Sorensen (2018))

family members or friends. Indeed imputed expenditure is negative for approximately 3% of households, which may explain a proportion of that. We discard both income and expenditure data for households in years in which their expenditure is negative. In online appendix J we test the robustness of our results to sample selection bias problems that these issues may give rise to.

As with income, we work with the residual of expenditure after normalizing by mean household income and controlling for the same observable features as income. We follow exactly the same steps as described in section 4.1.

Abildgren, Kuchler, Rasmussen, and Sorensen (2018) show that the mean levels of expenditure from this imputation method are close to those from the national accounts (see figure IV). They find relatively large differences at the household level between the consumer survey and imputed expenditure, although it is not clear that this is a problem with the imputation method as opposed to the survey measure. Indeed, for car purchases, for which highly accurate register data are available, the consumer survey shows significant underreporting, consistent with Koijen, Nieuwerburgh, and Vestman (2014), who find 30% underreporting of car purchases in the Swedish consumer survey. We believe that, with the exception of transaction-level data reported by financial aggregation applications, the imputation method we use results in some of the highest quality expenditure data available to researchers for the types of questions we are addressing.

17

4.3 Sample Selection

As our methodology requires income uncertainty to be relatively constant through the observed period and the young and old are likely to have predictable income trends unobservable to the econometrician, we limit the sample to households headed by an individual between the ages of 30 and 55 in 2008.[28] Our final sample contains 7.7 million observations from 2004 to 2015 from an age group population totaling 18.1 million. The selection criterion that reduces the sample size the most is the requirement that a household does not make a housing transaction for a period of five years. Table II shows summary statistics for all Danish households whose head fits into this age group as a whole as well as the sample we use in estimation. It is reassuring that both the mean and median values for after-tax income and consumption are similar in the estimation sample and the population. Our estimation sample has much lower standard deviations as a mechanical result of excluding the top and bottom 1% of the income and consumption distributions that contain extreme values. Sample selection shows up in homeownership and car ownership, as we exclude those households that buy a house at the end of a five year period but who otherwise would be counted as renters. As a result, our sample is, on average one year older than the population. Unhedged Interest Rate Exposure (URE) and Net Nominal Position (NNP) will be discussed in section 6, but again the significant differences here are due to the housing transaction criteria.

5 Income and Consumption Characteristics by Household Wealth

Liquidity constraints are the key microfoundation for the lack of consumption smoothing in heterogeneous agent models. In this section we look at the empirical relation between liquid wealth and the marginal propensity to expend (MPX) out of both permanent and transitory shocks to income. We find a strong monotonic negative relation. We also look at net wealth and find such a monotonic relation no longer holds.

Using our entire estimation sample we find a mean MPX out of transitory shocks of 0.50 and a mean MPX out of permanent shocks of 0.72. However, these averaged results hide a significant amount of heterogeneity. From the standpoint of consumption theory

---

[28]Online appendix B shows the assumption holds for this age group.

Table II Summary Statistics

|	Estimation Sample			Population (Age 30-55)		
	Mean	Median	Std Dev	Mean	Median	Std Dev
After Tax Income	59,261	57,804	28,819	58,312	53,304	68,799
Consumption	52,680	48,344	28,581	54,022	46,373	38,126
Liquid Assets	18,438	6,856	33,016	23,331	6,578	81,473
Net Worth	74,937	19,115	157,295	85,799	12,952	564,404
Homeowner	0.57	1.00	0.50	0.50	1.00	0.50
Car Owner	0.66	1.00	0.47	0.55	1.00	0.50
Higher Education	0.31	0.00	0.46	0.33	0.00	0.47
Age	43.5	44.0	7.1	42.5	42.0	7.3
URE	-28,052	-12,627	108,382	-47,589	-19,374	243,604
NNP	-109,685	-65,810	156,523	-158,321	-85,207	542,498
No. Household-year obs		7,664,360			18,050,340	

Notes: Values are 2015 USD. Age refers to the age in 2008 of the main income earner in the household. For the purposes of calculation of consumption in the population, top and bottom 1% in terms of consumption have been excluded. URE and NNP can only be calculated in the period 2009-2015 due to mortgage information being insufficiently detailed in the previous years.

it is the ability of households to self-insure with their own wealth that mostly determines how much they smooth their consumption over shocks. We divide our estimation sample into quintiles according to both liquid wealth (which we define as bank deposits[29]) and net wealth. In each case, wealth is measured as the mean household wealth holdings over the entire sample period.

Figure V shows the estimated income variances and MPXs for households in each quintile of liquid wealth.[30] Looking at the left-hand variance panel first, it is noticeable that income uncertainty, and particularly permanent income uncertainty, is highest for households in the lowest quintile of liquid wealth. This low buffer-stock, despite high income volatility, provides some evidence towards the idea that heterogeneous tastes (e.g. discount factors of risk aversion) may be more important than income risk in determining wealth held for precautionary saving. For households in the top three quintiles of liquid wealth, the similarity of their level of income risk is remarkable. Note that in contrast to standard estimates of the U.S. income process, permanent income variance in Denmark is slightly higher than transitory variance, likely due to the high levels of social insurance available in Denmark. The variance level, at just over 0.002 for these top three quintiles, represents a standard deviation of just below 5% of permanent income per year.

---

[29]The results are little changed using any other definition of liquid wealth as long as housing and debts are excluded. See online appendix J.

[30]For these graphs, and all similar ones in this paper, the 95% confidence intervals are shown above and below each quantile estimate.
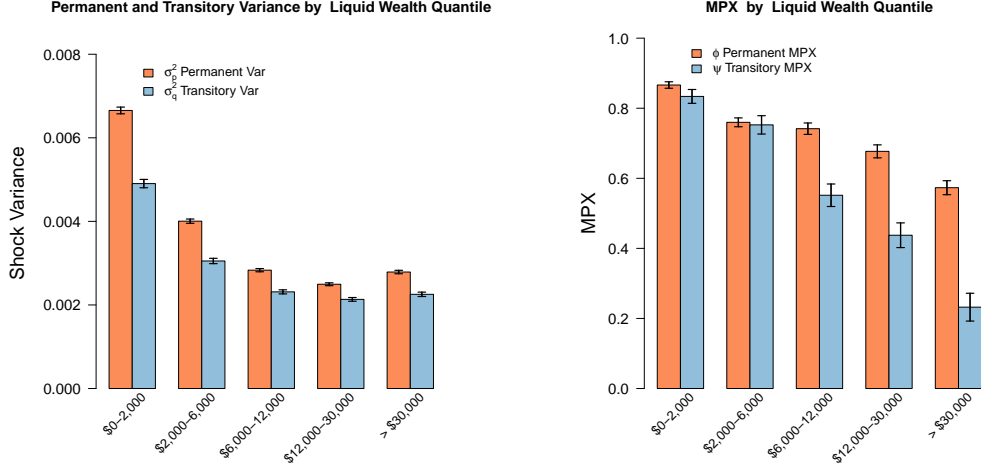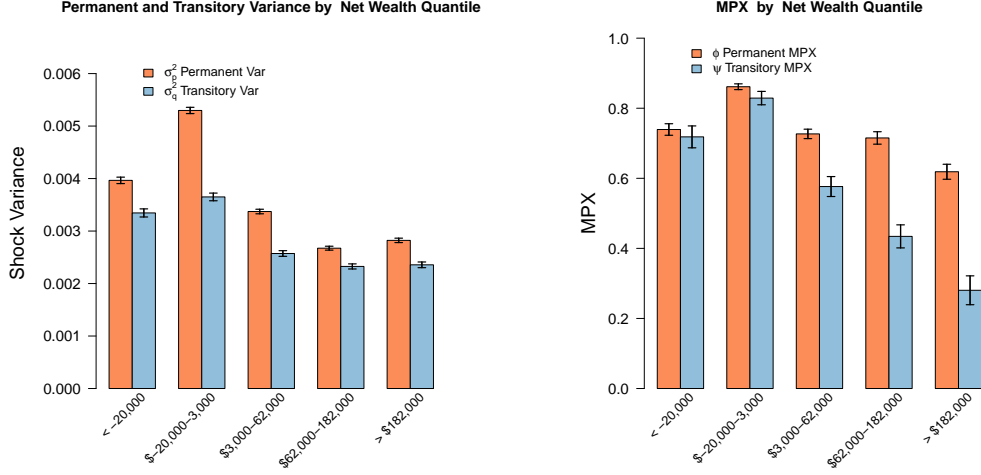
Figure V Variance and MPX by Liquid Wealth Quintile

Note that the estimates of income variance we obtain are highly sensitive to our treatment of outliers, but our MPX estimates do not change.[31]

The right-hand panel of figure V shows our estimates for the MPX out of permanent and transitory shocks by liquid wealth quintile. The lowest wealth quintile, who hold less than $2,000 in bank deposits on average over the sample period, look somewhat like hand-to-mouth consumers. They respond almost equally to permanent and transitory shocks, spending over 80% of an income shock in the year that it arrives. However, the fact that both permanent and transitory MPXs are very similar and significantly less than 1 suggests that these households may be more accurately modeled as saving in an illiquid asset such as housing or a pension following a rule of thumb (say, 20% of income) and then living hand to mouth on the remainder. As the quintile of liquid wealth increases, the MPX out of both transitory and permanent income decreases. In the top quintile, formed of households that maintained a mean bank balance above $30,000, the MPX out of permanent shocks is 0.57 and out of transitory shocks 0.23. From the point of view of theory, the responsiveness of spending out of permanent shocks in this quintile is low, while that of transitory shocks is high.

Figure VI shows the estimates for households grouped by quintiles of net wealth. Here the pattern is slightly different. The quintile with the highest MPX out of both transitory and permanent income is the second lowest, the quintile that contains zero net worth. Households in the lowest quintile—those with over $20,000 in net debt—do not seem

---

[31]See online appendix J for evidence of this.

Figure VI Variance and MPX by Net Wealth Quintile

to distinguish between permanent and transitory income shocks in their consumption responses, but their MPX for both is about 10 percentage points lower than the quintile with close to zero net wealth. The pattern for quintiles 3 to 5 looks similar to that for liquid wealth: the MPX out of transitory shocks falls sharply to around 0.28, while that out of permanent shocks also falls but more slowly to 0.62.

These results are broadly in line with the literature. The population mean of 0.5 for transitory MPX is a little higher than most estimates from table I, but, bearing in mind that our estimate includes durables and is best compared to a six-month MPC, it is certainly not an outlier. The MPX out of permanent shocks of 0.72 is also between the BPP estimate of 0.65[32] and the estimate of 1.0 from Gelman, Gorodnichenko, Kariv, Koustas, Shapiro, Silverman, and Tadelis (2016). The strength of the relationship between liquid wealth and MPC is similar to that found in Gelman (2016) and stronger than in Fagereng, Holm, and Natvik (2016).

5.1 Methodology and Result Intuition

Next we present some simple regressions of expenditure growth on income growth and compare them with what we would expect in some very well understood baseline models. We have found these regressions help to build intuition on why the method, given the

---

[32] The permanent "insurance" coefficient estimated by BPP does not suffer as much from the time aggregation problem as the transitory coefficient.

data, yields the results in figure V. They also help to show why the results are robust to many forms of misspecification.

We look at the estimate of β^N in the model

$$\Delta^N c_{it} = \alpha^N + \beta^N \Delta^N y_{it} + \varepsilon_{it}$$

where N, the number of years over which growth is measured, varies from 1 to 10. The identification of permanent and transitory MPCs in section 3.1 comes from the fact that transitory income shocks make up a relatively large proportion of the variance of income growth over a short period, while permanent income shocks dominate the variance of income growth over a long period. While the coefficients β^N in this section do not represent MPCs, we would expect their values for large N to be close the the permanent MPC, and for small N to be closer to the transitory MPC.
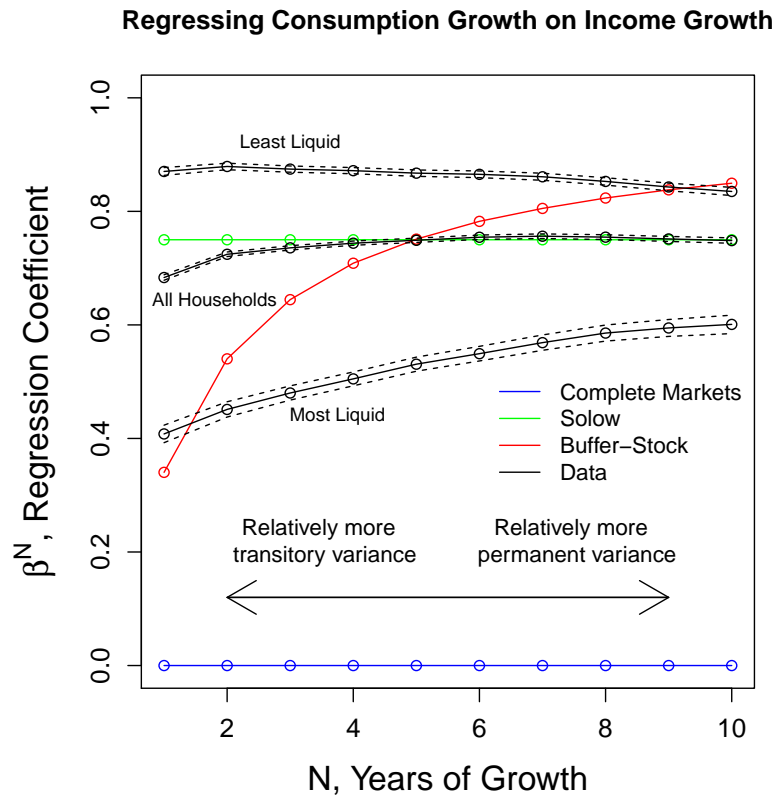
Figure VII shows what we would expect to see under three baseline models where households are subject to both transitory and permanent income shocks, as well as what we observe in the data. First, the blue horizontal line at zero shows what we would see in a complete markets model. With complete markets, all idiosyncratic shocks to income are insured against, resulting in no relation between idiosyncratic income and consumption growth. Second, the green horizontal line at 0.75 shows what we would see in a Solow model. Households in the Solow model do not optimize, but instead spend a constant proportion of their income each period—in this case set at 0.75—regardless of shock persistence. Third, the red, upward-sloping line shows the results for a typical buffer-stock saving model.[33] In this model, the consumption response to income growth over a one year period is small because households self-insure against the transitory shocks that dominate at this frequency. As the time period over which income growth is measured increases, the observed income growth is proportionally more permanent and self-insurance is not possible. The red line asymptotes toward 1.0 as N gets large.

The gray line, along with 95% confidence intervals, shows the results of these regressions using all households in the Danish sample. It is striking that the data appear to be closest to the Solow model, with only a small decrease in the regression coefficient over short periods. However, aggregating all households in this way hides a large degree of heterogeneity, particularly across households with different levels of liquid wealth.

The two black lines show the regression coefficients where the sample is restricted to households in the lowest and highest quintiles of liquid wealth (averaged over the observed period), respectively. For households in the lowest quintile there is no evidence

---

[33]These results come from the heterogeneous beta model in Carroll, Slacalek, Tokuoka, and White (2017), calibrated to match the distribution of liquid wealth in Denmark.

Regressing Consumption Growth on Income Growth

Least Liquid

All Households

Most Liquid

Complete Markets
Solow
Buffer–Stock
Data

Relatively more transitory variance

Relatively more permanent variance

β^N, Regression Coefficient

N, Years of Growth

Figure VII Regression Coefficients of Consumption Growth on Income Growth

of consumption smoothing: the consumption response to income growth over one year is both high and almost identical to that over 10 years, strongly suggesting the MPCs for this group out of transitory and permanent shocks are similar and high. Indeed, this is what we see in the lowest quintile of figure V—a result that is highly robust to misspecification. Households in the top quintile of liquid wealth show a clear upward slope in figure VII, indicating a substantial degree of consumption smoothing. The fact that the regression coefficient for this group appears to asymptote well below 1 also suggests, in contrast to standard buffer-stock models, that the MPC out of permanent shocks for liquid households is significantly lower than 1.

6 Monetary Policy and the Redistribution Channel

Auclert (2019) lays out a clear and intuitive theory as to how heterogeneity in the MPC out of transitory shocks affects the transmission mechanism of monetary policy. He identifies channels through which monetary policy can act, including those involving redistribution. He then uses this theory to identify a small set of sufficient statistics that help distinguish which of these channels are of quantitative importance.

While these statistics in theory are highly informative about the transmission mechanism of monetary policy, good data and MPC estimation methods are required to estimate them convincingly. Auclert states, "As administrative quality household surveys become available and more sophisticated identification methods for MPCs arise, a priority for future work is to refine the estimates I provide here." We are able to bring our new MPC estimation method, along with administrative data from Denmark, in order to estimate Auclert's sufficient statistics.

Our data have two significant advantages over previous efforts.[34] First, our sample is large, containing most households in Denmark. Second, we have detailed balance sheet information for the households. Furthermore, we are able to identify interest rate risk and nominal positions held by firms, foreigners, and the government so that the aggregate position is zero, as required in equilibrium. This allows us to avoid some problematic assumptions otherwise needed for aggregation of household data.

---

[34]Fagereng, Holm, and Natvik (2016) also estimate Auclert's sufficient statistics, imputing MPCs from lottery winnings in Norway, but they are limited by sample size. Ampudia, Georgarakos, Slacalek, Tristani, Vermeulen, and Violante (2018) look at differences in Auclert's statistics between European countries but do not attempt to estimate MPCs.

6.1 Distribution of MPX across NNP, URE, and Income

In this section we define a household's Net Nominal Position (NNP) and Unhedged Interest Rate Exposure (URE), and examine the distribution of households' transitory MPX along these dimensions, as well as income. In section 6.2 we show how these distributions can be used to quantify the importance of heterogeneity for monetary policy transmission.

The redistribution effects of monetary policy depend crucially on two household characteristics:

- **Net Nominal Position (NNP)** is the net value of a household's nominal assets and liabilities. Its relevance for analyzing the redistributive effects of monetary policy comes from the fact that an unexpected rise in the price level will decrease the real wealth of households with positive nominal assets, redistributing it to those with negative NNP (who now have less real debt). In our administrative data, we observe directly held nominal positions and their composition at the household level, including bank deposits and loans, bond holdings, and mortgages. In aggregate, the directly held NNP position of the household sector is negative, which as we will see is balanced in the national accounts by the financial sector as well as foreigners.

- **Unhedged Interest Rate Exposure (URE)** is the difference between all maturing assets (including income) and liabilities (including planned consumption).[35] For example, households with a large variable rate mortgage will likely have very negative URE. For these households, the entire value of their mortgage will be adjusted to the new rate. When the interest rate rises for one period they will see their disposable income (after mortgage payments) go down, and hence if they have a high MPX their spending will also decrease. To calculate URE, we assume all household bank deposits and loans have a variable rate that changes instantaneously. For mortgage debt we directly observe the amount resetting over the following year and assume that the new rate will only apply for half of the year.[36] For all other assets and liabilities we assume a maturity of five years. As

---

[35] We define "maturing" assets and liabilities as those that are due to have their interest rates reset, even if they contractually exist for a longer period. For example, a 30 year variable rate mortgage with annual interest rate fixation periods is "maturing" each year in our definition.

[36] See online appendix C for more details on the Danish mortgage market. Note that the prevalence of fixed-rate mortgages will strongly influence the distribution of URE. To the extent that the United States has more fixed-rate mortgages than Denmark, the interest rate exposure channel is likely to be smaller in the United States. Furthermore, household debt to income is about twice as large in Denmark as the United States.

with NNP we find households on aggregate have a negative URE position in our data, and this is counterbalanced by the interest rate position of the financial sector.[37]

Figure VIII shows how the MPX varies across households, according to their URE, NNP, and income. In each case, the value on the x-axis has been divided by the mean level of expenditure in the sample. The top panel shows the estimated MPX for each decile of unhedged interest rate exposure. The deciles on the left contain households most negatively exposed to a rise in interest rates, those in the middle deciles have little exposure, while the two top deciles on the right have the most to gain from an interest rate rise. We have included in this figure data on both rates of homeownership and median liquid assets for each decile. A pattern emerges in which we can roughly categorize the deciles into three groups following Violante, Kaplan, and Weidner (2014):

- **Wealthy Hand-to-Mouth:** The first five deciles contain households with high homeownership rates but few liquid assets. These households have relatively high MPXs, and it is likely that their wealth is locked up in illiquid assets (mostly housing) and that they have large mortgages.

- **Poor Hand-to-Mouth:** The next three deciles tend to be renters with very little in the way of liquid assets either. These households have very high MPXs and are close to being truly hand-to-mouth. As they have few assets, they have very little exposure to interest rates and cannot easily substitute consumption between periods; therefore, their consumption behavior is likely not affected by changes in interest rates directly.[38]

- **Wealthy:** The top two deciles contain households that are both likely to be homeowners and hold very large liquid asset balances. These are likely to be households that own their house outright without a mortgage and have been able to build up a large stock of liquid assets. Relative to the other deciles, they have low MPX and are likely able to use their assets to effectively smooth consumption.

The distribution of MPX with NNP follows a similar pattern. As mortgages in Denmark are a mixture of fixed and variable rates (see online appendix C for details on the Danish mortgage market), we can think of a typical household with negative

---

[37]See online appendix D for more details on how we calculate NNP and URE positions.

[38]Neither the interest rate exposure channel nor the intertemporal substitution channel will have much impact on their consumption. Monetary policy could impact their expenditure strongly through income effects.

URE or NNP as having a large mortgage, while those with positive URE or NNP are wealthy households with lots of liquid wealth. This pattern has not been evident in previous attempts to measure the distribution of MPX across these dimensions. Most importantly for the theory, the average MPX for those with negative URE and NNP positions is significantly greater than for those with positive URE or NNP. Note that the mean levels of both URE and NNP are negative for the households in our estimation sample, so even a constant (positive) MPX would result in interest rate hikes reducing their expenditure if not balanced by indirectly held exposures.[39]

The final chart in figure VIII shows the distribution of MPX with total household income. There is a clear downward trend. If the income of lower-income households decreases more than that of high-income households during a monetary policy contraction, then expenditure will go down by more than the mean income-weighted MPX that would be the result of a representative agent model.[40]

6.2 Theoretical Setup and Sufficient Statistics

Auclert's method is to consider individual households' consumption response to a monetary policy shock in which (i) the real rate of interest changes for one period by dR, (ii) the price level makes a one-time change of dP and then remains at the new level, and (iii) aggregate income makes a transitory change of dY. While the dynamics here are clearly stylized, and in particular lack any lag in the economy's response, we believe such a simple experiment to be highly informative as to the relative sizes of each transmission channel.

Auclert (2019) divides the effect of monetary policy on aggregate consumption into five distinct channels:

$$\frac{dC}{C} = \overbrace{\mathcal{M}\frac{dY}{Y}}^{\text{Aggregate Income Channel}} \overbrace{+\gamma\mathcal{E}_Y\frac{dY}{Y}}^{\text{Earnings Heterogeneity Channel}} \overbrace{-\mathcal{E}_P\frac{dP}{P}}^{\text{Fisher Channel}}$$

$$\underbrace{+\mathcal{E}_R\frac{dR}{R}}_{\text{Interest Rate Exposure Channel}} \underbrace{-\sigma\mathcal{S}\frac{dR}{R}}_{\text{Intertemporal Substitution Channel}} \tag{7}$$

---

[39]In contrast, Auclert (2019) finds a mean positive URE across households. We believe the difference is partly due to the prevalence of fixed-rate mortgages in the United States, but also due to underreporting of expenditures, especially in the PSID data.

[40]For comparison, the distribution of MPX out of permanent income shocks across these three dimensions can be found in online appendix K.

MPX by URE Decile
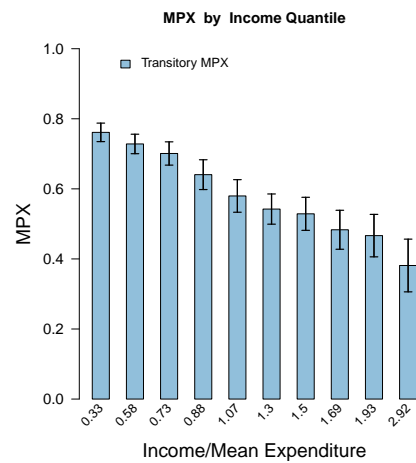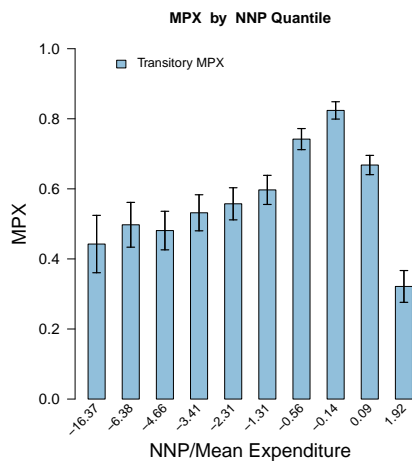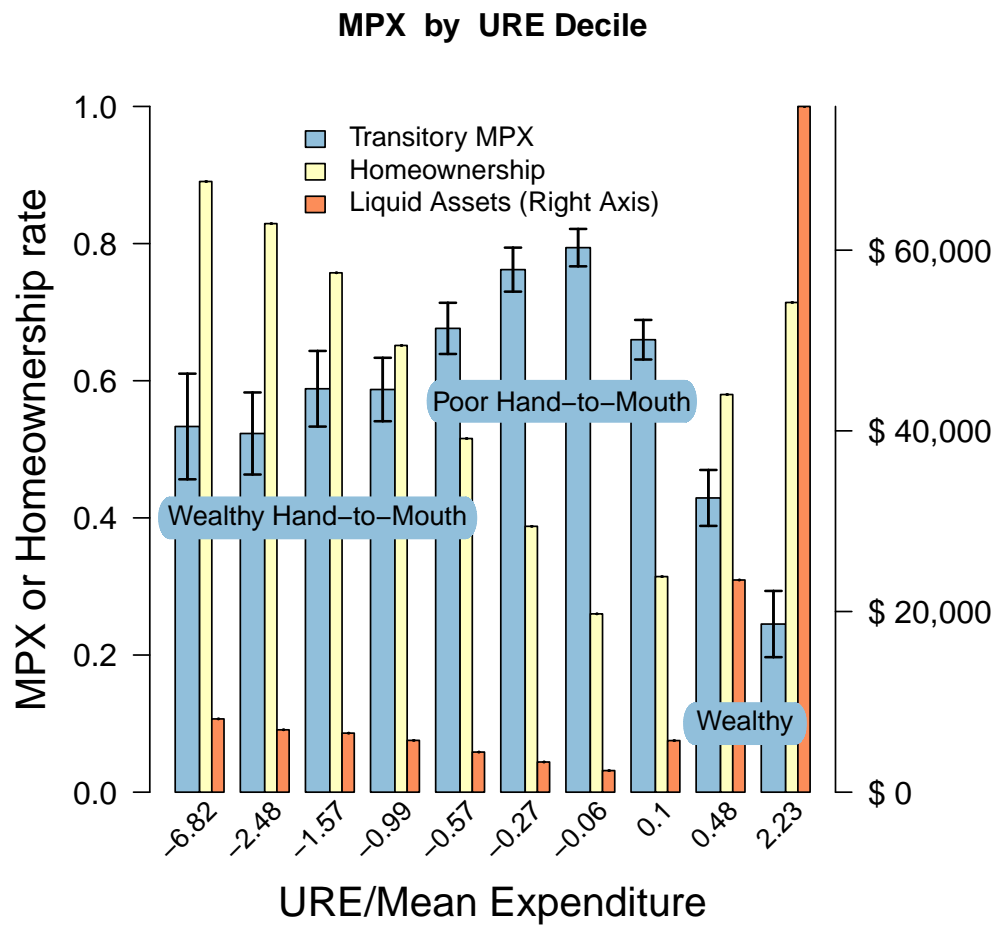
Figure VIII MPX Distribution by URE, NNP, and Income

where σ is the elasticity of intertemporal substitution, γ is the elasticity of relative income to aggregate income, and the five sufficient statistics, $\mathcal{M}$, $\mathcal{E}_Y$, $\mathcal{E}_P$, $\mathcal{E}_R$, and $\mathcal{S}$, are measurable in the data and defined in table III.[41]

In figure VIII, we estimated MPX's for households with heads between the ages of 30 and 55, excluding the young and the old. Furthermore, some of the URE and NNP exposures are held indirectly on households' balance sheets through pension funds and corporations, or by the government and foreigners, so that the URE and NNP exposure in our sample does not aggregate to zero. We allocate the aggregate URE and NNP exposure from our sample into seven bins so that the total exposure across the economy is zero. These bins include households with (i) young (<30) and (ii) old (>55) heads, and exposures held by households indirectly through (iii) pension funds, (iv) government, (v) nonfinancial corporates, (vi) financials, and (vii) exposures held by the rest of the world. Within each of these bins we assume no heterogeneity so that the MPX with respect to these exposures is constant. This assumption is conservative, and likely underestimates the size of the heterogeneous agent channels. Our assumptions on the level of these MPXs can be seen in table IV.

Table III Sufficient Statistics Definitions

Statistic	Definition	Description
$\mathcal{M}$	$\frac{1}{C}\left[\sum\limits_{i\in\text{Income deciles}} \text{MPX}_i Y_i + \sum\limits_{j\in\{\text{young,old}\}} \text{MPX}_j Y_j\right]$	Income-weighted MPX
$\mathcal{E}_Y$	$\mathcal{M} - \overline{\text{MPX}}\frac{Y}{C}$	Redistribution elasticity for Y
$\mathcal{E}_P$	$\frac{1}{C}\left[\sum\limits_{i\in\text{NNP deciles}} \text{MPX}_i \text{NNP}_i + \sum\limits_{j\in\text{bins}} \text{MPX}_j \text{NNP}_j\right]$	Redistribution elasticity for P
$\mathcal{E}_R$	$\frac{1}{C}\left[\sum\limits_{i\in\text{URE deciles}} \text{MPX}_i \text{URE}_i + \sum\limits_{j\in\text{bins}} \text{MPX}_j \text{URE}_j\right]$	Redistribution elasticity for R
$\mathcal{S}$	$1 - \frac{1}{C}\left[\sum\limits_{i\in\text{Consumption deciles}} \text{MPX}_i C_i + \sum\limits_{j\in\{\text{young,old}\}} \text{MPX}_j C_j\right]$	Hicksian scaling factor

Note: $\overline{\text{MPX}}$ is the mean MPX over all households in the economy. Y and C are aggregate household income and consumption respectively. Bins refers to the seven categories for which we have allocated URE and NNP exposures outside our estimation sample. {young,old} are the two bins that contain young and old households (the other five bins are only relevant for URE and NNP exposures as Y and C measure *household* income and consumption).

We define $\mathcal{E}_R$ as

$$\mathcal{E}_R = \frac{1}{C}\left[\sum_{i\in\text{URE deciles}} \text{MPX}_i \text{URE}_i + \sum_{j\in\text{bins}} \text{MPX}_j \text{URE}_j\right] \tag{8}$$

where i sums over the 10 deciles of URE, j over the seven bins defined above, and C is aggregate household expenditure in the economy. This method of dealing with the fact that aggregate exposure does not equal zero in the estimation sample is different than

---

[41]Here we are making the simplifying assumptions that σ and γ are common for all households; see Auclert (2019) for a discussion.

Table IV Aggregating Redistribution Elasticities

	MPX	NNP	URE	$\mathcal{E}_P$ component	$\mathcal{E}_R$ component
Sample	See Distribution	-204	-61	-0.75	-0.29
Young	0.5	-32	-15	-0.12	-0.06
Old	0.5	-23	6	-0.09	0.02
Pension Funds	0.1	137	37	0.10	0.03
Government	0.0	-85	-23	0.00	0.00
Non-financial Corp.	0.1	-49	-13	-0.04	-0.01
Financial Sector	0.1	223	61	0.17	0.05
Rest of World	0.0	33	9	0.00	0.00
Total		**0**	**-0**	**-0.73**	**-0.26**

Notes: NNP and URE numbers are in billions of 2015 USD. Pension Funds includes special saving such as children's savings accounts. See appendix D for detail.

the approach taken by Auclert. He assumes the residual exposure is distributed equally across households in the sample. By making use of the national accounts, we believe we are able to get a better handle on the likely MPXs to attach to this residual exposure.

6.3 Out of Sample MPX

The assumptions we make about the MPX of the young and the old, as well as out of indirectly held URE and NNP exposures, are shown in table IV. In each case we believe we have made conservative choices that will underestimate the size of the interest rate exposure channel of monetary policy. For the young we choose an MPX of 0.5, in line with the rest of the population. As the young have aggregate negative exposures, choosing an MPX on the low side is conservative. Similarly, for the old we choose an MPX of 0.5, which is on the high side for this age group. The assumption that there is no heterogeneity in MPX within these groups is also a very conservative assumption.

Much of the URE and NNP exposure is not held directly on the balance sheet of households but instead indirectly through pension funds, corporates, and the government. There is significant evidence that the MPX out of shocks to the value of pension wealth, stocks, or the government balance sheet is substantially lower than the MPX from income. We choose to use the estimate from Maggio, Kermani, and Majlesi (2019) that households' MPX from changes in stock market wealth is about 10%. This choice is the most quantitatively important for the sufficient statistics, as the bin containing the most exposure is the financial sector, which is positively exposed to interest rate

increases. This positive interest rate exposure may seem surprising because banks are typically thought to have long-term assets and short-term debt that would result in negative URE exposure. However, our findings are in line with those of Landier, Sraer, and Thesmar (2013), who find that the financial sector benefits from interest rate hikes overall. An important caveat is due here: we focus on the MPX out of changes in the assets indirectly held by households through the financial sector and do not assume any spending or lending response at the bank level. This assumption may be reasonable in good times when banks are not credit constrained, but will hold during a banking crisis. Financial frictions could possibly result in monetary policy being much less effective during a banking crisis as the interest rate exposure channel to household spending is counterbalanced by a channel from bank balance sheet interest rate exposure to lending.[42]

We choose an MPX of zero for government and the rest of the world. There is no evidence that households respond in any significant way to changes in the government's balance sheet, and furthermore a low MPX is a conservative assumption for the size of the heterogeneous agent channels. As Denmark is a very small part of the world economy, we assume that foreigners spend a negligible proportion of their wealth there.

6.4   Results

Our estimates of the five sufficient statistics, along with their standard errors, are shown in table V. The aggregate income channel is summarized by $\mathcal{M}$ that we estimate to be 0.52. This means that if income for all households in the economy increased by 1%, aggregate consumption growth would increase by 52 basis points, broadly in line with calibrations of saver-spender models designed to fit evidence from Campbell and Mankiw (1989). We find little role for the redistribution effect of income, $\mathcal{E}_Y$, despite the clear negative correlation between income and MPX seen in figure VIII.[43] $\mathcal{S}$, the Hicksian scaling factor, is 0.49, which reduces the size of the intertemporal substitution channel by close to half.

The two most interesting statistics are $\mathcal{E}_P$ and $\mathcal{E}_R$, both of which act through redistribution from households with low MPX to those with high MPX. $\mathcal{E}_P$ is estimated to be negative 0.73 suggesting that a one-time increase in the price level of 1% increases aggregate consumption growth by 73 basis points due to redistribution from those with large nominal assets to those with large nominal debts. This Fisher channel of monetary

---

[42]It should be noted that our analysis is all on the household side. Evidence suggests that firms are also sensitive to changes in cash flow; for example, see Blanchard, Lopez-de Silanes, and Shleifer (1994).

[43]Patterson (2019) finds a larger role for income heterogeneity by dividing households into groups that have differing income sensitivity to aggregate income.

Table V Sufficient Statistics

$\mathcal{M}$	$\mathcal{E}_Y$	$\mathcal{E}_P$	$\mathcal{E}_R$	$\mathcal{S}$
0.52	-0.03	-0.73	-0.26	0.49
(0.01)	(0.01)	(0.03)	(0.01)	(0.01)

policy is emphasized in Doepke and Schneider (2006). The interest rate exposure channel is also large. We estimate $\mathcal{E}_R$ to be negative 0.26, suggesting that a 1% increase in the interest rate decreases aggregate household expenditure growth by 26 basis points.

For both of these channels, but particularly the interest rate exposure channel, it is informative to compare them to the size of the intertemporal substitution channel. An increase in the real interest rate reduces aggregate consumption today by $\sigma \mathcal{S}$ multiplied by the percent point change in the rate. Reliable estimates of σ have been elusive to the economics profession, but there is very little evidence of a large positive number. Havranek (2015) provides a meta-study of the elasticity of intertemporal substitution and finds a mean of zero from studies using macrodata, and 0.3 to 0.4 for those using microdata. Many of these microlevel studies suffer from identification problems.[44] Best, Cloyne, Ilzetzki, and Kleven (2019) make use of mortgage notches in the United Kingdom to overcome some of these problems. They estimate the average elasticity of intertemporal substitution to be 0.1, which would result in a size of the intertemporal substitution channel of monetary policy being 0.05, over five times smaller than our estimate of the interest rate exposure channel.[45]

These estimates are significantly different to the results found in Auclert (2019). Our estimate of the Fisher channel is an order of magnitude larger, while our estimate of the interest rate exposure channel is over twice as large.[46]

---

[44]See Carroll (2001) for a critique of studies of the elasticity of intertemporal substitution.

[45]Our decomposition does not allow easy comparison of the interest rate exposure channel with the aggregate income channel, as we do not make assumptions about how much aggregate income changes. Cloyne, Ferreira, and Surico (2019) compare mortgagors with outright homeowners and find the aggregate income channel is larger than the direct effect of higher mortgage payments.

[46]The differences are mainly driven by two factors: (i) Our methodology uncovers more heterogeneity in MPX; (ii) we allocate URE and NNP exposures to sectors according to the national accounts, as opposed to assuming lump-sum rebates to households.
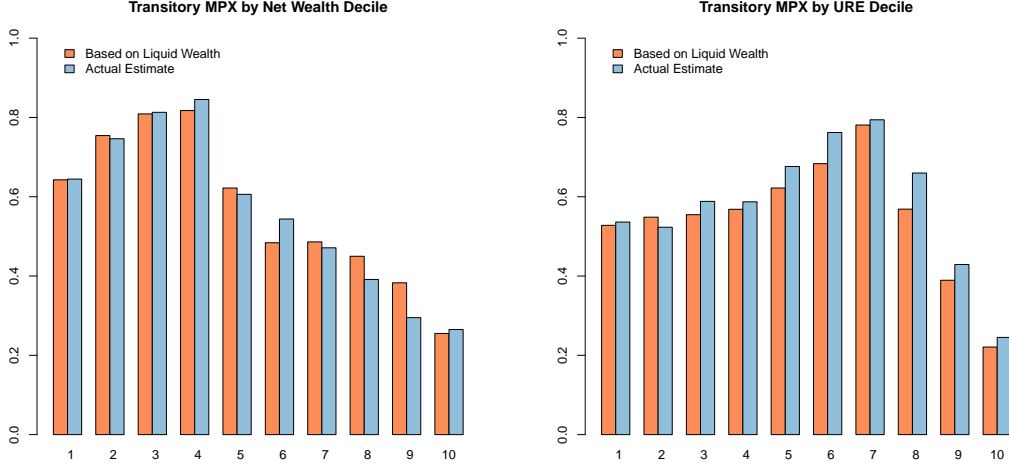
Figure IX Transitory MPX Interpolated From Liquid Wealth

7 Using Liquid Wealth to Estimate MPX and Application to the United States

Here we show that the estimate of a group's MPX obtained only from the median level of liquid wealth in the group is, for all practical purposes, the same as that obtained from running our full estimation procedure on that group. This allows us to plausibly extend our analysis beyond Denmark to measure the extent of MPX heterogeneity in the United States.

7.1 Can Median Liquid Wealth Predict a Group's MPX?

We proceed by first estimating the MPX for Danish households for each decile of liquid wealth. We then take the median liquid wealth for the group of households we are interested in, and linearly interpolate an MPX based on the position of this median liquid wealth for the group within the deciles of liquid wealth whose MPX are known.

Figure IX shows how estimates obtained in this way, using only the median liquid wealth of each decile, compares with the full estimation of transitory MPX for each decile of net wealth and URE. Median liquid wealth approximates the full estimation level and shape very closely. Table VI shows, for each of the dimensions we have previously looked at in this paper, that median wealth predicts the transitory MPX for each decile to around four percentage points, and the permanent MPX for each decile to around five

Table VI Mean Square Prediction Errors Using Interpolated MPX's

Interpolate using:	Transitory		Permanent	
	Absolute	Ratio	Absolute	Ratio
URE Deciles	0.05	0.07	0.06	0.06
NNP Deciles	0.04	0.09	0.05	0.05
Income Deciles	0.05	0.05	0.06	0.08
Consumption Deciles	0.04	0.07	0.06	0.06
Net Wealth Deciles	0.04	0.06	0.04	0.06

Notes: Mean square errors are for the interpolated values relative to the MPX estimated using the full estimation procedure. Interpolation uses either the absolute value of liquid wealth, or the ratio of liquid wealth to mean income over the sample period.

percentage points. Surprisingly, the absolute level of liquid wealth is better at predicting MPX than the ratio of liquid wealth the permanent income, shown in columns 2 and 4 of table VI.

7.2 An Application to U.S. Data

While there is no equivalent data source in the US, and in particular expenditure data is hard to come by, we can use the fact that liquid wealth is highly predictive of MPX to estimate the size of the monetary policy channels in the US. Specifically, we use the Survey of Consumer Finances (SCF) — which has detailed balance sheet information — to calculate households URE and NNP positions, along with liquid wealth.[47] We divide the population into deciles along each dimension, and estimate each deciles' MPX by interpolating the percentile of liquid wealth of the median household in each decile on the Danish MPX estimates by liquid wealth decile.

In the U.S., household debt is approximately half that found in Denmark. As a result, the Fisher channel is smaller (elasticity $\varepsilon_P = -0.25$). Furthermore, the fact that few mortgages adjust their interest rate within the year means the interest rate exposure channel is almost non-existent (elasticity $\varepsilon_R = -0.01$).

---

[47]The SCF does not contain saving data, which in the Danish data is a small part of URE, and we ignore this in our calculation of URE in the U.S.

8 Robustness

In the online appendix we address a number of identification concerns.[48] Here we focus on two that we think deserve highlighting: measurement error and durable expenditure.

8.1 Measurement Error

Our identification comes from estimating $\mathrm{Var}(\Delta^N \bar{y})$ and $\mathrm{Cov}(\Delta^N \bar{c}, \Delta^N \bar{y})$ using our observed data. For unbiased estimates of $\mathrm{Var}(\Delta^N \bar{y})$ we require no measurement error in our observed changes in labor income. For unbiased estimation of $\mathrm{Cov}(\Delta^N \bar{c}, \Delta^N \bar{y})$ we only require (further to no measurement error in income growth) that the measurement error in expenditure growth is uncorrelated with labor income growth. As our expenditure is imputed from income and changes in assets, this is potentially more of a concern than would be the case in survey data in which questions about consumption are not directly linked to those on income. We will examine potential sources of error in labor income and imputed consumption.

8.1.1 Labor Income

For most workers, labor income is well measured. Third party reporting, along with a high level of trust in government institutions, means that underreporting is likely very low. The black economy in Denmark is small, and to the extent that any growth in unreported income is uncorrelated with growth in reported income this will not bias our estimates.[49] In contrast to survey data, in which measurement error in income is likely to downwardly bias transitory MPX estimates, this is of very little concern in our data.

8.1.2 Imputed Expenditure

Expenditure is calculated as the residual of total household income (including interest and dividends) after pension contributions and the change in net wealth have been deducted. For households with simple financial lives (which we believe fits most of the Danish population), this should work well. There are a few scenarios that merit further investigation.

---

[48] Online appendix F looks a consumption persistence beyond two years; G goes into more detail on durables; H considers alternative income processes; I looks at time-varying risk; and J looks at alternative data definitions.

[49] Such income may show up as a change in net wealth and hence expenditure, but measurement error in the change in expenditure uncorrelated with the change in labor income will not bias our MPX estimates.

- **Stock Market Capital Gains:** Only 10% of Danish households directly own stocks or mutual funds.[50] In online appendix J we show that the qualitative patterns we observe are unchanged even when we completely remove these households from the sample. For households that do own stocks, we assume the return they receive is equal to a diversified portfolio of Danish stocks. Given that different households will have their own idiosyncratic portfolios, this methodology will result in significant measurement error. Baker, Kueng, Pagel, and Meyer (2018) show that the size of this measurement error is not only correlated with income and wealth, but also with the business cycle. Furthermore, Fagereng, Guiso, Malacrino, and Pistaferri (2019) show that some groups of investors consistently outperform the market, which would lead us to consistently underestimate their expenditure. Our concern, however, is that the *change* in measurement error of expenditure be correlated with the *change* in labor income. Consistently underestimating expenditure by the same amount is therefore not a problem for us. Furthermore, as we have removed all aggregate effects from the labor income residuals that we use in estimation, any measurement error correlated with the business cycle will be uncorrelated with our measure of changes in labor income. We see two potential ways in which mis-measuring stock returns may bias our results. First, if households have significantly invested in the stock of the firm they work for, which is likely only to be the case for high-level management. Second, to the extent that households invest their labor income gains halfway through the year, we will underestimate expenditure for those whose income increases, and overestimate it for those whose income decreases, leading us to underestimate the MPX. The size of this bias is limited by the size of excess expected returns, so our MPX estimate will be biased by no more than a few percentage points.

- **Family and Friends Transfers:** If a household receives a transfer of money from their parents, for example, imputed expenditure will be lower than true expenditure by this amount. Large transfers typically occur upon death of a parent, which is likely to be uncorrelated with the household head's labor income, or when purchasing a house—years that we have already excluded from our sample. However, to the extent that friends and family actively insure each other's labor income, our MPX estimates will be upward biased.

- **Off-Balance-Sheet Assets:** A larger concern is that some forms of saving may

---

[50]In our calculation we directly observe flows in and out of pension accounts, so these can be treated as off balance sheet in which capital gains do not affect our expenditure calculation.

be hidden off balance sheet. Our imputation method would interpret off-balance-sheet saving as expenditure, so our estimate of the MPX would increase one-to-one for each percentage point of saving out of income shocks performed off balance sheet. All Danish banks and brokers are required to report their clients' holdings, so off-balance-sheet assets are likely to be either offshore or nonfinancial assets. Such off-balance-sheet saving would be a large concern if we were focused on the expenditure of the super wealthy 0.1%, but is less so when dividing the population into quintiles or deciles as we have done.

8.2 Durables

A critique of our empirical methodology is that it does not take account of durable goods, while our data include all spending (except on real estate) and therefore include large and durable goods such as cars and home improvements. The empirical model assumes that in response to a transitive income shock, expenditure increases temporarily for up to two years, which is entirely consistent with a model that includes durable goods. However, the model assumes that in response to a permanent shock to income, expenditure increases once to a new permanent level. A model that included durable goods would instead imply a large one-off expenditure on durable goods to get the households up to their desired flow of durable good services, followed by a decrease back to a permanent level of spending that accounts for replenishing the higher level of depreciating durable goods.

In online appendix G we address this problem in two ways. First we show that our MPX estimates are unbiased in a simple model that includes durables, as long as we interpret the MPX out of transitory shocks to include durable expenditure (the correct definition for understanding aggregate demand) and the MPX out of permanent shocks to include only the consumption *flow* from durables. Second, we are able to construct a nondurable consumption proxy for each household using registry data on car purchases. This proxy has very large measurement error, but will result in unbiased estimates of the MPC (excluding durables) to both permanent and transitory shocks. The estimated MPCs by liquid wealth quintile are shown in figure X. The figure shows the estimates using the nondurable proxy are, as expected, lower than those including all expenditures, although the change in magnitude is similar in size to the overall fraction of durable expenditure, suggesting durables do not play a special role in expenditures following transitory shocks. For the top quintile, durables do appear to play an outsized role, accounting for about a third of the expenditure response to transitory shocks.
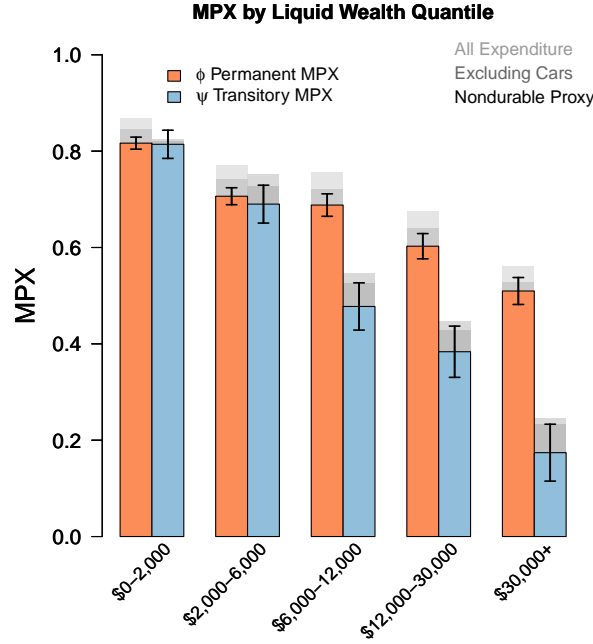
Figure X MPX Removing Cars and Using the Nondurable Proxy Panel

9 Conclusion

In this paper we have presented a new method to measure the sensitivity of consumption to permanent and transitory income shocks for different groups of households. Our focus has been to use this method to test the microfoundations of heterogeneous agent models and quantify the importance of consumption heterogeneity for monetary policy. With administrative data from Denmark, we have been able to dig into the distribution of MPC across wealth, URE and NNP in far more detail than has previously been possible. We find that MPCs vary systematically along these dimensions and in ways that are important for monetary policy transmission, although the current generation of heterogeneous agent models struggle to fit the high sensitivity to income that we observe.

Our hope is that the method we present in this paper, or variants of it, can also be of use to economists in a variety of fields. More and more high-quality microdata on consumption are becoming available, such as the administrative data used here, or the even more detailed transaction-level data available from financial aggregators. If this trend continues, as we hope it will, methods such as ours will become even more

valuable in bridging the gap between models and data.

References

ABILDGREN, KIM, ANDREAS KUCHLER, AMERICA SOLANGE LOHMANN RASMUSSEN, AND HENRIK SEJERBO SORENSEN (2018): "Consistency between Household-Level Consumption Data from Registers and Surveys," Danmarks Nationalbank Working Paper, no. 131.

AGARWAL, SUMIT, AND WENLAN QIAN (2014): "Consumption and Debt Response to Unanticipated Income Shocks: Evidence from a Natural Experiment in Singapore," *American Economic Review*, 104(12), 4205–30.

AIYAGARI, S. RAO (1994): "Uninsured Idiosyncratic Risk and Aggregate Saving," *Quarterly Journal of Economics*, 109, 659–684.

ALTONJI, JOSEPH, AND LEWIS M. SEGAL (1996): "Small-Sample Bias in GMM Estimation of Covariance Structures," *Journal of Business & Economic Statistics*, 14(3), 353–66.

AMPUDIA, MIGUEL, DIMITRIS GEORGARAKOS, JIRI SLACALEK, ORESTE TRISTANI, PHILIP VERMEULEN, AND GIOVANNI VIOLANTE (2018): "Monetary Policy and Household Inequality," SSRN Scholarly Paper ID 3223542, Social Science Research Network, Rochester, NY.

ARELLANO, MANUEL, RICHARD BLUNDELL, AND STÉPHANE BONHOMME (2017): "Earnings and Consumption Dynamics: A Nonlinear Panel Data Framework," *Econometrica*, 85(3), 693–734.

AUCLERT, ADRIEN (2019): "Monetary Policy and the Redistribution Channel," *American Economic Review*, 109(6), 2333–67.

BAKER, SCOTT R. (2018): "Debt and the Response to Household Income Shocks: Validation and Application of Linked Financial Account Data," *Journal of Political Economy*, 126(4), 1504–57.

BAKER, SCOTT R., LORENZ KUENG, MICHAELA PAGEL, AND STEFFEN MEYER (2018): "Measurement Error in Imputed Consumption," SSRN Scholarly Paper ID 3179117, Social Science Research Network, Rochester, NY.

BEST, MICHAEL CARLOS, JAMES S CLOYNE, ETHAN ILZETZKI, AND HENRIK J KLEVEN (2019): "Estimating the Elasticity of Intertemporal Substitution Using Mortgage Notches," *Review of Economic Studies*, Forthcoming.

BEWLEY, TRUMAN (1983): "A Difficulty with the Optimum Quantity of Money," *Econometrica*, 51(5), 1485–504.

BLANCHARD, OLIVIER JEAN, FLORENCIO LOPEZ-DE SILANES, AND ANDREI SHLEIFER (1994): "What Do Firms Do with Cash Windfalls?," *Journal of Financial Economics*, 36(3), 337–60.

BLUNDELL, RICHARD, LUIGI PISTAFERRI, AND IAN PRESTON (2008): "Consumption Inequality and Partial Insurance," *American Economic Review*, 98(5), 1887–1921.

BROWNING, MARTIN, AND M. DOLORES COLLADO (2001): "The Response of Expenditures to Anticipated Income Changes: Panel Data Estimates," *American Economic Review*, 91(3), 681–92.

BROWNING, MARTIN, AND SØREN LETH-PETERSEN (2003): "Imputing Consumption from Income and Wealth Information," *Economic Journal*, 113(488), F282–F301.

BUNN, PHILIP, JEANNE LE ROUX, KATE REINOLD, AND PAOLO SURICO (2018): "The Consumption Response to Positive and Negative Income Shocks," *Journal of Monetary Economics*, 96, 1–15.

CAMPBELL, JOHN Y., AND N. GREGORY MANKIW (1989): "Consumption, Income and Interest Rates: Reinterpreting the Time Series Evidence," NBER Chapters, National Bureau of Economic Research.

CARROLL, CHRISTOPHER, AND ANDREW SAMWICK (1997): "The Nature of Precautionary Wealth," *Journal of Monetary Economics*, 40(1), 41–71.

CARROLL, CHRISTOPHER, JIRI SLACALEK, KIICHI TOKUOKA, AND MATTHEW N. WHITE (2017): "The Distribution of Wealth and the Marginal Propensity to Consume," *Quantitative Economics*, 8(3), 977–1020.

CARROLL, CHRISTOPHER DIXON (2001): "Death to the Log-Linearized Consumption Euler Equation! (And Very Poor Health to the Second-Order Approximation)," *Advances in Macroeconomics*, 1(1).

CLOYNE, JAMES, CLODOMIRO FERREIRA, AND PAOLO SURICO (2019): "Monetary Policy when Households Have Debt: New Evidence on the Transmission Mechanism," *Review of Economic Studies*, Forthcoming.

COMMAULT, JEANNE (2017): "How Does Consumption Respond to a Transitory Income Shock? Reconciling Natural Experiments and Structural Estimations," Working Paper.

CORONADO, JULIA LYNN, JOSEPH P. LUPTON, AND LOUISE M. SHEINER (2005): "The Household Spending Response to the 2003 Tax Cut: Evidence from Survey Data," FEDS Discussion Paper 32, Federal Reserve Board.

41

CRAWLEY, EDMUND (2020): "In search of lost time aggregation," *Economics Letters*, 189, 108998.

DAVID, MARTIN, KENT H. MARQUIS, JEFFREY C MOORE, LINDA L. STINSON, AND EDWARD J. WELNIAK (1997): "Income Measurement Error in Surveys: A Review - Semantic Scholar," Discussion paper, U.S. Bureau of the Census, Washington.

DOEPKE, MATTHIAS, AND MARTIN SCHNEIDER (2006): "Inflation and the Redistribution of Nominal Wealth," *Journal of Political Economy*, 114(6), 1069–97.

FAGERENG, ANDREAS, LUIGI GUISO, DAVIDE MALACRINO, AND LUIGI PISTAFERRI (2019): "Heterogeneity and Persistence in Returns to Wealth," *Econometrica*, Forthcoming.

FAGERENG, ANDREAS, AND ELIN HALVORSEN (2017): "Imputing Consumption from Norwegian Income and Wealth Registry Data," *Journal of Economic and Social Measurement*, 42(1), 67–100.

FAGERENG, ANDREAS, MARTIN B. HOLM, AND GISLE J. NATVIK (2016): "MPC Heterogeneity and Household Balance Sheets," Discussion Paper, Statistics Norway, Research Department.

FUSTER, ANDREAS, GREG KAPLAN, AND BASIT ZAFAR (2018): "What Would You Do With $500? Spending Responses to Gains, Losses, News and Loans," NBER Working Paper 24386, National Bureau of Economic Research.

GANONG, PETER, AND PASCAL NOEL (2019): "Consumer Spending during Unemployment: Positive and Normative Implications," *American Economic Review*, 109(7), 2383–2424.

GARRIGA, CARLOS, FINN E. KYDLAND, AND ROMAN SUSTEK (2017): "Mortgages and Monetary Policy," *The Review of Financial Studies*, 30(10), 3337–3375.

GELMAN, MICHAEL (2016): "What Drives Heterogeneity in the Marginal Propensity to Consume? Temporary Shocks vs Persistent Characteristics," Working Paper.

GELMAN, MICHAEL, YURIY GORODNICHENKO, SHACHAR KARIV, DMITRI KOUSTAS, MATTHEW D. SHAPIRO, DAN SILVERMAN, AND STEVEN TADELIS (2016): "The Response of Consumer Spending to Changes in Gasoline Prices," NBER Working Paper 22969, National Bureau of Economic Research.

GELMAN, MICHAEL, SHACHAR KARIV, MATTHEW D. SHAPIRO, DAN SILVERMAN, AND STEVEN TADELIS (2014): "Harnessing Naturally Occurring Data to Measure the Response of Spending to Income," *Science*, 345(6193), 212–15.

GREENWALD, DANIEL (2018): "The Mortgage Credit Channel of Macroeconomic Transmission," SSRN Scholarly Paper ID 2735491, Social Science Research Network, Rochester, NY.

GUVENEN, FATIH (2007): "Learning Your Earning: Are Labor Income Shocks Really Very Persistent?," *American Economic Review*, 97(3), 687–712.

———— (2009): "An Empirical Investigation of labor income Processes," *Review of Economic Dynamics*, 12(1), 58–79.

HAUSMAN, JOSHUA K. (2012): "Fiscal Policy and Economic Recovery: The Case of the 1936 Veterans' Bonus," mimeo, University of California, Berkeley.

HAVRANEK, TOMAS (2015): "Measuring Intertemporal Substitution: The Importance of Method Choices and Selective Reporting," *Journal of the European Economic Association*, 13(6), 1180–1204.

HEATHCOTE, JONATHAN, FABRIZIO PERRI, AND GIOVANNI L. VIOLANTE (2010): "Unequal We Stand: An Empirical Analysis of Economic Inequality in the United States, 1967-2006," *Review of Economic Dynamics*, 13(1), 15–51.

HRYSHKO, DMYTRO, AND IOURII MANOVSKII (2018): "Income Dynamics and Consumption Insurance," Working Paper.

HSIEH, CHANG-TAI (2003): "Do Consumers React to Anticipated Income Changes? Evidence from the Alaska Permanent Fund," *American Economic Review*, 99(1), 397–405.

HUGGETT, MARK (1993): "The risk-free rate in heterogeneous-agent incomplete-insurance economies," *Journal of Economic Dynamics and Control*, 17(5), 953–69.

IMROHOROĞLU, AYŞE (1989): "Cost of Business Cycles with Indivisibilities and Liquidity Constraints," *Journal of Political Economy*, 97(6), 1364–83.

JAPPELLI, TULLIO, AND LUIGI PISTAFERRI (2010): "The Consumption Response to Income Changes," *Annual Review of Economics*, 2(1), 479–506.

———— (2014): "Fiscal Policy and MPC Heterogeneity," *American Economic Journal: Macroeconomics*, 6(4), 107–36.

JOHNSON, DAVID S., JONATHAN A. PARKER, AND NICHOLAS S. SOULELES (2006): "Household Expenditure and the Income Tax Rebates of 2001," *American Economic Review*, 96(5), 1589–610.

——— (2009): "The Response of Consumer Spending to Rebates During an Expansion: Evidence from the 2003 Child Tax Credit," Working Paper, The Wharton School.

KAPLAN, GREG, BENJAMIN MOLL, AND GIOVANNI L. VIOLANTE (2018): "Monetary Policy According to HANK," *American Economic Review*, 108(3), 697–743.

KAPLAN, GREG, AND GIOVANNI L. VIOLANTE (2010): "How Much Consumption Insurance beyond Self-Insurance?," *American Economic Journal: Macroeconomics*, 2(4), 53–87.

——— (2018): "Microeconomic Heterogeneity and Macroeconomic Shocks," *Journal of Economic Perspectives*, 32(3), 167–94.

KOIJEN, RALPH, STIJN VAN NIEUWERBURGH, AND ROINE VESTMAN (2014): "Judging the Quality of Survey Data by Comparison with "Truth" as Measured by Administrative Records: Evidence From Sweden," in *NBER Chapters: Improving the Measurement of Consumer Expenditures*, pp. 308–46. National Bureau of Economic Research.

KRUSELL, PER, AND ANTHONY SMITH (1998): "Income and Wealth Heterogeneity in the Macroeconomy," *Journal of Political Economy*, 106(5), 867–96.

LANDIER, AUGUSTIN, DAVID SRAER, AND DAVID THESMAR (2013): "Banks' Exposure to Interest Rate Risk and the Transmission of Monetary Policy," NBER Working Paper 18857, National Bureau of Economic Research.

LOW, HAMISH, COSTAS MEGHIR, AND LUIGI PISTAFERRI (2010): "Wage Risk and Employment Risk over the Life Cycle," *American Economic Review*, 100(4), 1432–67.

LUSARDI, ANNAMARIA (1996): "Permanent Income, Current Income, and Consumption: Evidence from Two Panel Data Sets," *Journal of Business and Economic Statistics*, 14(1), 81–90.

MAGGIO, MARCO DI, AMIR KERMANI, AND KAVEH MAJLESI (2019): "Stock Market Returns and Consumption," *Journal of Finance*, Forthcoming.

MANOVSKII, IOURII, AND DMYTRO HRYSHKO (2017): "How Much Consumption Insurance in the U.S.?," Working Paper 1584, Society for Economic Dynamics.

MEGHIR, COSTAS, AND LUIGI PISTAFERRI (2004): "Income Variance Dynamics and Heterogeneity," *Econometrica*, 72(1), 1–32.

MOFFITT, ROBERT, AND SISI ZHANG (2018): "Income Volatility and the PSID: Past Research and New Results," *AEA Papers and Proceedings*, 108, 277–80.

MOFFITT, ROBERT A., AND PETER GOTTSCHALK (2012): "Trends in the Transitory Variance of Male Earnings Methods and Evidence," *Journal of Human Resources*, 47(1), 204–36.

NIELSEN, HELENA SKYT, AND ANNETTE VISSING-JORGENSEN (2004): "The Impact of Labor Income Risk on Educational Choices: Estimates and Implied Risk Aversion," Working Paper, University of Aarhus.

OLAFSSON, ARNA, AND MICHAELA PAGEL (2018): "The Liquid Hand-to-Mouth: Evidence from Personal Finance Management Software," *The Review of Financial Studies*, 31(11), 4398–446.

PARKER, JONATHAN A. (1999): "The Reaction of Household Consumption to Predictable Changes in Social Security Taxes," *American Economic Review*, 89(4), 959–73.

PARKER, JONATHAN A, NICHOLAS S SOULELES, DAVID S JOHNSON, AND ROBERT MCCLELLAND (2013): "Consumer Spending and the Economic Stimulus Payments of 2008," *American Economic Review*, 103(6), 2530–53.

PATTERSON, CHRISTINA (2019): "The Matching Multiplier and the Amplification of Recessions," Mimeo, Department of Economics, MIT.

REITER, MICHAEL (2009): "Solving heterogeneous-agent models by projection and perturbation," *Journal of Economic Dynamics and Control*, 33(3), 649–665.

SAHM, CLAUDIA R., MATTHEW D. SHAPIRO, AND JOEL B. SLEMROD (2010): "Household Response to the 2008 Tax Rebate: Survey Evidence and Aggregate Implications," *Tax Policy and the Economy*, 24, 69–110.

SHAPIRO, MATTHEW D., AND JOEL SLEMROD (2009): "Did the 2008 Tax Rebates Stimulate Spending?," *American Economic Review*, 99(2), 374–79.

SOULELES, NICHOLAS S. (1999): "The Response of Household Consumption to Income Tax Refunds," *American Economic Review*, 89(4), 947–58.

——— (2002): "Consumer Response to the Reagan Tax Cuts," *Journal of Public Economics*, 85, 99–120.

VIOLANTE, GIANLUCA, GREG KAPLAN, AND JUSTIN WEIDNER (2014): "The Wealthy Hand-to-Mouth," *Brookings Papers on Economic Activity*, 2014(1), 77–138.

WONG, ARLENE (2016): "Population Aging and the Transmission of Monetary Policy to Consumption," 2016 Meeting Paper 716, Society for Economic Dynamics.

WORKING, HOLBROOK (1960): "Note on the Correlation of First Differences of Averages in a Random Chain," *Econometrica*, 28(4), 916–18.

Online Appendix

A   Identification with Time Aggregation

In this section we show how to derive equations 3 and 5 for the variance of income growth and covariance of income and consumption growth.

From equation 2 we have

$$\Delta^N \bar{y}_T = \int_{T-1}^{T} (T-s)dP_s + (P_{T-1} - P_{T-N}) + \int_{T-N-1}^{T-N} (s - (T-2))dP_s$$
$$+ \left(\int_{T-1}^{T} \int_{t-2}^{t} f(t-s)dQ_t dt - \int_{T-N-1}^{T-N} \int_{t-2}^{t} f(t-s)dQ_t dt \right) \tag{9}$$

Making use of the independent increment property of P_t and Q_t, we get

$$\mathrm{Var}(\Delta^N \bar{y}_T) = \int_{T-1}^{T} (T-s)^2 \sigma_P^2 ds + (N-1)\sigma_P^2 + \int_{T-N-1}^{T-N} (s-(T-2))^2 \sigma_P^2 ds$$
$$+ \mathrm{Var}\left(\int_{T-1}^{T} \int_{t-2}^{t} f(t-s)dQ_t dt \right) + \mathrm{Var}\left(\int_{T-N-1}^{T-N} \int_{t-2}^{t} f(t-s)dQ_t dt \right)$$
$$= (N - \frac{1}{3})\sigma_P^2 + 2\mathrm{Var}(\tilde{y}) \text{ for } n \geq 3 \tag{10}$$

The equivalent of equation 2 for consumption is

$$\Delta^N \bar{c}_T = \int_{T-1}^{T} (T-s)\phi dP_s + \phi(P_{T-1} - P_{T-N}) + \int_{T-N-1}^{T-N} (s - (T-2))\phi dP_s$$
$$+ \left(\int_{T-1}^{T} \int_{t-2}^{t} g(t-s)dQ_t dt - \int_{T-N-1}^{T-N} \int_{t-2}^{t} g(t-s)dQ_t dt \right) \tag{11}$$

Again making use of the independent increment property, we can calculate the covariance of income and consumption growth:

$$\mathrm{Cov}(\Delta^N \bar{c}_T, \Delta^N \bar{y}_T) = \int_{T-1}^{T} (T-s)^2 \phi \sigma_P^2 ds + \phi(N-1)\sigma_P^2 + \int_{T-N-1}^{T-N} (s-(T-2))^2 \phi \sigma_P^2 ds$$
$$+ \mathrm{Cov}\left(\int_{T-1}^{T} \int_{t-2}^{t} f(t-s)dQ_t dt, \int_{T-1}^{T} \int_{t-2}^{t} g(t-s)dQ_t dt \right)$$
$$+ \mathrm{Cov}\left(\int_{T-N-1}^{T-N} \int_{t-2}^{t} f(t-s)dQ_t dt, \int_{T-N-1}^{T-N} \int_{t-2}^{t} g(t-s)dQ_t dt \right)$$
$$= \phi(N - \frac{1}{3})\sigma_p^2 + 2\mathrm{Cov}(\tilde{c}, \tilde{y}) \text{ for } N \geq 3 \tag{12}$$

B Sample Selection

We choose to look at households whose head is between the ages of 30 and 55 in 2008, which is driven by the desire to remove households for which the assumption that most of the income growth is unexpected is not likely to be fulfilled. For the old and the young, individual households will likely have a lot of information about their income path that is not available to the econometrician (for example, the year in which they plan to retire, or the fact that they are on a specific career track with set expectations of promotion and pay raises). We also want to remove households whose income volatility is increasing or decreasing sharply. Figures B.1 and B.2 show how our estimates of both income variance and MPX vary with age. The dots represent the point estimate for each age, while the lines are the centered moving averages over the five nearest age groups. The solid black line shows the total variance of income growth over one year. It should not be surprising that income growth for households with heads in their 20's is highly volatile. This volatility plateaus around the age of 35 and stays at a constant level until retirement, at which point it temporarily grows before falling to an even lower level. We can see that while both transitory and permanent shocks to income are high early in life, permanent income shocks are particularly high while individuals find their place in the workforce. From the ages of 30 to 55, both transitory and permanent shocks are approximately the same size and remarkably stable. At retirement, shocks to permanent income rise—not surprising, as the model sees retirement itself as a shock—even as transitory income variance declines.

As the model assumes the variance to permanent and transitory shocks to be constant in the observed period, interpretation of the numbers outside of the 30 to55 age group needs to be treated with care. However, the figure clearly shows that within this age group the assumption of constant variance appears to be a reasonable one.

The dotted black line shows the variance of Δy, assuming no persistence in the transitory component. The fact that this line is slightly above the empirical variance of Δy is consistent with some persistence in the transitory component of income, justifying our decision to exclude growth over one and two years in our identification.

The level of both permanent and transitory shock variance for households aged 30 to 55 is approximately 0.0035, reflecting a standard deviation of 6%. Estimates using U.S. data are significantly higher, especially for the transitory shock variance (for example, Carroll and Samwick (1997) estimate 0.02 for permanent and 0.04 for transitory). This difference may be due to lower income inequality in Denmark, more progressive taxation,

**Permanent and Transitory Variance by Age**
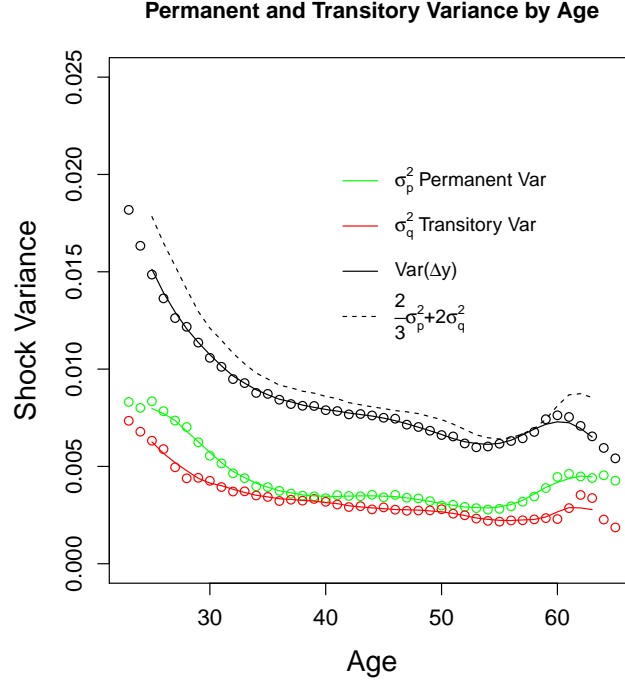
**Figure B.1** Permanent and Transitory Shock Variance by Age

and more generous unemployment insurance. The lower transitory variance will also be due to significantly reduced measurement error relative to the survey-based U.S. data.

C   The Danish Mortgage Market

Mortgage loans in Denmark are issued by specialized mortgage banks, which fully finance loans by issuing bonds. Interest rates are directly determined by sales prices at the bond market. That is, borrowers only pay the bond market interest rate plus a supplementary fee for the mortgage bank. Most loans are issued as 20- or 30-year loans, and households can only obtain loans from mortgage banks for up to 80% of the value at loan origination of properties used as permanent residences. The remaining (more insecure) part of the funding may be provided by commercial banks. The close link between loans and bonds, as well as fixed loan-to-value ratios, fast foreclosure procedures, full recourse, etc., mean that mortgage banks do not assume significant market risks. The status of Danish covered mortgage bonds as a safe asset class (AAA-rated by, e.g., S&P) implies that borrowers have access to very cheap real estate funding.
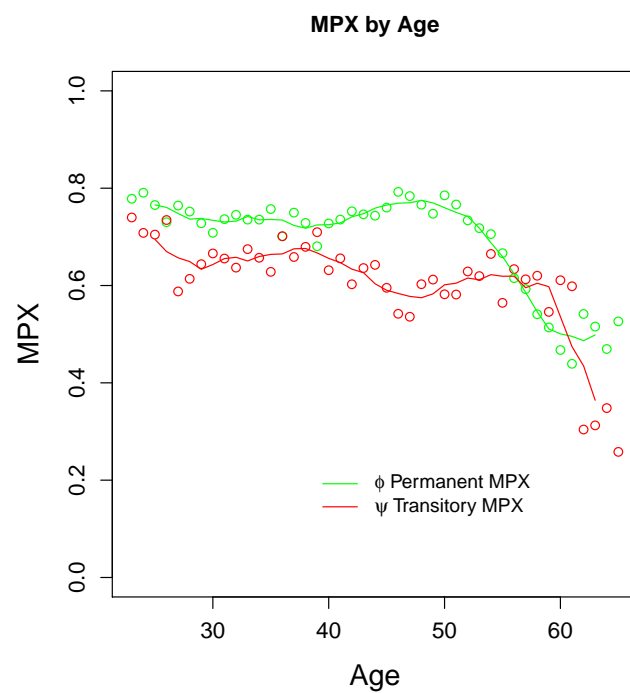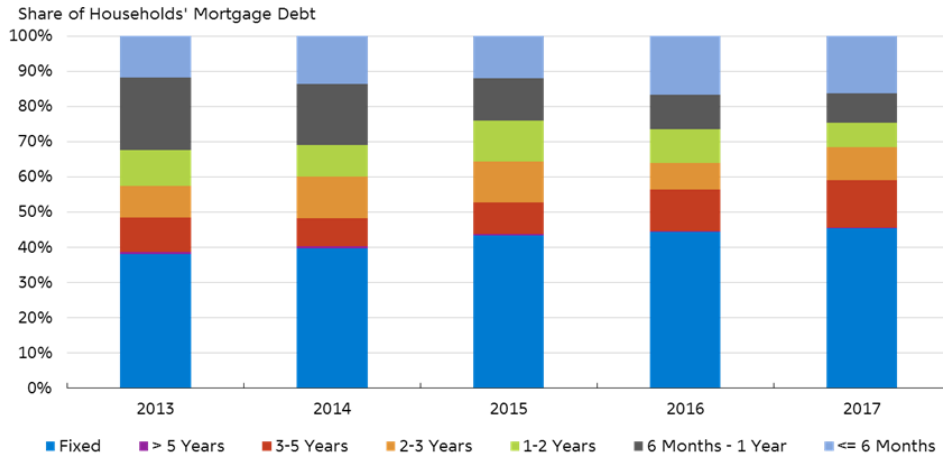
Figure B.2 MPX by Age
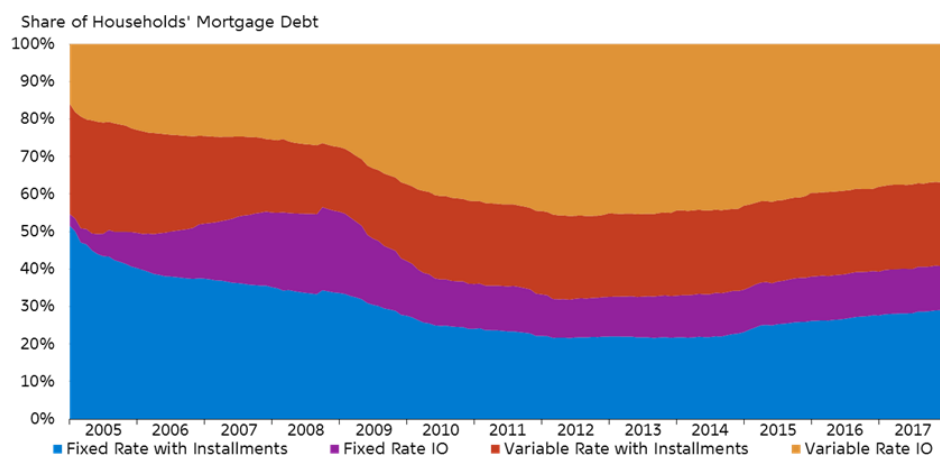
Figure C.1 Mortgage Debt by Type (All Households)

Source: Danmarks Nationalbank

Figure C.2 Mortgage Debt by Maturity (All Households Excluding Self-employed)

Source: Danmarks Nationalbank

The Danish mortgage system has been functioning for two centuries, but significant liberalization has taken place over the past 20 years. Variable interest loans were (re-)introduced in 1996, while interest only loans were introduced in 2003. These new loan characteristics are by now very popular; see figure C.1. In contrast to the United States, where most mortgage debt is fixed rate, 40% of mortgage debt in Denmark is variable rate, with interest fixation periods mostly between six months and five years. Fixed-rate loans come with an option for early redemption, which implies that in practice, refinancing of fixed-rate mortgages often takes place, both when interest rates decrease and increase. The latter may be attractive because borrowers have the option to repay their loan by purchasing the corresponding amount of bonds. When interest rates increase, the bond value decreases, so the option to repay the loan by purchasing the corresponding amount of bonds in essence acts as an equity insurance.

Around one-fourth of the total loan balance is due to have interest rates reset over a 12-month period (see figure C.2). This figure only comprises loans that automatically will have a new interest rate and not active decisions to refinance or extract equity.

D   Details on the Calculation of NNP and URE

The Net Nominal Position (NNP) and Unhedged Interest Rate Exposure (URE) for the various sectors in the Danish economy are calculated from our household-level dataset as well as the financial accounts from the national accounts statistics. All calculations

are based on average values over the years 2009 to 2015, deflated by the consumer price index.

D.1 NNP and URE for Households

The NNP for households is calculated as financial assets minus liabilities. As financial assets, we include bank deposits as well as the market value of securities (excluding shares). Liabilities include all debt to financial institutions (including credit card debt) as well as publicly administered student debt, tax debt and other debt to government bodies. These data are reported to the tax authorities by financial institutions on behalf of the households.

URE is calculated as annual savings (i.e. after-tax income minus expenditure) plus maturing assets minus maturing liabilities. As maturing assets, we include all bank deposits, thereby assuming that they are floating rate. We assume a maturity of five years for securities held by households and therefore include 20% of the value of securities. Regarding liabilities, we assume that all bank debt is floating rate. According to the interest rate statistics collected by Danmarks Nationalbank since 2013, on average 95% of bank debt from households is floating rate, most of which is tied either to a market reference rate or to the Danmarks Nationalbank rate on certificates of deposit, with immediate adjustment. For mortgage debt, we have detailed information allowing us to calculate the stock of debt which is due to have interest rates reset over the coming 12 months. Voluntary refinancing of mortgage loans, with or without extraction of additional equity, takes place to a large extent in Denmark. Our measure of maturing liabilities only includes the loans that are contractually due to have their interest rates reset, and we do not attempt to estimate the amount of additional refinancing. For remaining liabilities, which constitute very small amounts, we have no information regarding maturity, so we assume five years.

D.2 Other Sectors

NNP for the other sectors in the economy is obtained from the financial accounts statistics compiled by Danmarks Nationalbank. To most closely resemble the definition used in the household-level data, we define NNP as net assets (i.e., assets minus liabilities) in the following categories: "Currency and deposits", "Securities other than shares", "Loans", and "Trade credits and other accounts receivable/payable".

NNP for the whole economy should, in principle, sum to 0. However, the household-level microdata on bank deposits that we have access to is exclusive of certain types of savings (specialized children's savings accounts as well as some forms of pension savings accounts administered by banks), which are included in the financial accounts statistics. For the age group included in our sample, these types of accounts can be assumed to be largely illiquid. We therefore group those deposits (33 billion USD) together with the assets of pension funds (see table IV).[51]

URE for non-households is also based on the financial accounts. In the national accounts, we do not observe the maturity of different asset and liability classes. We hold household URE fixed at the values from the micro-level data and take advantage of the identity that total URE in the economy must be 0 to calibrate the maturity for the remaining sectors of the economy. This results in a maturity of assets and liabilities for non-households of 3.65 years.

# E   Comparison to Blundell, Pistaferri, and Preston (2008)

Are the differences in our estimates to those in BPP driven by the data or by the method? Here we apply BPP's original method to moments obtained from the Danish administrative data. Applied to the entire dataset, this method estimates the response to permanent income shocks to be 0.81 and to transitory income shocks to be 0.12. The response to permanent income shocks is in line with our method, while the response to transitory income shocks is significantly biased towards zero, although still larger than that obtained using PSID data.

Figure E.1 shows the results of using BPP's method on quintiles of liquid wealth. While the downward sloping pattern is evident, the magnitudes are very different to those from our method shown in figure V in the paper.

Our method differs from BPP in two respects: first we model time aggregation and second we impose that consumption responses to transitory income shocks are short lived, in contrast to the random walk behavior in BPP. Crawley (2020) shows how just the first of these, modeling time aggregation, significantly changes BPP's estimates in the PSID data. With the high MPX's that we estimate in our data, it is clear

---

[51] In practice, this amount is calculated as a residual, which may also reflect other minor differences between the household-level data and the national accounts statistics. For example, holdings of banknotes and coins are not observed in the microdata but are allocated based on certain assumptions in the financial accounts. For our exercise, the impact of such other differences is likely to be very small.
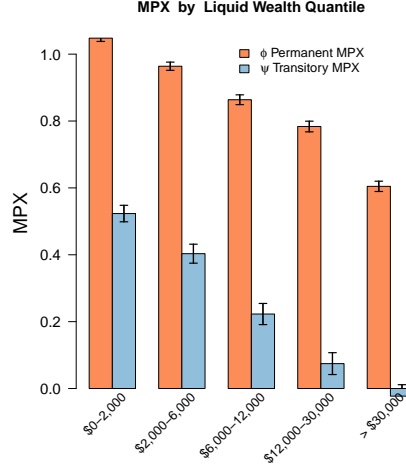
Figure E.1 Estimates by Liquid Wealth Quintile Obtained Using BPP's Methodology

that the random walk assumption cannot be valid. If we only make this first change and estimate again using the Danish administrative data, we estimate the response to permanent income shocks to be 1.18 and to transitory income shocks to be 1.50. These unreasonably high estimates come as a result of the assumption that consumption follows a random walk. In particular, the fact that the response to transitory income shocks is significantly greater than one comes from the method extending the short term response over the period of one year.

This exercise has shown that the original BPP method is both biased and sensitive to assumptions made about the path of consumption. In appendices F, H, and I we show that, in contrast, our method is relatively robust to misspecification.

F   Persistent Consumption Response

Our estimation procedure makes the assumption that the consumption response to a transitory income shock decays to zero in a period of two years or less. A slower decay will lead to a downward bias in our estimates of the transitory MPX. Figure F.1 shows the results of our estimation procedure on simulated data under two different assumptions about the transitory consumption response.

The exponential decay line assumes that the consumption flow following a transitory

shock decays exponentially.[52] We vary the decay rate to match a range of year 1 MPCs and assume that the entire transitory income is eventually consumed. For high MPCs, and especially those over 0.5, there is very little bias. However, for MPCs significantly below 0.5 our method results in downward-biased estimates. This bias arises because low MPCs, combined with exponential consumption decay, result in a relatively stable consumption flow over the first few years that has not declined close to zero after two years.

Empirical evidence suggests that in fact the consumption response to a transitory shock decays quickly in the first few months and then more slowly after that.[53] The "Fagereng et al." line in figure F.1 shows the MPC estimate in simulated data in which the consumption response decays according to the estimates made in Fagereng, Holm, and Natvik (2016). In this case, the fast decay in the first few months results in a smaller bias than the exponential case for low MPCs, while the fact that the decay is slower following these first months results in a larger bias for high MPCs. Overall it seems likely that our assumption about the persistence of the consumption response leads to a slight downward bias across the range of MPCs.

We also show that our MPX estimates are not very sensitive to the choice of N (years of growth in our identification equations) between 3 and 6, which lends further support to the fact that assuming a two-year limit does not bias our results too much.[54]

F.1   Details on Section F Simulations

For the simulations in section F we divided each year into 20 sub-intervals. Both permanent and transitory shocks occur each period, and the transitory shocks have no persistence. At an annual frequency the variance of permanent and transitory shocks are equal. Households spend their permanent income each period, along with their consumption response to the history of transitory shocks. For the exponential decay model, this is

$$c_t = p_t + (1 - \rho) \sum_{n=0}^{\infty} \rho^n \varepsilon_{t-n}$$

In Fagereng, Holm, and Natvik (2016) the T year MPC is estimated as a function:

$$\mathrm{MPC}_T = \theta_1 T^{\theta_2}$$

---

[52]Standard buffer-stock models give rise to a consumption response that decays very close to exponentially.

[53]Both Fagereng, Holm, and Natvik (2016) and Gelman (2016) provide evidence for this.

[54]Using N equal to 4 and 5 instead of 3, 4, and 5 allows us to extend the consumption response out to three years, at the expense of losing data and becoming more sensitive to misspecification of the income process.
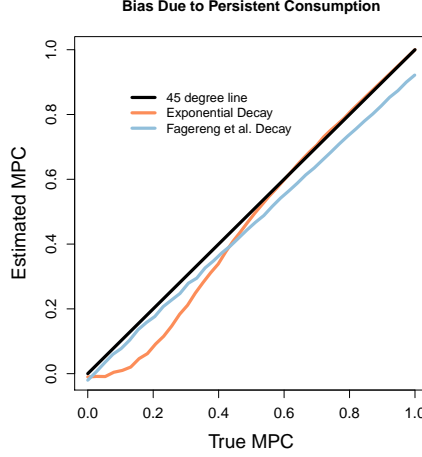
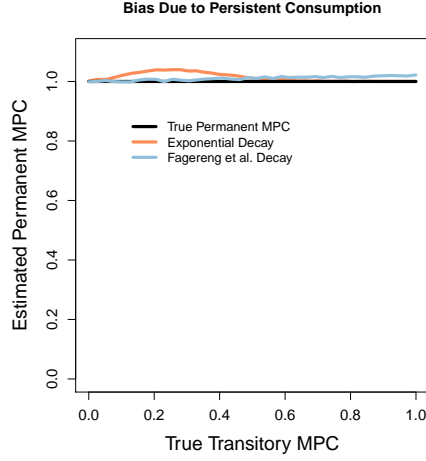**Figure F.1** Bias from Persistent Consumption

where θ_1 controls the size of the response and θ_2 the speed of decay. We vary θ_1 and choose $\theta_2 = 0.2142$ according to their estimate. In this model consumption in period t (measured in sub-intervals) is:

$$c_t = p_t + \theta_1 \sum_{n=0}^{\infty} \left((\frac{n+1}{20})^{\theta_2} - (\frac{n}{20})^{\theta_2} \right) \varepsilon_{t-n}$$

We then time aggregate both income and consumption over each 20-sub-interval period, choose a sample of 13 years, and run our estimation procedure with $N = 3, 4, 5$. The transitory MPC estimates are shown in figure F.1, and the permanent estimates are shown in figure F.2. The bias in permanent estimates is small across the range of transitory MPCs.

F.2   Estimates Using Different Values of N

Table F.1 ψ Estimates Using Different N

Bias Due to Persistent Consumption

Figure F.2 Bias from Persistent Consumption

					n_2		
		1	2	3	4	5	6
	1		0.58	0.59	0.59	0.60	0.60
	2			0.62	0.62	0.62	0.62
n_1	3				0.62	(0.62)	0.63
	4					0.62	0.64
	5						0.68
	6						

Table F.1 shows the estimates of the transitory MPX that we recover from our estimation sample when we just use $N = n_1, n_2$ in our identification equations 3 and 5. Remember in our main results we used GMM with $N = 3, 4, 5$ and we have circled $N = 3, 5$ to highlight where we get identification from in the paper. The purpose of this exercise is to show that the estimation results are not very sensitive to the values of N chosen, providing more evidence that the assumption we made that the transitory consumption response lasts less than two years is not biasing our results significantly. In fact, the results are not changed dramatically even when $N = 1, 2$, which suggests the majority of the transitory consumption response is very short-lived.

G Durables

In this appendix we expand on section 8.2 on durable expenditure. First, it will help to write down a simple model. The model will show that our empirical methodology continues to estimate the consumption response to permanent and transitory shocks, but that these need to be interpreted carefully. The model uses the same income process as section 3.1. Remembering the income process is made up of two martingale processes, P_t and Q_t, which may have jumps, instantaneous income is given by

$$dy_t = \left(\int_0^t dP_s \right) dt + dQ_t$$

while instantaneous expenditure now has both a durable and a nondurable component:

$$dc_t = \phi_{nd} \left(\int_0^t dP_s \right) dt + \phi_d dP_t + \psi dQ_s$$

Here we have assumed that the expenditure response to transitory shocks is instantaneous, but it would not change things to assume as before that the response decays to zero after two years. However, it is important that the durable component of the expenditure response to permanent shocks occurs instantaneously with the shock (or very soon after). Aggregating income and consumption annually gives

$$\Delta^N \bar{y}_T = \left(\int_{T-N-1}^{T-N} (s - (T - N - 1)) dP_s + \int_{T-N}^{T-1} dP_s + \int_{T-1}^{T} (T - s) dP_s \right)$$

$$+ \left(\int_{T-1}^{T} dQ_t - \int_{T-N-1}^{T-N} dQ_t \right)$$

$$\Delta^N \bar{c}_T = \phi_{nd} \left(\int_{T-N-1}^{T-N} (s - (T - N - 1)) dP_s + \int_{T-N}^{T-1} dP_s + \int_{T-1}^{T} (T - s) dP_s \right)$$

$$+ \phi_d \left(\int_{T-1}^{T} dP_t - \int_{T-N-1}^{T-N} dP_t \right)$$

$$+ \psi \left(\int_{T-1}^{T} dQ_t - \int_{T-N-1}^{T-N} dQ_t \right)$$

From this we can calculate the covariance:

$$\text{Cov}(\Delta^n \bar{c}_T, \Delta^n \bar{y}_T) = \phi_{nd} \text{Var}(\Delta^n \bar{y}_T)$$

$$+ \phi_d \left(\int_{T-1}^{T} (T - s) \sigma_P^2 dt - \int_{T-N-1}^{T-N} (s - (T - N - 1)) \sigma_P^2 dt \right)$$
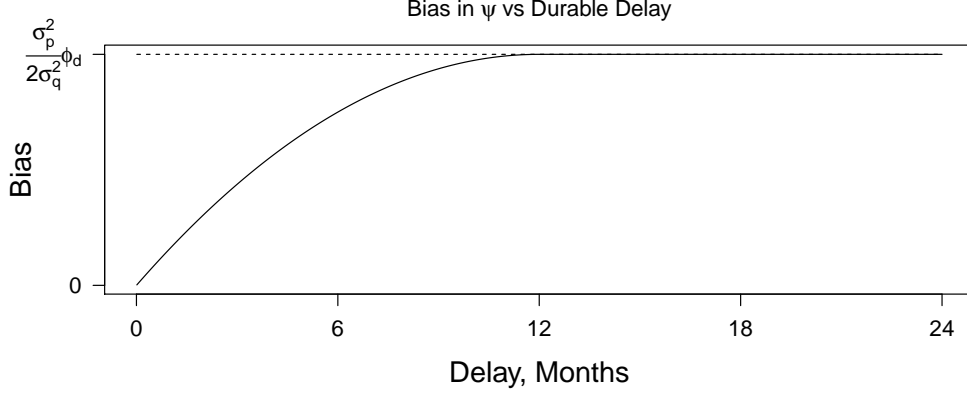
Figure G.3 Bias in Transitory MPX with Delay in Durable Goods Purchase

$$+ \psi \left(\int_{T-1}^{T} \sigma_Q^2 dt + \int_{T-N-1}^{T-N} \sigma_Q^2 dt \right)$$

$$= \phi_{nd}(n - \frac{1}{3})\sigma_P^2 + 0 + 2\psi\sigma_Q^2$$

So the durable component of the covariance cancels out, and our identification method correctly identifies ϕ_{nd} and ψ but is unable to identify ϕ_d.

However, if there is some delay between the household receiving the permanent income shock and purchasing the durable goods, then this introduces an upward bias into the estimate of transitory MPX. The size of the bias grows with the number of months delay between the permanent income shock and the durable goods purchase, plateauing after 12 months at a level of $\frac{\sigma_p^2}{2\sigma_q^2}\phi_d$. Figure G.3 shows how this bias increases with the delay.

In order to quantify how large this bias may be in practice, we make use of the car registry data available in Denmark. Using data on the current value of cars owned by a household, we perform the same residual calculation to find the change in car value that is unpredictable with the household characteristics we are able to observe. We then construct two new expenditure panels: one in which we remove expenditures on cars, and one in which we make a proxy for non-durable consumption by removing expenditures on cars multiplied by $\frac{1}{0.421}$ (car purchases make up 42.1% of durable expenditure in Denmark):

$$C_T^{\text{nocar}} = C_T - \Delta\text{CarValue}$$

$$C_T^{\text{nondurable}} = C_T - \frac{1}{0.421}\Delta\text{CarValue}$$

The second, nondurable proxy consumption panel, can be modeled as the true nondurable consumption panel with classical measurement error added. This classical

measurement error does not bias our estimates, so we can use this nondurable proxy panel to estimate an unbiased MPC out of transitory shocks, where the MPC does not include durable expenditures.

The results of this exercise can be seen in figure X. Even without bias, we would expect the nondurable proxy estimates to be lower than those including all expenditures, as the definition of transitory MPX changes over the three panels to exclude cars and then all durable goods. For the lower quintiles of liquid wealth it therefore looks as though the bias is likely very small, as nondurable goods make up 10% of spending and the MPX estimates are smaller by approximately 10% in this region. For the top quintile of liquid wealth there seems to be some bias, with the estimate of MPX for all expenditures decreasing from 25% to an MPC for nondurable goods of 17%.

While there is some evidence that our results may be biased upward for those in the top quintiles of liquid assets, this bias will only have a small effect on our overall conclusions. As the relevant number for the monetary policy exercise is the MPX rather than the MPC, we have chosen not to adjust our baseline results using this method and accept that a small bias may exist in our data. It should be noted that such a bias will cause the heterogeneous channels of monetary policy to appear smaller than they actually are.

H   RIP or HIP Income Process?

H.1   RIP or HIP Income Process?

Our method makes strong assumptions on the income process—namely, that there is no persistent idiosyncratic component to income growth and that the process contains a random walk. Guvenen (2009) shows that it is empirically difficult to distinguish between a 'Restricted Income Profile' (RIP) like this and a 'Heterogenous Income Profile' (HIP) income process, in which (i) shocks to income are much less persistent (e.g., AR(1) with $\rho \approx 0.8$), and (ii) households have a persistent idiosyncratic growth component. The reason the RIP and HIP processes are difficult to tell apart is that the two features (i) and (ii) act in opposite directions on the cross-section variance of income growth. The less persistent income shocks lead the cross-sectional income growth variance to not grow as fast as the HIP model, while the persistent idiosyncratic growth component leads the same variance to grow at a faster rate. The result is that the increase in variance of income growth over three to four years is approximately the same as the increase from

four to five years. To the extent that the consumption response to these semi-permanent shocks is similar to the response to the idiosyncratic persistent growth component,[55] our methodology will continue to provide reasonable estimates of the "permanent" MPX and the more familiar transitory MPX.

Both the Restricted Income Profile (RIP) and Heterogeneous Income Profile (HIP) processes can be described by the equations:

$$y_h^i = \beta^i h + z_h^i + \varepsilon_i^h$$
$$z_h^i = \rho z_{h-1]}^i + \eta_i^h$$

where i indexes the worker and h the years of experience. ε_i^h represents a transitory shock to income, while η_i^h is persistent. β^i represents an idiosyncratic persistent growth factor.

In the RIP model, $\beta^i = 0$ and ρ is usually estimated to be very close to 1 (in this paper we assumed $\rho = 1$). In the HIP model, β^i has a cross-sectional variance $\sigma_\beta^2 > 0$, and ρ is normally estimated to be significantly lower than 1, around 0.8. The reason these are difficult to tell apart is because the theory does not give a strong indication in which model the cross-sectional variance of income growth over N years should grow faster. In the RIP model with $\rho = 1$, the cross-sectional variance of income growth increases linearly with N. In the RIP model with $\rho \approx 0.8$, the growth in the cross-sectional variance of income growth will decrease due to the low ρ but increase due to the idiosyncratic β^i.

Figure H.1 shows the empirical values for income growth variance and the covariance of income and expenditure growth over N years. We have also plotted the fitted values for these statistics that are implied by our model when fitted to $N = 3, 4, 5$ as we do in the paper. We see the empirical variance and covariance decline slightly below the model fitted line as N becomes large, which fits with the finding that ρ in the RIP model is usually slightly below 1.0, around 0.98 or 0.99. We also note that around the region where we achieve our identification ($N = 3, 4, 5$), there is very little curvature in the empirical statistics, and the increase in both variance and covariance is close to linear.

While this linearity around $N = 3, 4, 5$ cannot help us distinguish between the RIP and HIP process, it does imply that our empirical methodology may be somewhat robust to misspecification along this dimension. If we assume that the expenditure response to a change in z_h^i and to the increase from the persistent idiosyncratic growth are equal to

---

[55]See Guvenen (2007) for an example of why this might be the case: if households do not know their own idiosyncratic growth ex-ante, a Bayesian learning process will be very slow, so households (at least initially) will react in similar ways to changes in income due to this persistent growth component as a true income shock.
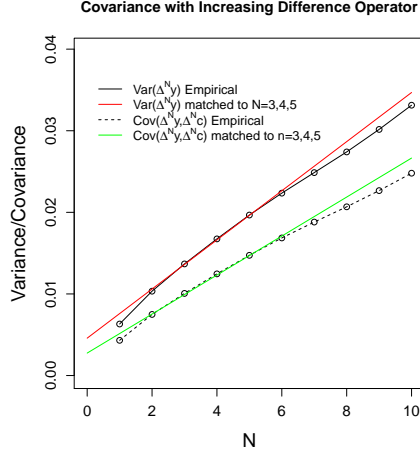
Figure H.1 Variance and Covariance with Years of Growth

ϕ, and the response to a transitory shock is ψ, that is:

$$\Delta^N c_h^i \approx \phi \Delta^N (\beta^i h + z_h^i) + \psi \Delta^N \varepsilon_i^h$$

Then, the fact that $\mathrm{Var}\big(\Delta^N(\beta^i h + z_h^i)\big)$ grows approximately linearly with N means that our empirical method will correctly identify ϕ and ψ.

A full investigation of the implications of different income processes is beyond the scope of this paper but would be a very useful exercise for future research.

I Time-Varying Risk

We have assumed that idiosyncratic risk remains constant over time. Given that our sample period covers the great recession, this may not be appropriate. Here we show how the variance of income growth has varied over time, peaking just after the crisis in 2010. In order to test how much this time-varying risk might bias our results, we simulated data with $\phi = 1$ and $\psi = 0.5$, with permanent variance equal to estimates from the data and transitory variance varying in order to match the time-varying income risk pattern observed in the data. When we run this simulation we find estimates of ϕ and ψ within 1% of their true values.

Figure I.1 shows how the standard deviation of income growth has changed over the sample period. From trough to peak, the standard deviation increases approximately 10%. In the simulation referred to in section I, we assume that both transitory income and transitory consumption response have no persistence. We divide each year into
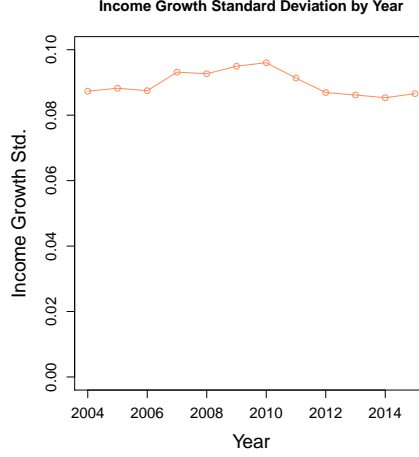
Figure I.1 Standard Deviation of Income Growth

20 sub-periods, choose the variance of permanent shocks to be 0.003, and allow time-varying transitory shocks to match the pattern in figure I.1. We choose values of $\phi = 1$ and $\psi = 0.5$ and apply our estimation procedure (that assumes constant variance) to the simulated data. We recover estimated values of ϕ and ψ to be 1.006 and 0.499, respectively.

J   Robustness

As would be clear from the main text, we have made a number of choices regarding both data and variable definitions as well as more methodological issues. In a series of graphs, this appendix presents a number of robustness checks aimed at assessing the extent to which our results are sensitive to the specific choices.

We begin with a number of robustness checks regarding our imputed expenditure measure, which may suffer from measurement error. In figure J.2, we compare our baseline estimates of the MPX to estimates based on different sample selection procedures. First, we exclude all households that own stocks corresponding to more than 10,000 USD (10% of households in our sample). Second, we do not remove households that have negative imputed expenditure. We remove those households in our baseline sample because negative expenditure is clearly not a good estimate of actual expenditure. However, for example, in the event that negative expenditure arises because of classical measurement error, removal of negative estimates may be asymmetric and introduce
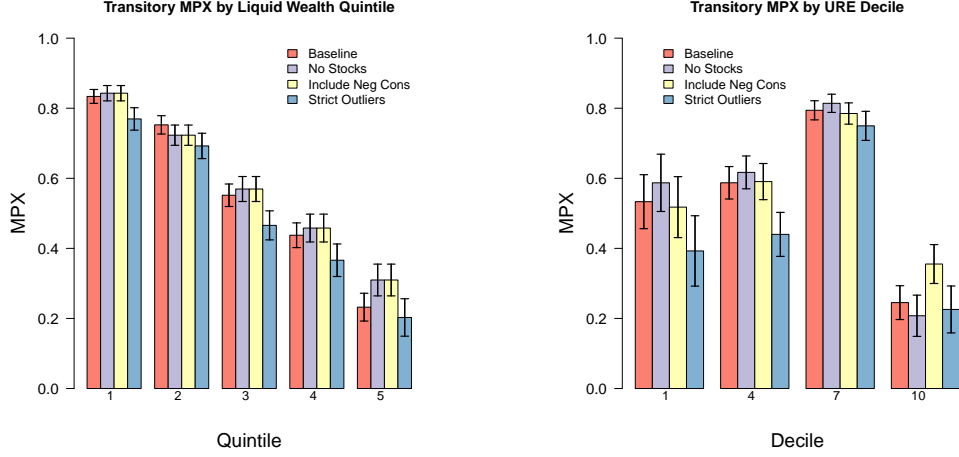
Figure J.2 Robustness of Liquid Wealth and URE Distributions

an upward bias in average imputed expenditure. Third, to check that large outliers do not drive our results, we remove observations in the top and bottom 2.5% in terms of level and change of income and expenditure. In the baseline calculations, we use only a 1% cutoff. Our results are qualitatively unchanged when using these alternative approaches to take account of measurement error. In terms of magnitudes of the estimated MPXs, the largest difference to the baseline results seems to be found when we include negative expenditure estimates. As expected, this makes the largest difference among the wealthier households. The specification of outliers also matters somewhat for the point estimates of MPX in certain groups of households, but differences are not large.

Another robustness check consists of specifying consumption and income in logs rather than in levels. The fundamental difference is that the log specification yields an elasticity rather than an MPX. Hence, some difference between level and log results must be expected for households that only spend a fraction of their annual income (typically wealthier households). Indeed, as expected, figure J.3 demonstrates that results hold qualitatively when specifying income and expenditure in logs rather than in levels, whereas estimated elasticities are higher than the MPXs for the wealthier households and those with high URE. Time-varying income risk may also potentially contribute to differences between results based on levels and logs. However, as shown in section I, this is not likely to be important in our setting.

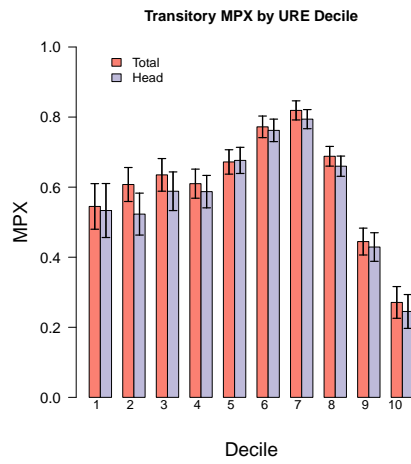As discussed in section 4.1, we use labor income of the head of the household as our
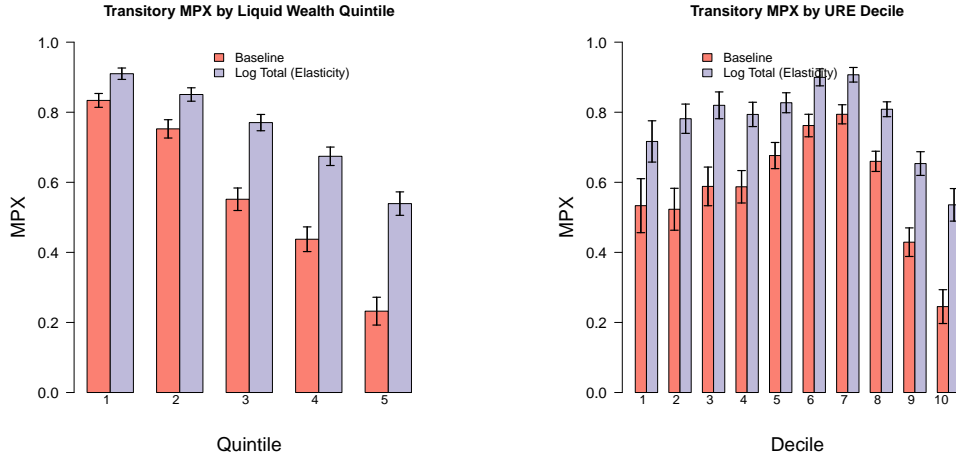
Figure J.3 Results Using Log Income and Expenditure

prime measure of income in line with previous literature. Various mechanisms—e.g., intra-household income insurance—may give rise to differences between results based on income of the head of household and total household income. However, figure J.4 demonstrates that there is virtually no difference in our results between using total household income and only the household head's income. Online appendix L briefly discusses the potential role that intra-household insurance may play, which we leave as an area for future research.

Finally, figure V shows the distribution of MPX by quintile of liquid wealth. It might be argued that the relevant level of liquid wealth is relative to income rather than in absolute terms. Figure J.5 demonstrates that results based on quintiles of liquid wealth divided by permanent income are similar. Also, results (not shown here) where deciles are based on a broader definition of liquid wealth—i.e., including stock and bond holdings—are similar to our baseline results.

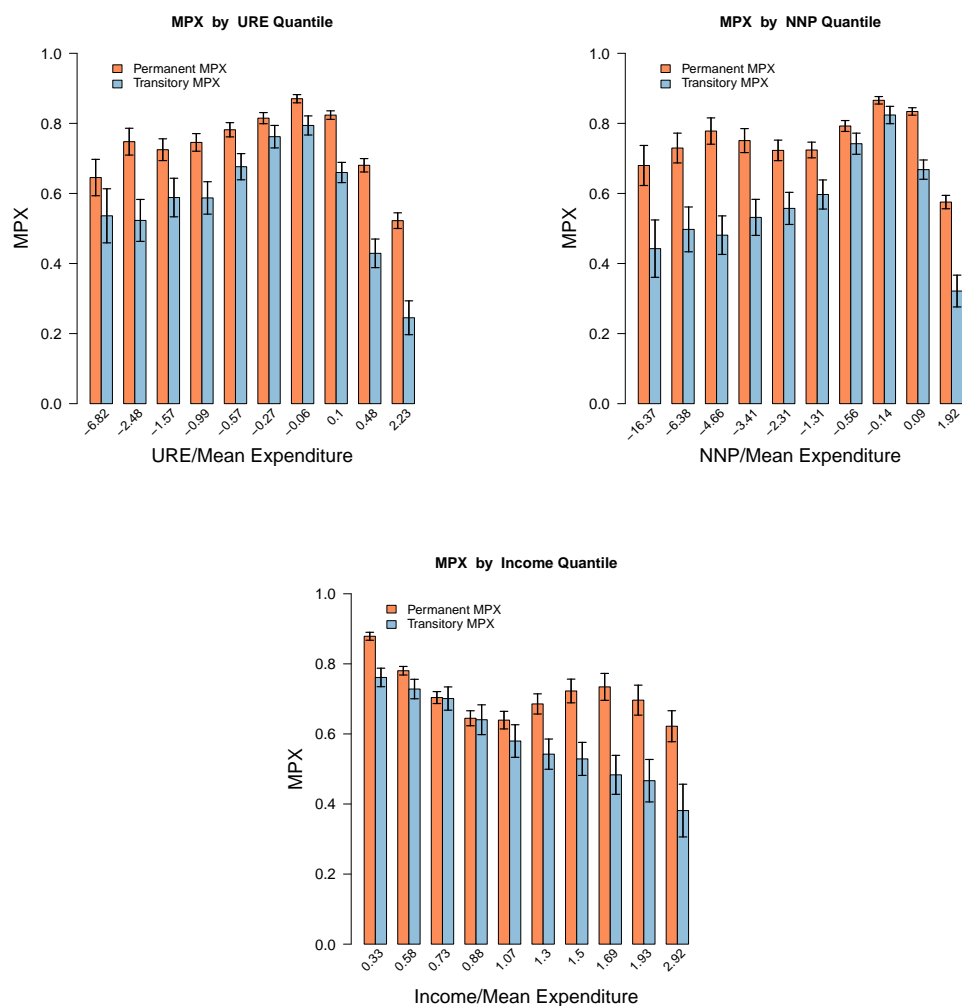# K   Distribution of Permanent MPX by NNP, URE, and Income

Figure K.1 shows the distribution of both transitory and permanent MPX by NNP, URE and income decile. The transitory numbers are a repeat of figure VIII.

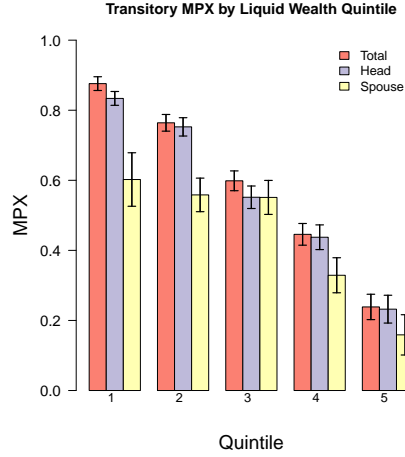**Figure J.4** Results Using Total Labor Income and Head Labor Income
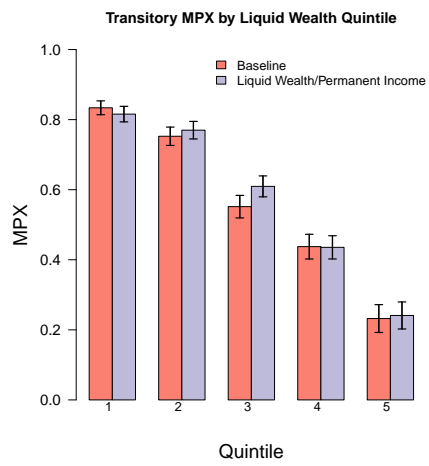
Figure J.5 Results Using Quintiles of Liquid Wealth over Permanent Income vs Liquid Wealth

Figure K.1 MPX Distribution by URE, NNP, and Income

Figure L.1 Results Using Total, Head, and Spouse Labor Income

L   Intra-household Income Insurance

As discussed in section 4.1, we use labor income of the head of the household as our prime measure of income, in line with previous literature. Figure J.4 demonstrates that results based on total household income and income of the head of household are similar. However, MPXs from transitory shocks to the income of the spouse are lower than MPXs from shocks to total income, in particular for the less wealthy households, as demonstrated in figure L.1. This indicates heterogeneity in the role that intra-household income insurance plays across different groups of households. We leave this interesting topic for future research.