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HOUSEHOLD DEBT AND MONETARY POLICY: REVEALING THE CASH-FLOW CHANNEL*

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We examine the effect of monetary policy on household spending when households are indebted and interest rates on outstanding loans are linked to short-term interest rates. Using administrative data on balance sheets and consumption expenditure of Swedish households, we reveal the cash-flow transmission channel of monetary policy. On average, indebted households reduce consumption spending by an additional 0.23–0.55 percentage points in response to a one-percentage-point increase in the policy rate, relative to a household with no debt. We show that these responses are driven by households that have some or a large share of their debt in contracts where interest rates vary with short-term interest rates, such as adjustable-rate mortgages (ARMs), which implies that monetary policy shocks are quickly passed through to interest expenses.

A fundamental question in macroeconomics is how monetary policy exerts its influence on the real economy. In standard macroeconomic models, the interest-rate channel is the primary transmission mechanism. According to this mechanism, forward-looking households change the slope of their consumption profiles when interest rates change. Although monetary policy indeed appears to affect the real economy, the empirical support for this mechanism is mixed, and the evidence indicates that the effects are both stronger and of a different character than predicted by the interest-rate channel. This suggests that other mechanisms may also be at work.[1] One such potential mechanism is the *cash-flow channel*.[2] According to this mechanism,

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The data and codes for this paper are available on the Journal website. They were checked for their ability to reproduce the results presented in the paper. The authors were granted an exemption to publish parts of their data because access to these data is restricted. However, the authors provided a simulated or synthetic data set that allowed the Journal to run their codes. The synthetic/simulated data and the codes for the parts subject to exemption are also available on the Journal website. They were checked for their ability to generate all tables and figures in the paper, however the synthetic/simulated data are not designed to reproduce the same results.

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[1] Attanasio and Weber (2010) and Jappelli and Pistaferri (2010) survey the empirical support for the consumption theories that underpin the interest-rate channel. Boivin *et al.* (2011) discuss the different transmission mechanisms that have been suggested in the literature and the (often weak) empirical support for these mechanisms.

[2] This terminology has previously been used by, e.g. Cloyne *et al.* (2020), whereas Berben *et al.* (2004) and Di Maggio *et al.* (2017) refer to the same channel as the 'income channel'. However, Boivin *et al.* (2011) do not mention this channel in their survey.

monetary policy has a direct effect on household spending through households' cash flows and disposable incomes. When the central bank raises its policy interest rate, the interest-rate expenses of households with debt tightly linked to short-term rates—such as adjustable-rate mortgages (ARMs)—rise, thus reducing the households' disposable income. If households are forward-looking and have good access to financial markets, such variations in cash flow need not result in tangible consumption responses. But, if households are myopic, or liquidity constrained, or for some other reason they are unable or unwilling to draw on savings or increase debt in response to temporarily lower disposable income, monetary policy-induced interest rate increases will reduce their consumption spending. Under these circumstances, monetary policy affects private spending through this cash-flow channel in addition to the conventional channels. In this paper, we assess the empirical support for this channel using administrative data on Swedish households. We argue that Sweden offers an ideal laboratory for three reasons. First, in Sweden, household debt is relatively high, and ARMs are common. Throughout our sample period, ARMs accounted for 30%–40% of the aggregate value of outstanding mortgage debt. These ARMs typically have an interest fixation period of only three months.[3] Second, ARMs are standard products on the Swedish mortgage market, and most households have adjustable rates on at least some share of their debt. That is, they are neither disproportionally held nor directly targeted to particular types of households. Moreover, the characterisation of the Swedish mortgage market is such that it is unlikely that our results are contaminated by important selection into different types of loan portfolios depending on household characteristics or spending behaviour.[4] In support of this notion, we find that households that we classify in our data as holders of ARMs are observationally similar along a variety of important dimensions to households holding fixed-rate mortgages (FRMs).[5] Third, studying the importance of this channel in Sweden offers an empirical setting with access to detailed household-level data. A common challenge in previous studies on the impact of monetary policy on consumption is the lack of suitable data sets that feature both a high-quality measure of consumption and data on households' wealth and balance sheets that are representative for the population. We overcome this problem by using administrative panel data from tax returns and other registry-based data. This data source provides us with detailed information on all income, assets and debt. As in Koijen *et al.* (2015), the details of these data enable us to impute a measure of consumption expenditure using the accounting identity that total consumption expenditure equals the sum of total income and capital gains minus the change in wealth. Furthermore, analysing responses at the level of the individual household mitigates the common problem when trying to evaluate the impact of monetary policy on economic outcomes that changes in monetary policy are endogenous to the development of the economy. In our setting, all households are affected by the same monetary policy, but if the cash-flow channel is important, the households' consumption responses vary, depending on their debt contracts and balance sheets.

Guided by theory, we examine how monetary policy affects consumption for households with a large debt-to-income ratio relative to households with a smaller debt-to-income ratio, and for

[3] According to Statistics Sweden's Financial Markets Statistics, the fraction of mortgages that had an interest-rate fixation period of one year or shorter at origination varied between 42% and 58% in 2003–7.

[4] In general, a possible concern is that households may select into ARMs based on household-specific characteristics that correlate with their overall exposure to macroeconomic factors. For theoretical arguments in this direction, see, e.g., Campbell and Cocco (2003), Campbell and Cocco (2015) and Badarinza *et al.* (2018) for recent empirical evidence.

[5] As further support of this notion, previous analysis has found that outcomes related to households' financial health, such as the probability of mortgage default, do not correlate with the choice of interest-rate fixation (Holmberg *et al.*, 2015).

households with ARMs relative to households with FRMs. We also examine how debt-to-income ratios and debt contracts interact with households' liquid assets-to-income ratios. We report three kinds of results that lend strong support to the importance of the cash-flow channel of monetary policy.

Our first result is that households with high levels of debt relative to their income respond substantially more to a change in the monetary policy interest rate than households with little or no debt. OLS estimates imply that when the central bank raises its interest rate by one percentage point, the average household, which has debt roughly equal to one year's disposable income, reduces its consumption by about 0.23 percentage points relative to a similar household with no debt.[6] This analysis faces a standard problem of a possible reverse causality when assessing the effects of macroeconomic policy: households respond to monetary policy, but monetary policy may also respond to the economic conditions of households. To overcome this issue, we measure innovations in monetary policy that are entirely due to policy shifts and not macroeconomic developments. This enables us to separate consumption responses to unanticipated changes in interest rates from consumption responses that are anticipated based on macroeconomic conditions. Following recent examples from the literature on monetary non-neutrality, we use monetary policy shocks as an instrumental variable for changes in the policy rate. Our IV estimates are 50%–100% greater than our OLS estimates. Translating our estimates into a relative marginal propensity to consume (MPC) out of changes in disposable income, or cash flow, they imply an MPC of 0.19–0.50. Importantly, our results are robust to using the average aggregate interest rate faced by households instead of the monetary policy rate.

Although our estimates depend on the exact empirical specification, they can be compared to regression estimates on data generated from model simulations. Our comparisons suggest that our estimates are at least three times as large as those from households with ARMs that respond optimally, according to standard neoclassical theory, to a temporary shock. Rather, our estimates are consistent with responses to a persistent shock to the interest rate where half of the households have ARMs (and the remainder have FRMs) and where half of the households respond optimally while the remainder display hand-to-mouth behaviour.

Our second result is that households' consumption responses crucially depend on the interest-rate fixation of their mortgages. Using a proxy measure for the shares of ARMs and FRMs in the loan portfolio of each household, we estimate responses by households' share of debt in ARMs. Separating the consumption responses of households along this dimension reveals a substantial difference in elasticities and MPCs out of an interest rate change. Households with a high share of ARMs respond strongly to a change in the policy rate, whereas households with a low share of ARMs (high share of FRMs) do not.

Our third result highlights the strong interaction between mortgage type and the level of liquid assets to income. We consistently reject that responses of households with ARMs and low liquid assets to income are equal to responses of households with ARMs and high liquid assets to income. In contrast, this is not the case for households with FRMs.

In sum, our findings are consistent with widespread hand-to-mouth behaviour among households. Furthermore, they suggest a high prevalence of relatively wealthy hand-to-mouth households. In line with this interpretation, we note that only 13% of the homeowners' total assets is in liquid assets, whereas 87% is tied to illiquid assets. Moreover, there is a strong negative

[6] Throughout the paper we use the terms 'consumption' and 'spending' interchangeably when referring to measured consumption spending.

correlation between debt and liquid assets. While the average homeowner has liquid assets corresponding to eight months of disposable income, homeowners with a high debt-to-income ratio have fewer than three months' worth of income in liquid assets.

Our paper contributes to the recent empirical literature on the relationship between household debt, mortgage markets and the transmission of monetary policy. Di Maggio *et al.* (2017) study a group of US households with mortgages that face interest rates that are held fixed for five years before being automatically adjusted. They exploit the staggering of such contracts to estimate consumption responses to changes in interest rates and find strong responses in car purchases to a change in interest expenses. An important difference between their study and ours is that we use a comprehensive expense-based measure of consumption rather than being limited to a measure of durable consumption such as car purchases. La Cava *et al.* (2016) explore the cash-flow channel in Australia using the large decline in interest rates early on in the financial crisis. They find that durable consumption responds more strongly to changes in cash flows for borrowers than savers, in particular for borrowers who hold debt with variable interest rates. Cloyne *et al.* (2020) study the response of expenditure and income to monetary policy in the United Kingdom and the United States.[7] In the absence of detailed balance sheet information, they use housing tenure status as a proxy for debt positions, finding that the consumption response to a temporary cut in interest rates depends on households' balance sheets. However, they argue that the general equilibrium effect of monetary policy on income is quantitatively more important than the direct effect of cash flows. In contrast to Cloyne *et al.* (2020), we are able to study responses across the distribution of debt positions even among households with the same housing tenure status and thus shed some further light on the mechanisms at work. Jappelli and Scognamiglio (2018) study the consumption responses to interest-rate reductions for holders of ARMs relative to those with FRMs in Italy following the financial crisis of 2007–9. In contrast to our study and other related studies, they find a very weak consumption response to a change in interest expenses and therefore limited support for the cash-flow channel. Using aggregate data, Calza *et al.* (2013) document that the transmission of monetary policy shocks to residential investment and house prices is stronger in countries with more flexible and developed mortgage markets and that responses in consumption are stronger in countries where there is a higher prevalence of ARMs.

The long period of extraordinarily expansionary monetary policy after the outbreak of the financial crisis has resulted in a discussion about the distributional impact of monetary policy (see, e.g., Bullard, 2014; Mersch, 2014; Bernanke, 2015). Our findings of heterogeneous effects of monetary policy on household spending complement a recent but growing literature studying heterogeneous and distributional effects of monetary policy. Recent empirical papers that more directly study the distributional impact of monetary policy include Sterk and Tenreyro (2018), Casiraghi *et al.* (2018) and Wong (2019), whereas Gornemann *et al.* (2016), Garriga *et al.* (2017) and Auclert (2019) are recent theoretical contributions to this literature. More generally, our study is related to an extensive literature studying household consumption responses to fiscal stimulus programmes, such as tax rebates, as well as other shocks to unearned income. This includes Shapiro and Slemrod (2003), Johnson *et al.* (2006), Agarwal *et al.* (2007), Shapiro and Slemrod (2009) and Parker *et al.* (2013), who study the effect of the 2001 and 2008 economic stimulus payments in the United States on consumer spending.[8] In all cases, the authors find

[7] Like in Sweden, ARMs make up a large share of the mortgages in the United Kingdom, whereas FRMs are more prevalent in the United States.

[8] Studies of consumption responses to other sources of shocks to disposable income include, e.g., Hsieh (2003), Stephens (2008), Qian (2014) and Agarwal and Kueng (2018).

a considerable consumption response to these income shocks, and the response is stronger for those who are more likely to be liquidity constrained. We view our paper as a monetary policy analogue to this work.

The remainder of the paper proceeds as follows. In Section 1, we provide a theoretical motivation for our empirical strategy, illustrating how the consumption behaviour underlying the cash-flow channel differs from the standard consumer theory behind the interest-rate channel. Section 2 provides details on the data we use in our analysis and the background to our empirical setting. In Section 3 we outline our empirical strategy, and in Section 4 we present our empirical results. Section 5 then summarises a range of checks that illustrate the robustness of our results. Section 6 concludes the paper. Some additional material, supplementary analyses and details of our theoretical framework are relegated to the Online Appendix.

1. Theoretical Motivation

Our analysis rests partly on theories of hand-to-mouth behaviour and partly on recent models in which mortgage contracts are a source of transmission of monetary policy. Deviations from standard consumption smoothing have been considered for a long time. Carroll and Kimball (1990) show that the average MPC increases in the presence of borrowing constraints and uncertainty. Campbell and Mankiw (1990) introduce 'rule-of-thumb' consumers as a potential explanation for the excess sensitivity of consumption. The role of mortgages in the transmission of monetary policy has also been discussed for a long time. Bernanke and Gertler (1995) and Mishkin (2007) point out that changes to short-term nominal interest rates affect households' mortgage burden, in turn affecting housing demand. Recently, models with mortgages demonstrate a more direct effect on households' overall consumption spending (see, e.g., Garriga *et al.*, 2017; Wong, 2019).

We structure our argument regarding the cash-flow transmission channel using two models. We first consider hand-to-mouth behaviour in a model of an infinitely lived household with no nominal rigidities (see Online Appendix A for full details). Consider a household whose financial wealth is small relative to its interest-only ARM, implying that net financial assets are approximately equal to minus the balance of the household's ARM.[9] Let d_t denote this mortgage debt. The intertemporal budget constraint reads $c_t - d_{t+1} = y_t - d_t(1 + r_t)$, where c_t is consumption, y_t is labour income and r_t is the real interest rate. By definition, hand-to-mouth households (henceforth HtM households) hold net financial assets constant. Hence, consumption obeys $c_t = y_t - r_t \cdot d_t$. In other words, the MPC out of a change to the short-term interest rate is equal to one. This is the response if a household is borrowing constrained or if it behaves in such a way for other reasons (e.g., due to deviations from rationality). To obtain a measure of the elasticity in the response, we log-linearise the consumption function around steady state to get

$$\Delta \log c_t \approx \theta \cdot \Delta \log y_t - \theta \cdot \frac{d}{y} \cdot \Delta r_t, \tag{1}$$

where θ is the inverse of the household's (steady-state) consumption-to-income ratio and $\frac{d}{y}$ the (steady-state) debt-to-income (DTI) ratio. This equation shows that the percentage consumption response to interest-rate changes is proportional to the household's DTI ratio. Note also that the response of HtM households does not depend on when information about the interest-rate change

[9] Notice that for the typical mortgage holder, gross financial assets are small relative to the value of the mortgage.

arrives. Their consumption responds when their cash flow changes, irrespective of whether the change was anticipated or not. In contrast, rational consumption smoothers have an identical elasticity in their consumption response, regardless of their DTI ratio (provided that wealth effects and the likelihood of becoming constrained in the future can be ignored).[10]

Let us now consider a more complex partial equilibrium model (see Online Appendix B for full details). In this model, building on Garriga *et al.* (2017), households' life spans are finite, there is persistence in interest-rate shocks, and mortgage contracts are nominal and in the form of either ARMs or FRMs. To mimic the Swedish setting, the FRM has a five-year interest-rate fixation period. Rational optimising households have access to a one-period nominal bond. The shocks to the short-term nominal interest rate may be equivalent to a real shock (i.e., inflation is unaffected) or partially nominal (i.e., positively correlated with inflation). In the extreme, the shocks are purely nominal, and the Fisher equation holds.[11]

We first consider optimising households' consumption response to a change in the nominal interest rate when inflation is unaffected. For optimising households with ARMs, the response is immediate and uniform across DTI ratios, as in the simpler model (ignoring differences in remaining life span that imply a small difference in wealth effects). The response is entirely a function of intertemporal substitution. For a temporary positive shock, optimising households intertemporally smooth consumption by borrowing some more in the one-period bond so that the consumption response is small (i.e., the optimal response requires access to a buffer). The greater the persistence of the shock in the interest rate, the greater the response in consumption. For optimising households with FRMs, the response is immediate too, provided that the shock is persistent and lasts longer than the interest-rate fixation period of the households' mortgage. Optimising households with FRMs strive to smooth consumption over time and achieve this by saving more and consuming less today. So, for optimising unconstrained households with either kind of mortgage contract, the consumption response is essentially independent of the DTI ratio but somewhat stronger for households with ARMs than households with FRMs. The magnitude of optimising households' responses depends on how inflation is affected. In the extreme case when the Fisher equation holds, households with ARMs are compensated exactly by opposing short-term and long-term wealth effects, and their consumption does not respond at all (although changes in the bond positions are large). In this extreme case, households with FRMs gain from higher inflation.

We now turn to HtM households. As in the simpler model, HtM households' consumption response is not uniform but rather proportional to the DTI ratio. HtM households with ARMs respond immediately, whereas HtM households with FRMs respond with a delay (i.e., only when the interest-rate fixation period ends). Finally, HtM households do not consider future inflation. Hence, the short-term consumption response of HtM households with ARMs is essentially independent from the shock's effect on inflation.

[10] Rational unconstrained households' responses can be thought of as obeying $\Delta \log c_t = \delta_t$, where δ_t is a time-fixed effect common to everyone.

[11] We focus on the income effect of Garriga *et al.* (2017) and abstract from the price effect on housing associated with housing transactions. This is consistent with our empirical approach, where we exclude households that transact housing (yet all households are exposed to a common house-price effect). Another related model is Wong (2019). In an incomplete markets model calibrated to the United States, she highlights the role of refinancing of FRMs for monetary policy transmission. In a counterfactual analysis, she also finds that the monetary policy transmission through mortgages is stronger in an economy with ARMs. Greenwald (2018) sets up a general equilibrium model with loan-to-value and payment-to-income constraints and studies monetary policy transmission in it. Auclert (2019) develops a consumer theoretic framework where households' net nominal positions and unhedged interest rate exposure matter for the response. See further discussion in Online Appendix B.

We highlight four implications from our model for household behaviour. First, HtM households' responses are approximately proportional to their DTI ratio, whereas optimising households' responses are independent of their DTI ratio (ignoring borrowing constraints) and smaller than HtM households' as long as the shock to the interest rate is not very persistent. Second, HtM households respond to both anticipated and unanticipated changes, whereas optimising households respond only to unanticipated changes. Third, how shocks to the nominal interest rate affect inflation matters little for the short-term consumption response of HtM households with ARMs. Fourth, we note that consumption of optimising households with ARMs responds more strongly than consumption of households with FRMs and that optimal responses of households with ARMs to interest rate increases require access to a buffer of liquid assets or credit.

2. Data and Institutional Background

2.1. *Data Description*

The main data set we use is the Swedish registry-based panel data set LINDA (Longitudinal INdividual DAta for Sweden). This data set is representative of the Swedish population, covering a random sample of 300,000 households and their members. Since in Sweden, as in other Scandinavian countries, each taxpayer has a unique personal identity number, we are able to construct a panel using several sources of administrative data. Our sample period covers 2000–7. During this period, Sweden levied a wealth tax that required every financial institution to provide the tax authority with comprehensive information on all taxpayers' wealth in addition to information on earnings and income.[12] The tax registers therefore include information on all taxable income and transfers, tax payments, liabilities and taxable wealth, including the value of real estate (i.e., houses, apartments, cabins), cash holdings in bank accounts, bonds, stocks and mutual funds.[13]

The market values of single-family houses and cabins are assessed by Statistics Sweden. They are a function of a long list of characteristics of the property and updated yearly using a price index constructed from transactions in a given municipality in each year. The market values of apartments (shares in co-op associations) are also assessed by Statistics Sweden but with more noise. The values of financial assets are detailed, and, for instance, each household reports each and every listed stock or mutual fund it holds in its tax filings (see Calvet *et al.*, 2007). The data set contains information on total household debt, which is the debt measure we use in the empirical analysis. The data set also contains information about annual interest expenses on that debt. Finally, the data set includes residential location for each household and various demographic variables.

The unit of analysis is the household, meaning that individual data have been aggregated to the household level using marital status, residential location and parent–child linkages (household identifiers are constructed by Statistics Sweden based on this information). Household characteristics, such as age and education, represent a household head, which we take as the oldest individual in the household unless more than one individual is of that same age, in which case we choose the oldest male.

[12] Most of this information was submitted automatically to the tax authority by employers, banks, public authorities and registers.

[13] For further details on the data set used in the current paper, see Koijen *et al.* (2015), and for a detailed account of the data collection process for LINDA, see Edin and Fredriksson (2000).

2.2. *Imputing Consumption*

We use this detailed data set to impute a measure of consumption expenses based on the approach first developed by Browning and Leth-Petersen (2003) and that has been adapted and applied to Swedish data in Koijen *et al.* (2015). This is a necessary step in our analysis, as our main outcome of interest is in terms of household spending.

A common way of describing a given household i's budget constraint in year t is as follows:

$$c_{i,t} = y_{i,t} + \Delta d_{i,t} - r^d_{i,t} d_{i,t-1} - \Delta a_{i,t} + r^a_{i,t} a_{i,t-1}. \tag{2}$$

That is, consumption, c, is constrained by disposable income, y, the change in outstanding debt, Δd, interest payments, $r^d d$, savings, Δa, and the household's returns $r^a a$. Based on the notion that the budget constraint can serve as an accounting identity in a given year, it can be used to impute a measure of consumption as total income net of change in wealth from the previous period. This is possible since all terms on the right-hand side of equation (2) are observable in our data. Mapping equation (2) into the detailed structure of our data gives the identity

$$c_{i,t} = y_{i,t} + \Delta d_{i,t} - r^d_{i,t} d_{i,t-1} - \Delta b_{i,t} - \Delta v_{i,t} - \Delta \psi_{i,t} - \omega_{i,t}, \tag{3}$$

where the household's disposable income, y_i, includes labour income, transfers and benefits (all net of taxes), and financial income; Δd is the change in debt; $r^d d$ is interest payments; Δb is the change in deposits on bank accounts; Δv is an active rebalancing of mutual funds, stocks and bonds; $\Delta \psi$ is changes in capital insurance accounts; and ω is contributions to private pension savings. Equation (3) is identical to the imputation method in Koijen *et al.* (2015), which describes the accuracy of this method through a comparison with additional information and surveys.[14]

2.3. *Sampling Restrictions*

Our household-level panel data set is outstanding in that it contains detailed information about the households' balance sheets at an annual frequency. Nevertheless, we impose a few restrictions on our sample, most of which are related to the construction of the consumption measure where we follow Koijen *et al.* (2015). First, we require households to be present for two consecutive years. Second, we drop households that transact in real estate or apartments because such events require additional careful adjustments that rely on additional non-registry-based data (see, e.g., the discussion in the Appendix of Sodini *et al.*, 2017). In addition, we exclude observations with outliers in disposable income, the DTI ratio, or the consumption measure. All in all, our sample corresponds to approximately 20% of the households in LINDA during 2002–7 that have a stable household structure and for whom all terms of equation (3) are observed. Table A.4 in Online Appendix C reports incremental changes to the sample as restrictions are imposed.

2.4. *The Swedish Mortgage Market*

Our proposed monetary policy transmission channel relies on a high prevalence of ARMs. Figure 1(a) displays the division of new mortgages in Sweden by the duration of interest-rate fixation, where ARMs are defined as those where interest rates are adjusted every three months

[14] Relative to Koijen *et al.* (2015), one refinement has been made that concerns bank accounts. Bank account deposits are reported only if certain criteria are met, and those changed in 2006. In 2000–5, a deposit in a bank account was reported in the Swedish tax records if the earned interest from that account exceeded SEK 100, while in 2006 and 2007, the deposit was reported only if the balance in the account exceeded SEK 10,000. Overall, the new rule implies an

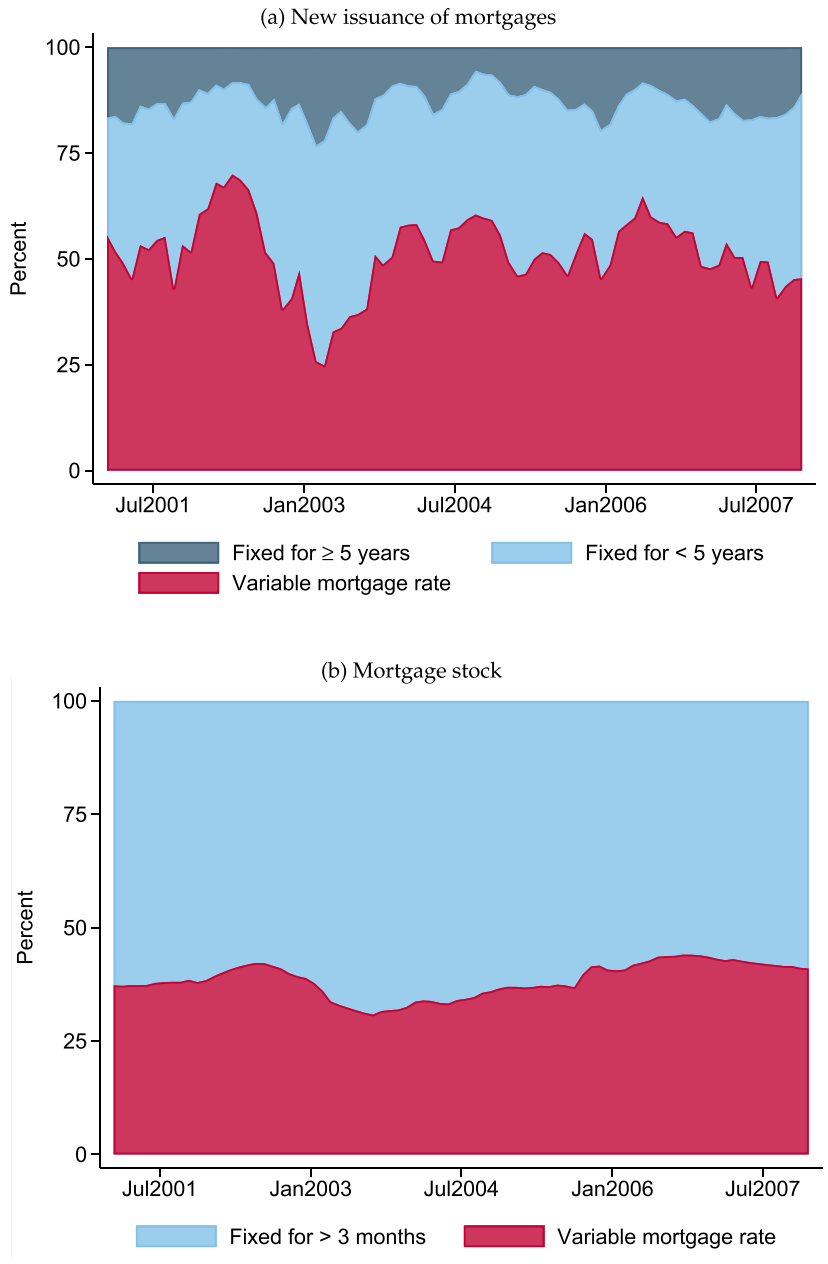


Fig. 1. *Mortgage Stock and New Issuances by Duration of Interest-Rate Fixation.*
Notes: Variable mortgage rate is defined as three months or shorter. Panel (a) plots the share of mortgage issuances by duration of interest-rate fixation. Source: Sveriges Riksbank (2012), Figure A18. Panel (b) plots the shares of the mortgage stock by duration of interest-rate fixation. Source: Sveriges Riksbank (2019), Figure A18.

or more frequently. The figure shows that a large share, almost half, of the new mortgages issued during our sample period were on adjustable rates. In terms of the total stock of the outstanding mortgage debt, Figure 1(b) reports that the value-weighted share of ARMs was between 30% and 40% during the sample period.[15] Furthermore, FRMs in Sweden have a fairly short interest-rate fixation period. Ninety percent of the new mortgages have a fixation period of fewer than five years. In addition to mortgage debt, a large percentage of other loans to households, such as car and consumption loans, have adjustable rates. This implies that lenders, at least partially, pass through a rise in their own borrowing costs by raising their interest rates. Taken together, these aggregate statistics imply that changes in the monetary policy rate are quickly passed through to changes in households' interest expenses.

An important characterisation of the Swedish mortgage market is that households frequently hold a combination of ARMs and FRMs, rather than one or the other. These components have different durations of interest-rate fixation which differ from that of the mortgage itself, meaning that their rates will be reset at different points in time (Sveriges Riksbank, 2014). There are two reasons for households to choose ARMs. First, interest rates on ARMs have historically often been lower than rates on FRMs. Second, if a household with an FRM wants to repay, refinance or change conditions on the mortgage—e.g., negotiate a new interest rate—it has to compensate the bank for the interest rate differential if market rates have fallen. In other words, the borrower bears the cost of refinancing to adjustable rates. In this way, households with FRMs cannot respond to decreasing interest rates by simply changing contract type during the interest-rate fixation period. Banks therefore frequently recommend a combination of FRMs vs. ARMs as this lowers the risk that the whole loan will be adjusted to a higher rate, while enabling households to benefit from decreasing interest rates. How households choose to divide their mortgages between FRMs and ARMs is then likely to depend on the prevailing market conditions when the mortgages were issued, for instance when the house was purchased, and this division was therefore already determined before we study the effects of interest rate changes on their consumption expenditure.

These aforementioned characteristics of the Swedish mortgage market lessen the concerns over selection into one type of mortgage contract relative to another. As discussed below and presented in Online Appendix D, we find evidence in our data that households we identify as holders of ARMs are observationally similar to FRM holders along a variety of important dimensions. In support of this notion, previous analysis has found that outcomes related to households' financial health, such as the probability of mortgage default, do not correlate with the choice of interest-rate fixation (Holmberg *et al.*, 2015). Moreover, across households with different cash-flow margins and DTI ratios, there are limited indicators of systematic differences in the duration of interest-rate fixation. Households with low cash-flow margins, if anything, do hold a somewhat lower share of their debt in ARMs (Finansinspektionen, 2017). Other things equal, this would imply that households with a larger share in ARMs should be better equipped to take on an unexpected increase in expenses, e.g., due to higher interest rates.

2.5. *Characteristics and Indebtedness of Swedish Households*

We wish to highlight some general characteristics of Swedish households and their balance sheets. Table 1 reports summary statistics for our sample as a whole as well as separated into renters

improvement in accuracy. However, to avoid overstating the savings between 2005 and 2006, we artificially implement the reporting rule of 2000–5 on the latter period as well when imputing consumption.

[15] Since then, the share with adjustable interest rates has continued to increase. In 2018, approximately 70% of outstanding mortgage debt had a duration of less than one year.

Table 1. *Summary Statistics.*

	All (1)	Renters (2)	Homeowners (3)
Sociodemographics			
Disposable income	251	180	303
	(141)	(89)	(149)
Disposable income a.e.	148	131	160
	(55)	(46)	(57)
Age	55	56	54
	(17)	(19)	(16)
Household size	2.26	1.77	2.62
	(1.48)	(1.33)	(1.49)
< High school (share)	15.31	19.58	12.22
High school (share)	61.05	62.77	59.79
> High school (share)	23.64	17.65	27.99
Consumption measure			
Consumption	241	180	285
	(137)	(93)	(147)
Consumption a.e.	143	132	151
	(58)	(50)	(61)
Balance sheet items			
Debt	284	65	444
	(422)	(121)	(486)
Debt to income	0.88	0.33	1.27
	(1.10)	(0.64)	(1.19)
Interest rate*	5.19	5.21	5.18
	(3.44)	(5.06)	(2.19)
Correlation measure*	0.37	0.18	0.46
	(0.55)	(0.61)	(0.49)
Interest share	4.10	1.14	6.24
	(5.36)	(2.54)	(5.82)
Illiquid assets	635	–	1,096
	(901)	–	(946)
Liquid assets	126	69	167
	(247)	(186)	(277)
Liquid assets to income	0.58	0.45	0.68
	(1.30)	(1.24)	(1.34)
Loan to value*	0.45	–	0.45
	(0.44)	–	(0.44)
Unique households	64,125	26,611	37,514

Notes: Values are in SEK 1,000 or percent (averages). Values in parentheses are SD; a.e. refers to adult equivalent. The scaling factor follows OECD, assigning a weight of 1 to the first household member, 0.7 to each additional adult and 0.5 to each child. Age and education refer to the household head. * There are fewer observations for the interest rate and the correlation measure. For the loan-to-value ratio the mean is reported excluding the top 1 percentile.

and homeowners. Homeowners are different than renters along essentially any dimension. For instance, they are more educated and have higher incomes. Adult equivalent disposable income differs by kSEK 29 and adult equivalent consumption by kSEK 19.[16] Homeowners have more liquid assets than renters, kSEK 167 compared with kSEK 69. However, most of their wealth is in illiquid assets. The average loan-to-value ratio is 0.45, and 87% of their total assets is in illiquid assets.

[16] The exchange rate during our sample period was approximately 7.50 SEK/USD, so kSEK 1 is approximately equal to USD 133.

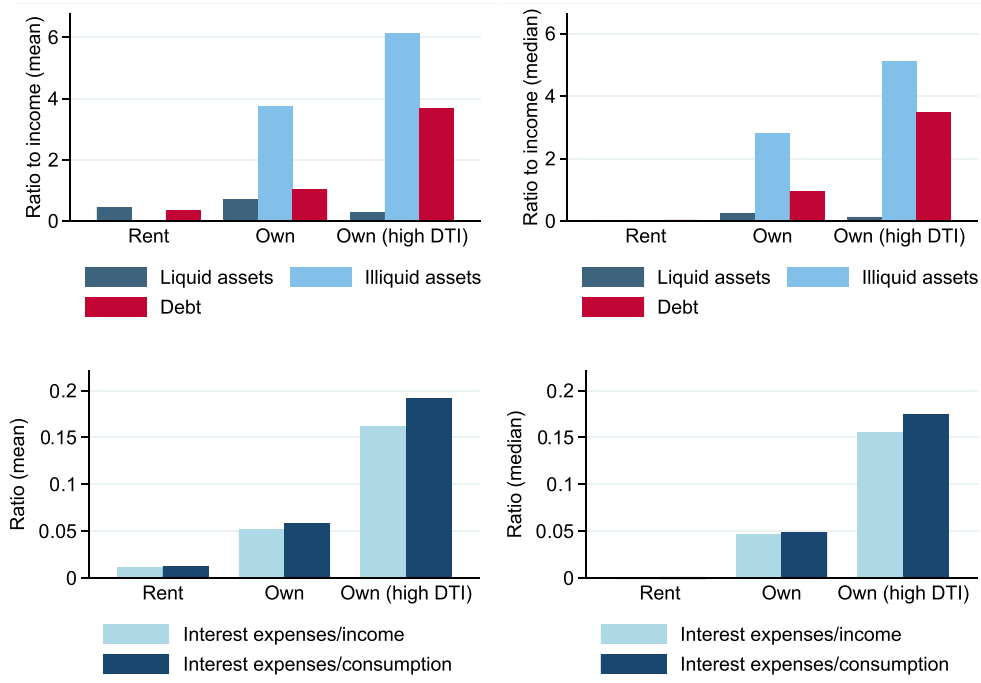


Fig. 2. *Assets, Debt and Interest Expenses.*
Notes: The figure displays renters' and homeowners' assets, debt and interest expenses normalised by disposable income. The second and third category report homeowners with a DTI ratio of less than 3 and equal or greater than 3, respectively. The last category is referred to as 'high DTI' homeowners; 9.1% of all homeowners belong to this category. The left-hand panels display means and the right-hand panels display medians.

Figure 2 graphically illustrates why homeowners in our sample with a high debt relative to income (DTI) are likely to be more sensitive to interest-rate changes than relatively less indebted homeowners. The top panels display the mean and median asset and debt balances in relation to disposable income for three groups: renters, homeowners with a DTI less than 3, and homeowners with a DTI equal to or greater than 3. The group of homeowners with a high DTI ratio comprises 9.1% of all homeowner observations. Whereas illiquid assets are relatively evenly distributed among homeowners—the mean is 4 for homeowners and 6 for the high DTI homeowners—liquid assets are less evenly distributed. The average homeowner has liquid assets worth approximately eight months of disposable income. In contrast, the high DTI homeowners have fewer than four months of disposable income. These statistics relate to a growing literature (e.g., Kaplan *et al.*, 2014) emphasising the importance of the liquidity of households' wealth for understanding consumption responses to income shocks and the significant share of wealthy HtM households in the population.

The bottom panels of Figure 2 display a cross-sectional variation in interest expenses relative to disposable income and consumption. High DTI homeowners spend on average 16% of their yearly disposable income on interest expenses. A doubling of the interest rate that homeowners face thus implies that the typical high DTI homeowner would deplete its liquid assets within one year unless it adjusts its income or consumption. These households are wealthy in terms of

illiquid wealth but hold very little liquid wealth. Thus, these households are likely to have a high propensity to consume out of transitory income and also likely to not react strongly to anticipated changes in future income. Another measure of interest-rate risk is the ratio of liquid assets to interest expenses. There are substantial differences in this ratio between renters, homeowners and high DTI homeowners. The median homeowner has liquid assets that are 2.6 times higher than its annual interest expenses, whereas this ratio is only 0.86 for the median high DTI homeowner, meaning that its annual interest expenses are larger than its liquid assets.

Combined with a high prevalence of ARMs, these empirical patterns lend support to our hypothesis of the sensitivity of a significant share of indebted households to changes in interest expenses.

3. Empirical Strategy

3.1. *Econometric Specification*

In Section 1, we outlined our theoretical argument for the cash-flow channel being operational among HtM households that have a large share of their debt in ARMs. The theory predicts that, for these households, the magnitude of the consumption response is approximately proportional to the DTI ratio. Building on these theoretical predictions, our main regression specification is

$$\Delta \log c_{i,t} = \alpha_i + \delta_t + \beta \Delta r_t \times DTI_{i,t-2} + \mathbf{X}'_{i,t}\gamma + \varepsilon_{i,t}, \tag{4}$$

where $\Delta \log c_{i,t}$ denotes the percentage change in consumption spending of household i in year t. The variable Δr_t is the change in the relevant interest rate, which, depending on the specification, is either the monetary policy interest rate (i.e., the *repo rate*) or an aggregate household interest rate measured by Statistics Sweden using data on all loans to households.[17] The variable $DTI_{i,t-2}$ is the household's DTI ratio, which we lag by one year so it is predetermined with respect to $c_{i,t-1}$. We denote individual fixed effects and year fixed effects by α_i and δ_t, respectively. Individual fixed effects capture any time-invariant cross-sectional heterogeneity, such as in borrowing behaviour or portfolio choice. Year fixed effects capture all common macroeconomic effects and responses to aggregate shocks, including intertemporal consumption responses of optimising households. The vector $\mathbf{X}_{i,t}$ collects a set of controls, including demographic characteristics, a fourth-order polynomial in age, the number of children, and the change in the number of children, and an interaction between Δr_t and dummy variables for being young (< 40), being old (≥ 60) and having children, aimed at accounting for characteristics that may, on average, interact with the sensitivity to changes in aggregate interest rates.

The coefficient β captures the effect of the cash-flow channel. It measures consumption responses to changes in the interest rate that vary systematically due to differences in DTI. If all households are optimisers, theory predicts $\beta = 0$. Conversely, if all households are HtM and obey equation (1), theory instead predicts that β will be equal to the average income-to-consumption ratio (θ), which is likely to be close to 1.[18] In Online Appendix B we illustrate that regression

[17] Note that this specification relates household spending to an aggregate interest rate, Δr_t, with no subindex i. Specifically, it does not use a measure of a household-specific interest rate. Thus, we avoid a potential bias that would arise if unobserved idiosyncratic events, for instance negative news about future income, affect both the household's consumption path and the household's creditworthiness. We provide further discussion on this in Subsection 4.2 and report the results from other alternative specifications in the Online Appendix.

[18] In our sample, the average income-to-consumption ratio is 0.98.

estimates of β will approximately capture the average response in our sample, determined by the population weights of each household and mortgage type.

3.2. *Identification Using Monetary Policy Shocks*

Under the cash-flow channel, HtM households respond to interest-rate changes when their cash flow changes, irrespective of whether the change was anticipated or unanticipated. The coefficient β in equation (4) captures this effect. Importantly, our empirical specification includes both year fixed effects—accounting for the overall aggregate effects of monetary policy on household spending, including intertemporal substitution of consumption—and household fixed effects—accounting for time-invariant individual differences, including those in consumption growth. As a result, β captures consumption responses due to cross-sectional variation in interest-rate sensitivity, less the aggregate effect.

However, our empirical analysis faces the standard problem of reverse causality in estimating the effects of monetary policy, namely that households respond to monetary policy, but monetary policy may also respond to the economic conditions of households. In particular, this concern arises if the central bank responds to macroeconomic developments that relate to the conditions of more indebted households.

To overcome this issue, we separate unanticipated changes in interest rates from those that are anticipated based on macroeconomic conditions and use this measure of monetary policy shocks as an instrumental variable in our estimation. To this end, we measure monetary policy shocks using an approach similar to what is used in recent literature studying monetary non-neutrality, including Gürkaynak *et al.* (2005), Gertler and Karadi (2015), Hanson and Stein (2015) and Nakamura and Steinsson (2018), building on an approach pioneered by Kuttner (2001) and Cochrane and Piazzesi (2002). Using data at high frequency, this literature seeks to identify innovations in monetary policy that are due entirely to policy shifts and not to macroeconomic development. Following this approach, we use a tight window of time around a monetary policy announcement to isolate the effect of a policy surprise on market interest rates. During our sample period, the Swedish market on futures, called STINA, was still undeveloped and illiquid. Unlike the aforementioned studies that use data from the USA and measure shocks using changes in the federal funds' futures, we are unable to measure shocks using futures. Instead, we therefore use the yield at a daily frequency of a one-month Swedish Treasury bill, computing the difference in the interest rates between the beginning and end of the days when a monetary policy announcement is made.[19]

3.3. *Threats to Identification*

The identification strategy of using monetary policy shocks as instruments for changes in interest rates does not come without challenges. The key challenge we face is the discrepancy between the frequencies at which we measure monetary policy shocks and observe changes in spending. Following precedent from earlier work, including Romer and Romer (2004), Coibion (2012) and

[19] The lack of futures data also prevents us from exploring responses to shocks of different persistence—e.g., by separating policy shocks into a 'target factor' and a 'path factor' following Gürkaynak *et al.* (2005). This might allow us, for example, to separately identify responses of HtM households to temporary shocks from responses of optimising households. However, as highlighted in the Online Appendix B, the latter are likely to be small.

Cloyne *et al.* (2020), we time-aggregate the monetary policy shocks to a yearly frequency by summing measured shocks from all announcements in a year.[20]

In Figure 3, we document two important correlations for our empirical strategy. First, in Figure 3(a) we document that the average interest rate on household debt closely follows the monetary policy rate, which is expected given the large share of debt with adjustable interest rates. Second, in Figure 3(b), we document how our measure of monetary policy shocks covaries with the monetary policy rate. While, as expected, the magnitude of these unanticipated changes in monetary policy rates is considerably smaller than the overall changes in interest rates, there is a strong positive co-movement of the shocks and the policy rate over the sample period. This validates the use of the shocks as an instrumental variable in our estimation.

The second challenge we face using this identification strategy is the concern that monetary policy shocks may influence consumption through channels other than interest rates, violating the exclusion restriction. While we cannot rule out this possibility, we argue that such effects would need to run through channels that affect households differentially across the DTI distribution since all aggregate channels through which monetary policy shocks, and changes in interest rates more generally, affect consumption are captured by year fixed effects.

In addition to the aforementioned challenges, one caveat to our empirical analysis is that, given the data at hand, we are not able to observe if households refinance their debt or adjust their amortisation in response to interest-rate changes. Any strategic response of that kind would be subsumed into the estimated cash-flow effect. But since borrowers must compensate the bank for changes in market interest rates when refinancing an FRM, we argue that it is unlikely that refinancing correlates systematically with changes in the monetary policy rate. Moreover, for highly indebted and constrained households that consume most of their disposable income, a decrease in the short-term interest rate implies an increase in disposable income and by that increased consumption possibilities, which are likely to be highly valued. Therefore, we expect any strategic refinancing, which is costly in Sweden, to come from less constrained (less indebted) households, which would bias our estimate of β towards zero.

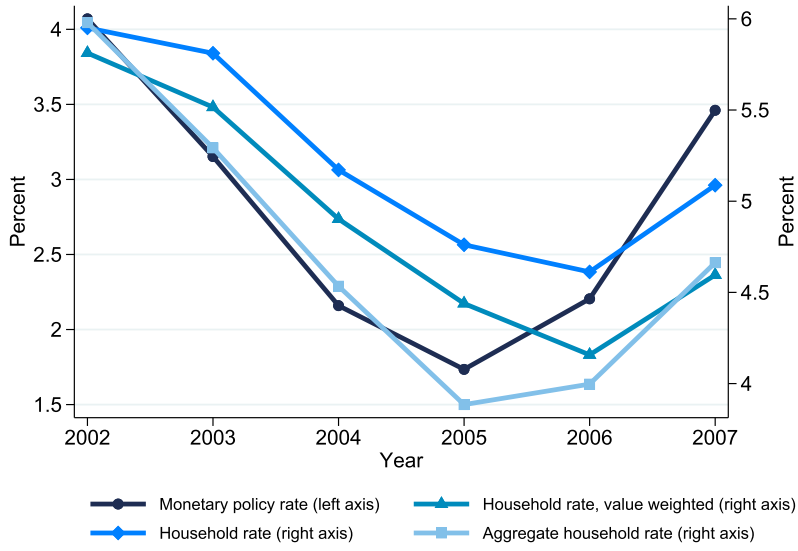
4. Results

4.1. *Consumption Responses to Interest-Rate Changes by Indebtedness*

Table 2 reports OLS estimates of consumption responses to changes in the monetary policy (repo) interest rate based on the regression equation (4). Column (1) reports a coefficient estimate of β of -0.26. The interpretation of this estimate is that the average household, which has a DTI of 0.88, reduces its consumption spending by an additional 0.23 percentage points (0.88×0.26) in response to a one-percentage-point increase in the monetary policy rate, relative to a household with no debt. Households that differ in their indebtedness and therefore, according to our hypothesis, in their consumption sensitivity to monetary policy, may also differ in their holdings of liquid assets. If households with high DTI hold disproportionally more liquid assets, our measure of the cash-flow channel will be muted. To investigate the importance of this effect, column (2) controls for the ratio of liquid assets to income, lagged in the same way as the DTI ratio. The coefficient estimate is only marginally affected by this control. In columns (3) and

[20] We acknowledge that this method allocates equal weights to shocks independent of whether they occur early or late in the year. We have explored the robustness of our results to alternative approaches to aggregation, including using within-year duration weights, which provides more weight to shocks the earlier in the year that they occur. We find that this does not have a meaningful effect on our estimates.

(a) Repo rate and household interest rates



(b) Monetary policy shocks and changes in the repo rate

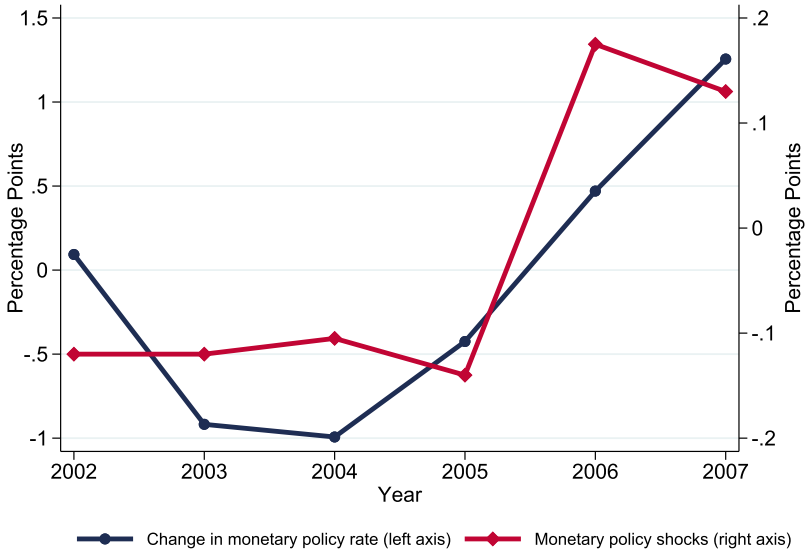


Fig. 3. *Repo Rate, Household Interest Rates and Monetary Policy Shocks.*
Notes: Panel (a) plots the repo rate, the average household interest rate in our sample (both equally and value weighted), and an aggregate household interest rate from Statistics Sweden. Panel (b) plots the monetary policy shocks and changes in the repo rate.

Table 2. *Consumption Responses to Changes in the Monetary Policy Rate.*

	(1)	(2)	(3)	(4)
	OLS			
	All households		Homeowners	
$\Delta r \times DTI$	−0.260***	−0.266***	−0.199***	−0.211***
	(0.058)	(0.058)	(0.075)	(0.075)
Liquid assets to income	No	Yes	No	Yes
Mean DTI	0.88	0.88	1.27	1.27
Observations	265,642	265,642	153,964	153,964
Clusters (households)	64,125	64,125	37,514	37,514
	IV			
	All households		Homeowners	
$\Delta r \times DTI$	−0.400***	−0.400***	−0.413***	−0.415***
	(0.078)	(0.078)	(0.103)	(0.103)
Liquid assets to income	No	Yes	No	Yes
Mean DTI	0.88	0.88	1.27	1.27
Observations	265,642	265,642	153,964	153,964
Clusters (households)	64,125	64,125	37,514	37,514

Notes: Each column in both panels presents results from a separate regression estimate of equation (4). In all regressions, Δr is the year-on-year change in the monetary policy (repo) interest rate, set by the Central Bank's monetary policy committee. The top panel presents results estimated using ordinary least squares (OLS). The bottom panel presents results estimated using instrumental variables (IV), where changes in interest rates are instrumented with monetary policy shocks. All regressions include individual fixed effects, year fixed effects and a set of controls containing a fourth polynomial in age, the number of children, change in number of children and interactions between the change in the monetary policy interest rate and *young* (dummy for < 40), *old* (dummy for ≥ 60) and *children* (dummy for having children). Robust SE, clustered at the household level, are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

(4), we repeat these regressions for the sample of homeowners. The heterogeneous response of homeowners with different DTI ratios is about the same as in the greater population. The estimated coefficient is between −0.20 and −0.21, indicating that the average homeowner with a DTI of 1.27 reduces its consumption spending by an additional 0.27 percentage points (1.27 × 0.21) in response to a one-percentage-point change in the monetary policy rate, relative to homeowners without debt.[21] These results imply that indebtedness matters not only in terms of the relative responses of (indebted) homeowners and renters, as found in Cloyne *et al.* (2020), but also within the group of homeowners where more indebted households reduce their consumption spending disproportionately relative to those less indebted.[22]

As we have emphasised above, consumption responses operating through the cash-flow channel occur in response to any interest expense-driven change in cash flow, irrespective of whether the change was anticipated or not. However, in estimating these responses, we face the problem of reverse causality: households respond to monetary policy-induced interest changes while monetary policy may also respond to the economic conditions of households. While we account

[21] A potential concern with using DTI lagged two years is that the behaviour of households that make large changes to their DTI between t and $t-2$ is ill-measured. To evaluate the implication that this might have for our estimates, we exclude households with large increases (top 10%) and decreases (bottom 10%) in the DTI ratio. Our main estimates are robust to this exclusion. The estimated coefficients are somewhat more negative compared with Table 2.

[22] We also consider the potentially different consumption responses to a change in the monetary policy rate among households in different parts of the DTI distribution. In a regression specification alternative to equation (4), instead of including the DTI ratio in levels we construct five indicator variables for quantiles of the DTI distribution and interact these indicators with the change in the monetary policy rate. The estimated coefficients are negative for all five groups and largest in absolute value for the two upper quantiles. Results are available upon request.

for all aggregate effects of monetary policy on consumption by including year fixed effects, there is still a concern that monetary policy responds to the conditions of highly indebted households. We address this issue by using monetary policy shocks—variations in the policy rate not driven by changes in macroeconomic conditions—as instruments for changes in the interest rate.

The bottom panel of Table 2 presents two-stage least squares estimates of equation (4), where changes in interest rates are instrumented with monetary policy shocks. This isolates consumption responses to changes in interest rates that are unanticipated. Columns (1)–(4) report coefficient estimates of β between -0.40 and -0.42. This implies that, on average, households in the full sample reduce their consumption spending by an additional 0.35 percentage points in response to a one-percentage-point increase in the monetary policy rate, relative to a household with no debt. For homeowners, the corresponding figure is 0.53. Compared to the OLS estimates, these estimates are 50%–100% greater and imply considerably stronger cash-flow effects. This may reflect that the IV estimates capture only responses to unanticipated changes in interest expenses, which changes the composition of households who adjust their consumption. It may also reflect that the OLS estimate is biased towards zero due to strategic responses, such as refinancing or amortisation. All in all, our results are consistent with the presence of households that display HtM behaviour, as discussed in Section 1. More precisely, our estimates can be compared to regression estimates on data generated from model simulations, reported in Online Appendix B. Comparisons suggest that our estimates are at least three times as large as those generated by optimal consumption responses of households with ARMs and of similar magnitude to estimates on model data based on a configuration such that 50% of households have ARMs (the remainder having FRMs) and 50% of households respond optimally (the remainder displaying HtM behaviour). As an alternative to our estimates of responses to changes in the policy rate, Table 3 documents responses to changes in the aggregate interest rate faced by households. This rate, which we obtain from Statistics Sweden, is the average interest rate across all loans to households. By focusing on responses to this interest rate, we ignore the first step in the transmission of monetary policy into households' interest payments. As documented in Figure 3(a), the average interest rates on household debt closely follow the monetary policy rate.[23] Column (1) in the top panel of Table 3 reports a coefficient estimate of β of -0.62. This implies that a one-percentage-point increase in the lending rate reduces the consumption spending of the average household by an additional 0.55 percentage points (0.62×0.88) relative to those without debt. As for the response to changes in the policy rate, the magnitudes are similar when controlling for holdings of liquid assets and restricting the sample to homeowners only. The difference between the estimates in Tables 2 and 3 reflect that responses to changes in the monetary policy rate are muted due to an incomplete transmission to household interest rates. This is expected as changes in the policy rate get transmitted into changes in household interest rates only for those with adjustable rates.

In the bottom panel of Table 3, we report IV estimates where the average household interest rate is instrumented with monetary policy shocks. These estimates are similar to the OLS estimates in magnitude—if anything, slightly smaller in absolute value—implying that consumption responds equally strongly to anticipated and unanticipated changes in interest expenses, as predicted by the

[23] To further gauge the pass-through of monetary policy rates to interest on household debt, we estimate a regression of the change in the average household rate on the change in the policy rate, which gives a coefficient estimate of about 0.68.

Table 3. *Consumption Responses to Changes in Aggregate Household Interest Rate.*

	(1)	(2)	(3)	(4)
	OLS			
	All households		Homeowners	
$\Delta r \times DTI$	−0.622***	−0.631***	−0.594***	−0.616***
	(0.087)	(0.087)	(0.114)	(0.114)
Liquid assets to income	No	Yes	No	Yes
Mean DTI	0.88	0.88	1.27	1.27
Observations	265,642	265,642	153,964	153,964
Clusters (households)	64,125	64,125	37,514	37,514
	IV			
	All households		Homeowners	
$\Delta r \times DTI$	−0.529***	−0.528***	−0.538***	−0.539***
	(0.111)	(0.111)	(0.146)	(0.146)
Liquid assets to income	No	Yes	No	Yes
Mean DTI	0.88	0.88	1.27	1.27
Observations	265,642	265,642	153,964	153,964
Clusters (households)	64,125	64,125	37,514	37,514

Notes: Each column in both panels presents results from a separate regression estimate of equation (4). In all regressions, Δr is the year-on-year change in the average household interest rate computed by Statistics Sweden based on all loans to households. The top panel presents results estimated using ordinary least squares (OLS). The bottom panel presents results estimated using instrumental variables (IV), where changes in interest rates are instrumented with monetary policy shocks. All regressions include individual fixed effects, year fixed effects and a set of controls containing a fourth polynomial in age, the number of children, change in number of children and interactions between the change in the average household interest rate and *young* (dummy for < 40), *old* (dummy for ≥ 60) and *children* (dummy for having children). Robust SE, clustered at the household level, are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

theory set out in Section 1. The estimates of β can be translated into a relative MPC out of changes in disposable income, or cash flow, as a result of a change in the interest rates. Under a perfect pass-through of interest rate changes to households' interest payments, the above estimates imply an MPC of 0.19–0.50 from a one-unit increase in interest expenses.[24]

4.2. *The Role of Mortgage Types*

Our point of departure, theoretically motivated by Section 1, is that if the interest rates on household debt are tightly linked to short-term interest rates, changes in monetary policy will have a direct effect on households' interest expenses, which will translate into a reduction in household consumption expenditure if they are HtM households. This is what we refer to as the cash-flow channel. We argue that Sweden offers an ideal setting for evaluating the importance of this channel due to generally short interest-rate fixation periods and, in particular, a high prevalence of ARMs. However, our analysis until now has not differentiated between households with different types of mortgage contracts. We now provide more direct evidence illustrating how

[24] The average consumption in our sample is kSEK 241, and average debt is kSEK 284. A one-percentage-point increase in the interest rate reduces household cash flows by $0.01 \times 284 =$ kSEK 2.84 under perfect pass-through. According to the estimate in Table 2, top panel, Column (1), the average reduction in consumption to a one-percentage-point interest rate increase is $0.26 \times 0.88 \times 0.01 \times 241 = 0.55$. This implies an MPC of 0.19 (0.55/2.84). Similar calculations based on the IV estimates in the bottom panel imply an MPC of 0.30–0.34. The estimates in Table 3 imply an MPC of 0.40–0.50. For homeowners, we use a consumption value of kSEK 285 and a debt value of kSEK 444 in these calculations.

our estimates of differential consumption responses to interest-rate changes operate through the cash-flow channel.

Since our data originate from tax records and do not include any contract details, we do not directly observe which households have a mortgage with an adjustable rate, a fixed rate or more than one mortgage and a mixture of the two (which is common). We also do not directly observe the interest rate that the household pays on its debt. Instead, our approach is to first compute the implied household-specific interest rate using information on interest expenses and the amount of debt. Then, for each household, we calculate the correlation between its implied interest rate and the monetary policy rate. We use this correlation as a proxy for the impact of changes in the monetary policy rate on the interest expenses of that particular household—or to which extent each household has adjustable- or fixed-rate mortgages.

More precisely, we first calculate the interest rate $r^d_{i,t}$ for household i in year t as total interest expenses divided by average debt (in t and $t-1$):

$$r^d_{i,t} = \frac{interest\ payment_{i,t}}{0.5 \cdot debt_{i,t} + 0.5 \cdot debt_{i,t-1}}. \tag{5}$$

Based on definition (5), we construct value-weighted and equal-weighted household interest rates in our sample. Figure 3(a) illustrates the evolution of these rates and how they co-move with the monetary policy rate and the aggregate household interest rate reported by Statistics Sweden. Over our sample period, the household rates display the same U shape as the monetary policy rate, which highlights the prevalence of ARMs. The value-weighted rate almost perfectly tracks the monetary policy rate with some lag. The equally weighted rate also tracks the fluctuation well, but the level is too high, indicating that small credits carry a higher interest.

As we discuss in Subsection 2.4, it is very common in Sweden to hold a portfolio of loans with different durations of interest-rate fixation. Therefore, in our setting, holding debt with adjustable rates is not a binary variable. To obtain a proxy measure for how closely a household's interest rates react to short-term rates—i.e., what is the prevalence of ARMs vs. FRMs in households' debt portfolios—we compute the correlation between household-specific interest rates, $r^d_{i,t}$, and the monetary policy rate. We document the cross-sectional distribution of these correlation coefficients in Figure A.11 in Online Appendix C. Consistent with a high prevalence of ARMs, the median correlation in the population of homeowners is 0.61.[25]

To evaluate the differential consumption response of holders of ARMs vs. FRMs, we estimate an extended version of regression equation (4). First, we construct five indicator variables for quantiles of the correlation distribution, $Interest\ fixation_{iq}$, where $q = 1$ denotes the quantile with the lowest correlation—interpreted as reflecting households with loan portfolios consisting mainly of FRMs—and $q = 5$ denotes the quantile with the highest correlation—interpreted as reflecting households with high prevalence of ARMs in their loan portfolios. We then run the

[25] One obvious concern is that few observations are used for each household in computing these correlations. However, measurement error due to misclassification into ARMs vs. FRMs would result in an attenuation bias, as the differential responses would be muted. Another concern, which we highlight in Subsection 3.3, is that changes in computed interest rates due to the resetting of interest rates cannot be separated from changes due to refinancing or loan repayment. This explains, for example, the fact that we estimate a negative correlation coefficient for some households.

Table 4. *Consumption Responses by Interest-Rate Fixation.*

	(1)	(2)	(3)	(4)
	OLS		IV	
Interest fixation$_1$ × Δr × *DTI*	−0.102	−0.107	0.000	−0.004
	(0.147)	(0.147)	(0.193)	(0.193)
Interest fixation$_2$ × Δr × *DTI*	−0.072	−0.074	−0.447***	−0.448***
	(0.132)	(0.132)	(0.168)	(0.168)
Interest fixation$_3$ × Δr × *DTI*	−0.381***	−0.384***	−0.492***	−0.495***
	(0.141)	(0.141)	(0.176)	(0.176)
Interest fixation$_4$ × Δr × *DTI*	−0.438***	−0.439***	−0.383**	−0.385**
	(0.129)	(0.129)	(0.174)	(0.174)
Interest fixation$_5$ × Δr × *DTI*	−0.440***	−0.448***	−0.438**	−0.444**
	(0.145)	(0.144)	(0.193)	(0.193)
Interest fixation$_1$ × Δr	0.626***	0.608***	−0.322	−0.312
	(0.205)	(0.205)	(0.271)	(0.271)
Interest fixation$_2$ × Δr	0.626***	0.611***	0.391	0.405
	(0.225)	(0.225)	(0.296)	(0.296)
Interest fixation$_3$ × Δr	0.520**	0.507**	−0.024	−0.009
	(0.249)	(0.249)	(0.323)	(0.323)
Interest fixation$_4$ × Δr	0.272	0.262	−0.532	−0.508
	(0.245)	(0.245)	(0.329)	(0.329)
Interest fixation$_5$ × Δr	0.421*	0.421*	−0.215	−0.189
	(0.237)	(0.237)	(0.320)	(0.320)
Liquid assets to income	No	Yes	No	Yes
Observations	265,642	265,642	265,642	265,642
Clusters (households)	64,125	64,125	64,125	64,125

Notes: Δr is the year-on-year change in the monetary policy (repo) interest rate, set by the Central Bank's monetary policy committee. *Interest fixation*$_q$ refers to five indicator variables for quantiles of the distribution of correlation coefficients between the household-specific interest rate and the monetary policy rate; see the main text for details. All specifications include individual fixed effects, year fixed effects and a set of controls containing a fourth polynomial in age, the number of children, change in number of children and interactions between the change in the monetary policy interest rate and *young* (dummy for < 40), *old* (dummy for ≥ 60) and *children* (dummy for having children). Robust SE, clustered at the household level, are in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

following regression:

$$\Delta \log c_{i,t} = \alpha_i + \delta_t + \sum_{q=1}^{5} \lambda_q \textit{Interest fixation}_{iq} \times \Delta r_t \times \textit{DTI}_{i,t-2} + \sum_{q=1}^{5} \eta_g \textit{ Interest fixation}_{iq} \times \Delta r_t + \mathbf{X}'_{i,t}\gamma + \varepsilon_{i,t}. \quad (6)$$

Table 4 reports estimates of regression equation (6). For the two groups with the lowest correlation—higher prevalence of FRMs—the λ_q coefficients are not statistically significant. For the groups with higher correlation—higher prevalence of ARMs—the estimated responses are negative and stronger at the top of the distribution. There is a statistically significant difference between each of the two top quantiles and the bottom two quantiles. Comparing the OLS and IV estimates, the estimates are similar in magnitude although the IV estimates at the lower quantiles are larger (in absolute value) than the corresponding OLS estimates.[26]

In order to compute 'quantile effects' from these estimates, we multiply the estimates of λ_q by the average DTI ratio for that quantile group and add the corresponding η_q coefficient

[26] Table A.9 in the Online Appendix reports results for a sample restricted to homeowners, finding similar results although the coefficients are less precisely estimated.

estimate.[27] We find that the heterogeneity in responses between the quantiles is sizeable: households with higher prevalence of ARMs display the strongest responses. The difference in elasticities across the quantiles is approximately 0.90. Furthermore, *F*-tests imply that the differences between the top three quantiles relative to the bottom two are statistically significant. This implies that the responses reported so far are driven not only by differential responses of more indebted households but among them by those with a higher prevalence of debt with adjustable interest rates.

Figure 4 graphically illustrates our findings. The figure plots yearly changes in the repo rate—displaying a distinct U shape during 2002–7—as well as the difference in consumption growth for households with similar levels but different composition of debt. Figure 4(a) plots the median consumption growth of homeowners that belong to the high DTI group minus the median consumption growth of homeowners that belong to the high DTI group and have an interest-rate correlation with the repo rate above median (i.e., a proxy for having ARMs). Figure 4(b) displays the same group differences in means instead of medians. In line with our regression estimates, the figure shows a strong positive correlation between this measure and the repo rate. As the repo rate increases, consumption falls behind among the highly indebted homeowners with ARMs.

4.3. *The Role of Liquid Assets*

So far, we have focused on differential responses due to differences in DTI ratios and interest-rate fixation on the mortgage. We now analyse the role of a third characteristic of the household balance sheet, namely the level of liquid assets to income. Kaplan *et al.* (2014) emphasise that having low levels of liquid wealth is associated with hand-to-mouth behaviour, and one of the takeaways of Section 1 is that access to a buffer is critical for optimal consumption responses to increases in the mortgage interest rate.

To examine how liquid assets shape consumption responses, we group households by three characteristics: DTI ratios, interest-rate fixation and liquid assets to income. For DTI, we define households as being either below or above median. For interest-rate fixation, a correlation below the median is taken as a proxy for the household having an FRM, and a correlation above is taken as a proxy for the household having an ARM. This is broadly consistent with the aggregate shares. In addition, we classify households as having either low or high liquid assets to income. Again, the cut-off is at the median. Based on these three balance sheet characteristics, we form eight ($2 \times 2 \times 2$) groups of households.

Panel A of Table 5 reports summary statistics for the groups. On average, households with high DTI ratios have higher levels of disposable income than those with low DTI ratios, more household members, and a household head that is slightly younger. Higher DTI ratios are also associated with higher levels of illiquid wealth, i.e., higher real estate value. Looking within groups with similar DTI and liquid assets-to-income ratios, households with ARMs and FRMs appear similar.

We extend our baseline regression (4) to include a sum of terms, $\sum_{k=1}^{8} \omega_k \, Group_{ik} \times \Delta r$. The coefficient ω_k is an estimate of group k's response to changes in the monetary policy rate.[28] Panel B of Table 5 reports OLS estimates from this regression. Groups 1 and 8 are the two

[27] The average DTI ratios for the different quantiles are {0.83, 1.17, 1.36, 1, 36, 1.23}. To illustrate, for the top quantile (i.e., the highest correlation) the group response is equal to $-0.440 \times 1.23 + 0.421 = -0.120$.

[28] We also include the lagged value of liquid assets to income in the vector of control variables, as in previous extensions of (4). Notice that the difference to (6) is that this regression specification only contains one set of terms with

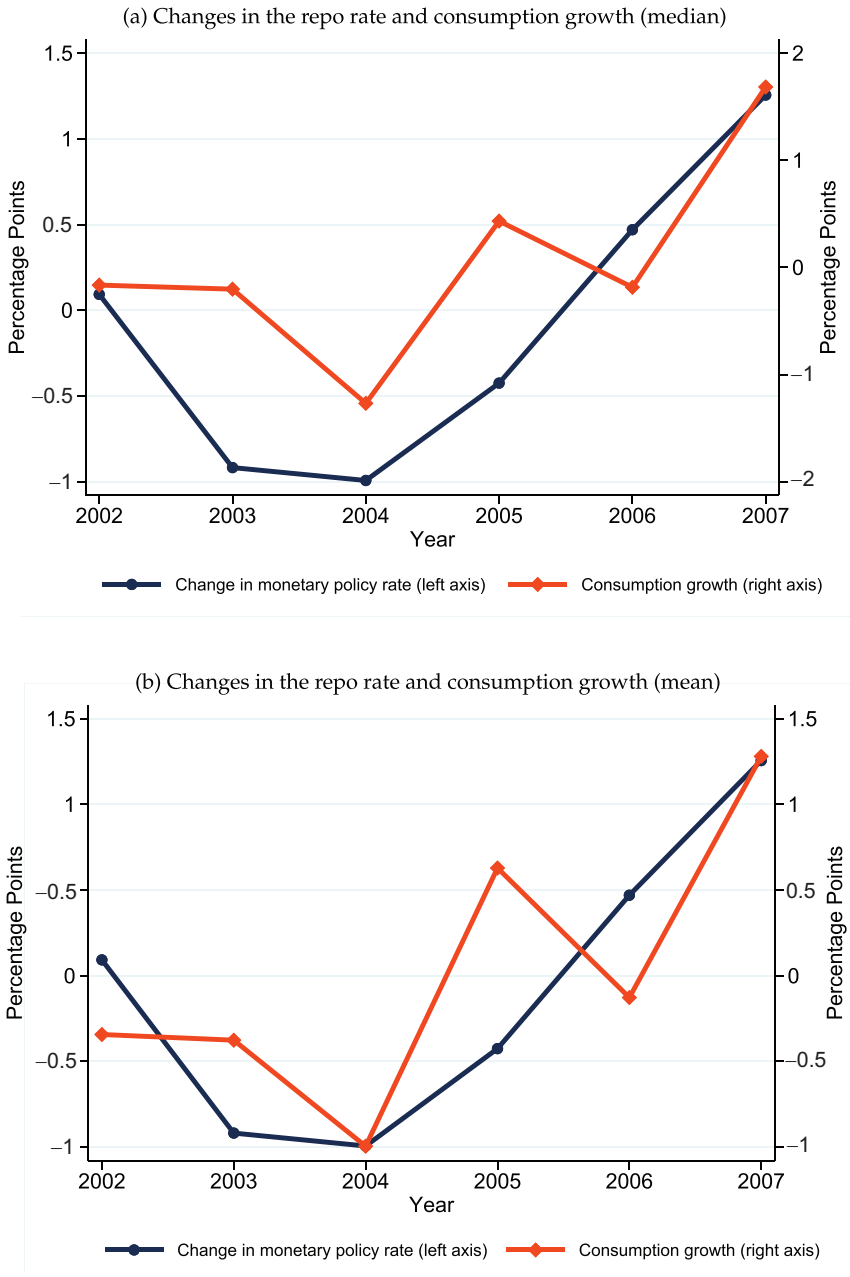


Fig. 4. *The Repo Rate and Relative Consumption Growth.*
Notes: Panel (a) depicts relative consumption growth measured as the median consumption growth among homeowners with a high DTI ratio minus the median consumption growth of homeowners with a high DTI ratio and an interest-rate correlation with the repo rate above median—a proxy for households with ARMs. Panel (b) depicts the same group differences evaluated at the mean instead of the median.

Table 5. *Consumption Responses by Liquid Assets to Income.*

	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6	Group 7	Group 8
DTI	High	High	High	High	Low	Low	Low	Low
Mortgage type (proxy)	ARM	ARM	FRM	FRM	ARM	ARM	FRM	FRM
Liquid assets to income	Low	High	Low	High	Low	High	Low	High
	A. Summary statistics							
Disposable income	308	359	278	344	211	260	207	257
Age	47	50	46	49	50	56	49	56
Household size	2.84	3.01	2.68	2.92	2.12	2.02	2.27	2.03
Consumption	290	331	265	314	210	255	208	253
Debt	573	605	470	563	49	49	45	42
Debt to income	1.77	1.66	1.61	1.60	0.22	0.18	0.21	0.16
Interest rate	5.26	4.71	4.98	4.87	6.90	5.52	6.72	5.62
Interest share	8.58	7.56	7.42	7.60	1.37	0.95	1.24	0.79
Illiquid assets	873	1,390	623	1,254	114	578	83	517
Liquid assets	23	196	20	189	12	241	10	227
Liquid assets to income	0.07	0.57	0.06	0.57	0.05	0.92	0.04	0.87
Loan to value	0.74	0.52	0.72	0.52	0.27	0.13	0.23	0.12
Observations	34,045	36,249	33,381	26,775	14,716	11,108	22,548	13,420
Households	11,158	11,828	10,828	9,074	4,892	3,962	7,156	4,704
	B. Consumption responses (OLS)							
$Group_k \times \Delta r$	−0.691***	−0.236	0.326*	−0.069	0.207	0.943***	0.672***	0.965***
	(0.201)	(0.207)	(0.195)	(0.226)	(0.228)	(0.305)	(0.192)	(0.283)
F-test	0.060		0.120		0.041		0.354	
	C. Consumption responses (IV)							
$Group_k \times \Delta r$	−1.792***	−0.547*	−0.786***	−0.571*	−0.878***	0.736*	−0.309	1.125***
	(0.280)	(0.287)	(0.267)	(0.307)	(0.303)	(0.409)	(0.254)	(0.367)
F-test	0.000		0.519		0.001		0.000	

Notes: $Group_k$ refers to eight indicator variables for group defined by DTI, interest duration and liquid assets to income. Δr is the year-on-year change in the monetary policy (repo) interest rate. The OLS and IV regressions include individual fixed effects, year fixed effects and a set of controls containing a fourth polynomial in age, the number of children, change in number of children, the twice lagged value of liquid assets to income, and interactions between Δr and *young* (dummy for < 40), *old* (dummy for ≥ 60) and *children* (dummy for having children). The number of observations is 265,642, and the number of households is 64,125. Robust SE, clustered at the household level, are in parentheses. The *F*-tests report p-values for equality of coefficients between two groups with equal debt to income and mortgage type but different liquid assets to income. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. See Table 1 for further details on the summary statistics.

polar extremes from our classification, and this is reflected in our estimates. The coefficient estimates vary from −0.69 to 0.97. For the other groups, estimates are in-between. Since the groups are small and our imputed measure of consumption is noisy, some caution is warranted. We therefore complement the point estimates with *F*-tests of equality of estimates across groups. We report tests of equality for groups with similar DTI ratios and mortgage types but different levels of liquid assets to income. The tests indicate that for households with ARMs we can reject equal responses (columns 1 vs. 2; columns 5 vs. 6), whereas this is not the case for households with FRMs (columns 3 vs. 4; columns 7 vs. 8). Groups with ARMs and low liquid assets to income display different responses relative to households with high liquid assets to income. This is consistent with households with low liquid assets displaying HtM behaviour, hence facing difficulties responding optimally to sudden increases in interest expenses. For households with high DTI ratios but FRMs, there is no direct effect on expenses in the short run; only future expenses are directly affected if the interest rate increase is expected to be long lasting, and spending responses are independent of liquid assets. Consistent with this, the *F*-tests cannot reject equal responses.

Panel C of Table 5 reports IV estimates. Consistent with previous analysis in the paper, the IV estimates display greater variation than the OLS estimates. One reason could be comparably weak first-stage effects for FRM households. We focus on equality of coefficients rather than point estimates. As in Panel B, we reject equality for households with ARMs and similar DTI ratios but different levels of liquid assets to income (columns 1 vs. 2; columns 5 vs. 6). In our IV estimation there is also a difference between households with low DTI and FRMs that have different liquid assets-to-income ratios (columns 7 vs. 8). However, some caution is warranted since the OLS and IV estimates of Group 7 are quite different. We conclude that responses of households with ARMs to a greater extent depend on their liquid assets to income.

5. Robustness

In this section we document a range of statistics and checks to evaluate the robustness of our results.

5.1. *Savings in Bank Deposits*

As discussed in Section 2, we only observe bank account deposits in our data if certain criteria are met. Essentially, small bank deposits are not reported. As a result, some savings responses to interest rate changes are unobserved. Mechanically, this will lead to an overstatement of measured spending in equation (3). If savings responses of this kind are homogeneous across households, this measurement error will wash out with the year fixed effects in our regressions in the same way as intertemporal consumption responses of optimising households do. However, if less indebted households are more likely to have unmeasured savings responses than those more indebted, which is likely, there will be an upward bias in the estimate of coefficient β. We have carried out analysis to assess this potential bias. We first document in Figure A.10 of Online Appendix C that the monetary policy rate and the bank deposit rate are positively correlated. Regressing changes in the aggregate deposit rate on changes in the monetary policy

Δr_t, which simplifies interpretation. Unlike the specification in (6), the classification prior to estimation already takes into account the household's DTI.

rate gives a coefficient estimate of 0.62, implying high pass-through. Then we study the relationship between bank deposit rates and flows into bank account deposits and find there to be a positive correlation. From this evidence we conclude that there is cause for concern about this bias.

To quantify the bias, we estimate our main regression, (4), on a restricted sample. We first impute spending as if all household-year observations associated with unreported bank deposits are missing. This is a very severe restriction as we drop all observations when households have no, or close to no, bank deposits. Using this restricted sample, we estimate our main regression specification, which yields a coefficient estimate of −0.17. Relative to the comparable baseline estimate in Table 2, this estimate is 35% smaller in absolute value. We argue that this is likely an upper bound on the size of this bias. This sample restriction not only excludes households with unmeasured savings responses but also HtM households who are likely to respond strongly in terms of spending. Furthermore, many of these households are highly indebted. So, simply removing those households from the data will overstate the bias. We therefore conduct another robustness exercise. We treat observations with no reported bank deposits as missing only if the household has a DTI ratio below the median. Under this restriction, we obtain a coefficient estimate of −0.23, which is 12% smaller (in absolute value) than the comparable baseline estimate. We conclude that this is the likely magnitude of the bias resulting from this type of measurement error.

5.2. *Heterogeneity in Consumption-to-Income Ratios*

The theoretical motivation for our empirical analysis, described in Section 1, implies that if all households are HtM consumers, the consumption response to a change in interest rates that directly translates into a change in interest expenses will be proportional to the consumption-to-income ratio (see equation (1)). While our empirical specification (4) captures the response of households to interest rate changes that vary in their effect by households' indebtedness, it assumes that individuals' consumption-to-income ratio is constant and subsumed in the individual fixed effects. However, it is possible for there to be household-level variation in consumption-to-income ratios that is correlated with the consumption responses to changes in interest expenses. This would bias our estimates. We investigate this concern in Tables A.5–A.8 in Online Appendix D, finding relatively similar but, if anything, somewhat stronger responses when accounting for individuals' consumption-to-income ratios.

5.3. *Heterogeneity in Income Growth*

As Section 1 describes, for HtM consumers, consumption moves closely with changes in interest rates but also with changes in income. If changes in monetary policy directly affect not only interest payments but also labour income, the effect is that our empirical specification might not only measure the consumption response to changes in interest payments as a result of changes in the policy rate but also the response to a change in income from changes in monetary policy. To shed some light on this concern, we estimate equation (4) including income growth as an additional explanatory variable. As documented in Tables A.5–A.8 in Online Appendix D, the estimates are largely unaffected by the inclusion of this control. While this exercise implies that our results are robust to the aforementioned concern, we are cautious when interpreting the results as including income growth as a control may itself introduce a bias to our estimates. As

income growth should rather be thought of as an outcome variable itself, it is a 'bad control' in the language of Angrist and Pischke (2008) and therefore not included as a control in our main specification.

5.4. *Further Analysis of Interest-Rate Flexibility*

In order to evaluate our results on the differential responses by our measure of interest-rate fixation, we compare the characteristics of homeowners at the two sides of the spectrum. Table A.3 in Online Appendix D reports differences across households based on whether they have a correlation above or below the median. We denote these groups as holders of ARMs and holders of FRMs, respectively. We find that households with ARMs have higher income and consumption on average, but they also have more household members than holders of FRMs. Households with ARMs have more debt as well as more illiquid assets, but, importantly from the perspective of our analysis, there is no statistical difference in liquid assets. While the groups are statistically different along some dimensions, the differences are economically small. This is consistent with the conventional Swedish view that an ARM is not an exotic mortgage product and that households tend to hold more than one mortgage, often with interest rates of different duration.

To further evaluate the non-linearities in responses by interest rate flexibility, Table A.10 in Online Appendix D reports estimates of equation (6), where instead of interactions based on five quantile groups we use a continuous correlation measure (i.e., the triple interaction $Corr_i \times \Delta r_t \times DTI_{i,t-2}$). The estimates imply that households holding only ARMs ($Corr_i \approx 1$) respond to a one-percentage-point increase in interest rates by reducing their consumption by about 0.4–0.5 percentage points more than households holding only FRMs. These results are somewhat stronger when restricting the sample to only include homeowners.[29]

5.5. *Household-Specific Interest Rates*

To this point, our analysis has focused on consumption responses to aggregate interest rates. There are two reasons for this choice. First, our aim is to shed light on a transmission mechanism of monetary policy that operates through the direct effect of changes in policy rates on households' interest expenses. Since the pass-through to household interest rates is not perfect, estimating responses to changes in household interest rates directly moves us further from this goal. Second, as our data include neither details about loan contracts nor refinancing of loans, we cannot separate changes in interest expenses that are due to changes in the policy rate from those due to other factors.

To evaluate the implication of this restriction, Table A.11 in Online Appendix D reports consumption responses to two measures: individual households' interest rates and their total interest expenses. Columns (1) and (2) report estimates of equation (4), where the interest rate is the household-specific interest rate rather than the monetary policy rate. The coefficient estimate implies a similar but somewhat weaker response than what is reported in Subsection 4.1. The

[29] As discussed above and reported in Figure A.11 in Online Appendix D, some households have a negative correlation, which may result from changes in interest rates due to refinancing or loan repayment. When restricting the sample to households with a non-negative correlation, the coefficient estimates are broadly similar and, if anything, stronger than for the full sample.

estimates imply that the average household reduces its consumption spending by an additional 0.25 percentage points (1.41 × 0.18) in response to a one-percentage-point increase in its average interest rate, relative to a household with no debt. The results, as before, are robust to controlling for differences in liquid asset holdings. Columns (3) and (4) report estimates from an alternative specification where we relate the change in consumption directly to changes in households' interest expenses. The coefficient estimates, which can be interpreted as the MPC out of a one-unit increase in interest expenses, imply an MPC of about 0.22. Possibly consistent with our concerns, both sets of estimates are in the lower range of estimates reported in Subsection 4.1.

6. Conclusion

Using detailed data on consumption and balance sheets of Swedish households, we find evidence of the cash-flow channel of monetary policy transmission. Households with higher levels of debt relative to their income respond more strongly to changes in the policy interest rate than those that are less indebted. This is true even among homeowners and households with high levels of illiquid wealth, but with disproportionally little liquid wealth. Our results document that these responses are driven by households that hold some or a large share of their debt in contracts where interest rates are linked to short-term rates, such as ARMs, and are therefore at short notice directly exposed to monetary policy changes.

Our results highlight the importance of channels of monetary policy transmission other than the conventional interest-rate channel. The findings indicate that monetary policy is more potent in economic environments where households holding high levels of debt relative to their income face interest expenses that closely follow the monetary policy rate as well as where households have small buffers of liquid assets or restricted access to credit. We demonstrate this in a setting where households are relatively highly indebted and loan and mortgage contracts with variable interest rates are standard and non-exotic, covering nearly half of the outstanding debt during our sample period. Under such conditions, monetary policy can have a stronger effect on real economic activity than what is predicted by conventional estimates where transmission operates first and foremost through intertemporal substitution.

It is necessary to emphasise the limitations of our study and the generalisability of our results. Our empirical analysis is directed and limited to illustrating the cash-flow effect of changes in interest rates and cannot speak directly to the effects that monetary policy may have on the supply of credit. This may be an important channel, particularly at times when central banks make large changes to their policy rates. More generally, we are unable to characterise the general equilibrium effect of the cash-flow channel on aggregate consumption in the economy, as has been highlighted in recent and contemporaneous research (Cloyne *et al.*, 2020). Another channel that we have not been able to incorporate into our analysis, but is likely to be important, is that monetary policy may have heterogeneous effects on household consumption by affecting the distribution of wealth in the economy. This mechanism has been highlighted in recent theoretical work (Auclert, 2019). Empirically evaluating these other mechanisms remains a task for future research.

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Additional Supporting Information may be found in the online version of this article:

Online Appendix
Replication Package

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