Monetary Policy when Households have Debt: New Evidence on the Transmission Mechanism[*]

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Abstract

This paper offers a new perspective on the transmission of monetary policy using household micro-data for the U.S. and U.K.. Following a temporary cut in interest rates, households with mortgage debt increase their spending significantly, home-owners without debt do not adjust expenditure at all and renters increase spending but by less than mortgagors. Income, however, rises considerably for all households. We show that the balance sheets of these housing tenure groups differ markedly in their composition of liquid versus illiquid wealth and this heterogeneity in liquidity holdings, together with a sizable general equilibrium effect on income, is crucial for explaining our results. In contrast, differences in demographics, the elasticity of intertemporal substitution, wealth effects and the response of mortgage and rental payments are unable to account for the heterogeneity in expenditure we document.

JEL classification: E21, E32, E52.
Key words: monetary policy, household balance sheets, liquidity constraints.

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1 Introduction

How monetary policy affects the real economy is one of the oldest and most intensively investigated topics in macroeconomics. In many standard macroeconomic theories, interest rate changes primarily affect household consumption through intertemporal substitution. The recent financial crisis, however, has sparked a lively debate about whether other mechanisms could be equally, or even more, important for the transmission of monetary policy.

A significant part of this debate has focused on household indebtedness and whether macroeconomic policies affect consumption by redistributing resources between borrowers and savers. In particular, an influential set of papers have highlighted the role of mortgage debt in the amplification of macroeconomic shocks (Mian et al. (2013), Mian and Sufi (2014)), and how the composition of the household balance sheets affects the transmission of monetary policy (Auclert (2015), Kaplan et al. (2015)). Despite this, the literature is still lacking a systematic empirical investigation of the how differences in household balance sheets may affect the propagation of changes in macroeconomic policy. Our contribution is therefore to provide a novel set of empirical stylized facts and to use these to evaluate different theoretical channels of monetary transmission.

Using household survey data from the U.S. and the U.K. we find strong heterogeneity in the response of expenditure to interest rates. We show that households with mortgage debt increase their expenditure considerably following a temporary cut in interest rates. In contrast, outright home-owners without debt do not change their spending at all. This heterogeneity exists over and above any heterogeneity coming from demographic factors. Renters also increase their expenditure, although by less than mortgagors.

In the second part of the paper, we examine which the theoretical channels can explain our results. We show that the heterogeneous response of expenditure is not driven by heterogeneous changes in resources across groups. While the fall in mortgage payments is significantly larger in the U.K. than in the U.S., these changes

are too small to account for the magnitude of the rise in mortgagors' expenditure in either country. Rental payments also go up and therefore cannot explain the increase in renters' expenditure. Furthermore, income increases significantly for *all* housing tenure groups, implying that heterogeneity in the response of expenditure is not driven by heterogeneous movements in income. We also show that our results cannot be explained, qualitatively or quantitatively, by differences across households in the elasticity of intertemporal substitution, the revaluation of nominal assets and liabilities via expected inflation (the so-called Fisher effect) and other financial wealth effects. Of course, this does not mean these channels play no role; simply that, *on their own*, they cannot account for the novel aspects of the transmission mechanism that we uncover.

Differences in the composition of household balances sheets, together with a sizable general equilibrium effect on income can, however, explain our findings. We show that the three housing tenure groups differ markedly in their composition of liquid and illiquid assets. Renters have little wealth and, being younger and poorer, fit the typical description of liquidity constrained households. But using wealth survey data, we also show that mortgagors tend to hold little liquidity, despite owning sizable illiquid assets. Indeed, between 40 and 50% of households with mortgage debt live with net liquid wealth below half of their monthly income, consistent with the presence of a significant proportion of 'wealthy' hand-to-mouth consumers (Kaplan and Violante (2014))). In contrast, outright home-owners have ample amounts of liquid and illiquid assets.[1] Finally, for income to rise for all groups, and for this to account for the magnitude of the mortgagors' expenditure response, there needs to be a sizable rise in aggregate demand. Indeed, we show that non-household components of expenditure also increase. In particular, the response of private investment to a monetary policy shock is large and significant, while the response of fiscal policy and net exports appears more limited.

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[1]The notion that debtors are borrowing constrained and exhibit high marginal propensities to consume, can also be found in theoretical contributions such as Eggertsson and Krugman (2012) and Iacoviello (2005).

Our evidence is therefore consistent with *two*, very different, types of liquidity constrained and hand to mouth households. That renters may be liquidity constrained is perhaps unsurprising. But, importantly, we show that mortgagors also have low liquidity despite being well-off. Since mortgagors are a much larger share of the population, these findings have important implications for how monetary policy affects the economy.

To examine the disaggregated effects of monetary policy we need good quality micro data on expenditures and income, together with information on household balance sheets over a long period of time. Few, if any, datasets include this information. Instead, we use rich micro data from the U.K. Living Costs and Food Survey and the U.S. Consumer Expenditure Survey which are extensive and have been run for many decades. The drawback is that they are not longitudinal and, more importantly for our purposes, lack explicit information about wealth. Our methodological innovation is to use a household's housing tenure status — specifically whether they rent, own their home with a mortgage or own without a mortgage — as a predictor of their debt and asset positions, a fact that we document using lower frequency surveys on household wealth. Housing tenure status is then used to aggregate individual households into a group with debt (mortgagors), a group without debt (outright home-owners) and a group of low wealth households (renters). To tackle the potential reverse causality problem between interest rates and our dependent variables, we measure monetary policy shocks using the identification strategy of Romer and Romer (2004) for the U.S. and applied to the U.K. by Cloyne and Huertgen (2016).

Two issues are worth noting before proceeding. First, households are not randomly assigned to be mortgagors, outright owners or renters. Mortgagors may respond more than outright owners not because they have debt, but because some other trait (correlated with having debt) makes them more responsive to interest rate changes. At the very minimum, our balance sheet proxy (i.e. housing tenure) is still a strong predictor of the households most affected by monetary policy. But we show that the heterogeneity we uncover exists over and above any effect from demographics.

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Furthermore, we explicitly use the heterogeneity we uncover to investigate which channels might be driving our results, showing that other household characteristics are inconsistent with our evidence. A second concern is the possibility of endogenous transitions from one tenure status to another over time. In addition to grouping by actual housing tenure, we therefore use a variant of the Attanasio et al. (2002) propensity score approach which explicitly addresses this issue.

Related literature. This papers relates to at least four strands of the literature. First, we provide empirical support for theories that highlight the role of household balance sheet channels in the transmission of economic shocks, including Kaplan et al. (2015), Auclert (2015), Bayer et al. (2015), Ragot (2014), Eggertsson and Krugman (2012), Iacoviello (2005) and Greenwald (2016).

Second, we relate to the large body of evidence on the relationship between the mortgage market, credit and real activity, with Mian et al. (2013), Mian and Sufi (2014), Guerrieri and Iacoviello (2014), Aladangady (2014), Jorda et al. (2014), Baker (2013), Justiniano et al. (2015, 2014) and Cloyne and Surico (2016) being recent examples.

Third, a growing literature has studied how the structure of the mortgage market affects the transmission of monetary policy. Using aggregate data for selected OECD economies, Calza et al. (2013) show that countries with a higher debt to GDP ratio, an ability to extract housing equity and a higher share of adjustable rate mortgages tend to exhibit a larger consumption response on average, although their empirical approach does not identify the marginal contribution of each of these features separately. The theoretical model in Garriga et al. (2013) predicts that under adjustable mortgage rates the change in consumption is only slightly larger than under fixed rates when the monetary policy shock is temporary (as in this paper). The difference can, however, become much larger if the policy shock is very persistent (as in Keys et al. (2015) and Di Maggio et al. (2015)).

Fourth, our findings complement evidence from an increasing number of studies

that explore heterogeneity in the effects of monetary policy including Coibion et al. (2012), who study the effects on inequality, Gornemann et al. (2012), who study the role of earning risk, Sterk and Tenreyro (2015), who highlight a Fisher-type channel in an OLG model with open market operations, and Wong (2015), who shows that the behavior of levered home-owners with a mortgage is important for explaining the aggregate non-durable consumption response to a monetary policy shock after 1993 in the U.S..

While we also share an interest in mortgage debt, repayments and heterogeneity in general, relative to these studies, we focus on a far broader set of household-level variables, including durable expenditure, mortgage and rental payments, gross and net income. We show that considering all these responses together is important for evaluating and disentangling different theoretical channels of monetary transmission.

Structure of the paper. The rest of the paper is structured as follows. Section 2 presents the datasets, discusses the identification of the monetary policy shocks and sets out the empirical specification. The baseline estimates are reported in Section 3, together with two extensions that control for endogenous compositional changes and demographic factors. In Section 4, we assess which theoretical channels are most consistent with all our results. Section 5 briefly discusses what components of demand drive the increase in household income. Section 6 concludes. The Appendix contains some further results and robustness exercises.

2 Data and empirical framework

In this section, we describe our main sources of household survey data and the variables we use. We then lay out the strategy to group individual observations into pseudo-cohorts using housing tenure status to proxy a household's debt and asset position. We also discuss the identification of the monetary policy shocks. Finally, we present our empirical specification.

2.1 Household survey data

In order to investigate how different types of consumers change their expenditure in response to changes in monetary policy, we use household survey data with a rich coverage of expenditure variables. For the U.K. we use the Living Costs and Food Survey (LCFS).[2] For the U.S. we use the Consumer Expenditure Survey (CEX). Both surveys contain detailed information on weekly expenditures on non-durable goods and services, durable goods (excluding housing and rental-related costs) and household income. The latter is defined as labor income (wages and salaries) plus non-labor income (income from investments and social security payments), net of taxes paid by each household. In the appendix, we provide a more detailed description of the variable definitions and the sample restrictions.

In addition, the survey provides information on two additional useful sets of variables (i) demographics, in particular household size and the year of birth of the household head, (ii) mortgage payments for households with outstanding debt and rental payments for renters. The information on birth years will be used to verify that the heterogeneity we uncover across housing tenure groups does not reflect (omitted) differences in life-cycle positions (as opposed to genuine differences in debt positions that are independent of demographics). The information on household size will allow us to conduct the analysis at the per capita level. The mortgage and rental payments data will be used to quantify the extent to which changes in repayments can account for movements in expenditure.

We convert the data into a quarterly time series using the date of interview. The resulting series is then deflated by the Retail Prices Index (excluding mortgage interest payments) for the U.K. and the Consumer Price Index for the U.S. to convert the data into real series.[3] Our sample covers 1975 to 2007 for the U.K. and 1981 to

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[2]This survey was previously known as the Expenditure and Food Survey (EFS) and the Family Expenditure Survey (FES).

[3]Previous research finds evidence consistent with possible trends in the under-reporting of expenditure for more affluent households. This can lead to a divergence between expenditure measures aggregated in the CEX and those from national statistics over time (Aguiar and Bils (2015)). To ensure consistency between the survey data and the national statistics, while allowing for different

2007 for the U.S.. The key variables of interest are available in the FES from the mid-1970s whereas the CEX begins in 1981. Our focus is on the effects of interest rate changes, so we deliberately stop just prior to the financial crisis, excluding the period of unconventional monetary policy.

2.2 Grouping households into pseudo-cohorts

The first empirical challenge we face is that, to our knowledge, no U.S. and U.K. datasets contain disaggregated information on *both* (i) wealth and household balance sheets and (ii) a rich array of expenditure categories at the household level over a sufficiently long period of time. Unfortunately, the LCFS and the CEX are no exceptions but they do record detailed expenditure and income data as well as information on housing tenure status, namely whether a household lives in rented accommodation, is an owner-occupier with a mortgage or owns the property outright without a mortgage.

Housing tenure is a useful proxy for the balance sheet positions of households in both the U.S. and the U.K.. Mortgagors, by definition, have sizable debt but also sizable wealth (although this is tied-up in their house). Outright owners have sizable housing and other financial wealth. In contrast, renters typically have low wealth in general. The housing tenure distinction therefore makes it possible to bypass the lack of household balance sheet information in surveys that feature rich expenditure and income data.[4] Accordingly, and in keeping with the tradition of Browning et al. (1985), we employ a grouping estimator to aggregate individual observations into pseudo-cohorts by housing tenure.

It is worth discussing two potential concerns about grouping households. The first concern is about endogenous changes in group composition. Specifically, a household

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trends in under-reporting (and other characteristics) across groups, we do two things. First, we rescale expenditure for each cohort in the survey data by the ratio of the national statistics series to the corresponding series aggregated from the CEX/LCFS. The adjusted expenditure series for each cohort are reported in Appendix Figure C.2. Second, our cohort-specific regressions feature cohort-specific time trends. These time trends are meant to absorb low-frequency variation in both under-reporting and other characteristics that are specific to each group.

[4]In Section 4.3 we discuss the balance sheet composition of each of these housing tenure groups.

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may change housing tenure status in response to a monetary policy shock. The second concern is about selection. The assignments into the housing tenure groups are not random and some other (unobserved) characteristics may, potentially, be responsible for the heterogeneous responses we find.

In terms of compositional changes, the time series of the tenure shares in Figure 1 are clearly slow-moving. The variation in monetary policy we exploit, however, occurs at a much higher frequency (as can be seen from Figure 2). In Section 3, we provide formal evidence that compositional change is unlikely to be driving our results. Specifically, we show that the monetary policy shocks do not significantly affect the shares of households in each housing tenure group. Furthermore, we show that our findings are not affected by using the propensity score method of Attanasio et al. (2002), which deals with possible endogenous compositional change.

Turning to the issue of selection, three factors seem to make this less serve in our context. First, we explicitly consider demographic and life cycle factors. In Section 3, we divide each housing tenure cohort into further sub-groups using the age of the household-head. We show that the expenditure response of middle-aged mortgagors is still significantly different from the response of middle-aged outright owners, despite the more stable age range. The heterogeneity we uncover based on housing tenure therefore exists over and above any possible heterogeneity purely due to demographic factors.[5] Second, in Section 4, we explicitly consider which other mechanisms might be driving our results and show that alternative mechanisms struggle to explain our empirical findings. The composition of household balance sheets therefore seems an important part of why some households respond strongly to interest rate changes while others do not. Finally, grouping households by housing tenure can be motivated from various theoretical frameworks. For example, the distinction between consumers

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[5]Selected descriptive statistics for all housing tenure groups are reported in Appendix A. There are some small differences across the distributions of per-capita income and across the shares of post-compulsory educational attainment between mortgagors and outright owners. While the age difference between these two groups seems larger, (i) the age distributions for mortgagors and outright owners still overlap significantly and (ii) as already noted, the heterogeneous responses across housing tenure groups are not overturned in the sensitivity analysis of Section 3.3 where we further control for demographics.

with and without mortgage debt fits well with the predictions of a range of theoretical models that would imply heterogeneous expenditure responses to a monetary policy shock, including Iacoviello (2005), Eggertsson and Krugman (2012), Auclert (2015) and Kaplan et al. (2015).

2.3 Identification of monetary policy shocks

Our goal is to examine the effect of monetary policy on the spending and income of different *groups* of households. As such, we face the usual macroeconomic reverse causation problem: the economy responds to movements in monetary policy, but monetary policy also responds to developments in the macroeconomy. To identify unanticipated changes in the short-term interest rate we need a monetary policy shock series that can be used for estimation.

There is a vast literature on the identification of monetary policy changes. Older approaches, mainly developed for the United States, relied on timing restrictions and a Choleski decomposition of the variance-covariance matrix of the residuals from a Vector Autoregression, such as Christiano et al. (1996, 1999). But when applied to the United Kingdom, this method produces a large rise in inflation following a monetary contraction, the so-called price puzzle, even after controlling for variables shown to ameliorate this issue for the U.S. (Cloyne and Huertgen (2016)).

Another popular approach for the U.S. was introduced by Romer and Romer (2004). This method first constructs a measure of the target policy rate (since the effective Federal Funds Rate is moved around by other factors than just policy decisions) and then regresses the change in the target rate around the policy decision on a proxy for the information set available to the policymaker just prior to that decision. This information set includes a range of real time indicators and forecasts to reflect the forward-looking nature of monetary policy. Cloyne and Huertgen (2016) construct a measure for the U.K. employing this methodology and show that it improves on conventional VAR methods. Hence, we use an updated version of the Romer and Romer (2004) shock series for the U.S. (whose original analysis ended in 1996) and

the Cloyne and Huertgen (2016) shock series for the UK.[6] One particular advantage of using shocks based on the Romer and Romer (2004) method is that we have two comparable series across the two countries we study.

The shock series match the micro-data sample periods, which are from 1975 to 2007 for the U.K. and 1981 to 2007 for the U.S.. The shock series deliberately stop before the recent financial crisis, when the policy rate hit the zero bound in both countries.[7] To boost the number of household observations used to generate the pseudo-cohorts at each point in time, we aggregate household survey variables to a quarterly frequency. The original shock series are monthly but, following Romer and Romer (2004) and Coibion (2012), we sum up the monthly innovations to obtain a quarterly series. The construction of the U.K. series also allows for a break in 1993 reflecting the adoption of the inflation targeting framework. The monetary policy shock series for the two countries are shown in Figure 2.

Cohort-specific Granger causality. The shock series we use should already be regarded as monetary innovations from a macroeconomic perspective. But there is still a concern that the monetary policymakers might have been reacting to the conditions in particular groups. While some of this should be captured in the policymakers' forecasts, for example if they were concerned about developments in the housing market, it is useful for our purposes to test whether the U.S. and U.K. shock series can be predicted by movements in cohort level consumption and income. Finding that these shocks are unpredictable on the basis of cohort level concerns would therefore be reassuring. Specifically, we conduct Granger causality tests based on a VAR which contains consumption, expenditure and income per capita for each household group.

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[6]Unfortunately, the length and frequency of the sample we consider prevents us from using a high frequency identification strategy as in Gertler and Karadi (2015).

[7]Results using the Romer and Romer (2004) shocks might be sensitive to the period in the early 1980s when monetary policy was conducted somewhat differently (Coibion (2012)). This is also the period where the share of floating rate mortgages was unusually high in the U.S. Accordingly, in Appendix F, we verify that our findings are not sensitive to starting the sample in 1985. This chimes with independent evidence from Wong (2015), who also provides support for a household balance sheet effect on non-durable consumption across U.S. households using the high-frequency identification in Gertler and Karadi (2015) over a shorter sample starting in 1993.

We cannot reject the hypothesis that the cohort-specific time-series from household survey data (as well as the aggregate time-series from national statistics) do not Granger cause the monetary policy shocks in each country.

2.4 Empirical specification

Using the two monetary policy shock series, our empirical specification closely resembles Romer and Romer (2004). Accordingly, we regress the variable of interest on a distributed lag of the monetary policy shocks. As in Romer and Romer (2004), we also control for the lagged endogenous variable as is common in exercises with relatively short samples. Specifically, we estimate the following equation:

$$X_{i,t} = \alpha_0^i + \alpha_1^i trend + B^i(L)X_{i,t-1} + C^i(L)S_{t-1} + D^i(L)Z_{i,t-1} + u_{i,t} \qquad (1)$$

where $X_{i,t}$ is real non-durable consumption, durable expenditure or income recorded by households interviewed at time t.[8] The monetary policy shocks are denoted by S and Z is a vector of quarterly dummies. The α terms represent intercepts and coefficients on a time trend polynomial, with a break in 1993 for the U.K. and no break for the U.S.. Finally, $i \in Mortgagors, OutrightOwners, Renters$ refers to the relevant housing tenure group. The orders of the lag polynomials are chosen using an optimal lag length criteria, namely the corrected AIC.[9] Standard errors are bootstrapped using a recursive wild bootstrap following Mertens and Ravn (2013).[10]

3 The heterogeneous response of expenditure

This section reports strong heterogeneity across housing tenure groups in the response of expenditure to a monetary policy shock. Mortgagors respond the most, outright

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[8]Households interviewed at time t are typically asked to report expenditure over the previous three months (with the exception of non-durable consumption in the LCFS which refers to the previous two weeks). To eliminate some of the noise inherent in survey data, $X_{i,t}$ is smoothed with a backward-looking (current and previous three quarters) moving average.

[9]We have also explored a more general specification where X is a vector of variables, but with similar results. In addition, we have experimented with including the contemporaneous value of the shock and with assuming different types of trend. In all cases, our results are robust.

[10]The wild bootstrap in Mertens and Ravn (2013) randomly multiplies draws of the residuals by -1 and 1 and is robust to heteroskedasticity.

owners do not change expenditure at all, and the response of renters is closer to that of mortgagors. We first show results based on actual housing tenure status. We then consider a more sophisticated propensity score approach that tackles concerns about endogenous changes in group composition. Finally, we explore the role of demographics by restricting the age range within each housing tenure group. In all cases, we simulate the effects of a *temporary* 25 basis points (bp) cut in the policy rate. The gray areas on the charts show the 90% confidence intervals. For reference, in Appendix Figure C.1, we report the aggregate responses of expenditure to a monetary policy shock using national accounts data. We show that these are consistent with the disaggregated results using household survey micro-data.

3.1 Results based on housing tenure

In this section, we present the results from estimating our benchmark specification (1) using cohort-level data. Figures 3 and 4 present our baseline estimates for the response of non-durable consumption and durable expenditure across housing tenure groups. The left, middle and right-hand columns show the different responses for mortgagors, outright owners and renters respectively.

Beginning with the response of non-durable consumption in Figure 3, the response of mortgagors tends to be larger than the adjustment made by outright owners. Specifically, the response of households with mortgage debt in the U.K. peaks at 0.3% after about 10 quarters but the response of outright owners without debt is never statistically different from zero. The response of non-durable expenditure for renters is similar to that of mortgagors at around 0.2%. This suggests that the behavior of mortgagors and renters drives the aggregate results for non-durables in Appendix Figure C.1. For the U.S. the pattern is similar, with the peak effects for renters and mortgagors between 0.2% and 0.3%.

Clear evidence of heterogeneous effects across groups can also be seen in the response of durable expenditure in Figure 4. The heterogeneity between housing tenure groups is now starker. The response of U.K. mortgagors' expenditure peaks at around

1.2%, but the reaction of outright owners' durables is statistically indistinguishable from zero. The bottom row paints a similar picture for the U.S., with the significant and persistent response of households with debt peaking around 1.2%, and driving the aggregate durable response reported in Appendix Figure C.1. The results for renters' durable expenditures are, however, typically smaller (especially in the U.K.) and less precisely estimated than for the other cohorts, possibly reflecting the heterogeneous composition of the renter group. Still, the durable expenditure of renters tends to increase, especially in the U.S.

Given that there are differences in the average levels of expenditure and income across groups, it is useful to consider what these IRFs imply in dollar terms. This will also be useful later when we quantify the contribution of different transmission mechanisms. In Table 1, we convert the IRFs into an equivalent dollar change in expenditure over the four year forecast period.[11] This can be seen as the overall dollar adjustment in the short-run as a result of the change in monetary policy.[12] The overall effects on non-durable and durable expenditure in the two countries are shown in the first two columns of Table 1. The other columns will be discussed in Section 4.

Table 1 shows that the total dollar change in mortgagors' expenditure (non-durables and durables) is very similar across the two countries, with U.K. mortgagors spending around 70 dollars (about 10%) more than those in the U.S. following a cut in interest rates of the same magnitude. The dollar changes for the other two housing tenure groups are also similar across countries and spending categories, with the possible exception of the response of durable expenditure for renters. Overall, mortgagors display the largest and most significant overall dollar adjustment whereas outright owners experience a small and insignificant change.[13] This difference is not

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[11]More detail on the calculation can be found in the notes to Table 1

[12]The response of the policy rate to the monetary policy shock tends to be slightly more persistent in the U.S. than in the U.K., as can be seen in Coibion (2012) and Cloyne and Huertgen (2016). To make the magnitudes comparable, we rescale the U.K. numbers by the ratio of the cumulated response of the U.S. Federal Funds Rate and the cumulated response of the U.K. Bank Rate. This is like rescaling by the relative movement in the long-rate.

[13]The absolute magnitudes of the numbers in Table 1 may seem small but we are considering a

only economically meaningful but also statistically significant.

In summary, in response to a cut in interest rates, the response of expenditure tends to be large and significant for mortgagors but small and insignificant for outright owners, with the heterogeneity being more pronounced for durable goods. The response of renters tends to be closer to the response of mortgagors than outright owners, especially for non-durable goods and services.

3.2 A propensity score approach

To interpret our estimates as the causal effect of monetary policy on the expenditure of mortgagors, we need that the policy change does not cause households to move from one housing tenure status to another. Note that this is likely to be more problematic, if anything, for the U.K. survey data which consist of repeated cross-sections, than for the U.S. survey data where, given the short panel dimension, we already consider only those households who have not changed housing tenure status between interviews. The risk of group transitions seems limited given the slow-moving housing tenure shares that we report in Figure 1 and the small (25bps) monetary policy change we consider. But, in this section, we assess the empirical relevance of possible changes in group compositional.

One simple way to tackle this issue is to look at the response of the housing tenure group shares to an unanticipated cut in interest rates. In Appendix Figure E.1, we show that none of the group shares respond significantly, indicating that changes in monetary policy do not seem to trigger significant endogenous changes in the housing tenure status.[14]

A more formal approach is to apply the Attanasio et al. (2002) propensity score method. Rather than grouping households based on actual housing tenure, this ap-

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small and *temporary* change in interest rates (i.e. a change of 25 basis points on impact, which then returns to zero after about six to eight quarters). The size of our monetary policy shock is therefore about twelve times smaller and more than six times less persistent than the shock in Keys et al. (2015) and Di Maggio et al. (2015).

[14]While it may be theoretically possible that the inflows into one group might be offset by its outflows, it would seem difficult to think that *at the same time*, for example, some renters become mortgagors and other households with debt become renters following a monetary policy shock.

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proach groups households based on the probability of being a mortgagor using fully predictable household characteristics. Specifically, we run a probit regression over the full sample to generate individual predicted probabilities of having a mortgage based on a high order polynomial in age, education, a time trend and their interactions.[15] For households observed in quarter t, we compute the probability that they had a mortgage in the previous quarter. For these two periods, we classify households as 'likely' or 'unlikely mortgagors' if the probability in the first of the two periods is larger or smaller than the share of mortgagors in the sample.[16] We then take the growth in consumption across these two quarters for each group, from which we can construct the implied consumption series for each of the groups that we then use for estimation.

The results of this exercise are shown in Figures 5 and 6. As can be seen, the main findings of our earlier analysis are not overturned. The likely mortgagor group still exhibits stronger expenditure responses than the unlikely mortgagors, with the difference being particularly pronounced for durables and in line with the results on actual housing tenure. Changes in group composition are therefore unlikely to drive the heterogeneous expenditure responses we found above.

3.3 Controlling for demographics

One possible concern is that the housing tenure distinction may simply be picking up (omitted) life-cycle effects. Although we can include demographic controls in our regressions, given the grouping strategy, this does not condition on demographics in the usual sense. Conditioning requires further splitting the groups by demographics. To explore this possibility, we follow the micro-econometric literature and construct

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[15] To maximize the number of households in each quarter and cohort, we place no restrictions on the birth year of the household head in this exercise but include age among the demographic variables in the probit regressions. To sharpen the comparison we do not include renters in the analysis in this section. While these restrictions do not affect the point estimates from the propensity score method significantly, they improve their accuracy.

[16] As time variation in the probability of being a mortgagor may induce changes in the group composition, we use a constant threshold for determining the group of 'likely' mortgagors.

birth cohorts.[17] We regard households as 'older' if the head was born before 1935, as 'middle-aged' if the head was born in the interval [1935, 1949] and as 'younger' if the head was born after 1949. This strategy therefore produces groups with a more stable age range than our baseline grouping strategy. For example, while the maximum possible age gap within the middle-aged group at each point in time is 14 years, it is important to note that the median age gap is around 6 years.

Before reporting the results of this exercise, it is useful to consider the demographic composition of the housing tenure groups. As shown in Appendix Figure B.2, a greater share of mortgagors are younger and there is a prevalence of outright home-owners without mortgage debt among the older households. But, importantly, not all younger households are mortgagors and not all older households are outright home-owners. Furthermore, the middle-aged cohort is populated by even shares of all housing tenure groups.

Within each housing tenure group, we sub-divide households into birth cohorts. We then consider two separate experiments to explore whether age/life-cycle consid-erations could be driving our heterogeneous responses. First, we investigate whether the response of middle-aged mortgagors is similar to the response of middle-aged outright owners.[18] Second, we examine whether excluding households with a retired head makes any difference to our results.

Comparing the first and the second columns in Figures 7 and 8 reveals that the ex-penditure responses for middle-aged mortgagors are large and significant, while those for middle-aged outright owners are small and statistically indistinguishable from zero. This is true both in terms of the magnitude and in terms of the significance of the point estimates (both for non-durable consumption and durable expenditure). Consistent with the results in the previous section, middle-aged renters generally

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[17]The objective in a pseudo-panel is to construct a group with relatively stable composition over time. Directly grouping by age does not satisfy this requirement as we would be constructing a representative household who never ages.

[18]Unfortunately, there are neither enough mortgagors in the older birth cohort nor enough outright owners in the younger birth cohort for us to look at these two other sub-groups. Furthermore, we find little heterogeneity within the renter group.

respond less than mortgagors but significantly more than outright owners. The heterogeneity across housing tenure groups therefore exists even for cohort groups with a more stable age range. In the Appendix Figures D.1 and D.2, we also consider restricted samples from the LCFS and the CEX where we exclude households with a retired head. The results are very similar to our baseline findings above.

In summary, are results in the previous section are not overturned when considering the impact of demographics. In particular, the heterogeneous responses associated with housing tenures status appear to hold over and above any possible heterogeneity associated with age or birth cohort.

4 Inspecting the transmission mechanism

In the previous section, we showed significant heterogeneity in the response of expenditure to monetary policy shocks across housing tenure groups. Our evidence is also not driven by heterogeneity in demographics. Furthermore, in Table 2, we showed that the balance sheets of the three housing tenure groups differ markedly in their composition of liquid and illiquid wealth.

In this section, we ask what mechanism(s) may be responsible for the heterogeneous effects on expenditure across groups. To do so, we consider a rich set of household-specific variables in the micro-data as well as other, relevant, aggregate variables in the national statistics. One of our contributions is to show that our disaggregated evidence can be used to evaluate the predictions of different theoretical mechanisms, many of which would be observationally equivalent using aggregate data.

There are a number of channels that might account for our findings. For instance, the heterogeneous response of expenditure may simply reflect heterogeneity in windfalls — either because mortgage and rental payments fall, or because monetary expansions increase earnings only for mortgagors and renters. An alternative hypothesis is that differences in expenditure might reflect differences in liquidity holdings: some households may exhibit larger marginal propensities to consume (MPC)

if they face costs accessing liquidity. In addition, a higher sensitivity of consumption to interest rates for some groups could reflect a higher elasticity of intertemporal substitution. Finally, the effects may represent wealth redistribution across households from movements in goods and asset prices.

To preview our results, while these mechanisms are likely to play some role in the transmission mechanism in general, most of them cannot explain the heterogeneity we find in the data. Our evidence, however, is consistent with heterogeneity in liquidity holdings across households, together with a sizable general equilibrium effect on income. In particular, we show that our findings are consistent with *two*, very different, types of hand-to-mouth households: low wealth renters and wealthy mortgagors.

4.1 Mortgage and rental payments

A fall in interest rates can affect the resources available to households by affecting interest payments and rents. These windfalls from lower interest or rental payments might then be spent on durables and non-durables in a way that could potentially rationalize the expenditure heterogeneity we documented in the previous section.[19] In our survey data, we observe both variables and we therefore explore how mortgage and rental payments react to monetary policy changes.

In Figure 10, we report the percentage change in mortgage payments (top row) and rents following a temporary interest rate cut of 25 basis points. Mortgage payments fall significantly in both countries but the effect is considerably faster and larger in the U.K., peaking at around 0.7% versus 0.4% for the U.S.

To compare these effects to the change in expenditure, it is useful to convert the IRFs into a 'windfall' in dollars. This is shown in the third column of Table 1. A few points are worth noting. First, the dollar benefit of this change for the average mortgagor is nearly three times larger in the U.K. than in the U.S. Interestingly, the absolute difference between the two point estimates is also consistent with the differ-

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[19]Note that financial income is already included in our measure of household income. Any cash-flow effect on savers is therefore already captured in our income responses. Since outright owners do not alter their expenditure, this section focuses on cash flows for mortgagors and renters.

ence in total expenditure for mortgagors in the two countries (columns 1 and 2).[20] The relative size and speed of the adjustment in mortgage payments is intuitive given the historic prevalence of adjustable rate mortgages in the U.K. market (Besley et al. (2013)). Finally, and most importantly, the dollar fall associated with lower mortgage payments is considerably smaller than the dollar increase in total expenditure — the expenditure changes are four and nine times larger than the changes in mortgage payments in the U.K. and the U.S. respectively. This suggests that the windfall from lower mortgages payments is quantitatively too small to account for the magnitude of the rise in the expenditure of mortgagors.

The response of rental payments tells a similar story. On the one hand, a monetary expansion lowers the user cost of housing. On the other hand, a fall in interest rates could lead to a rise in house prices — as we document in Appendix Figure G — and encourage the demand for renting relative to buying. The bottom row of Figure 10 shows that, in the data, the latter effect dominates and rental payments increase. Renters' still increase their expenditure on non-durable and durable goods, however.

In summary, mortgage payments move significantly more in the U.K. than in the U.S., but the magnitudes are much smaller than the overall dollar increase in expenditure. Furthermore, movements in rental payments do not free up extra resources because they actually increase following a monetary expansion. These cash flow effects are, *on their own*, unlikely to explain why mortgagors respond differently from outright owners. They also cannot explain why renters respond differently from other households without mortgage debt. Interestingly, our findings are consistent with the theoretical predictions in Garriga et al. (2013), who show that the distinction between adjustable and fixed rate mortgages implies a small difference in the

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[20]The size of the dollar change in the average mortgage payments is consistent with a back-of-the-envelope calculation using an *effective* mortgage duration of ten years, an *effective* loan to value ratio on outstanding debt of 0.5 and, for the U.K., the average house value from the Land Registry since 1995 (and from Halifax before then) as well as a share of mortgages on adjustable rates of 45%. This yields an average change in U.K. mortgage payments of 168 US dollars. Replacing the UK share of mortgage contracts on adjustable rates with a share of 15% for the U.S., we obtain a value of 56 dollars. Note that our results are an average of the effect on loan rates for newly originated mortgages and the effect on existing adjustable and fixed rate loans.

consumption responses to a *temporary* monetary policy shock (as we consider here) but a large difference to a *very persistent* monetary shock (as considered in empirical work by Keys et al. (2015) and Di Maggio et al. (2015)).

4.2 The response of income

Household expenditure may respond directly to interest rate changes, for example through substitution, income or wealth effects. But the overall response of expenditure may also be affected by the general equilibrium effects monetary policy has on GDP and household income.[21] If the income of some groups is more sensitive to macroeconomic conditions, then the heterogeneity in resource windfalls associated with a monetary expansion may be the driver of heterogeneity in the expenditure responses across groups (Gornemann et al. (2012)).

We examine this mechanism by looking at the response of household income, both gross and net of taxes. As noted earlier, our survey data include labor and non-labor income (which includes, for example, financial returns for asset holders). In Figure 9, we report the confidence bands and point estimates for the response of income net of taxes. We also report the point estimates for gross income.[22]

Income net of taxes responds significantly for all three housing tenure groups and increases by a similar proportion across countries and cohorts. The peak effects are typically between 0.3 and 0.4%. It is useful to note that, despite the expenditure heterogeneity across groups, there is limited heterogeneity in the IRFs for income. This is consistent with the notion that the increase in household resources triggered by the monetary policy shock is likely driven by a general increase in aggregate demand. In other words, expenditure heterogeneity does not simply reflect earnings heterogeneity. Furthermore, converting these percentage changes into dollar amounts reveals that, in both countries, the dollar increase in mortgagors' income is of the

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[21]This includes any multiplier effects where the rise in consumption boosts GDP, feeding back into consumption and so forth.

[22]We only report the point estimates for gross income purely for presentational convenience. The confidence bands for this measure largely overlap with those of net income.

same order of magnitude as the dollar change in mortgagors' total expenditure (final column of Table 1).

Disposable income may also be affected by changes in taxes if fiscal policy responds to the fall in interest rates. This could be the case if, for instance, lower interest payments on government debt encourage tax cuts. But Figure 9 shows that the movements in gross and net income are very similar, with the difference being insignificant. The majority of the movement in net income is therefore driven by the response of gross income and not by movements in taxes.[23]

The change in household *resources* following an interest rate cut is driven by changes in household income. But the increase in aggregate income is shared across all housing tenure groups, consistent with a general equilibrium effect of monetary policy on factor demand. Heterogeneity in the response of income therefore does not explain our results for expenditure. This means that consumption responds strongly for mortgagors and renters, even relative to the change in resources for these groups.

4.3 Liquidity shortages for the poor and the 'wealthy'

In the previous sections, we have shown that income increases for all groups but expenditure increases only for renters and mortgagors. Furthermore, the dollar change in expenditure for these two groups is of a similar order of magnitude to the dollar change in their income (Table 1). While renters would seem to fit the traditional characterization of hand-to-mouth households, the strong sensitivity of consumption to income for mortgagors is hard to reconcile with traditional one-asset models. A number of recent theories, however, emphasize the role of debtors and balance sheets in the transmission of monetary policy. For example, in Eggertsson and Krugman (2012) the presence of debt constraints facing borrowers generate heterogeneous MPCs, a mechanism that can vary over time if (as in Iacoviello (2005)) the credit limit is tied to the value of the asset used as collateral.[24] In Auclert (2015), incomplete markets

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[23]Gross income responds slightly less than net income, consistent with Mountford and Uhlig (2009) who show that a monetary expansion is associated with a fall in net taxes receipts.

[24]Cloyne et al. (2015) present a model where this type of balance sheet constraint generates theoretical responses consistent with the heterogeneity in this paper.

and debt limits imply that borrowers with unhedged positions (such as mortgagors) exhibit a high MPC following changes in monetary policy. In Kaplan et al. (2014, 2015), consumers can be wealthy but still behave in a constrained manner if most of their wealth is tied-up in an illiquid asset (such as housing).

Prima facie evidence that both renters and mortgagors may face liquidity shortages is provided by Table 2. At the one end of the spectrum, renters have little 'cash on hand' and no housing wealth, consistent with the notion of poor hand to mouth consumers. At the other end, outright owners have sizable financial and housing wealth and seem unconstrained. In-between, mortgagors, in both countries, have little liquid assets despite having significant housing equity. As such, they appear to fit well the definition of 'wealthy' hand-to-mouth (WHTM) households put forward by Kaplan and Violante (2014).

To explore this hypothesis in more detail, we draw on independent evidence on household wealth from the British Household Panel Survey (BHPS) and the U.S. Survey of Consumer Finance (SCF) for the (multi-year) waves that correspond to our baseline samples.[25] Following Kaplan and Violante (2014), we define a household as 'wealthy' hand-to-mouth if at any given point in time both (i) their net illiquid wealth is positive and (ii) their net liquid wealth is less than half of their total monthly labor income.[26] In Figure 11, we report the share of 'wealthy' hand-to-mouth mortgagors

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[25]These surveys do not contain wealth information at a sufficiently high frequency to be used for our main analysis and they lack rich consumption data over a long period of time. While there are more SCF waves than reported here, there are only three waves of the BHPS over our sample period (but we have confirmed that similar results emerge from the SCF waves that we have not reported).

[26]When constructing the relevant household income and wealth measures, we select variables to make the concepts of net liquid and illiquid wealth as consistent as possible across the two datasets. The BHPS only reports quantities for overall investment and debts whereas for specific assets it only records whether these are held or not. The SCF, in contrast, does provide quantities for particular assets and overall investment. Furthermore, the assets on which information is provided differ slightly between surveys. Accordingly, net liquid wealth in the U.K. is constructed as total amount of liquid savings and investments (National Savings Bank Accounts and Cash ISAs or TESSAs, Premium Bonds, Stocks and shares ISAs or PEPs) minus non-mortgage debt (Hire purchase agreements, Personal Loans, Credit and store cards, DWP Social Fund loans). Following Kaplan and Violante (2014), net liquid wealth in the U.S. is the value of checking, saving and MM accounts, directly held mutual funds, stocks, bonds and t-bills, net of outstanding unsecured debt. Net illiquid wealth in the U.K. is measured using a binary variable which takes value one if housing equity> 0 or the household has positive investments in (relatively) illiquid instruments such as National Saving

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for pairs of temporally close waves in the BHPS and SCF. The message from this chart is that between 40% and 50% of households with debt hold a value of liquid assets below half of their monthly income and therefore are likely to face liquidity shortages. In the second panel of Figure 11, we also show that most WHTM agents in the sample do have a mortgage.

In summary, renters have low wealth and fit the typical description of liquidity constrained households. Their strong consumption response may not, therefore, be surprising. But, importantly, a significant share of mortgagors have low liquidity despite having high wealth. Mortgagors may therefore become hand to mouth by purchasing a large housing asset with debt. These balance sheet characteristics, coupled with the significant increase in after-tax income for all groups, suggest that heterogeneity in liquid asset holdings is fully consistent with the heterogeneity in expenditure documented earlier.

4.4 Intertemporal substitution

Differences in the elasticity of intertemporal substitution (EIS) may also explain heterogeneity in the expenditure responses across housing tenure groups. There is a large literature estimating the EIS across households and several papers have argued that the EIS is likely to be increasing with wealth (see Guvenen (2006) and Attanasio and Weber (2010) for an overview). Blundell et al. (1994) report evidence that the EIS is larger for more affluent households and that wealth is a more important driver of EIS heterogeneity than demographics. More specifically, Attanasio et al. (2002) estimate that the EIS is around one for stock-holders — who are wealthier households — but only between 0.1 and 0.2 for households who do not participate in financial markets.

As shown in Table 2, outright owners tend to be richer than renters and have more financial assets than mortgagors. They also tend to be well-off households

Certificates, NS/BS insurance bonds, private pensions, non-regular savings. Net illiquid wealth in the U.S. is the value of housing equity (housing value - mortgage debt) plus pension/retirement funds, life insurance, saving bonds and certificate of deposits.

in general. Based on the empirical evidence above, one would therefore expect the outright owners to have a high EIS. But to explain our results, the outright owners would need to have a very low sensitivity of consumption to interest rates and thus a *very low* EIS. Similarly, mortgagors and renters would need to have a considerably higher EIS to be, even qualitatively, consistent with our results.[27] Since mortgagors do not, on average, have sizable net financial wealth (Table 2) and renters tend to be poorer, we conclude that heterogeneity in the intertemporal elasticity of substitution is unable to explain the heterogeneity in expenditure we find.

4.5   Fisher and other wealth effects

By raising inflation, expansionary monetary policy may revalue both sides of the household balance sheet, leading to a redistribution of wealth from net savers to net borrowers (see, for example, the large and permanent change in inflation considered in Doepke and Schneider (2006)). As shown in Table 2, our housing tenure grouping identifies mortgagors as net borrowers and outright owners as net savers.

There are at least two reasons why a Fisher effect does not easily explain the heterogeneous response of expenditure to small and temporary monetary policy shocks that we find. First, if this channel was driving the dynamics of expenditure, then the impulse responses for expected inflation (and inflation) would look broadly similar to the impulse responses for expenditure. Consistent with the wider macro literature (e.g. Romer and Romer (2004) and Christiano et al. (1999)), however, Figure 12 shows that the effect of an interest rate cut on inflation and one-year-ahead expected inflation build over time, with small and insignificant responses in the near-term and more significant effects only after the first seven quarters. In contrast, the adjustment in durable expenditure is already large and significant within the first two years for both countries. Second, if a Fisher effect is driving our results, we would expect the spending responses to be of a similar magnitude but with opposite signs for borrowers and savers (i.e. mortgagors and outright home-owners in our sample). Durable

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[27]Best et al. (2015) estimate a low EIS for mortgagors in the United Kingdom.

expenditure, however, moves in the same direction across all groups and, although outright owners' non-durable expenditure does fall slightly, the non-durable responses are never statistically different from zero.

Finally, a fall in interest rates is also likely to raise the price of financial assets, including housing, and this may lead to additional wealth effects. It seems unlikely, however, that these effects alone could explain the heterogeneity in expenditure we find. First, we consider a small (25 bps) and short-lived fall in the interest rate which is unwound within two years. Furthermore, any effect on asset prices should be more beneficial for those with a larger amount of net financial assets. Since outright owners — the group holding the largest amount of net financial assets — do not adjust their expenditure at all, it is unlikely that these pure wealth effects could be, on their own, the main driver of our results.

5   What is behind the movement in income?

In the previous sections, we have shown that heterogeneity in the access to liquidity combined with a sizable general equilibrium effect of monetary policy on income can account for the evidence presented so far. Furthermore, in Table 1 we have also shown that the dollar changes in income are not only significant for all groups but are also larger than the dollar changes in expenditure. This implies that the overall increase in labor and capital income across the three housing tenure groups is larger than the overall household expenditure increase in the economy. In other words, the change in interest rates must trigger a response in the other components of aggregate expenditure. In this section, we therefore explore the response of government spending, private investment and net exports using aggregate data.

In the left column of Figure 13, we do not find evidence that government spending reacts to a monetary policy shock. In contrast, the middle column reveals that investment does respond strongly and significantly, which is consistent with the evidence in several empirical macro studies (e.g. Christiano et al. (1999)). This is important because it reveals that the extra demand driving the increase in household income

comes from the private sector rather than from the government.

The extra demand also does not come from the rest of the world. In theory, the effect of monetary policy on net exports could be ambiguous, depending on whether the expenditure switching effect (from the exchange rate depreciation) increases exports *by more* than the rise in imports coming from increased household expenditure on both foreign and domestic goods. In the right column of Figure 13, we find that net exports fall in both countries, with the adjustment being slightly larger for the United States. While this is consistent with the U.K. being a more open economy, it suggests that the income effect (on expenditure) dominates the expenditure switching effect.

In summary, the boost to household income following a relaxation of monetary policy reflects increased demand from the domestic private sector rather than expenditure by the government or the rest of the world.

6   Conclusions

Does household indebtedness matter for the transmission of monetary policy? And if so, why? Our analysis suggests that it does matter: the reason is that households with mortgage debt tend to hold little liquid wealth relative to their income and therefore exhibit hand-to-mouth behavior despite owning sizable illiquid assets. Outright owners, in contrast, hold sufficient liquid assets and therefore hardly change their spending at all. Renters — whose financial circumstances are closer to the traditional characterization of liquidity constrained households in one-asset models — also exhibit behavior consistent with a high marginal propensity to consume. We reach these conclusions by performing a detailed and extensive evaluation of the effects of temporary interest rate changes using survey micro-data for the U.S. and U.K., grouping households by their housing tenure status to elicit their debt and asset positions.

Our novel stylized facts are some of the first comprehensive results on how monetary policy affects expenditure and disposable income across households. Further-

more, our findings can shed light on the specific channels through which interest rates affect consumption. A range of theoretical mechanisms, such as heterogeneity in the elasticity of intertemporal substitution, heterogeneous wealth effects and movements in mortgage and rental payments struggle to explain, on their own, the heterogeneous expenditure responses we find in the data. In contrast, limited access to liquidity for two very different types of households, indebted mortgagors and renters, combined with a general increase in income for all housing tenure groups, is able to account for our evidence. Given that households with mortgage debt are the largest group in the population, our results provide a novel mechanism through which monetary policy affects the economy.

We find that the overall dollar change in income is significantly larger than the response of consumer spending for all housing tenure groups. Other components of aggregate demand must therefore respond to monetary policy. Our evidence shows that private investment plays a key role in delivering the extra resources for households to spend. The results in this paper highlight that differences in the composition of household balance sheets represent an important amplification mechanism for the transmission of monetary policy. Investigating whether a similar channel exists for the corporate sector is the next step in our research agenda.
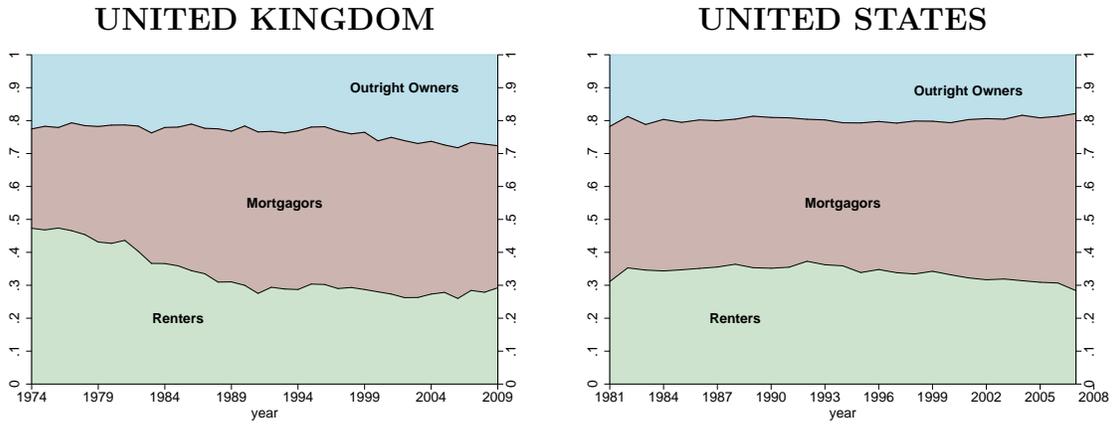
Figure 1: Share of mortgagors, outright home owners and renters in the U.K. (source: FES/LCFS, 1975-2009) and the U.S. (source: CEX, 1981-2009).
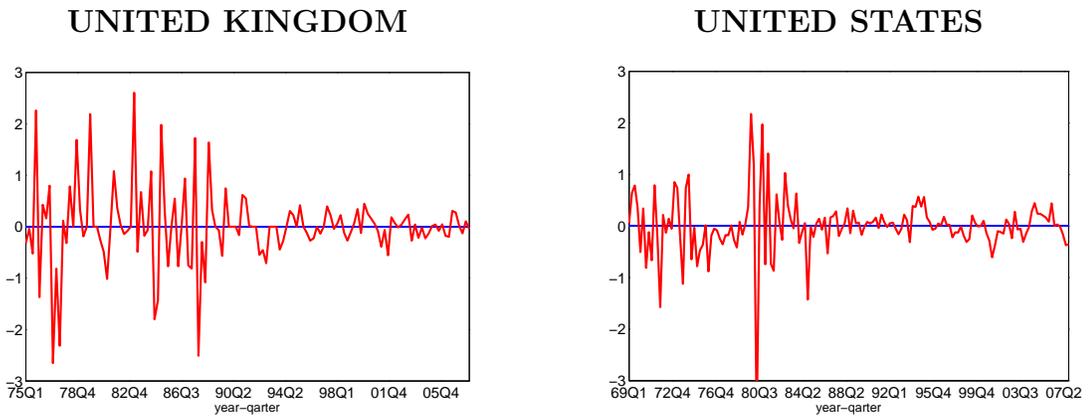
Figure 2: Monetary policy shocks series. United Kingdom: Cloyne and Huertgen (2016); United States: updated version of Romer and Romer (2004).
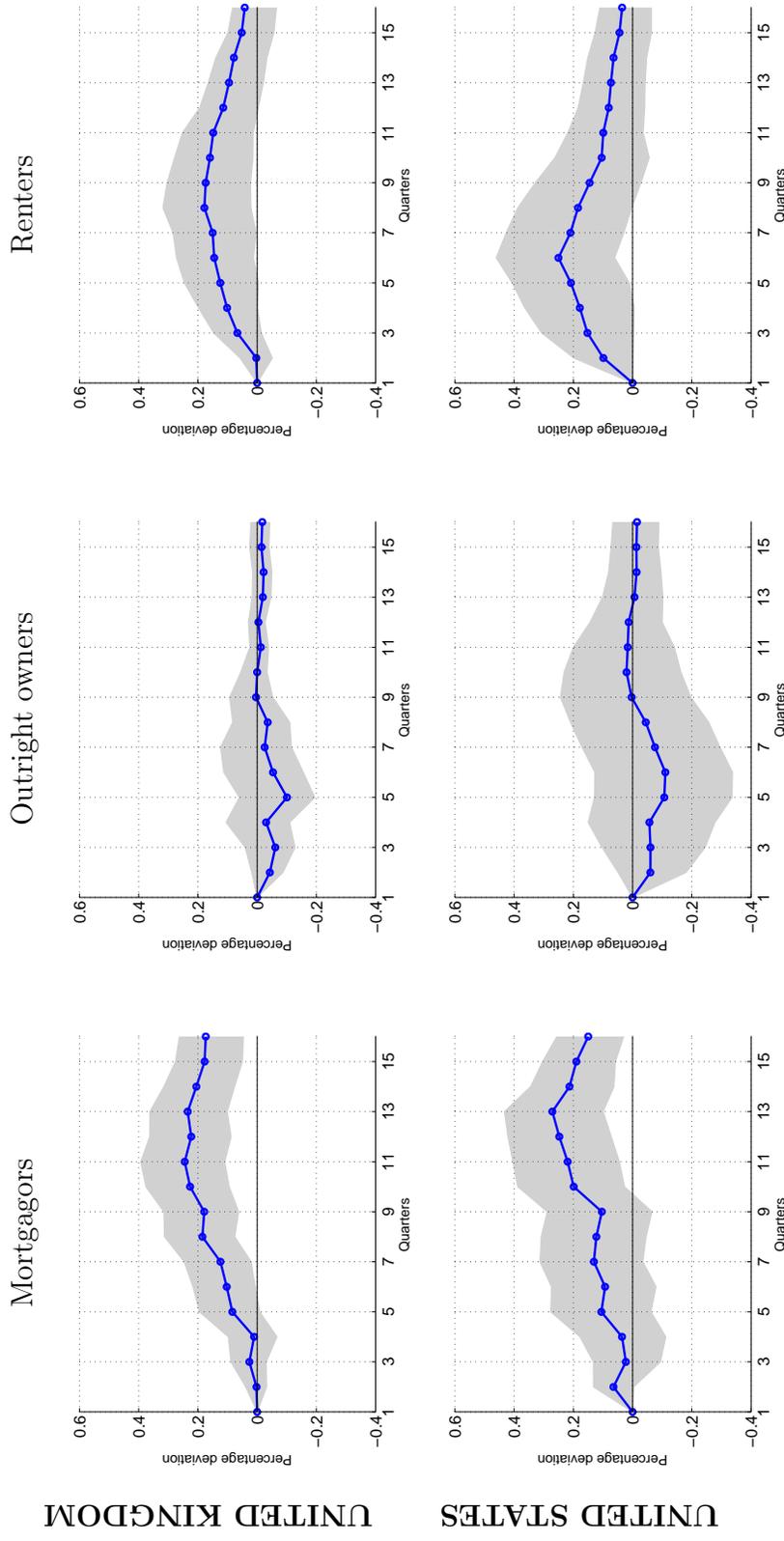
Figure 3: Dynamic effects of a 25 basis point unanticipated interest rate cut on the consumption of non-durable goods and services by housing tenure group. Grey areas are bootstrapped 90% confidence bands. Top row: UK (FES/LCFS data). Bottom row: US (CEX data).
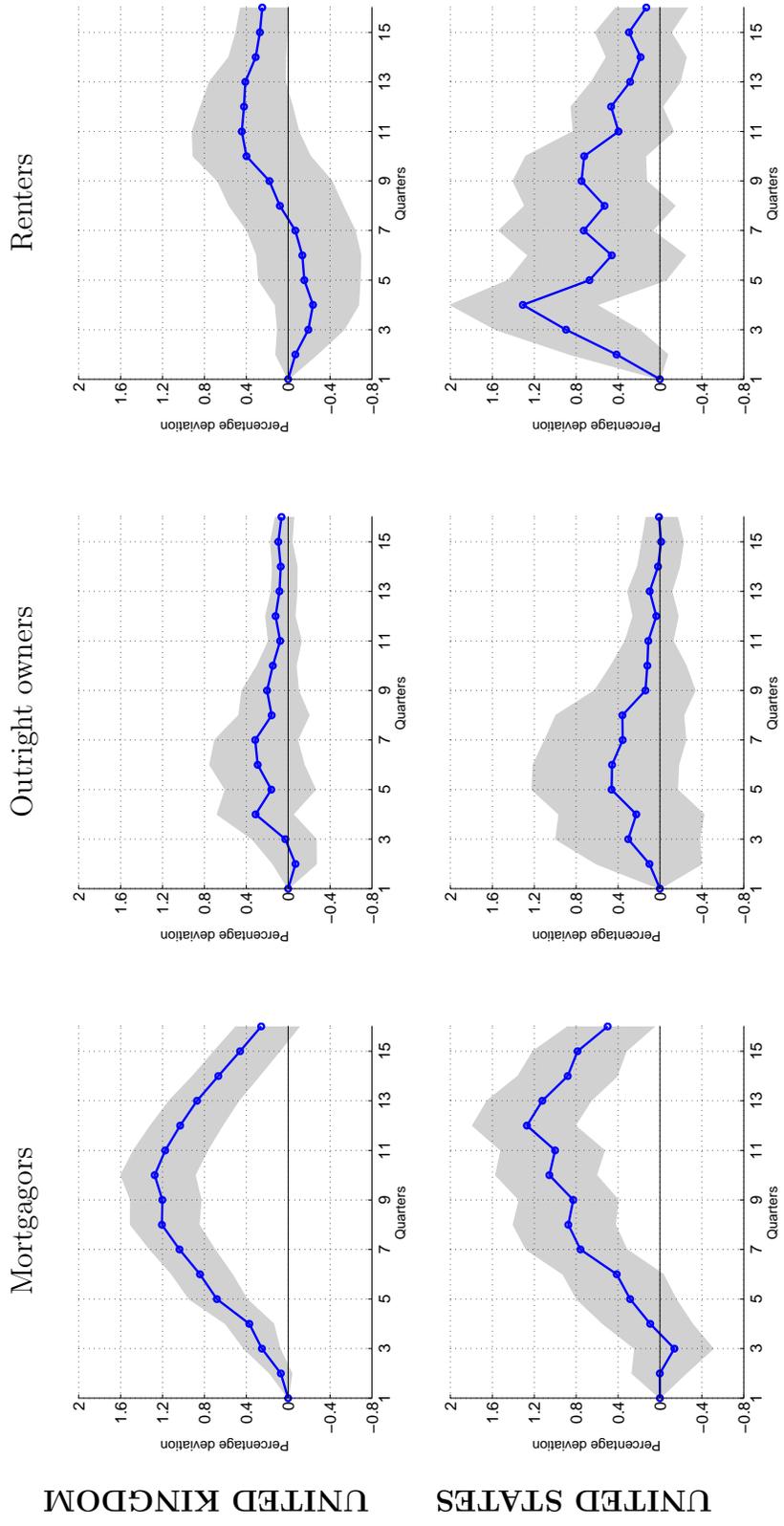
Figure 4: Dynamic effects of a 25 basis point unanticipated interest rate cut on the expenditure of durable goods by housing tenure group. Grey areas are bootstrapped 90% confidence bands. Top row: UK (FES/LCFS data). Bottom row: US (CEX data).
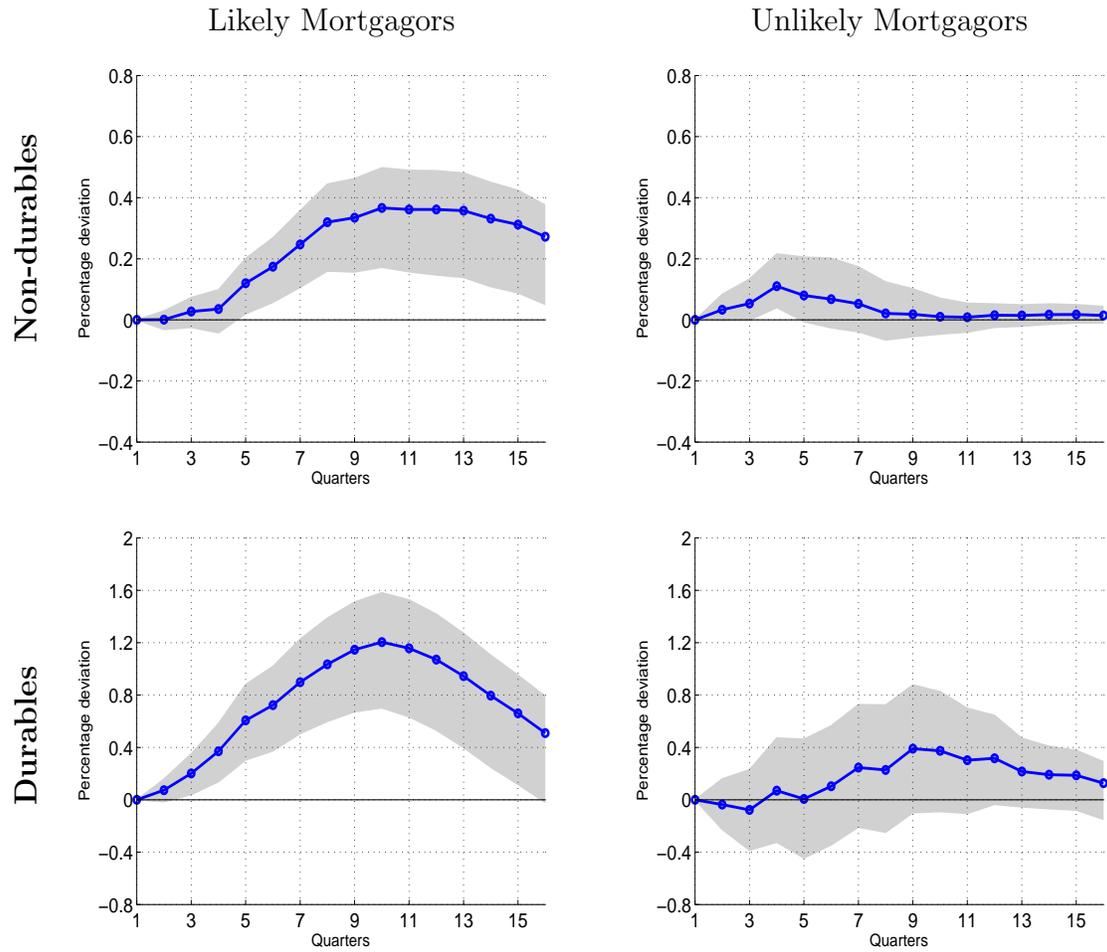
Figure 5: UK results for non-durable, durable and income net of taxes for "likely" and "unlikely" mortgagors, excluding renters. Groups computed following Attanasio et al (2002) propensity score approach using a fixed probability threshold. Grey areas are bootstrapped 90% confidence bands.
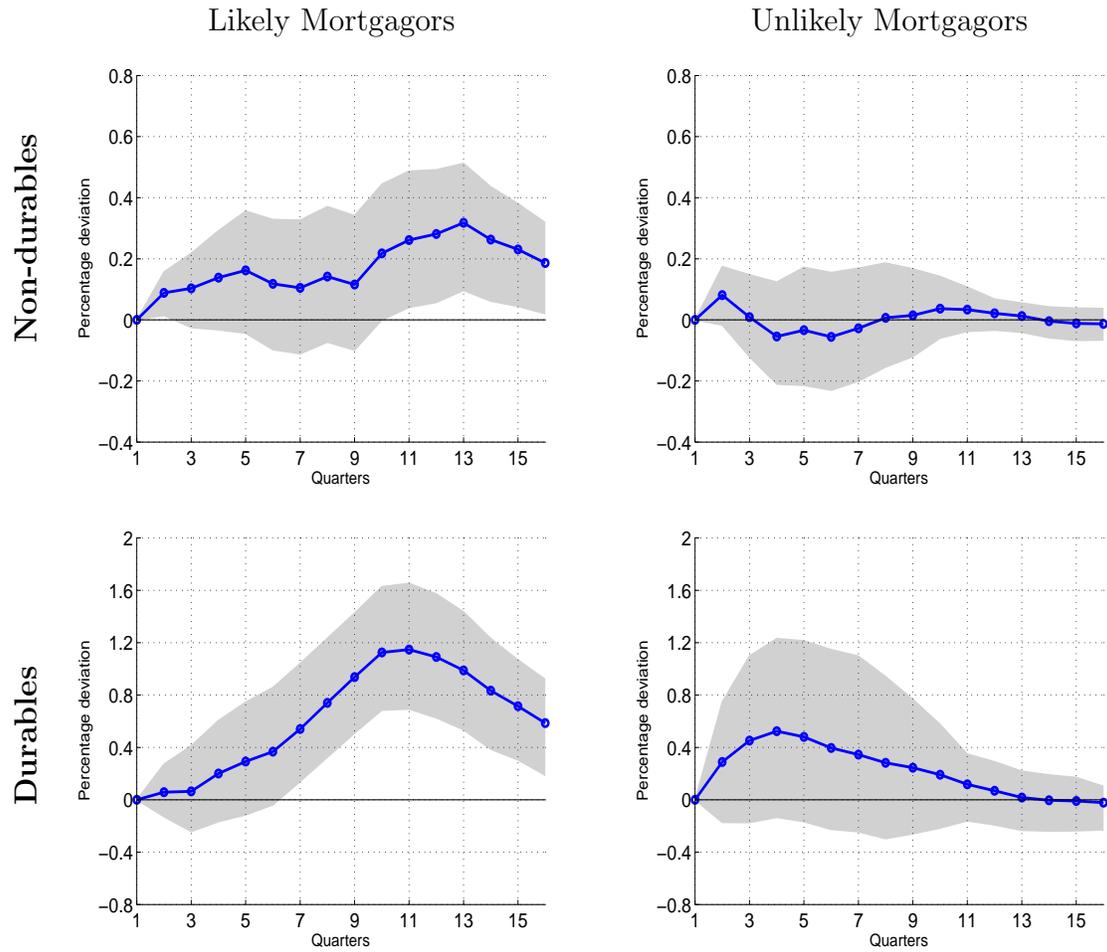
Figure 6: US results for non-durable, durable and income net of taxes for "likely" and "unlikely" mortgagors, excluding renters. Groups computed following Attanasio et al (2002) propensity score approach using a fixed probability threshold. Grey areas are bootstrapped 90% confidence bands.
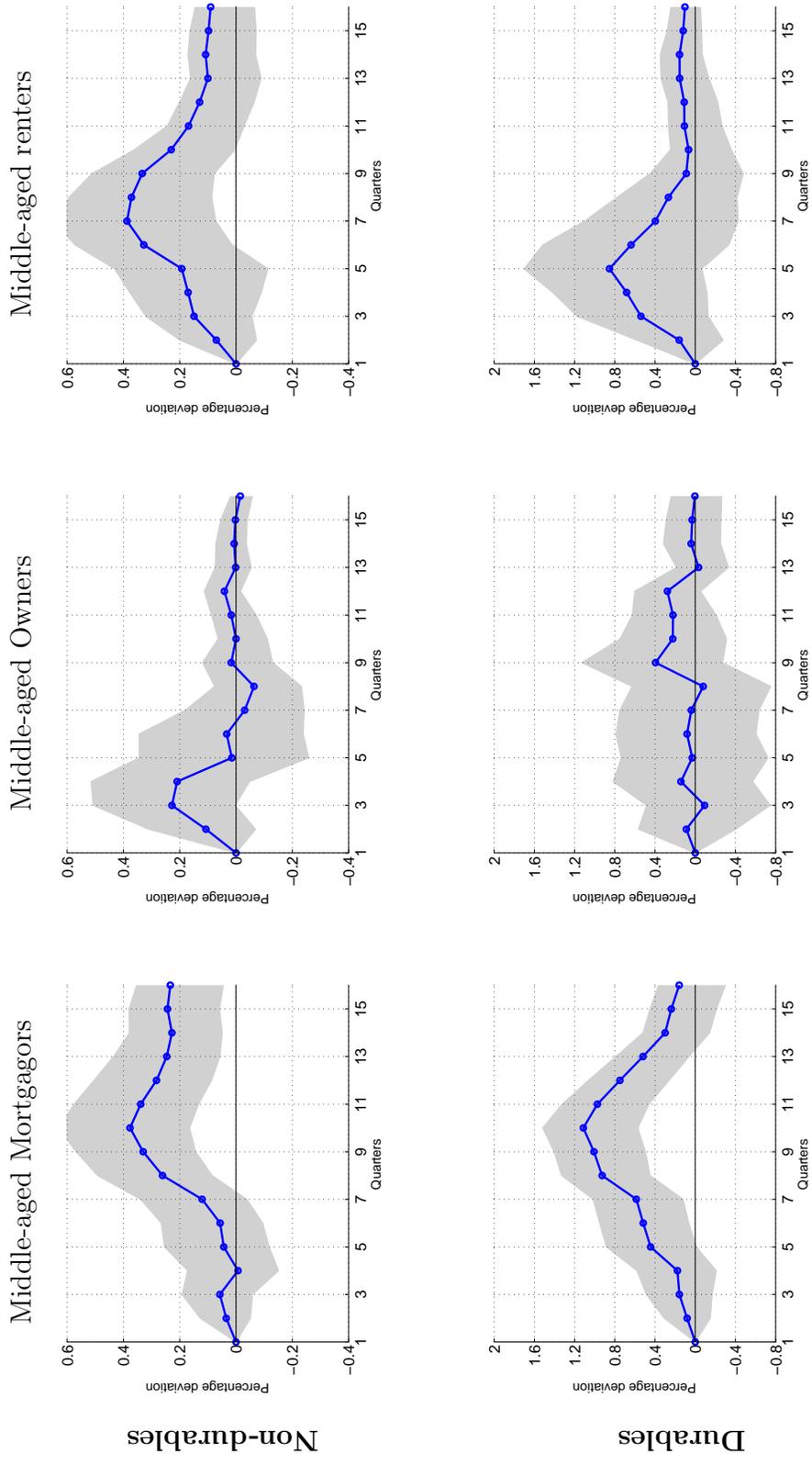
Figure 7: Dynamic effects of a 25 basis point unanticipated interest rate cut on non-durable consumption (top) and durable expenditure (bottom) for mortgagors born after 1949 (left column), mortgagors born between 1935 and 1949 (middle column) and outright owners born before 1935 (right column). Grey areas are bootstrapped 90% confidence bands. U.K. data: FES/LCFS (1975-2007).
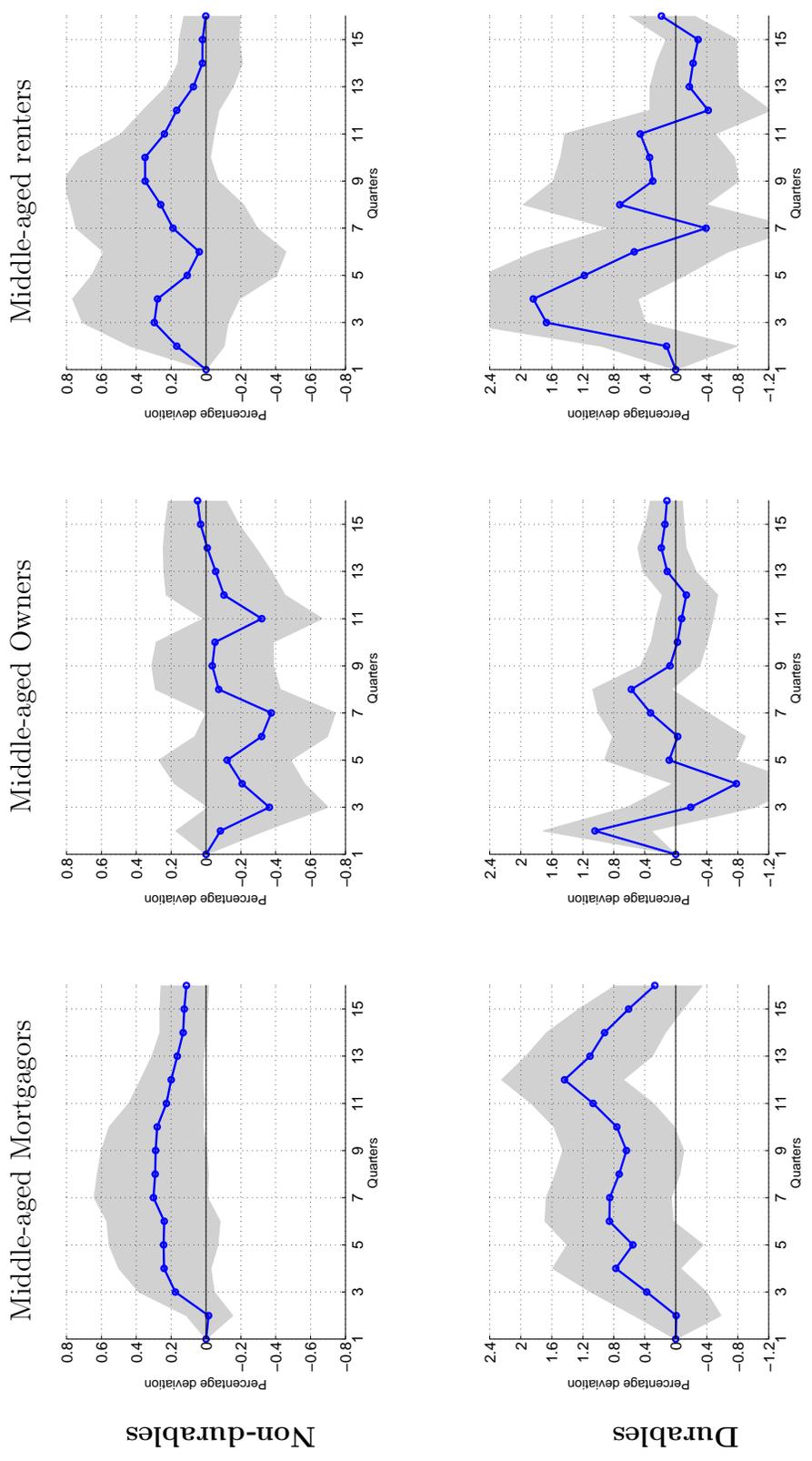
Figure 8: Dynamic effects of a 25 basis point unanticipated interest rate cut on non-durable consumption (top) and durable expenditure (bottom) for mortgagors born after 1949 (left column), mortgagors born between 1935 and 1949 (middle column) and outright owners born before 1935 (right column). Grey areas are bootstrapped 90% confidence bands. U.S. data: CEX (1981-2007).
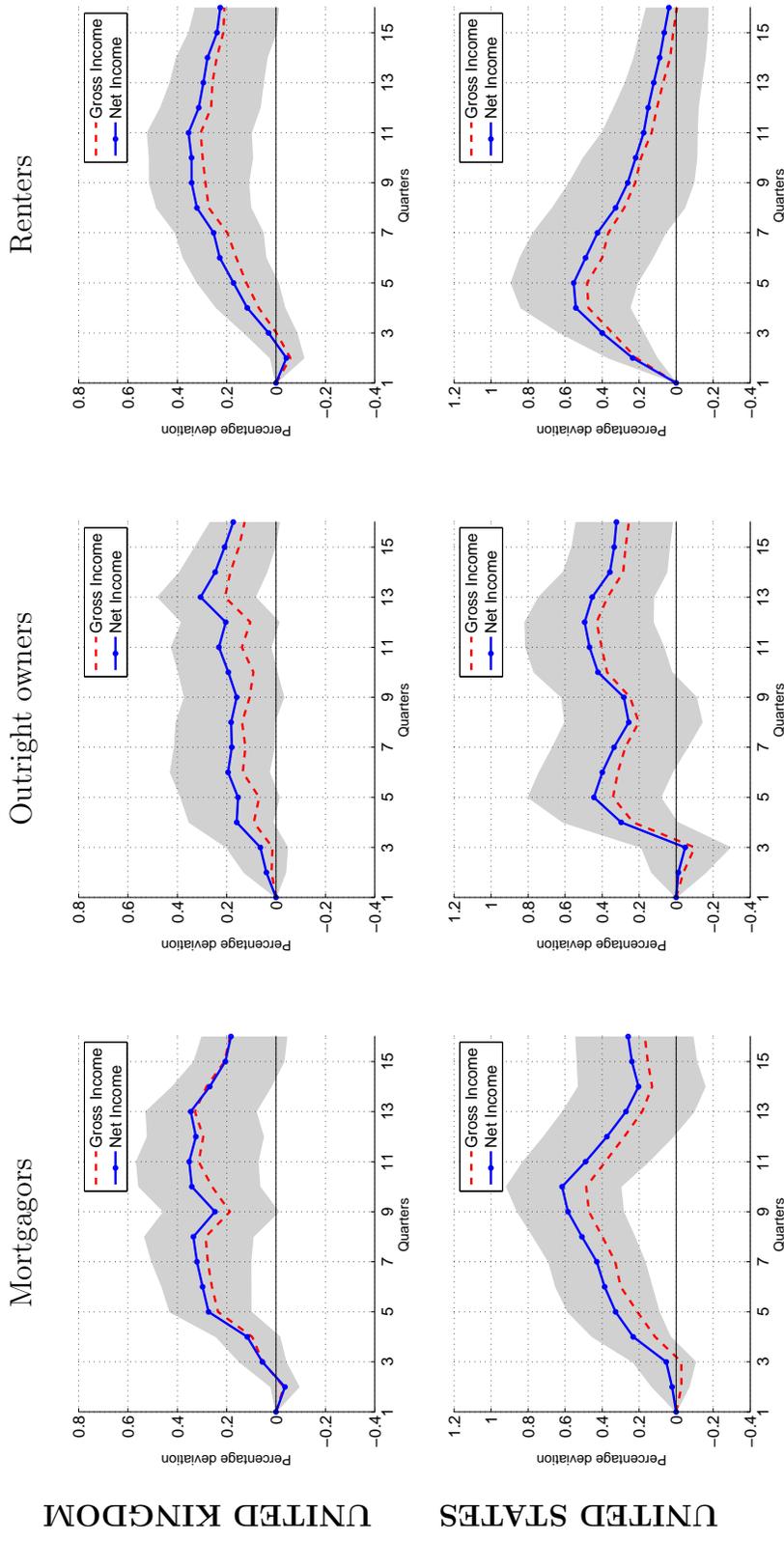
Figure 9: Dynamic effects of a 25 basis point unanticipated interest rate cut on net income (blue) and gross income (red). Mortgagors (left), outright owners (center) and renters (right). Grey areas are bootstrapped 90\% confidence bands for net income. Top row: UK (FES/LCFS data). Bottom row: US (CEX data).
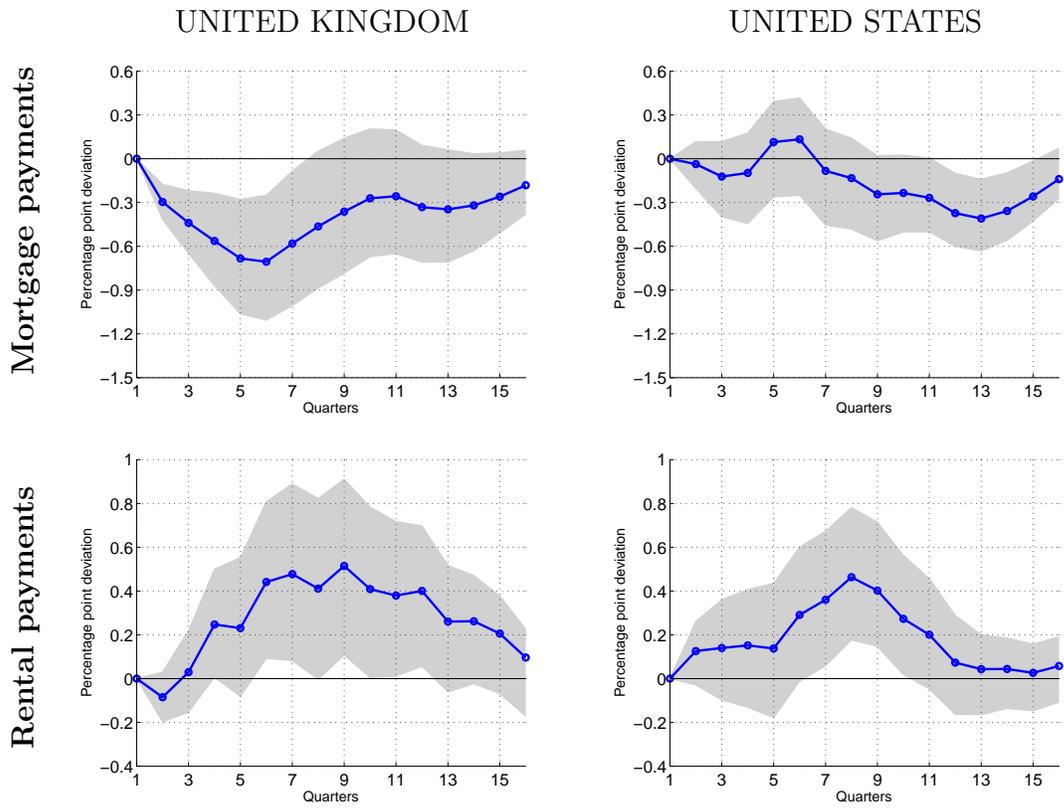
Figure 10: Dynamic effects of a 25 basis point unanticipated interest rate cut on mortgage and rental payments. Grey areas are bootstrapped 90% confidence bands. Left: UK (FES/LCFS data). Right: US (CEX data).
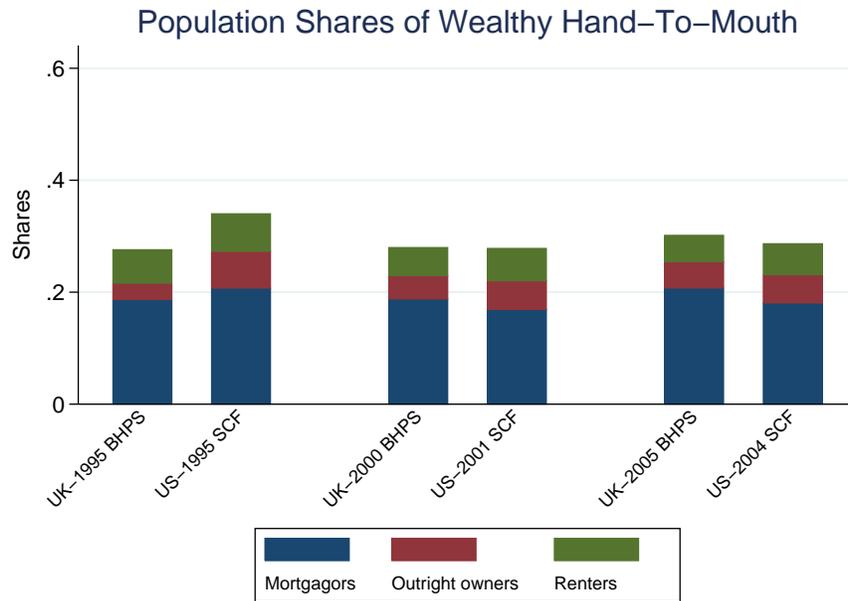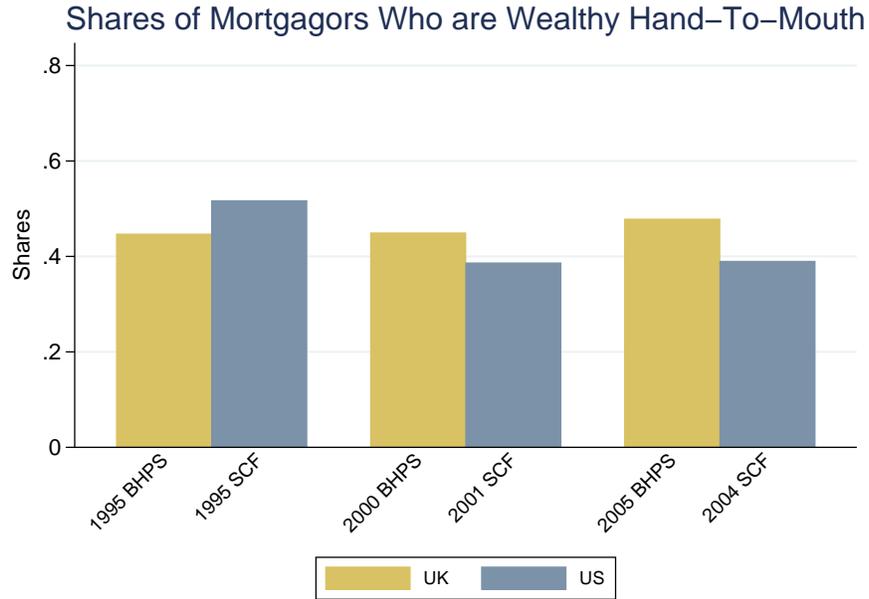
Figure 11: Shares of Wealthy Hand-To-Mouth (WHTM) mortgagors. U.K. (U.S.) data: 1995, 2000, 2005 waves of the British Household Panel Survey (Survey of Consumer Finances). A household is defined as WHTM if at any given point in time both (i) their net illiquid wealth is positive and (ii) their net liquid wealth is less than half of their total monthly household labor income.

UNITED KINGDOM   UNITED STATES
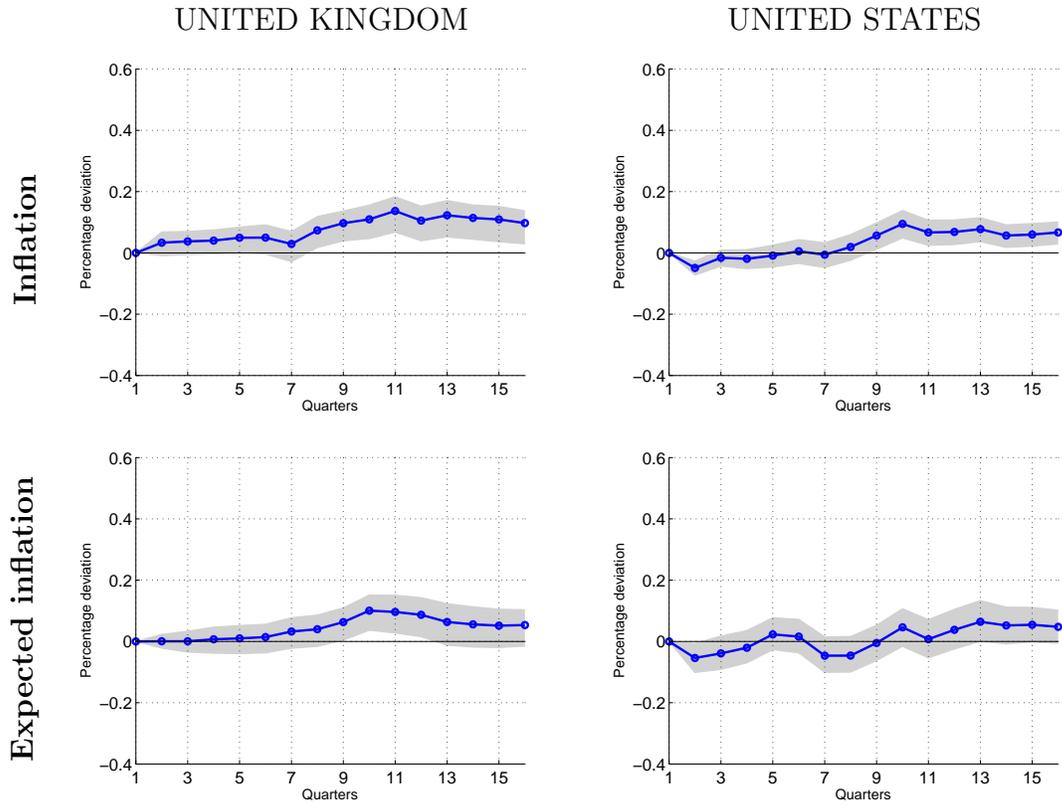
Figure 12: Dynamic effects of a 25 basis point unanticipated interest rate cut on CPI inflation (top row) and expected (1yr) inflation (bottom row). Left: UK. Right: US.
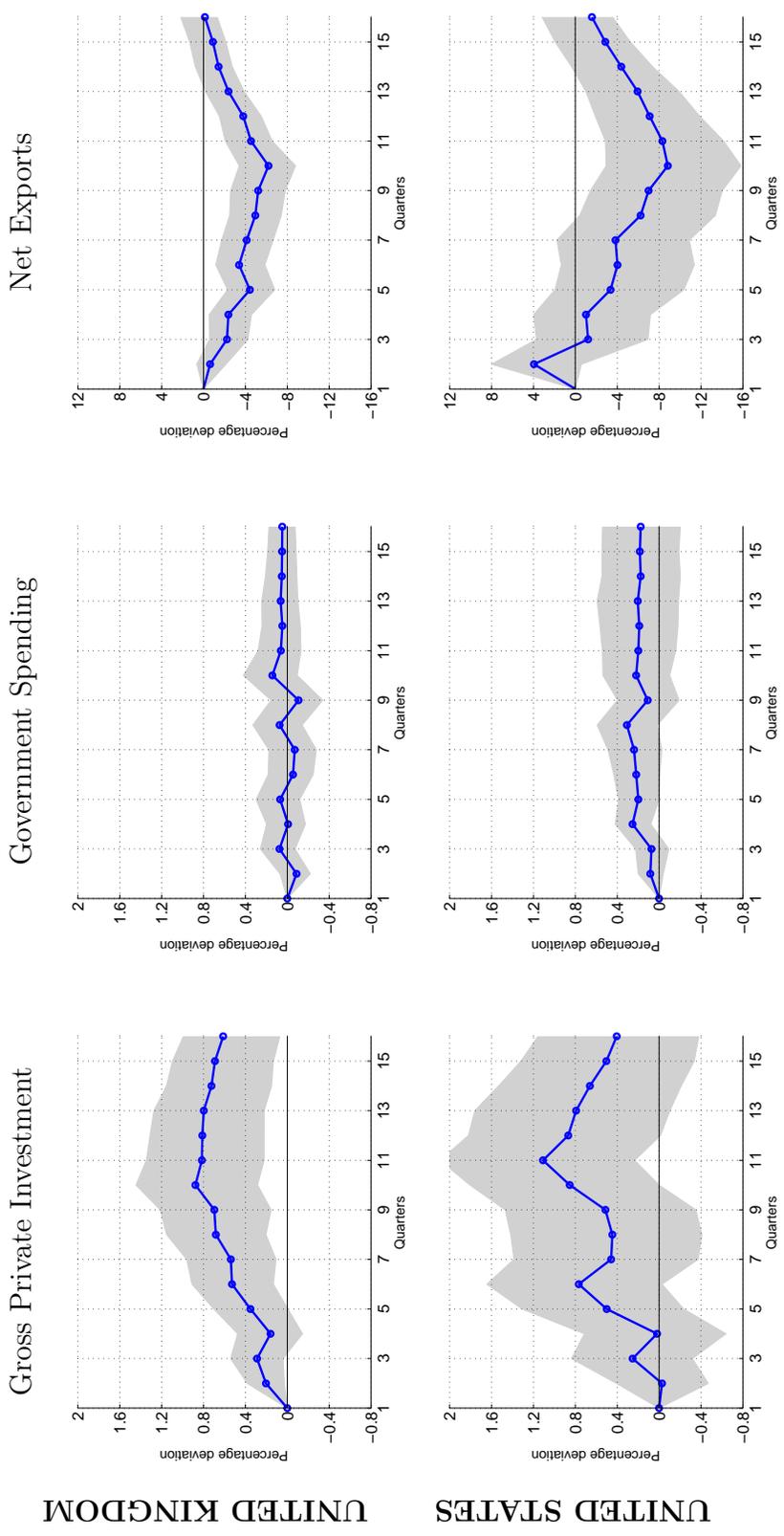
Figure 13: Dynamic effects of a 25 basis point unanticipated interest rate cut on gross private investment (first column), government expenditures (consumption plus investment; second column), and net exports (X-M) (third column). Top row: UK (aggregate ONS data). Bottom row: US (aggregate NIPA data).

Table 1: CUMULATIVE CHANGES OVER FOUR YEARS IN US$

Panel A: United Kingdom

	Non-durable consumption	Durable expenditure	Mortgage or rental payments	After-tax income
Mortgagors	308.3	292.3	-166.4	695.9
	[112.8, 516.1]	[189.2 , 369.0]	**[-272.2 , -41.7]**	[186.5 , 1105.1]
Outright owners	-62.6	46.5		451.7
	[-148.2 , 77.4]	[-24.6 , 107.6]		[122.5 , 797.2]
Renters	155.3	19.0	64.7	397.3
	[17.9 , 261.8]	[-36.5 , 62.9]	[4.4 , 118.7]	[94.2 , 596.1]

Panel B: United States

	Non-durable consumption	Durable expenditure	Mortgage or rental payments	After-tax income
Mortgagors	305.8	229.3	-56.3	757.3
	[58.3 , 554.3]	[122.0 , 350.8]	**[-112.8 , -4.3]**	[196.8 , 1302.0]
Outright owners	-72.3	54.8		585.3
	[-324.8 , 186.0]	[-10.5 , 127.8]		[83.3 , 1012.8]
Renters	223.3	123.5	64.8	439.3
	[32.3 , 412.3]	[30.3 , 213.8]	[9.8 , 121.5]	[112.8 , 699.8]

Note: The Table reports the overall dollar change in expenditure over the 4 year period following a temporary 25bps cut in monetary policy, together with the bootstrapped 90% confidence bands. The magnitudes are per household averages. To compute these numbers we cumulate the IRFs and divide by the average value of each variable for each cohort in each country (shown in Appendix Table B.1). We use the U.S. price level in 2007 and the average exchange rate between pounds sterling and U.S. dollars in that year. Using the average exchange rate over the full sample would make the dollar changes for the U.K. some 10% lower but would not affect the fact that the estimated values in each column have the same order of magnitude across the two countries.

Table 2: Household Financial and Housing Wealth for the UK and US.

	UK BHPS: 1995		US SCF: 1995	
	Median	[p25 , p75]	Median	[p25 , p75]
Net Financial wealth				
Outright owners	2,000	[0 , 10,000]	3,505	[165 , 18,455]
Mortgagors	150	[-753 , 3,450]	1,505	[-695 , 8,705]
Renters	0	[-25 , 559]	5	[-545 , 2,005]
Net Housing wealth				
Outright owners	59,000	[42,000 , 80,000]	86,000	[52,000 , 142,000]
Mortgagors	28,000	[10,000 , 51,750]	43,500	[20,000 , 86,000]
Renters	0	0	0	0

	UK BHPS: 2000		US SCF: 2001	
	Median	[p25 , p75]	Median	[p25 , p75]
Net Financial wealth				
Outright owners	3,300	[0 , 15,000]	8,305	[1,005 , 40,915]
Mortgagors	375	[-1,600 , 6,400]	4,605	[105 , 22,701]
Renters	0	[-68 , 1,000]	145	[-255 , 2,425]
Net Housing wealth				
Outright owners	70,000	[47,000 , 110,000]	111,500	[65,000 , 203,500]
Mortgagors	36,000	[16,000 , 70,000]	62,000	[25,000 , 126,000]
Renters	0	0	0	0

	UK BHPS: 2005		US SCF: 2004	
	Median	[p25 , p75]	Median	[p25 , p75]
Net Financial wealth				
Outright owners	3,050	[0 , 17,034]	5,580	[605 , 56,805]
Mortgagors	0	[-3,250 , 5,000]	2,500	[-95 , 25,505]
Renters	0	[-455 , 500]	70	[-345 , 2,305]
Net Housing wealth				
Outright owners	150,000	[100,000 , 220,000]	145,000	[76,000 , 265,000]
Mortgagors	97,000	[56,250 , 152,000]	77,000	[32,000 , 168,000]
Renters	0	0	0	0

Note: *Net financial wealth* (NLW): (i) from the BHPS, the value of savings and investments net of outstanding non-mortgage debt; (ii) from the SCF, the value of checking, saving and MM accounts, directly held mutual funds, stocks, bonds and t-bills, net of outstanding unsecured debt. *Net housing wealth* is the household's estimate of the property value net of any outstanding mortgage and home equity lines of credit. Trimmed at the top 5 percentiles of the NLW distribution.

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ON-LINE APPENDIX

(not intended for publication)

A Data sources and definitions

A.1 National statistics

The aggregate time-series for the U.K. and the U.S. come from the Office for National Statistics (ONS) and the National Income and Product Accounts (NIPA), respectively. Personal consumption expenditure on non-durable goods and services expenditure, personal consumption expenditure on durable goods and disposable income are all seasonally adjusted at the source. The series are divided by total population to obtain per-capita values. The deflator used for the U.K. (U.S.) is the Retail Price Index excluding mortgage interest payments (Consumer Price Index).

A.2 Household survey data

For the U.K., we use the Living Costs and Food Survey (formerly known as Family Expenditure Survey) from 1975 to 2007 (1978 to 2007 when we use educational attainment for the probit regressions). For the U.S., we use the Consumer Expenditure Survey (interview section) from 1981 to 2007.

Household expenditure. *Non-durable goods and services*: includes food, alcohol, tobacco, fuel, light and power, clothing and footwear, personal goods and services, fares, leisure services, household services, non-durable household goods, motoring expenditures and leisure goods. Between 1982 and 1987, food at home in the CEX is adjusted following Aguiar and Bils (2015). *Durable goods*: durable household goods, motor vehicles and durable leisure goods. This includes expenditure such as furniture and furnishings, electrical appliances and audio-visual equipment.

Mortgage payments and income. *Mortgage payments*: includes both interest payments and capital repayments (not available individually over the whole sample). *Net income*: sum of labor- and non-labor household income net of taxes paid.

Restrictions. We exclude households: (i) that do not report income, (ii) that report negative net income, (iii) that are in either the top or the bottom 1% of either the non-durable or the durable expenditure distributions of each housing tenure group at any quarter and (iv) whose head is either below 25 years old or above 74 years old. Finally, for the CEX, which features a short panel dimension, we only keep households that have not changed housing tenure status between interviews. The LCFS comprises repeated cross-sections and thus each household is observed only once. For comparability across countries and over time, all household variables in the LCFS for the U.K. (in the CEX for the U.S.) are deflated by the Retail Price Index excluding mortgage interest payments (Consumer Price Index) and divided by the household size to obtain real values in per-capita terms.
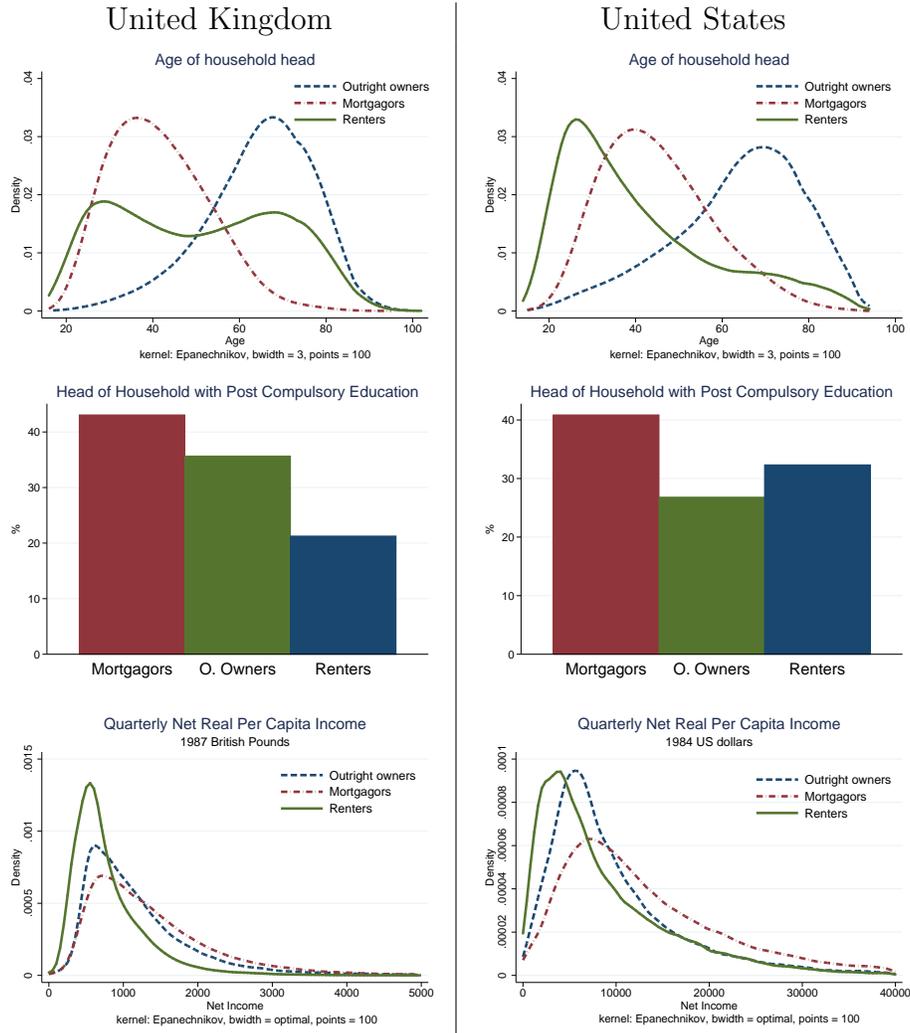
B   Descriptive statistics

Figure B.1: Distribution of Demographic Characteristics Across Housing Tenure Groups. Top row: age of the household head at the time of interview; middle row: share of household heads who completed *more than* compulsory education; bottom row: real per capita income net of taxes. Left: U.K. (LCFS), 1975q1-2007q4. Right: U.S. (CEX), 1981q1-2007q4.
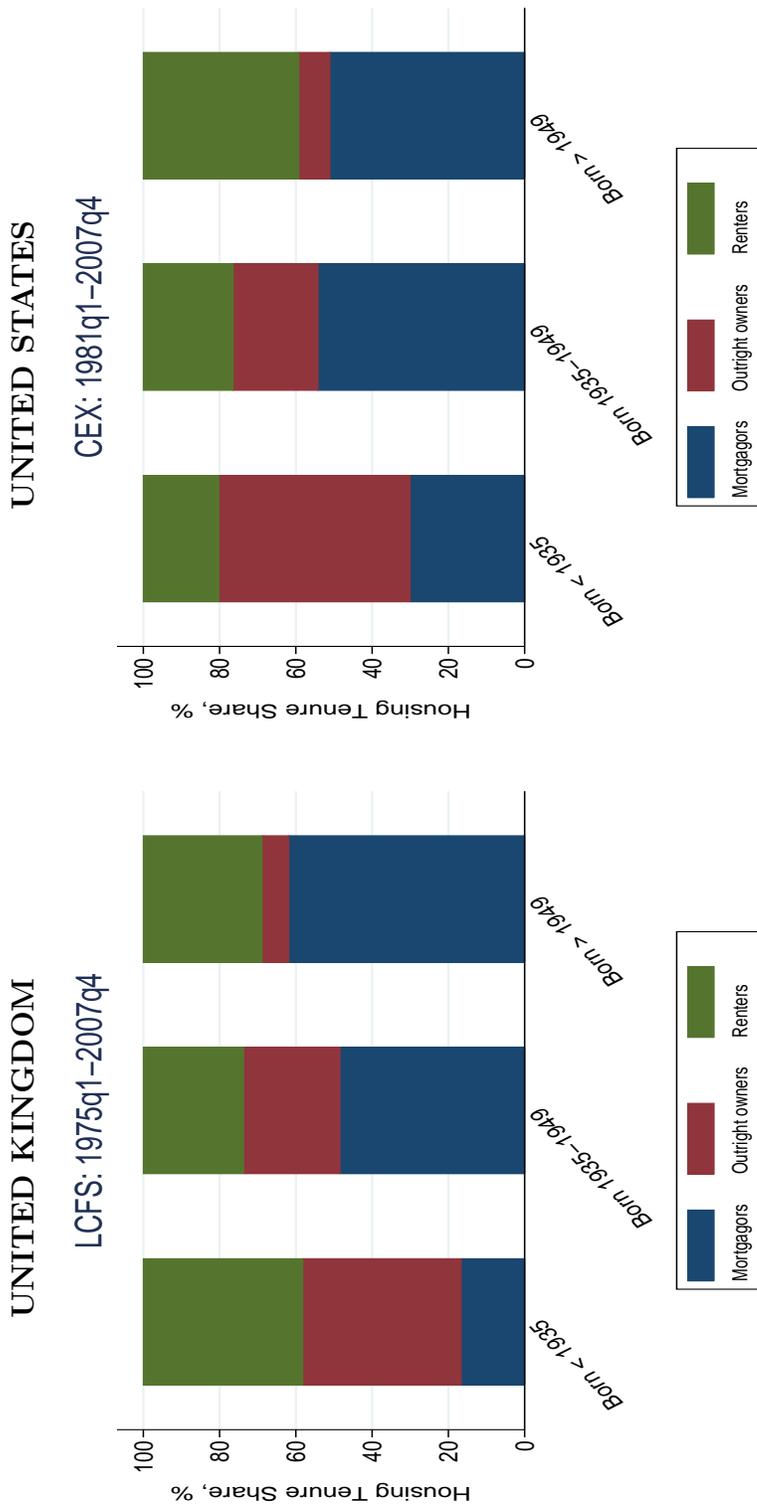
Figure B.2: The share of mortgagors, outright home owners and renters for different birth cohorts in the US and the UK. Birth cohorts are defined by the birth year of the household head. Three birth cohorts are considered: younger households, middle aged households and older households. The year cut-offs are chosen to ensure there are enough observations in each cohort group. The cut-offs also ensure that each birth cohort has approximately the same average age across the two countries.

Table B.1: Mean Quarterly Household Expenditures and Income over the full sample, in 2007 US$

	Panel A: United Kingdom			
	non-durable expenditure	*durable expenditure*	*mortgage or rent payments*	*after-tax income*
Mortgagors	10,202	1,842	1,950	15,555
Outright Owners	10,030	1,625		13,617
Renters	6,807	750	1,613	9,310

	Panel B: United States			
	non-durable expenditure	*durables expenditure*	*mortgage or rent payments*	*after-tax income*
Mortgagors	14,767	2,470	2,352	20,500
Outright Owners	14,482	2,032		16,495
Renters	11,945	1,542	2,397	14,340

Note: Data for the UK comes from the Living Costs and Food Survey (LCFS) between 1975q1-2007q4; data for the US comes from the CEX between 1981q1-2007q4. The values in the table are average household level expenditures and income by cohort. In a given quarter these are constructed as a weighted average of all households within each cohort. Entries refer to sample averages converted into 2007 US dollars.

C Evidence from official national statistics

It is useful to examine the aggregate response of non-durable expenditure, durable expenditure and household income from the U.K. and U.S. official aggregate statistics. These results are presented in Figure C.1. A cut in the policy rate raises durable expenditure, non-durable expenditure and disposable income. More specifically, a 25 basis point monetary policy expansion leads to (i) a persistent (but small) rise in non-durable consumption, which peaks at around 0.2% after about 10 quarters for the U.K. and 0.1% after 11 quarters for the U.S., (ii) a larger *percentage* increase in durable expenditure (peaking at 1.2% for the U.K. and 1% for the U.S.), consistent with the evidence in Barsky et al. (2007) and Sterk and Tenreyro (2015) and (iii) a rise in household income (that reaches its maximum at 0.4% in the U.K. and just below 0.3% in the U.S.). While the U.K. aggregate variables tend to exhibit a slightly larger adjustment, the differences between the two countries are not large or significant.
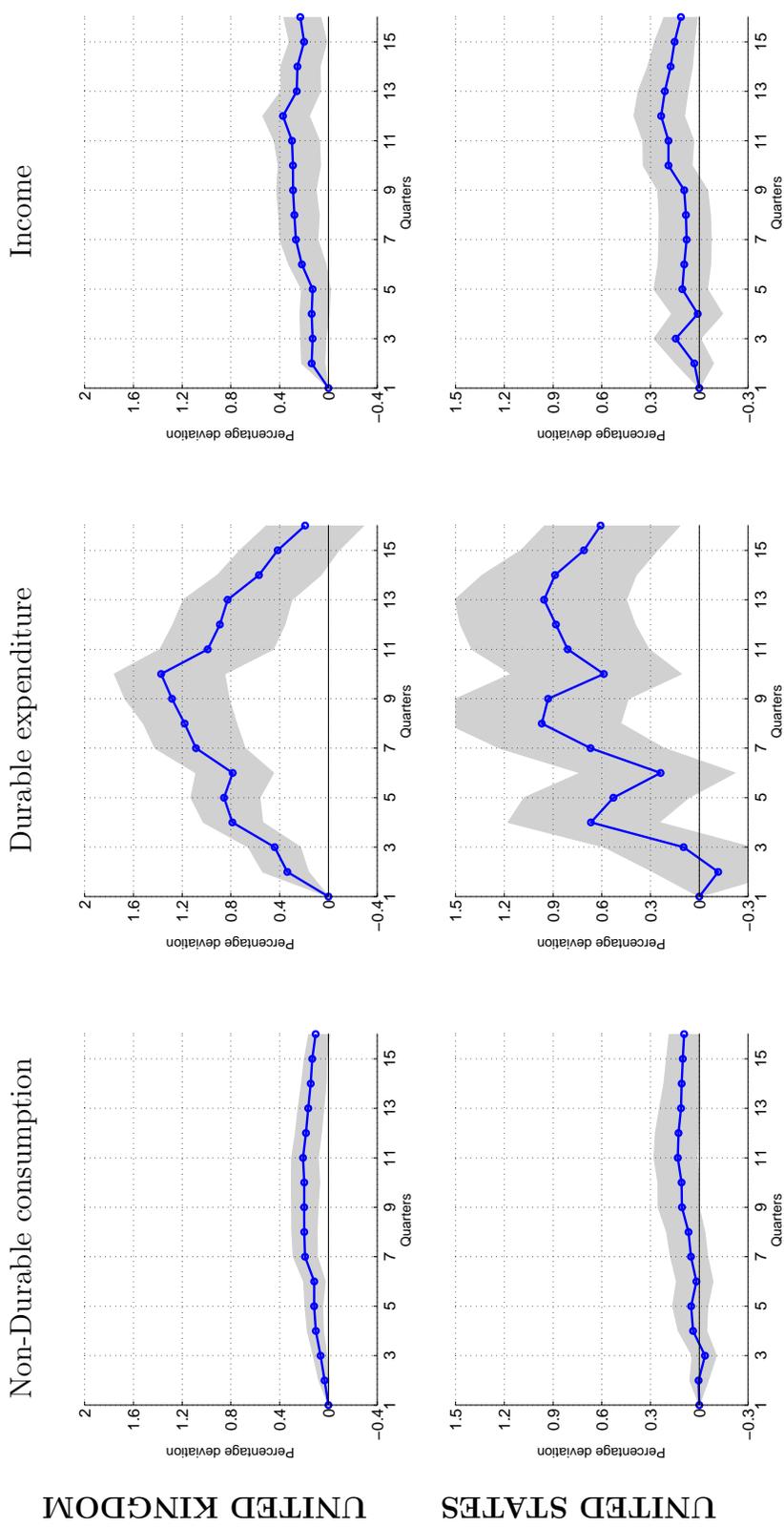
Figure C.1: Dynamic effects of a 25 basis point unanticipated interest rate cut on the consumption of non-durable goods and services, the expenditure on durable goods and household income from official aggregate national statistics. Grey areas are bootstrapped 90% confidence bands. Top row: UK, data range 1975q1-2007q4. Bottom row: US, data range: 1981q1-2007q4.

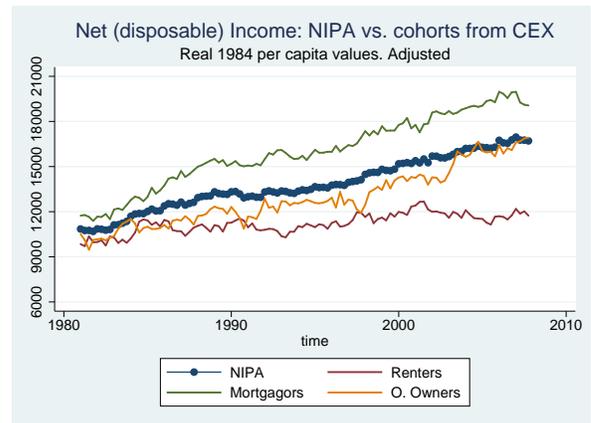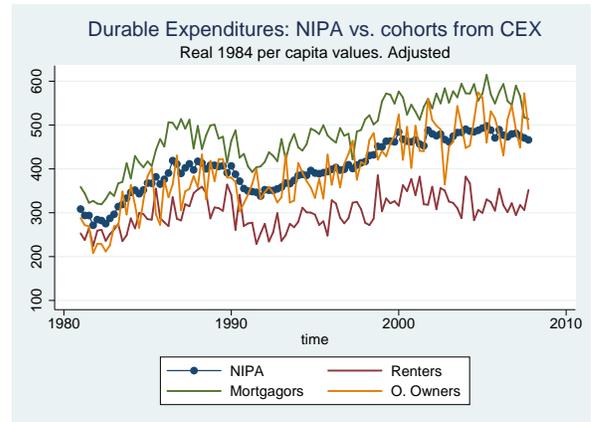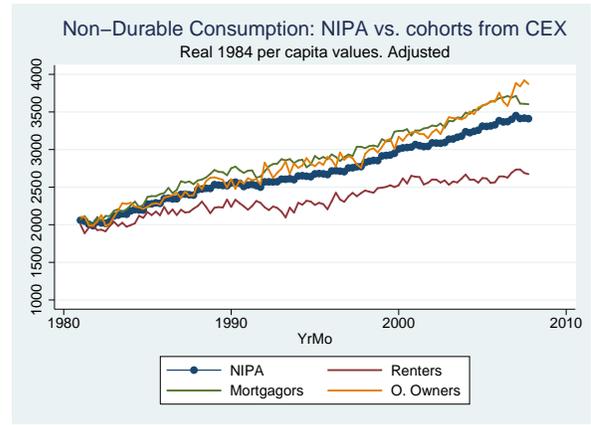**UNITED KINGDOM**
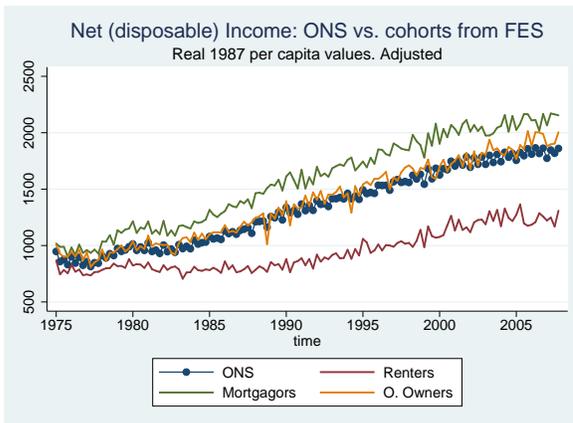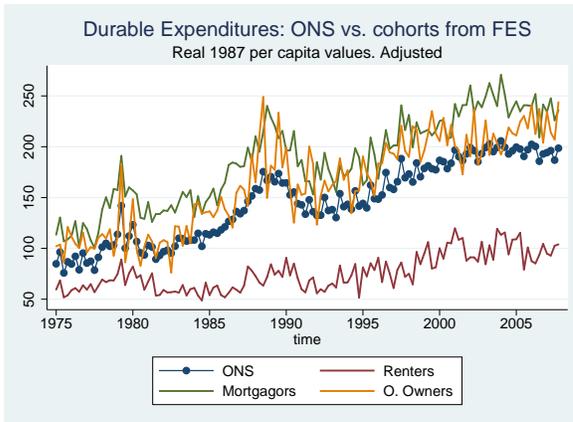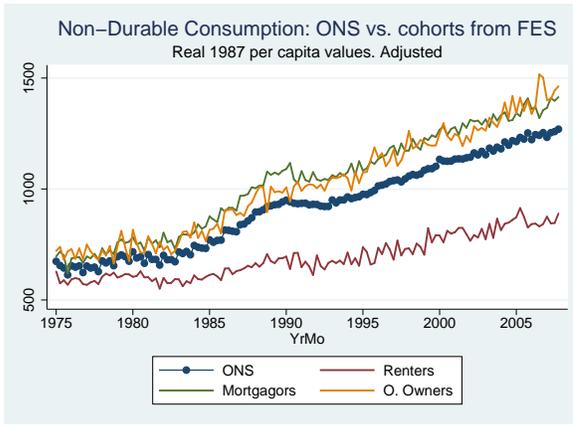
UNITED STATES

Figure C.2: Time series for real non-durable, durable and income per capita by cohort group from the LCFS/CEX, together with the equivalent aggregate measure form national statistics (US NIPA/UK ONS).
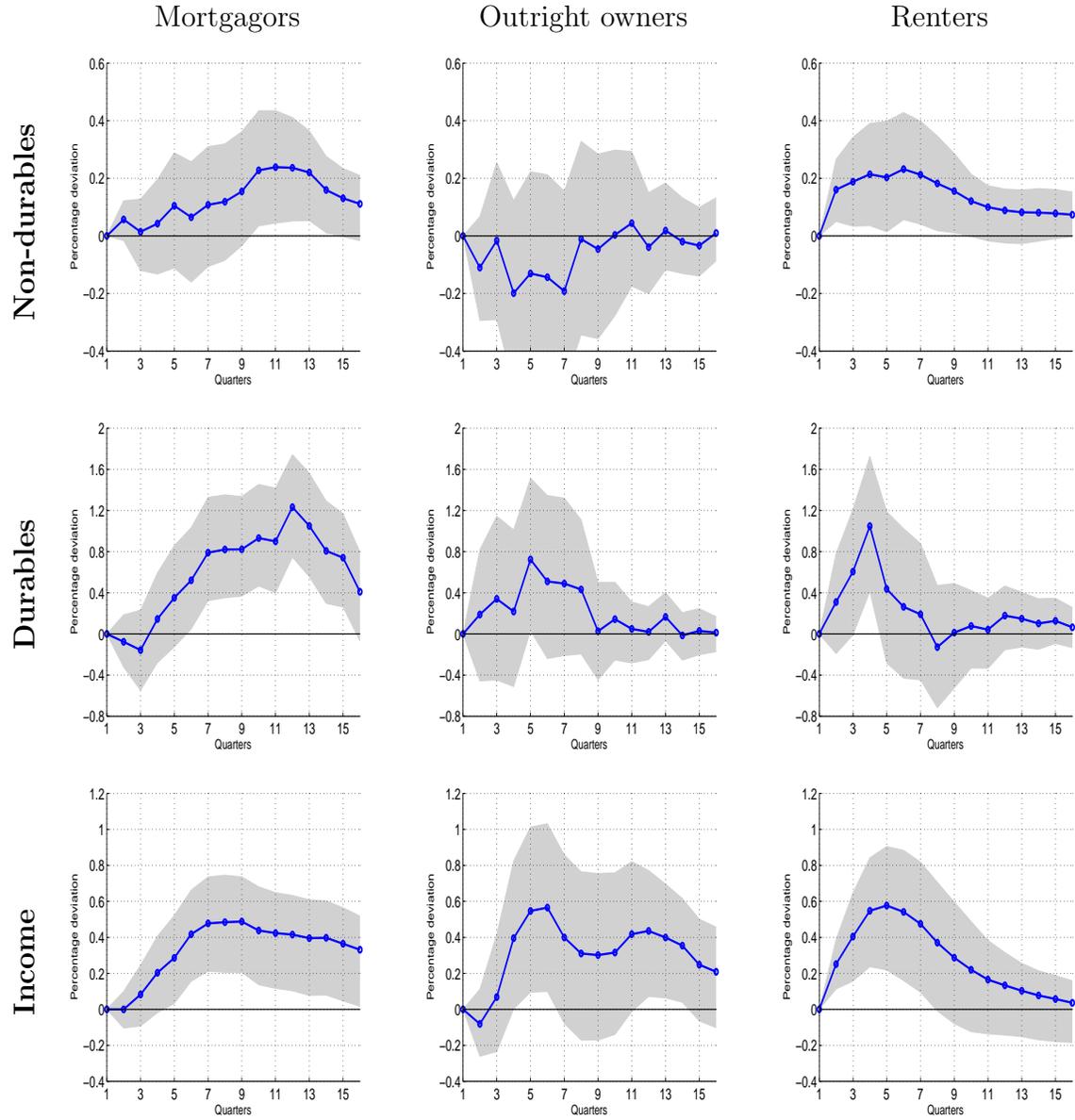
D   Excluding retired households
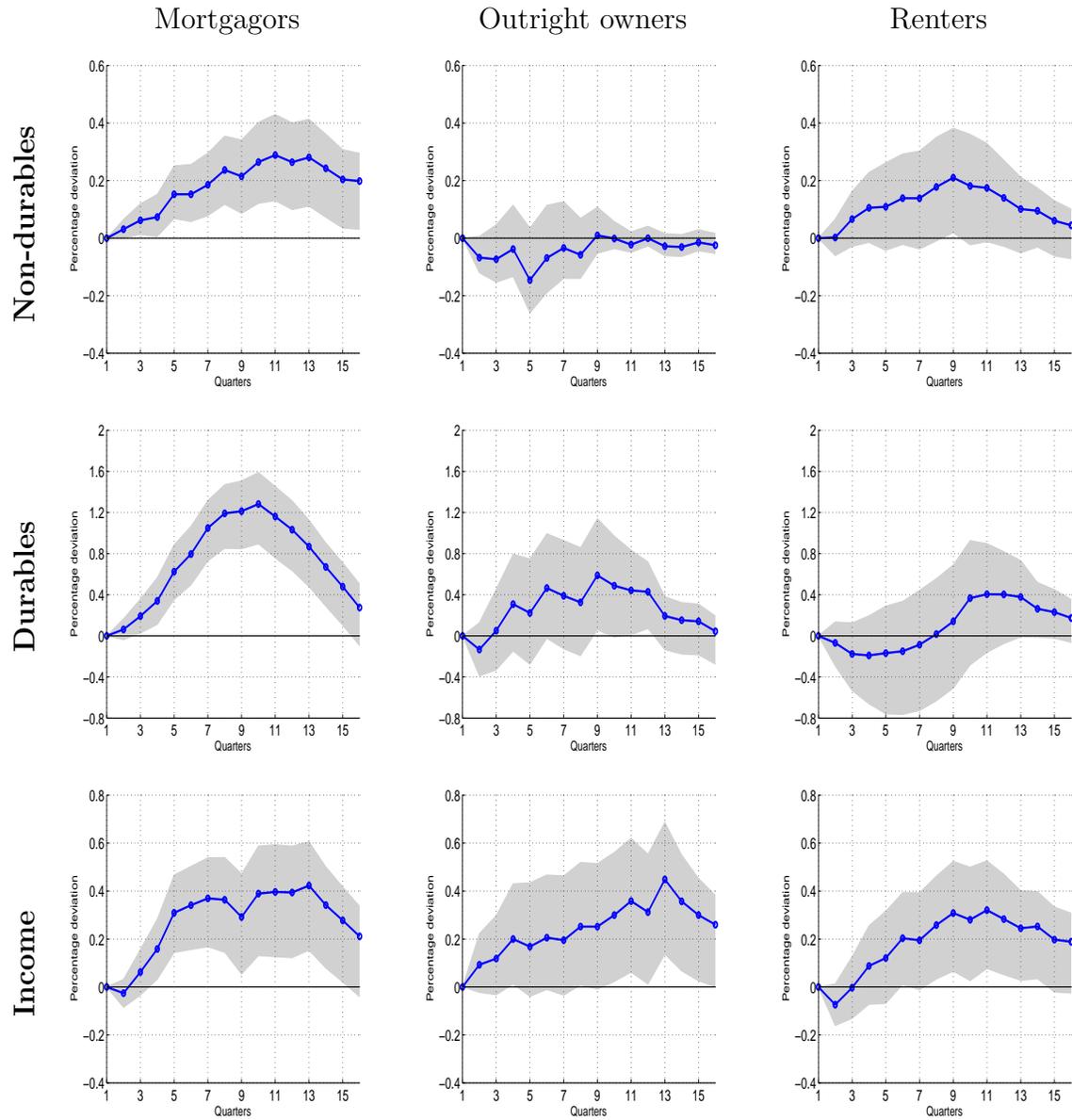
Figure D.1: Dynamic effects of a 25 basis point unanticipated interest rate cut on non-durable consumption (ND), durable expenditure (D) and income net of tax for households below age 65. U.K. data: FES/LCFS (1975-2007). Grey areas are boot-strapped 90% confidence bands.

Figure D.2: Dynamic effects of a 25 basis point unanticipated interest rate cut on non-durable consumption (ND), durable expenditure (D) and income net of tax for households below age 65. U.S. data: CEX (1981-2007). Grey areas are bootstrapped 90% confidence bands.
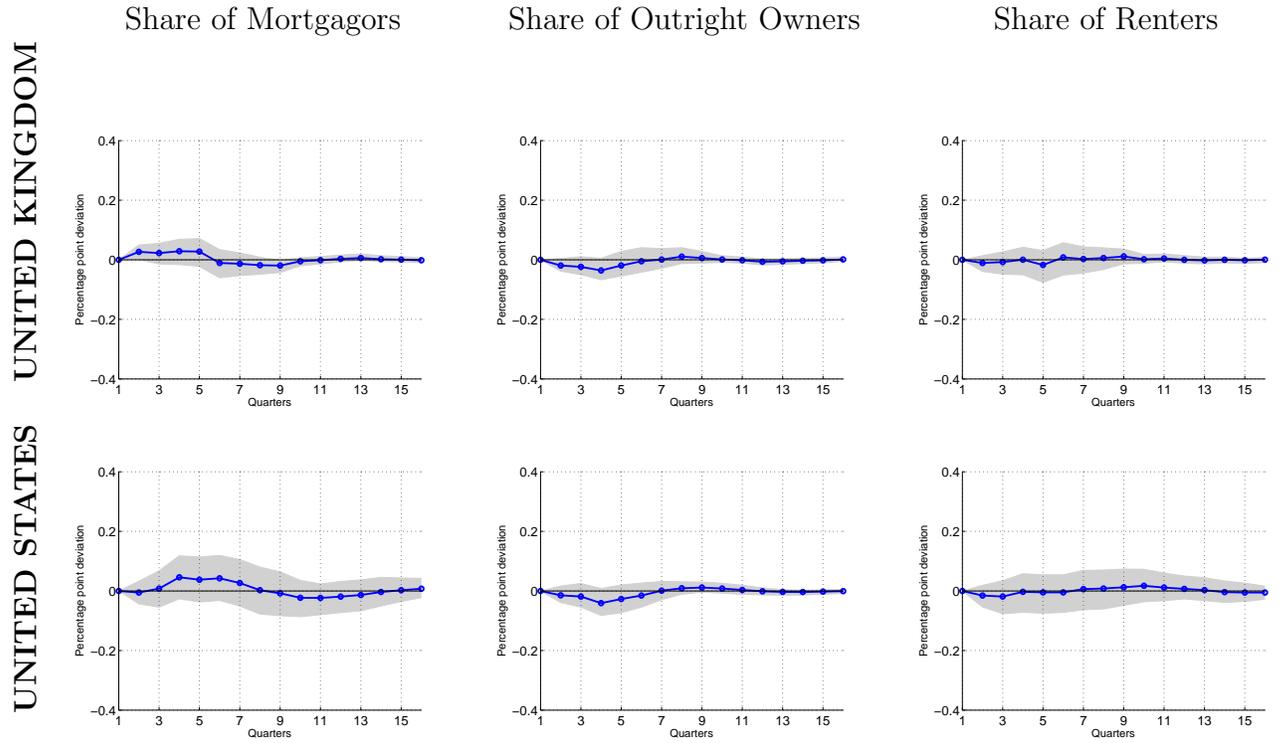
E   Compositional change

| | Share of Mortgagors | Share of Outright Owners | Share of Renters |

Figure E.1: Dynamic effects of a 25 basis point unanticipated interest rate cut on the share of mortgagors, outright owners and renters. Grey areas are bootstrapped 90% confidence bands. U.K. data: LCFS (1975-2007) in top row; U.S. data: CEX (1981-2007).
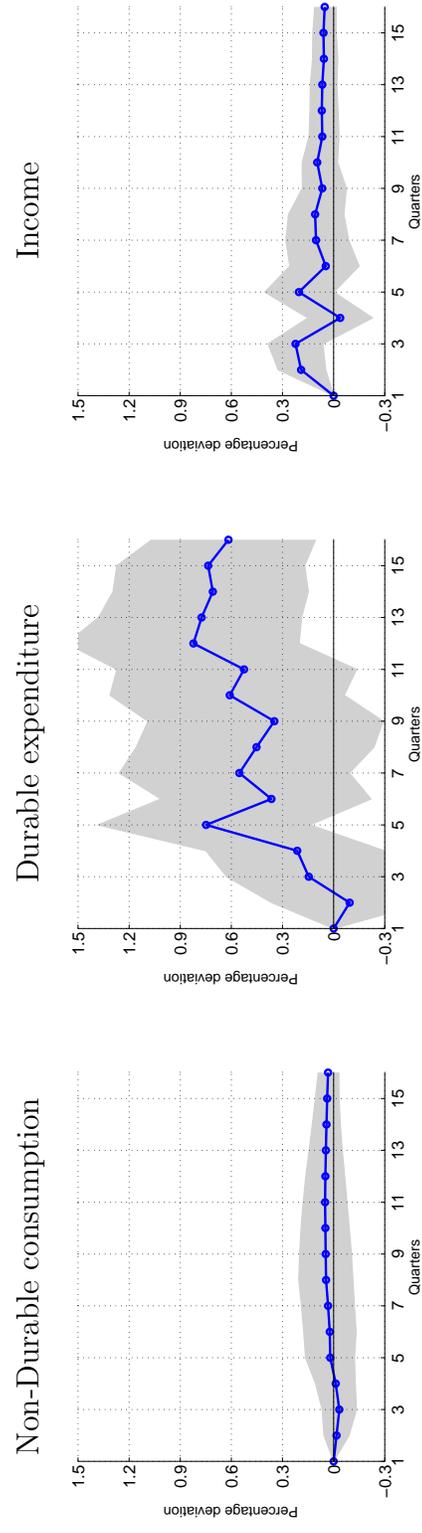
F   US results post-1985

Figure F.1: Dynamic effects of a 25 basis point unanticipated interest rate cut on the consumption of non-durable goods and services, the expenditure on durable goods and household income from official aggregate national statistics (NIPA). Grey areas are bootstrapped 90% confidence bands. Sample: 1985q1-2007q4.
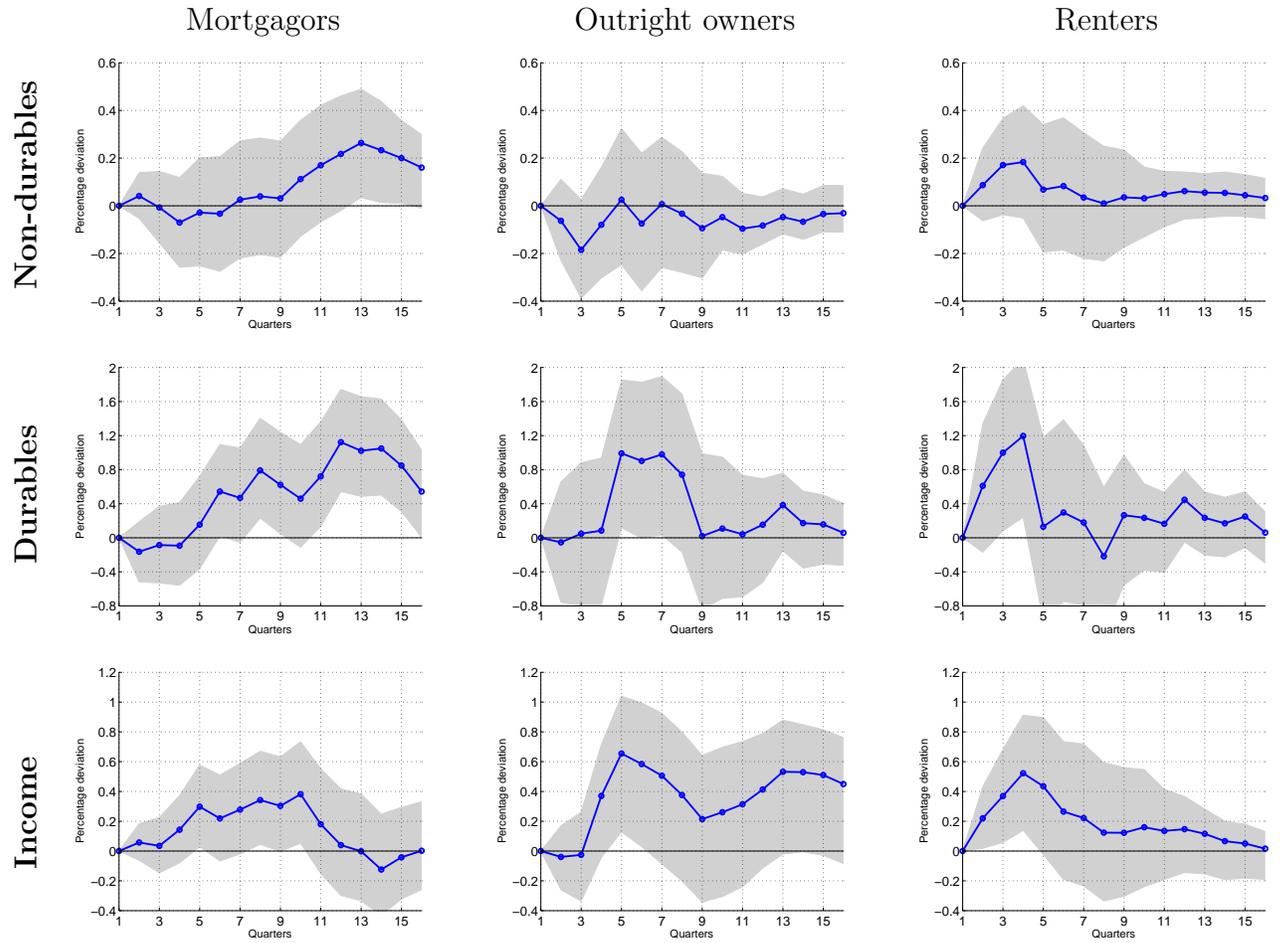
Figure F.2: Dynamic effects of a 25 basis point unanticipated interest rate cut on consumption of non-durable goods and services, expenditure on durable goods, and income. Mortgagors (left), outright owners (center) and renters (right). Grey areas are bootstrapped 90\% confidence bands. Data from the CEX for the US. **Data range: 1985q1-2007q4**.

G   The response of house prices
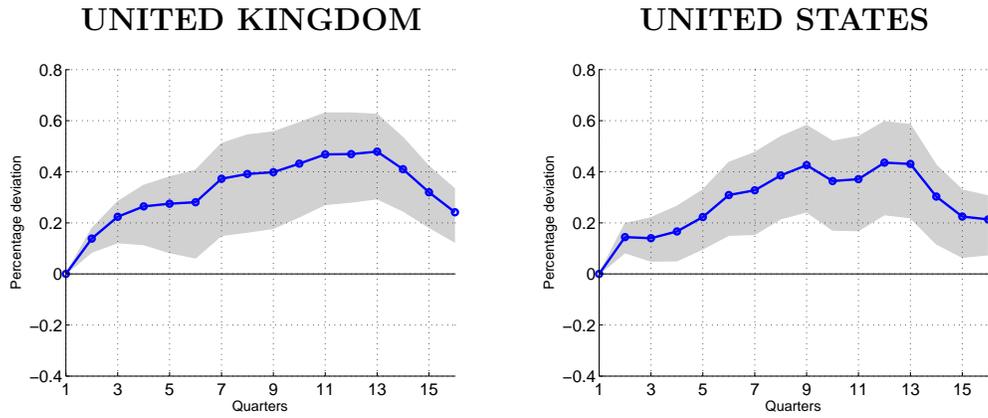
UNITED KINGDOM

UNITED STATES

Figure G.1: IRF for aggregate real house prices. Grey areas are bootstrapped 90% confidence bands. Left: U.K. (1975-2007). Right: U.S. (1981-2007).