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Household Debt and the Dynamic Effects of Income Tax Changes

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Using a new narrative measure of fiscal policy shocks for the U.K., we show that households with mortgage debt exhibit large and significant consumption responses to tax changes. Homeowners without a mortgage, in contrast, do not adjust their expenditure, with responses not statistically different from zero at all horizons. We compare our findings to the predictions of traditional and newer theories of liquidity constraints, providing a novel interpretation for the aggregate effects of tax changes on the macroeconomy.

Key words: Fiscal policy, narrative tax changes, household debt

JEL Codes: E21, E32, E62

1. INTRODUCTION

The persistent rise in mortgage debt prior to the recent financial crisis has drawn considerable attention to the role of private indebtedness in the transmission of macroeconomic shocks in advanced economies. On the empirical side, Mian *et al*. (2013), Dynan (2012), and the IMF (2012) report that high levels of household debt are likely to have amplified and prolonged the Great Recession of 2007–8. On the theoretical side, Eggertsson and Krugman (2012), Andres *et al*. (2015), and Kaplan and Violante (2014) present heterogeneous agent models where fiscal policy is more effective the larger the proportion of debt-constrained households.

A common presumption behind these studies is that debtors are more likely to face liquidity constraints and, therefore, adjust their consumption significantly in response to conditions that unexpectedly change their income. An important implication is that it is not net wealth *per se* that determines the consumption response to fiscal policy: households who made a large durable purchase, such as housing, may well be wealthy and liquidity constrained at the same time. Despite the importance of this transmission mechanism for both policy and academic research, little is known empirically about whether the effects of tax changes on consumption vary with a household's debt position.

Looking at the empirical association between consumption, income, and debt is complicated by at least three factors. First, since consumption and income are jointly determined we need to isolate the exogenous component of income changes. Secondly, survey data with good coverage of expenditure typically lack equally detailed and reliable information on wealth over a sufficiently long period of time. We, therefore, need to identify a proxy for the household debt position. Thirdly, commonly used surveys are either repeated cross-sections, like the Family Expenditure Survey (FES) for the U.K., or overlapping panels with a short time series dimension, like the Consumption Expenditure Survey for the U.S. We, therefore, need to group individual observations into aggregate pseudo-cohorts.

To address the endogeneity of income changes, we identify exogenous variation in taxes using the narrative approach pioneered by Romer and Romer (2010) and applied to aggregate tax changes in the U.K. by Cloyne (2013). More specifically, we construct a new series of changes in household income tax for the U.K. that are exogenous to *both* macroeconomic *and* cohort-level fluctuations. The U.K. appears a natural choice for our purposes because there have been a large number of income tax changes in the last 40 years and detailed official documents allow us to identify individual tax measures and their motivations.

To elicit individual debt positions, we propose to group households by housing tenure (whether households are mortgagors, outright owners, or renters) using the U.K. FES and compare the consumption response of homeowners with mortgage debt with the response of outright home owners (*i.e.* those without a mortgage). The motivation for this novel grouping strategy is threefold. First, mortgages are the most prominent form of household debt, in both incidence and value. Secondly, the extensive margin of whether a household holds a mortgage is likely to be less prone to measurement errors than the intensive margin of its outstanding value (which is recorded consistently in the FES only over a shorter period of time). Thirdly, looking at housing tenure allows us to investigate the dynamic effects of tax changes on the consumption of "social renters;" (*i.e.* those renting from local authorities or housing associations). This is a group with virtually no net wealth, low income and only compulsory education and, therefore, fits the *traditional* stereotype of liquidity constrained households in one-asset models.[1]

A potential drawback of grouping households by their housing tenure status is the possibility of endogenous transitions from one tenure status to another over time as a result of any tax change. The very gradual rate at which ownership has risen in the U.K. suggests this may be less of a concern. But, to verify this, we show that our results are robust to using the grouping strategy proposed by Attanasio *et al*. (2002), which explicitly addresses the possibility of endogenous movements between groups and compositional change.

A more significant limitation of our empirical design is that households in our sample are not randomly assigned to mortgagor, outright owner, or social renter status. Indeed, we document some systematic differences between these groups, for instance, in terms of demographics and educational attainment, suggesting the possible presence of a selection issue: mortgagors may be responding differently from outright owners not because they have a mortgage, but because some other trait that makes households more responsive to tax changes is present disproportionately in the mortgagor population. Nevertheless, it is difficult to identify obvious candidates for such a trait, especially in light of our findings that the response by outright owners is insignificant and small, that the heterogeneity in the consumption adjustment across birth and education cohorts is limited, and the fact that we also present additional results which line up, quantitatively and qualitatively, with the theoretical predictions of a model based on liquidity constraints (but tend to accord less

1. It is worth noting, however, that social renters account for only around 20% of the British population and thus they seem unlikely, on their own, to account for the large and persistent effects of tax changes on the aggregate economy typically found in empirical macro studies.

well with the predictions associated with alternative explanations). Furthermore, even if we were unable to establish conclusively that the effect of the tax cut is due to liquidity constraints, our results may have important policy implications in that they highlight a new variable (mortgagor status) which performs exceptionally well in identifying the group of population most likely to respond to policy changes.

This article makes two main contributions to the fiscal policy literature. First, we document that the dynamic effects of exogenous income tax changes on private consumption are highly heterogeneous across housing tenures: mortgagors exhibit the largest and most significant response, outright homeowners hardly adjust their expenditure at all and social renters change their consumption somewhat less than mortgagors. Secondly, we provide empirical support for the notion that household mortgage debt positions play a role in the transmission mechanism of fiscal policy. Specifically, as noted above, we show that our findings are consistent with the qualitative and quantitative predictions of a consumption model where accessing illiquid wealth (such as housing) is subject to transaction costs. Since mortgagors tend to account for between 40% and 50% of the population over the sample, our evidence suggests that this *new* type of liquidity-constrained household can make a substantial contribution to the large effects of tax changes reported in earlier empirical macro studies.

Related literature. This article contributes to a growing body of empirical research on consumption heterogeneity, including Anderson *et al*. (2015), De Giorgi and Gambetti (2012a), Ercolani and Pavoni (2012), and Misra and Surico (2014) among others. The findings from these studies have been interpreted as supportive of theories of precautionary saving, partial insurance, and limited participation. Our results highlight the role of an additional channel in the transmission of structural shocks: mortgage debt positions. This appears consistent with a framework where the decision to purchase a large durable good through borrowing makes some households "wealthy" hand-to-mouth, as in Kaplan and Violante (2014).

Our results also relate to a range of empirical contributions on the macroeconomic effects of fiscal policy on real activity. While our estimates are consistent with those reported by Romer and Romer (2010), Mertens and Ravn (2012), Caldara and Kamps (2008, 2012), Mountford and Uhlig (2009), and Cloyne (2013), among others, our approach allows us to identify which households drive the aggregate result as well as which individual characteristics tend to predict a higher sensitivity of consumption to income changes.

While we consider the heterogeneous effects of income tax changes, another strand of the literature has looked at the heterogeneous effects of government spending across different industries (Nekarda and Ramey, 2011), the state of the business cycle (Auerbach and Gorodnichenko, 2012, 2013; Owyang *et al*., 2013) and across households (De Giorgi and Gambetti, 2012b; Giavazzi and McMahon, 2012).

Paper layout. Our analysis is structured as follows. Section 2 presents our new series of narrative-identified tax changes and describes how we use the household survey data to construct expenditure measures by housing tenure. Section 3 finds pervasive heterogeneity in the consumption responses to tax changes across housing tenures and demonstrates robustness across an extensive list of modifications to our baseline specification. Section 4 examines the relationship between mortgage debt and liquidity constraints, and the extent to which this is consistent with our findings. We show that this channel compares favourably with the alternative explanations assessed in Section 5. The Appendices contain a description of the data and further econometric results. A Supplementary Appendix presents the narrative evidence supporting the construction of our exogenous income tax series.

2. IDENTIFICATION

In this section, we present our identification strategy and the data sets we employ. We first discuss the narrative data on U.K. tax changes and the way we exploit these to construct an exogenous income tax measure. We then move to the household survey data and the grouping strategy used to construct time series of consumption for pseudo-cohorts based on housing tenure status.

2.1. *U.K. income tax changes and the narrative approach*

A key identification challenge we face is that tax changes may affect consumption and other macroeconomic variables but common measures of taxes are also affected by the state of the economy. This may be because tax revenues are affected automatically by the cycle or because discretionary policy actions are taken in response to macroeconomic or cohort-level economic conditions. Since our household tenure groups are large shares of the population, the simultaneity between fiscal policy and consumption prevents consistent estimation.

To address the identification problem, we employ a narrative approach following Romer and Romer (2010) for the U.S. and Cloyne (2013) for the U.K. We use detailed documentation from historical sources to identify "exogenous" legislated changes in tax policy from the motivations given by lawmakers at the time of the policy intervention.[2] Unfortunately, the narrative measures of *aggregate* tax changes used in earlier contributions contain changes to a variety of taxes such as income, consumption, and capital taxes, each of which may affect household groups differently (*e.g.* stamp duty, vehicle excise duty etc.). Ideally, we seek tax changes that affect all housing tenure groups. We, therefore, focus on specific changes to household income taxes.

Income tax in the U.K. is payable on a wide range of income including earnings from employment, property, interest, retirement pensions, and some social security benefits. Further details of the U.K. income tax system are provided in Appendix A. To focus on changes that affect all income taxpayers, we collect a data set of changes affecting the lowest bracket of income tax. Specifically, we consider changes in the tax free allowances (that determine the level of income above which income tax starts to be paid), the basic rate of income tax (currently 20%) and the income bands defining the basic rate. We refer to this group of tax changes as the allowance and basic rate of income tax.[3]

Our starting point is the narrative record of tax interventions in the U.K. reported by Cloyne (2012). More specifically, we work back through all the original documents to collect the specific set of income taxes. While some of this information is already contained in Cloyne (2012), in many cases there is not enough detail for our new purpose. For instance, the types of tax changes (*e.g.* allowances and basic rate changes) are not specifically categorized in a manner that is readily suitable for our purpose.

Our narrative analysis isolates around 140 changes in the allowances and basic rate of income tax.[4] For the quantitative magnitude of each change, we follow Romer and Romer (2010) and use the revenue forecasts from the Budget documents. The focus is on the change in tax liabilities

2. The idea has been applied to government spending (Ramey and Shapiro, 1998; Ramey, 2011), monetary policy (Romer and Romer, 1989, 2004) and fiscal consolidations (Guajardo *et al*., 2011).

3. Mertens and Ravn (2013) split the Romer and Romer data set into corporate and personal tax liabilities and study the macroeconomic effects of these tax changes. As we examine sub-groups of the population, we need to construct a more specific measure of income tax changes.

4. This measure is even more conservative than the narrative series used in Cloyne and Surico (2013) as we drop a small number of tax changes that, while not explicitly directed to a specific housing tenure group, might be argued to have affected unevenly other (small) groups in society.

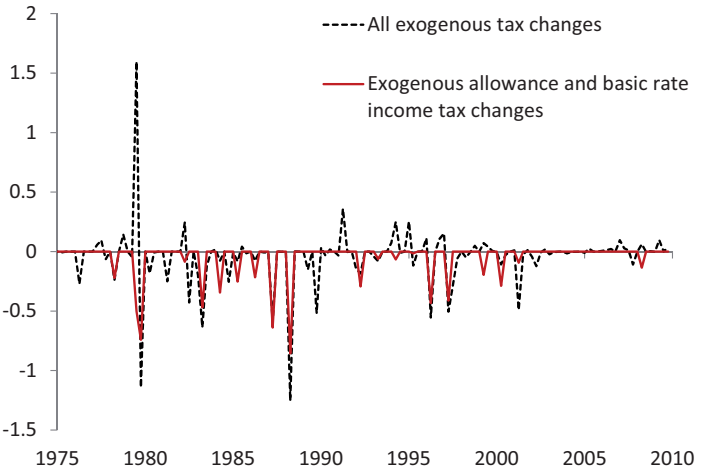

FIGURE 1
Tax liability changes over GDP: income tax measure (solid) versus all exogenous tax changes (dashed)

rather than any short-run revenue effect due to the timing of revenues reaching the Treasury. Consequently, we use the "full year" revenue estimate, which is the projected ongoing annualized effect on tax liabilities. This value is assigned to the implementation date of the policy change. The main sources for the policy changes and revenue estimates is the Financial Statement and Budget Report which is published alongside the Budget speech. As explained by Mertens and Ravn (2012), the implementation date might be anticipated whenever the policy announcement takes place some time before its implementation. We address this possibility in Section 3 and show that our findings are robust to considering only tax changes implemented on announcement.

As in Romer and Romer (2010) and Cloyne (2013), we categorize all the individual tax changes as either exogenous and endogenous based on whether they were motivated as a response to changing macroeconomic conditions (*e.g.*, GDP, consumption or other spending decisions). Unlike Cloyne (2013), however, we also examine whether the policy changes followed fluctuations in the circumstances of particular housing tenure groups. Our new measure is, therefore, also exogenous to developments in these specific cohorts. Much more detail regarding the sources, methods and supporting narrative evidence can be found in the Supplementary Appendix. Here, we briefly note that, in categorizing the given motivations, we use a variety of U.K. government, parliamentary and historical documents, and speeches. The main source is either the Chancellor's Parliamentary speech recorded in Hansard (the official parliamentary record), or the Economic and Fiscal Strategy Report published with more recent Budgets.

Individual exogenous income tax changes are assigned to quarters and aggregated. Figure 1 shows, as the solid line, our newly compiled tax series scaled by nominal GDP, together with the aggregate tax change series in Cloyne (2013) as the dashed line. There have been a sizeable number of income tax changes and many of these have been quite large, providing good variation in our narrative tax series over time. The large majority of these legislated changes were reforms designed to encourage long-run economic performance, sharpening incentives, and lowering the overall burden of taxation. Reassuringly, the correlation between all other exogenous *income* tax changes and our measure of changes to the allowance and the basic rate is low, at 0.15. Similarly, the correlation with all other exogenous tax changes is -0.07. This suggests that changes in our measure were not contemporaneously offset by changes in the higher rates of income tax.

Two features of the narrative construction of a tax shock series are worth emphasizing. First, the official Budget documents report projections for the *change* in annual tax liabilities as a result of the legislated policy action (rather than the absolute effect on the levels). Secondly, narrative-identified tax shocks (including ours) tend to exhibits virtually no persistence. These two features imply that the empirical model, discussed below, mechanically simulates the dynamic effect of a one pound tax cut as a shock that lowers, by one pound, the level of taxes paid *in each year* of the forecast period.[5] Given the frequency with which income taxes have changed over our sample period, however, it would seem difficult to interpret our shocks as permanent.

In Romer and Romer (2010) and Cloyne (2013), the tax measure can be thought of as the change in an aggregate average tax rate. For our purpose, however, it makes less sense to divide income tax liabilities by aggregate GDP as this would not reflect an average tax rate. Instead, we transform our nominal tax liabilities series into a (real) income tax change per taxpayer. We divide the (narrative) projected change in nominal liabilities by the Retail Prices Index (RPIX) and the total number of individual income taxpayers. Over a 3-year horizon, this amounts to an average tax change of about 700 pounds sterling per household at 2009 prices.

2.1.1. Cohort-specific Granger causality tests. The narrative account in the Supplementary Appendix suggests that our newly constructed income tax changes should, if truly exogenous, be unpredictable on the basis of either cohort-specific or macroeconomic conditions and we now verify this. Specifically, we conduct Granger causality tests based on a vector autoregression (VAR) which contains the change in consumption and income per capita for each household group, the change in real GDP and government spending per capita, the central bank's policy rate, the change in the Financial Times Stock Exchange Index (FTSE) and RPIX inflation. Reassuringly, we could not reject the hypothesis that the cohort-specific variables do not Granger cause our income tax series: the p-values using various lag lengths were high, over 0.4, for 4, 6, and 8 lags.

2.2. *Household consumption data*

The focus of our analysis is on whether households with mortgage debt respond more to income tax changes than those without. For this purpose, we need both good quality household expenditure data and information on household debt positions. Household expenditure data are obtained using 32 waves, from 1978 to 2009, of the Living Costs and Food Survey, commonly known as the FES. This survey has high quality, detailed information on expenditure, and household characteristics.[6] Each wave contains around 7,000 households, generating over 200,000 observations in total (Appendix B).

Ideally, we would like to observe individual balance sheet positions *and* expenditure. Unfortunately, there are no micro data sets that jointly record detailed information on consumption and wealth over a sufficiently long time period and the FES is no exception. To construct a pseudo-panel, it is, therefore, common to use a grouping estimator along the lines proposed by Browning *et al*. (1985). Given our focus on mortgage debt, housing tenure appears a natural dimension to aggregate households in three pseudo-cohorts, therefore, bypassing the lack of

5. In contrast to Mertens and Ravn (2013) for the U.S., tax revenue data for the U.K. are not sufficiently detailed to construct a national accounts quarterly counterpart of the specific subgroup of income taxes we consider (nor the tax base) over a long period of time. We have verified however, that our measure generates persistent movements in *total* income tax revenues (from national accounts) as a share of GDP, although the estimates become less precise at longer horizons.

6. While the survey has run from 1968, educational attainment is only available from 1978.

household debt data. Since loans secured on housing represent the majority of household debt, we are particularly interested in whether homeowners with mortgage debt react more to tax changes than homeowners without.

As another interesting group, we consider a third tenure category: those living in accommodation rented from local authorities or housing associations. For short, we refer to these households as "social renters". These households tend to be poorer, have only compulsory education and — as we will show using less-frequent data from the British Household Panel Survey (BHPS)—have little liquid or illiquid wealth.[7] The social renters, therefore, fit the demographic characteristics of those more likely to be credit constrained in the traditional sense used in earlier empirical contributions. For this reason, we see the "social renter" group as a useful comparison.

We focus on non-durable goods and services expenditure. Since we examine the response of consumption to tax changes, we only want to include taxpayers in our sample. The FES contains information on income taxes paid and we, therefore, exclude households who reported they did not pay any tax. However, there may be some measurement error associated with this reporting. After excluding these households, we also then drop any remaining households whose income was below the threshold for paying income tax.[8]

In our sample, mortgagors represent about 45% of the observations, on average, while social renters and owners outright represent around 20% and 25% each. Private renters averages around a 10% share but over several quarters around the middle of the sample their number appears too low (below sixty) to draw any reliable inference and are, therefore, excluded.

Each household is assigned to a quarter based on the date of the interview. To account for some (unrealistically) high or low values of consumption, for each quarter and tenure group we drop the top and bottom 1% of observations.[9] Then, we sum up the individual survey responses within each cohort by quarter using household weights that ensure representativeness in the British population, divide by the number of people in the household to generate a per capita measure and divide by the retail prices index excluding mortgage repayments (RPIX). To address seasonality, we use the annual change in quarterly expenditure for each group.

One issue using a grouping estimator is that the dimension along which the aggregation is performed needs to be fully predictable over time. In our case, we do not know whether a household with a particular tenure status had the same tenure status in the previous period or whether it will still have the same tenure status in the next period. Figure 2 shows that there has been some variation in the shares of the tenure groups over time, especially before 1986, although the dynamics of these series appear to be relatively slow moving. To ensure robustness of our findings, we also consider grouping households according to their predicted probabilities of having a mortgage based on exogenous observables. More specifically, in the next section, we complement the evidence using actual housing tenure groupings with results based on the propensity score grouping strategy proposed by Attanasio *et al*. (2002). We also assess the sensitivity of our findings to using shorter samples that start between the mid-1980s and the early 1990s which, according to Figure 2, were characterized by a greater stability in the evolution of tenure shares.

7. Unlike the FES, the BHPS has limited consumption coverage, mostly on food expenditure.

8. These two strategies largely identify the same group of households, whose dominant share (around 70%) is made-up of social renters. Non-taxpaying social renters represent about 40% of the total number of social renters in the whole FES sample. We have verified that the non-taxpayers' consumption response to our income tax shock is never statistically different from zero.

9. Similar but less precise estimates for the unrestricted sample are in Cloyne and Surico (2013).

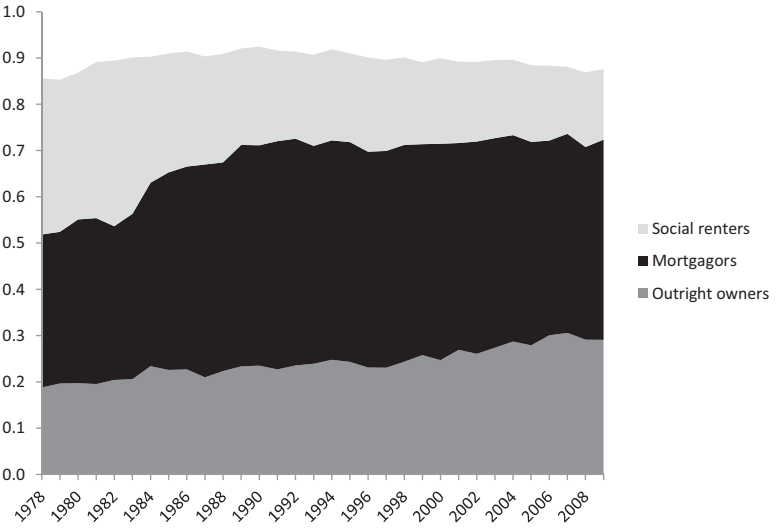

FIGURE 2
Shares of social renters, mortgagors, and outright homeowners.

Before turning to the estimation, it is useful to examine the demographic properties of our pseudo-cohorts. For each group, Figure 3 shows kernel density estimates of age (*i.e.* the difference between interview year and household head's birth year) and weekly household real income per adult as well as the shares of households with different education levels and the shares of households with positive non-mortgage debt. Mortgagors were born on average in later years (so tend to be younger in our sample), tend to be more educated and appear relatively richer. It is worth noting, however, that the distributions of the three tenure groups overlap significantly.[10] In particular, the estimated densities for both groups of homeowners are characterized by a long right tail. This means that average income of outright owners is relatively closer to the average income of mortgagors rather than social renters.[11] Finally, the mortgagor group has the largest share of households with positive non-mortgage debt, while outright owners have the lowest.[12]

3. THE HETEROGENEOUS EFFECTS OF TAX CHANGES

In this section, we document significant heterogeneity in the dynamic effects of fiscal policy by reporting the estimated consumption responses across actual housing tenure groups. To assess concerns of possible endogenous changes in group composition, we then show that our findings are not overturned when households are grouped according to their predicted probability (based on a non-linear polynomial in demographics, education and time trends) of having the same housing tenure status in the previous year (which we do not observe in the repeated cross-sections of the FES).

10. A similar picture emerges considering household disposable income.

11. The sub-sample evidence in Figure 8 of Appendix C reveals that the distributions of education and age have not changed much over time. In contrast, income has become more unequal, although this has largely been a feature of the top of the income distribution. Consequently, it has affected the average income of the owners and mortgagors but less so the average income of social renters.

12. This is based on the shorter FES sample 1986–2009 for which non-mortgage debt is available.

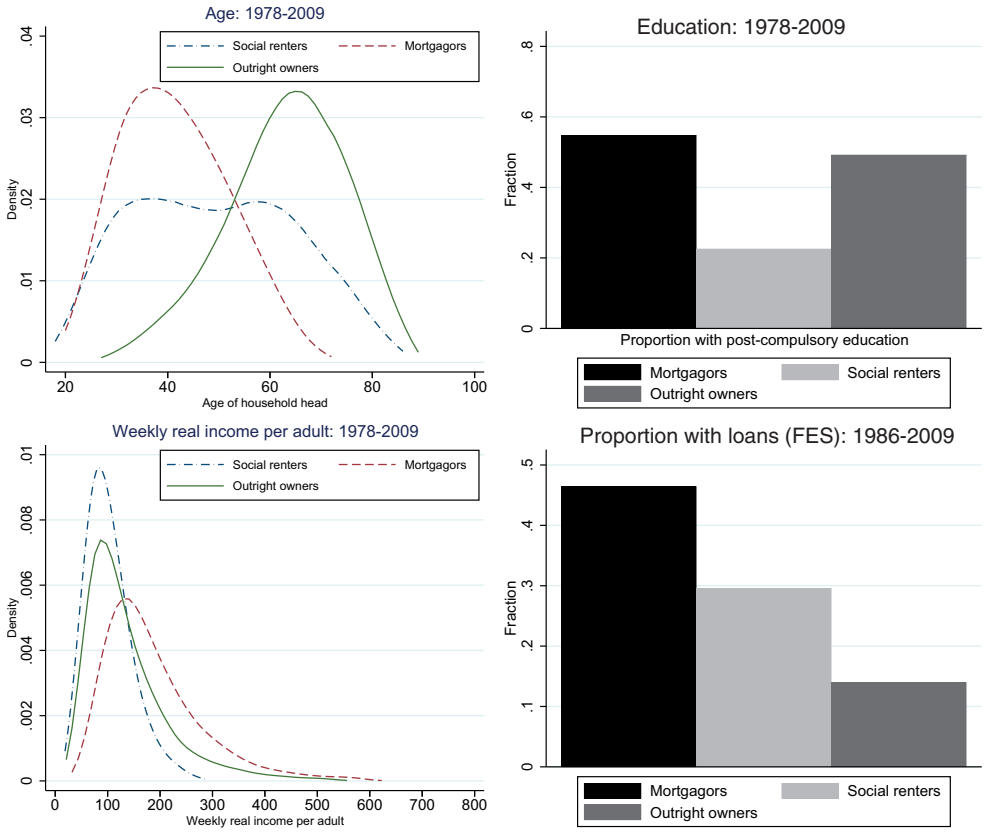

FIGURE 3

Distributions of age and income and proportions of households with post-compulsory education and non-mortgage loans across housing tenures

For each tenure group, we estimate a separate VAR. The VAR includes the change in group-specific consumption, the change in real GDP, the level of the central bank's policy rate and the change in real government spending. We use four lags of these endogenous variables.[13] In line with the empirical literature on narrative measures of fiscal shocks, the first twelve lags and the contemporaneous value of our newly compiled exogenous measure of income tax changes enter the VAR as exogenous variables. The contemporaneous values of the share of households with post-compulsory education and the average difference between interview year and birth year in each quarter are added as further exogenous regressors to control for other unrelated life cycle factors. In Cloyne and Surico (2013), we show that the results below are robust to running the single-equation specification used in Romer and Romer (2010) as well as to using the method of seemingly unrelated regressions.

3.1. *Actual housing tenure*

The point estimates of the dynamic effects of the income tax cut on non-durable expenditure for each housing tenure group are reported in Figure 4 as solid lines with circles. The shaded

13. Similar results are obtained using shorter or longer lag lengths.

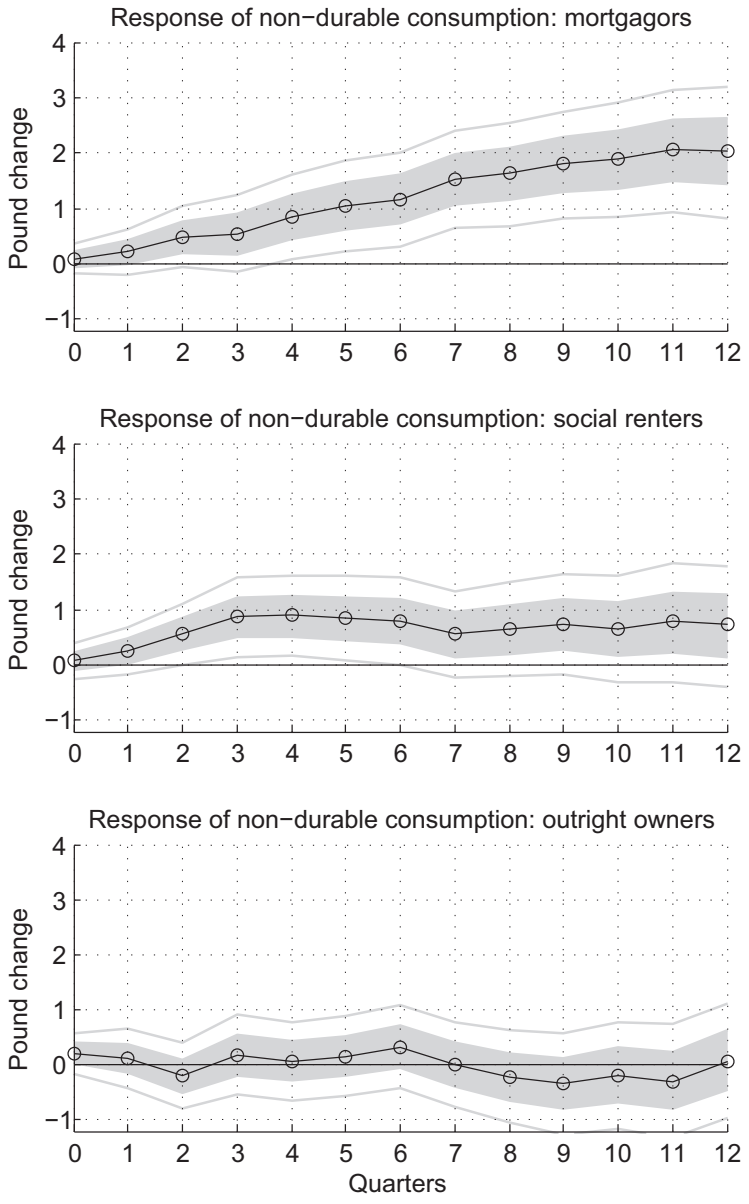

FIGURE 4

Dynamic effects of a per-taxpayer liability change in the allowance and basic rate of income tax on per capita non-durable goods and services consumption across housing tenures using a VAR in non-durable consumption per capita change, real GDP per capita change, real government spending per capita change, and Bank Rate specified as in Section 3. Shaded areas (grey lines) represent 68% (95%) confidence bands over 10,000 bootstrap repetitions. Sample: 1978–2009

area represents the 68% confidence intervals based on 10,000 bootstrap repetitions; the grey lines show the 95% intervals.

Each point of the impulse response function represents the effect on annual consumption following a one pound tax change, relative to what would have happened in the absence of the shock.[14] Since our shock series reflects changes in taxes, this is equivalent to a shock that lowers the level of taxes by one pound *in each* of the 3 years of the forecast period. The response at the end of each year can, therefore, be seen as the fiscal multiplier in that year of the simulation, although—as discussed in Section 2—we do not regard these tax changes as permanent.

The first row shows that the consumption of mortgagors responds significantly at the 5% level beyond the third quarter and reaches a peak around two pounds after 3 years. In contrast, the response of owners without mortgage (in the last row) is never statistically different from zero, even at the 32% significance level, and peaks below 0.4 or 40 pence at six quarters after the shock. Finally, the point estimates in the second row suggest that social renters change their non-durable expenditure by slightly less than one pound, with responses that are significant at 5% between quarters 3 and 6.[15] As owners without a mortgage tend to have a significantly higher gross income than social renters, it seems unlikely that the heterogeneity in Figure 4 is driven by possible heterogeneity in the tax change, although we return to this issue in the sensitivity analysis below.

Turning to the overall magnitudes, the peak effect for mortgagors is equivalent to a peak fiscal multiplier on consumption of 2 in the third year. The associated cumulative multiplier over 3 years (*i.e.* the overall change in consumption relative to the overall changes in taxes) is 1.29. It is important to note that these numbers reflect the general equilibrium effect of the tax stimulus on the economy: Figure 14 Appendix E shows that income also responds strongly (roughly twice as much as mortgagors' consumption), with cumulative multipliers for household income between 2 and 3 depending on the specification. Importantly, income moves for all tenure groups but, as we will show in Section 4, the relative response of consumption is large and significant for mortgagors but not for outright homeowners. In that section, we also show that the absolute magnitudes of our estimates are consistent with the tax multipliers typically found in macroeconomic narrative studies such as Romer and Romer (2010).

In summary, our estimates suggest that housing tenure is highly correlated with the characteristics that drive the heterogenous response of household consumption to an exogenous income tax change. Specifically, whether a household has mortgage debt seems an important candidate for explaining how tax changes affect consumption. In Section 4, we will show that the composition of the mortgagors' asset portfolio—in particular the lack of significant liquid net wealth—could indeed make this group more responsive changes in their income.

3.2. *Propensity score methods*

The shares of social renters, mortgagors, and outright owners have varied slowly over time, as shown in Figure 2. Still, if households have chosen to move to another housing tenure status in response to the fiscal shock, this may distort our inference. Furthermore, the changes in the time series of consumption that we construct for each pseudo-cohort can only make sense in the absence of significant compositional changes across housing tenure groups. To assess the empirical relevance of these concerns, we adapt the methodology proposed by Attanasio *et al*. (2002) to generate individual predicted probabilities of having mortgage debt over a number

14. The response of annual consumption is constructed by cumulating the impulse response function for the annual change in quarterly expenditure, which was estimated using VAR specification discussed above.

15. The finding of heterogeneity across groups is robust to using the log difference of consumption, as opposed to the consumption change. Under this transformation, however, the size of the responses are difficult to interpret as we only observe the tax liability *changes* projected by HM Treasury.

of subsequent periods. 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. For households observed in quarter t, we compute the probability that they had a mortgage four quarters earlier. 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 that period. We then take the difference in consumption across these two quarters for each group.

By running the probit specification over the full sample and opting for a time-varying threshold, the propensity score method of Attanasio *et al*. (2002) is tailored to minimize the effects of endogenous changes across tenure groups and classification errors. However, the composition of the likely mortgagor group may still change either because the probability of ownership changes over time or because the cut-off point changes. To deal with this second set of issues, we follow the recommendations in Attanasio *et al*. (2002) and run also a probit regression for every year of our sample and, more importantly, we use a fixed cut-off equal to the average share of mortgagors in the total population over the full sample, although we have verified that the results below do not hinge upon the specific value chosen. By using a classification threshold that does not vary with time, the method minimizes the possible biases induced by changes in group composition. To make the contrast between outright owners and mortgagors sharper we have excluded renters from this exercise.[16]

The results of this exercise are displayed in Figure 5. The left column reports the estimates that use a full-sample probit regression and a time-varying cut-off while the right column refers to impulse responses that address possible compositional changes using year-by-year probit regressions and a fixed threshold. Consistent with the evidence based on actual housing tenure, the response of the "unlikely" mortgagors is never significant at the 5% level. In contrast, the response of the "likely" mortgagors remains significant at the 5% level after four quarters and, in line with the estimates in Figure 4, peaks at around 2 after 3 years. It is still the case that the 95% confidence bands for the likely mortgagors do not include the point estimates for the unlikely mortgagors after six quarters. We, therefore, conclude that the heterogeneous consumption responses between households with and without mortgage debt is robust to presence of possible endogenous changes in group composition.[17] We return to the issue of compositional change in Section 5.

Income also responds for both groups, as shown in Figure 14 of Appendix E. In fact, using the propensity score method, the income response is very similar for likely and unlikely mortgagors, reinforcing the result that the differential responses of consumption are not driven by differential movements in income. We discuss this further in Section 4.

3.3. *Sensitivity analysis*

We now conduct a range of sensitivity tests to ensure the robustness of our main results. For brevity, we list here the main findings and report the full set of results in the Appendix.

16. In Cloyne and Surico (2013), we report similar but less precise estimates when including renters.

17. In the Online Appendix, we use the yearly panel dimension of the BHPS between 1991 and 2009—when the BHPS starts and when our sample ends, respectively—to verify that our tax shocks do not have statistical power predicting whether a household changes housing tenure. Similarly, using the FES over our full sample at quarterly frequency, our tax shocks do not predict whether a household belongs to a specific tenure group. Furthermore, in both regressions, the coefficients on age, family size, and educational attainment are very significant but the adjusted R^2 is never above 0.04 for the BHPS and 0.2 for the FES.

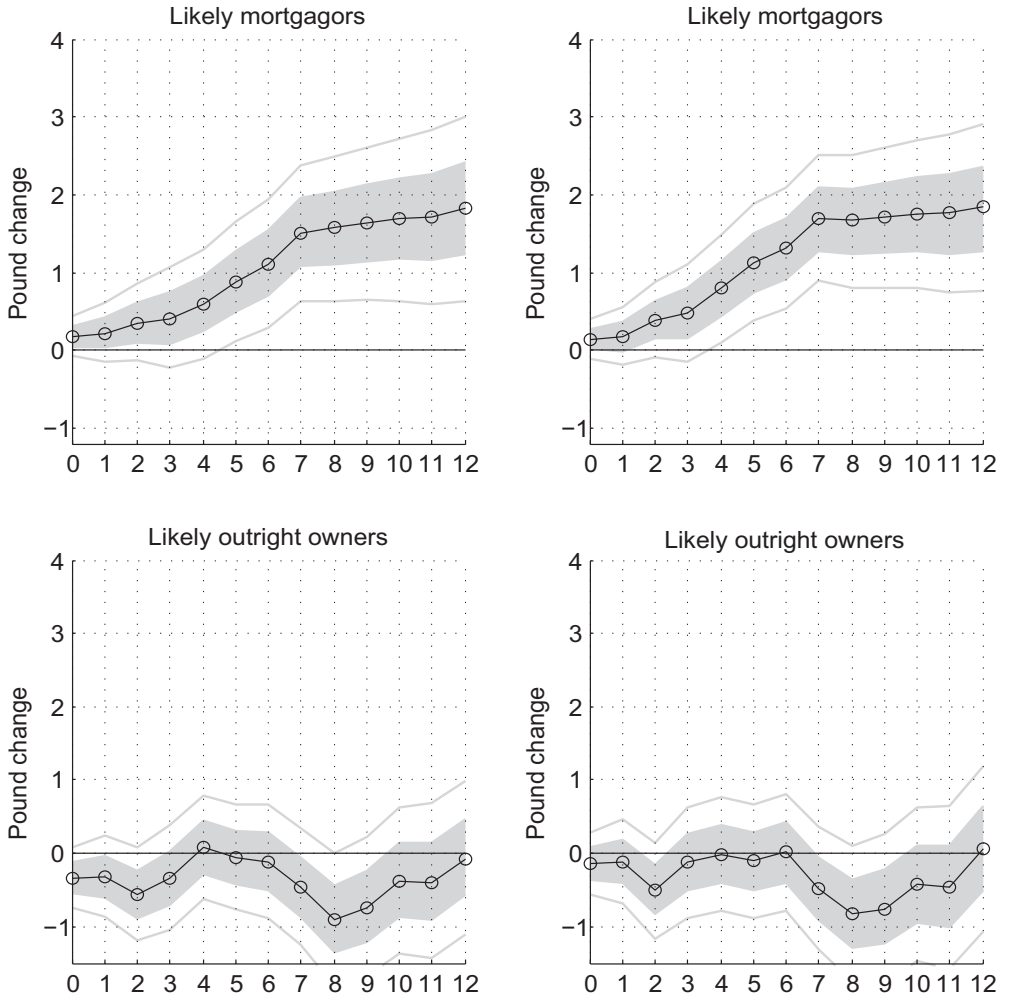

FIGURE 5

Dynamic effects of a per-taxpayer liability change in the allowance and basic rate of income tax on per capita non-durable goods and services consumption across predicted housing tenures (based on probit estimation) using a VAR in non-durable consumption per capita change, real GDP per capita change, real government spending per capita change, and Bank Rate. Left (right) column refers to full-sample (year-by-year) probit and time-varying (fixed) classification cut-off for 'likely' and 'unlikely' mortgagors. Shaded areas (grey lines) represent 68% (95%) confidence bands over 10,000 bootstrap repetitions. Sample: 1978–2009

3.3.1. Other tax changes. In considering a subset of tax changes, one possible concern is that these are correlated with other tax changes whose omission may then distort our inference. In Figure 9 of the Appendix, we show that our findings are robust to adding the first twelve lags and the contemporaneous values of any *other* exogenous tax changes identified by the narrative method. Furthermore, we have verified that our shock series does not trigger a significant response of these other tax changes over the 3-year forecast period.

3.3.2. Anticipation. One issue is whether the tax changes we consider were anticipated. In Figure 10, we, therefore, re-estimate the baseline specification but use the "unanticipated" component of our exogenous income tax changes. We follow Mertens and Ravn (2012) by defining an unanticipated change as one that was implemented within 90 days of announcement. The evidence from this exercise suggests that our earlier findings are broadly confirmed.[18]

3.3.3. Sub-sample stability. Another sensitivity test is to analyse whether the shape of the responses for each group varies over time. In Figure 11, we show that this is not the case by looking at estimates over increasingly shorter samples that start in 1986, 1988, 1990, and 1992, respectively. These years have been chosen as the beginning of the sub-samples because, according to Figure 2, the largest changes in the housing tenure status occurred between the 1980s and the early 1990s.

3.3.4. Spending categories. Figure 12 shows that excluding semi-durable categories (such as "apparel", "health", and "reading") from non-durable goods and services in the second column of "strictly non-durables" does not affect the heterogeneity or the size of the consumption responses, suggesting our main findings are not driven by the more durable component of non-durable expenditure. In addition, we record larger (and far less precise) point estimates for the responses of non-housing expenditure, but these do not seem statistically different from the responses in Figure 4. Interestingly, Figure 12 also shows that spending on necessities such as food hardly responds across the three housing tenure groups.

3.3.5. Size of the tax change. Finally, to examine the extent to which our findings may reflect (omitted) heterogeneity in income, we use the cohort-specific average level of income to construct a measure of the exogenous tax liability change that reflects possible variation in the tax base and, therefore, in the average income tax rate across housing tenure groups. Figure 13 shows that our earlier findings of heterogeneity are robust also to this exercise.

4. EXPLORING THE ROLE OF MORTGAGE DEBT

In the previous section, we documented substantial heterogeneity in the consumption responses to tax changes. In this section, we provide evidence that our findings are consistent with a model where liquidity constraints are associated with having mortgage debt.

We begin by summarizing some of the main testable predictions from a recent theoretical framework developed by Kaplan and Violante (2014) and then confront these predictions with the data. In this model, there are transactions costs associated with accessing illiquid wealth, and this would make households with mortgage debt more responsive to income tax changes. We go on to show that our estimates are consistent with the magnitudes reported in earlier empirical and theoretical contributions on the effects of fiscal policy. In contrast, in the next section, we argue that alternative hypotheses, including demographics, impatience, rational inattention, risk aversion, and compositional changes are unlikely to fit all our findings.

18. Unfortunately, there are only seven tax changes over our full sample which can be deemed as anticipated—too few to be able to carry out a similar analysis for this subgroup.

4.1. *Insights from a model of hand-to-mouth households*

A popular explanation for the sizeable effects of fiscal policy found in the empirical literature is that a fraction of households are hand-to-mouth, meaning they consume a significant fraction of any additional income they receive. The conventional wisdom is that these households are typically renters, have a young household head, have lower educational attainment, and are on a low income. While this view is often entertained in policy and academic circles, it has been shown that for an empirically plausible fraction of these households (typically around 10%), the standard one-asset model has a hard time replicating the sizeable consumption response of households to a fiscal stimulus (once the model distribution of net worth is calibrated to match the distribution observed in the data).

To tackle this challenge, Kaplan and Violante (2014) propose a partial equilibrium model of consumption in which households can store wealth in two forms: a liquid asset (such as bank accounts) and an illiquid asset (such as housing). The crucial feature of their framework is that the illiquid asset carries an exogenously higher rate of return but can be accessed only by paying a transaction cost. When this assumption is built into an incomplete-markets life cycle economy, Kaplan and Violante (2014) show that a significant fraction of households optimally choose to spend a large fraction of any positive change in their income, despite holding sizeable amounts of illiquid wealth. The intuition is that the welfare loss of not smoothing consumption turns out to be smaller than the cost of accessing their illiquid wealth or of holding large balances of cash and foregoing the higher return on the illiquid asset. They refer to these agents as "wealthy hand-to-mouth".

4.2. *Testable predictions*

Using a quantitative model parameterized to replicate a number of life cycle and cross-sectional features of advanced economies (see also Kaplan *et al*., 2014), a numerical implication of the analysis in Kaplan and Violante (2014) is that for transaction costs close to $1000, the "wealthy hand-to-mouth" households display a marginal propensity to consume (MPC) around 0.43 while unconstrained agents are characterized by a MPC of about 0.07. This specific value for transaction costs seems in line with both their favourite estimate for the U.S. and our back of the envelope calculation of £700 for the U.K. (Cloyne and Surico, 2013). The Kaplan–Violante model also generates a further theoretical prediction: the consumption response of "wealthy hand-to-mouth" households should only be large when the income change is small relative to the transaction costs of assessing their illiquid wealth. In the face of sufficiently large income changes, in contrast, households are better off paying the transaction costs and re-optimizing their plans, producing a MPC close to zero. In this case, households pay the extra resources into their illiquid asset despite the existence of transaction costs. In this section, we evaluate both of these testable predictions.

4.2.1. Liquid versus illiquid net wealth. Traditional explanations for a significant consumption response to an unexpected income change emphasizes net wealth as an important driver of heterogeneous behaviour. In short, wealthier households are less likely to be liquidity constrained. Since this argument is typically made in the context of one-asset models, the academic and policy discussion seems to have implicitly abstracted from the distinction between liquid and illiquid assets. To the extent that most household wealth is held in the form of housing, and, therefore, not immediately accessible, looking at *liquid* net wealth (as opposed to *total*) may shed light on the heterogeneous consumption responses across the three tenure groups.

TABLE 1
Net Wealth Position by Housing Tenure

	Panel A: 2005 BHPS in 2005 Pounds				
	p25	*Median*	*p75*	*Mean*	*Obs*
NET FINANCIAL WEALTH					
Social renters	−400	0	150	470	1,337
Mortgagors	−3,250	0	4,600	3,014	3,179
Outright owners	0	3,000	21,540	18,293	2,385
NET HOUSING WEALTH					
Social renters	0	0	0	0	1,337
Mortgagors	55,000	95,000	150,000	113,257	3,179
Outright owners	100,000	150,000	230,000	189,385	2,385
	Panel B: 2000 BHPS in 2000 pounds				
	p25	*Median*	*p75*	*Mean*	*Obs*
NET FINANCIAL WEALTH					
Social renters	−300	0	250	959	1,959
Mortgagors	−2,000	82	5,500	4,197	3,234
Outright owners	0	3,500	20,000	15,889	2,009
NET HOUSING WEALTH					
Social renters	0	0	0	0	1,959
Mortgagors	16,000	38,000	71,400	54,572	3,234
Outright owners	50,000	75,000	120,000	97,284	2,009
	Panel C: 1995 BHPS in 1995 pounds				
	p25	*Median*	*p75*	*Mean*	*Obs*
NET FINANCIAL WEALTH					
Social renters	−50	0	400	1,185	944
Mortgagors	−750	193	3,690	4,209	2,024
Outright owners	25	3,000	16,540	15,711	1,054
NET HOUSING WEALTH					
Social renters	0	0	0	0	944
Mortgagors	29,000	10,000	55,000	37,162	2,024
Outright owners	44,000	60,000	85,000	72,069	1,054

Notes: Net financial wealth is the value of savings and investments less outstanding non-mortgage debt. Housing wealth is the household's estimate of the property value net of any outstanding mortgage. *p*25 is 25th percentile and *p*75 is 75th percentile.

Table 1 reports summary statistics for the distributions of financial and housing wealth by tenure group, using the only 3 years for which the BHPS has asked questions on the household's financial position. Following Crossley and O'Dea (2010), net financial wealth is defined as the value of saving and investment net of non-mortgage debt and is meant to provide a measure of the stock of *liquid assets*.[19] Net housing wealth is the difference between the property value estimated by the household and the value of any outstanding mortgage.

19. "Saving" includes: savings or deposit accounts, national savings bank accounts and cash ISAs (or TESSAs). "Investment" comprises: national savings certificates, premium bonds, unit trusts/investment trusts, stocks and shares ISAs (or PEPs), shares, national savings bonds (capital, income, or deposit) and other investments (gilts, government, or company securities). "Non-mortgage debt" refer to: hire purchase agreements, personal loans, credit and store cards, catalogue or mail order purchase agreements, Department for Work and Pensions (DWP) social fund loans, overdrafts and student loans.

Three important findings emerge from Table 1. First, social renters—who account for about 20% of the sample—are characterized by little *liquid* financial net wealth and no housing wealth. Together with the fact that they tend to be younger than the other groups and have a higher proportion of households with compulsory education only, social renters appear to fit the traditional stereotype of liquidity constrained households. Secondly, outright owners—who make up around 25% of the population—score high in both financial and housing wealth and seem unlikely to face significant credit constraints. Thirdly, mortgagors—approximately 45% of the population—seem in-between the other two groups as they have low *liquid* net wealth but high housing wealth. Indeed, in each of the 3 years, more than 50% of mortgagors hold either non-positive financial net wealth or only a small positive amount. As the vast majority of mortgagors have at least some equity in their house, their *total* net wealth tends to be high, although it might not be immediately accessible. The mortgagor group, therefore, seems to fit well the characteristics of wealthy hand-to-mouth households.

4.2.2. Small versus large tax changes. To verify whether mortgagors are indeed "wealthy" hand-to-mouth as in Kaplan and Violante (2014), Figure 6 shows the response of non-durable consumption for this tenure group to "smaller" and "larger" tax changes. For any given quarter, we define a change in our tax change series as "smaller" ("larger") if the associated household income change over the subsequent 3 years is below (above) 1,200 pounds in 2009 prices. While the results below are not specific to this value, the choice of the 1,200 pounds cut-off allows us to split our baseline measure into two groups whose averages are sufficiently far apart, at about 500 pounds and 1,500 pounds, respectively.

A key finding from Figure 6 is that mortgagors significantly adjust their non-durable consumption only in response to "smaller" tax cuts. The response to "larger" changes, in contrast, is never statistically positive. Furthermore, the difference between the two dynamic effects is sizeable and often significant, especially over the second year and towards the end of the forecast period. The dashed blue line with squares repeats the point estimates for mortgagors' from Figure 4. The comparison between the blue line with squares and the black lines with circles reveals that the baseline response is always above the point estimates of the response to the *larger* tax changes (as well as outside the associated 95% confidence band) but it is typically below the point estimates of the response to the *smaller* tax changes (becoming even statistically different over part of the second year forecast horizon). In summary, the size of the income change does appear to trigger different consumption behaviour.[20] An interesting implication of this finding is that, for a "wealthy hand-to-mouth" household, there may be an optimal tax cut size to stimulate the economy.

4.2.3. Quantitative assessment. To explore further the interpretation of mortgagors as "wealthy hand-to-mouth" households, we compare the magnitude of the cumulated response of consumption relative to the cumulated response of income implied by our estimates to the magnitude of the MPCs reported in the partial equilibrium model of Kaplan and Violante (2014). It is important to note that, however, that the significant response of income for all housing tenure groups, reported in Appendix E, strongly suggests that the overall magnitudes of both the consumption and income responses reflect (at least partially) the general equilibrium effect of the tax cut.

20. We also repeated this exercise for outright owners and social renters, finding little evidence for a significantly different response to the two types of tax changes for these housing tenure groups.

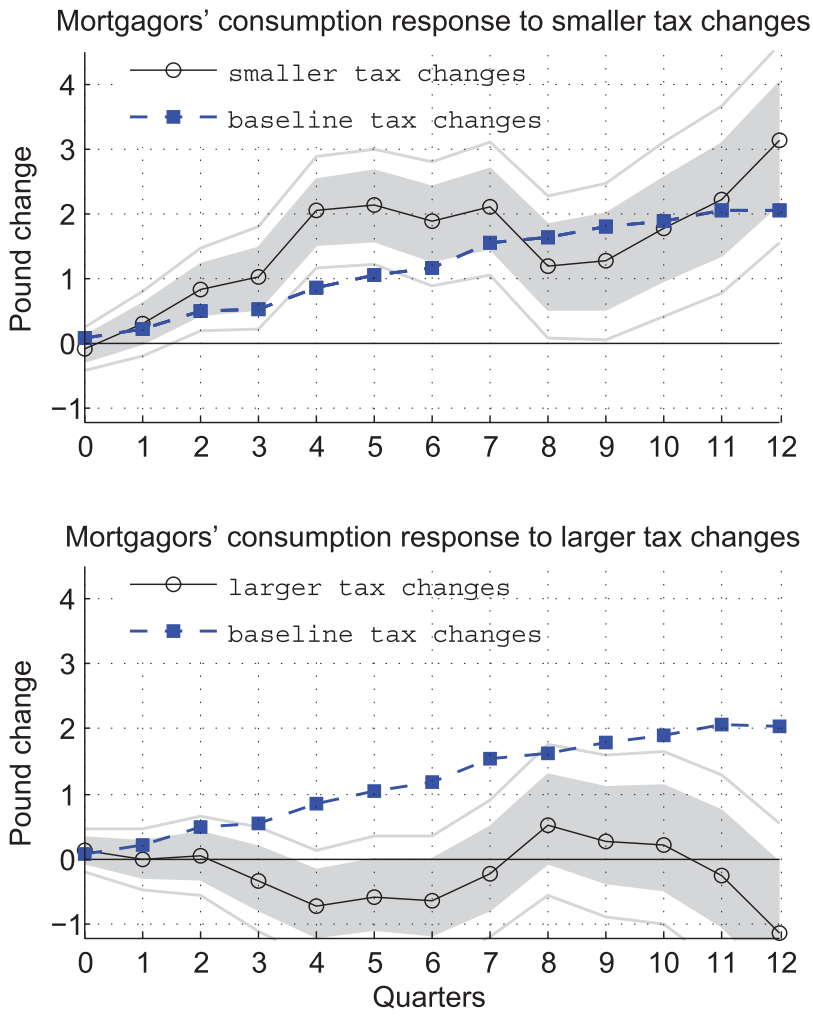

FIGURE 6

Dynamic effects of a per-taxpayer liability change on per capita non-durable goods and services consumption for mortgagors using a VAR in non-durable consumption per capita change, real GDP per capita change, real government spending per capita, change, and Bank Rate. An average tax change in the "smaller tax changes" ("larger tax changes") category corresponds to a liability change in the personal allowance and the basic rate of about 500 pounds (1,500 pounds) per household over 3 years (at 2009 prices). Shaded areas (solid lines) represent 68% (95%) confidence bands over 10,000 bootstrap repetitions. The blue dashed line with squares refers to the point estimates for the non-durable consumption response of mortgagors in the baseline specification at the top of Figure 4. Sample: 1978–2009

Panel A of Table 2 reports the ratio of the discounted total consumption change to the total discounted income change triggered by the tax change over the 3-year period. Panel B (C), reports the total discounted consumption (income) change over the 3-year period relative to the total discounted tax change (about three pounds in our simulations) simulated over the same

TABLE 2
Quantitative Comparison with Earlier Contributions

	Panel A: Consumption relative to income	
	(likely) mortgagors	(likely) outright owners
PROPENSITY SCORE		
(time-varying threshold)	0.54*** [0.12, 1.55]	0.06 [−0.44, 0.43]
(fixed threshold)	0.57*** [0.22, 1.27]	0.10 [−0.75, 0.66]
ACTUAL TENURE	0.55*** [0.22, 1.49]	0.14 [−3.58, 3.55]
Kaplan–Violante	*0.43*	*0.07*
	Panel B: Consumption relative to taxes	
	(likely) mortgagors	(likely) outright owners
PROPENSITY SCORE		
(time-varying threshold)	1.06*** [0.23, 1.88]	0.12 [−0.63, 0.90]
(fixed threshold)	1.30*** [0.52, 2.11]	0.16 [−0.64, 0.98]
ACTUAL TENURE	1.33*** [0.55, 2.16]	0.10 [−0.62, 0.87]
Romer–Romer	*PCE: 0.89*	
	Panel C: Income relative to taxes	
	(likely) mortgagors	(likely) outright owners
PROPENSITY SCORE		
(time-varying threshold)	1.97*** [0.62, 3.33]	2.13*** [0.87, 3.40]
(fixed threshold)	2.29*** [1.08, 3.53]	1.68*** [0.38, 2.98]
ACTUAL TENURE	2.40*** [0.85, 3.96]	0.70 [−1.00, 2.50]
Romer–Romer	*GDP: 1.78*	

Notes: Panel A reports the ratio of the present value of the non-durable expenditure change to the present value of the disposable income change in the 3-years after the shock. Panel B (C) records the ratio of the discounted total non-durable expenditure (disposable income) change to the discounted total tax change over the same horizon. PCE (GDP) stands for per capita personal consumption expenditure (gross domestic product) from national accounts. The rows for Romer and Romer (2010) refer to the U.S.; similar findings are obtained for the U.K. following Cloyne (2013). Squared brackets display the [5th, 95th] percentiles of the distribution of interest across 10,000 bootstrap repetitions. Asterisks denote significance at the 5% level.

period.[21,22] For the sake of comparison, the row labeled Kaplan and Violante (2014) reproduces the quantitative predictions of their model for hand-to-mouth households (under the column "likely mortgagors") and non-hand-to-mouth households (under the column "likely outright owners") assuming transactions costs around $1,000 (see figure 5(b) in their paper). The rows labeled Romer and Romer (2010) will be discussed in the next section.

The results in Panel A of Table 2 show that for every pound of additional income, mortgagors spend a significant fraction on consumption—around 55 pence—across the three estimation procedures used earlier (based on either predicted probabilities or actual tenure groups).

21. Denoting X_t the annual change of a quarterly variable, the discounted value (relative to the tax change) over a 3-year forecast horizon is $(\beta X_{t+4}+\beta^2 X_{t+8}+\beta^3 X_{t+12})/(\beta+\beta^2+\beta^3)$, where the discount factor is $\beta=1/(1+r)$ with r being the average annual rate of interest.

22. The income responses to the tax change reported in Appendix E are based on the VARs of Section 3, using group-specific income rather than GDP.

Interestingly, the confidence bands around the point estimates for ("likely") mortgagors always include the MPC of 0.43 that Kaplan and Violante (2014) report as a plausible value for the wealthy hand-to-mouth households in their quantitative model. But the same bands never include the MPC of 0.07 for non-hand-to-mouth agents. The response of consumption relative to income for the ("likely") outright owners, in contrast, is never statistically significant and is quantitatively similar to the MPC of the unconstrained households in Kaplan and Violante (2014). We conclude that, once the general equilibrium effects are taken into account, our estimates are consistent with the magnitudes predicted by a quantitative consumption model where transaction costs make it costly for mortgagor-type households to access their illiquid asset.

4.3. *Comparison with the empirical macro literature*

Using a narrative identification approach, a growing body of empirical research in macroeconomics has estimated the response of aggregate consumption and GDP. For example, in the seminal contribution by Romer and Romer (2010) the cumulative multiplier (*i.e.* the cumulative effect on the variable of interest relative to the size of the tax change) is around 0.89 for consumption and 1.78 for output.[23] Their GDP multiplier at the peak is around 3. Cloyne (2013) finds similar magnitudes for the U.K.[24] In Panels B and C of Table 2, we compare the findings of this macro literature with the estimates in our aritcle (for different household groups using survey micro data).

In Panel B, the cumulative consumption multiplier is large and significant only for the ("likely") mortgagors. The associated confidence bands also never include the smaller and insignificant value for the ("likely") outright owners. Interestingly, the estimated response of aggregate personal consumption expenditure (PCE) from Romer and Romer's specification sits in-between the set of point estimates for the two tenure groups. Furthermore, the results in Panel C show that the income multiplier is not quantitatively different between households with debt and households without debt (with the possible exception of the imprecise point estimates based on actual housing tenures) and that the implied cumulative multiplier for GDP from Romer and Romer (2010) appears consistent with the estimates we report for the two groups. Our findings are, therefore, consistent with the absolute effects on consumption and output reported in the empirical macro literature on tax changes.

5. OTHER EXPLANATIONS

Sections 3 and 4 contain two main messages. First, that the consumption of households with mortgage debt is the most responsive to changes in taxes. Secondly, that our qualitative and quantitative findings are consistent with a model of "wealthy hand to mouth" consumers.

One may be concerned, however, about a potential selection issue, namely that mortgage debt is correlated with some other (more structural) trait which makes households more responsive to tax changes. In this section, we, therefore, discuss the extent to which other explanations may fit our findings. Since the behaviour of social renters seems to square with the predictions of traditional liquidity constraint models, we focus here on the differential responses of mortgagors and outright

23. These numbers refer to the cumulative effect on GDP and consumption replicated using Romers' data set available online and the exact empirical specifications used in their aritcle. To make the data more comparable with our micro data, we use per capita series rather than the aggregate series cited in the original article. However, the numbers are very similar when using the non-per capita measures: 0.88 and 1.61 for consumption and GDP, respectively.

24. In Cloyne and Surico (2013), we also find similar results for aggregate consumption using official consumption data and using an aggregate series constructed from our micro data.

owners. In particular, we will show that demographics, rational inattention, impatience, and risk seem to provide less compelling explanations for all our empirical results. Finally, we provide further evidence to show that our results are unlikely to be explained by compositional change.

5.1. *Demographics*

A long-standing approach in micro-econometrics has proxied the presence of liquidity constraints with the household head's birth year and educational attainment. However, whether these more traditional dimensions provide sharper evidence of heterogeneity than housing tenure remains an empirical question and one we tackle in this section.[25]

The answer provided by Figure 7 is based on VAR specifications in which households are grouped depending on whether the head is born after 1955 (first row), between 1930 and 1955 (second row), or before 1930 (third row). The columns then split these pseudo-cohorts further, depending on whether the household head attained only compulsory or also post-compulsory education. The point estimates suggest that the "younger" (born after 1955) and the "middled-aged" more educated tend to change their non-durable consumption by a larger amount than the other groups. But the heterogeneity reported in Figure 7 appears more muted and far less precise than the heterogeneity in Figure 4 where housing tenure was associated with the presence of liquidity constraints.

Why might grouping by housing tenure deliver clearer evidence of heterogeneity in the response of consumption?[26] To answer this, it is useful to revisit the demographic statistics in Figure 3. Specifically, we compute the shares of mortgagors and outright owners within each birth year/education pseudo-cohort. These shares are also reported in each panel of Figure 7. On the one hand, the largest consumption responses using the birth year/education split occur for cohorts characterized by the largest share of mortgagors. On the other hand, in none of the panels in Figure 7 is the share of mortgagors greater than 70%, which potentially explains the larger confidence intervals relative to Figure 4: each birth year/education pseudo-cohort pools together households with different debt positions and thus different consumption responses. In Appendix F, Figures 15–17 show that similar results and interpretations arise when we group households by birth year and education separately.[27]

5.2. *Rational Inattention*

An alternative explanation for the mortgagors' differential reaction to small and large tax changes might be provided by rational inattention. To the extent that the "smaller" changes are small relative to household income, mortgagors may rationally choose to pay little attention to the extra resources and not re-optimize their consumption-saving plan. On the other hand, as we have already mentioned, the response of outright homeowners does not vary significantly with the size of the tax change despite this group having an average income that is only slightly lower than the mortgagors'. We conclude that rational inattention is unlikely to explain all our findings.

25. A further advantage of grouping households by housing tenure, relative to using birth year, liquidity, leverage or, income is that we do not need to take a stand—prior to estimation—on the specific (and somewhat arbitrary) threshold levels below which a household is considered to be, for example, younger, poorer, or more levered.

26. The considerable variation in the rate at which different birth cohorts transition into home ownership reported in Bottazzi *et al*. (2010) appears another plausible candidate to reconcile the estimates based on the two grouping strategies in Figures 4 and 7.

27. In that Appendix, we also show that (1) older mortgagors (born before 1955) still respond more than older outright owners (also born before 1955), (2) the response of mortgagors is significant for both "younger" and "older" cohorts, but (3) the response of outright owners is never significant.

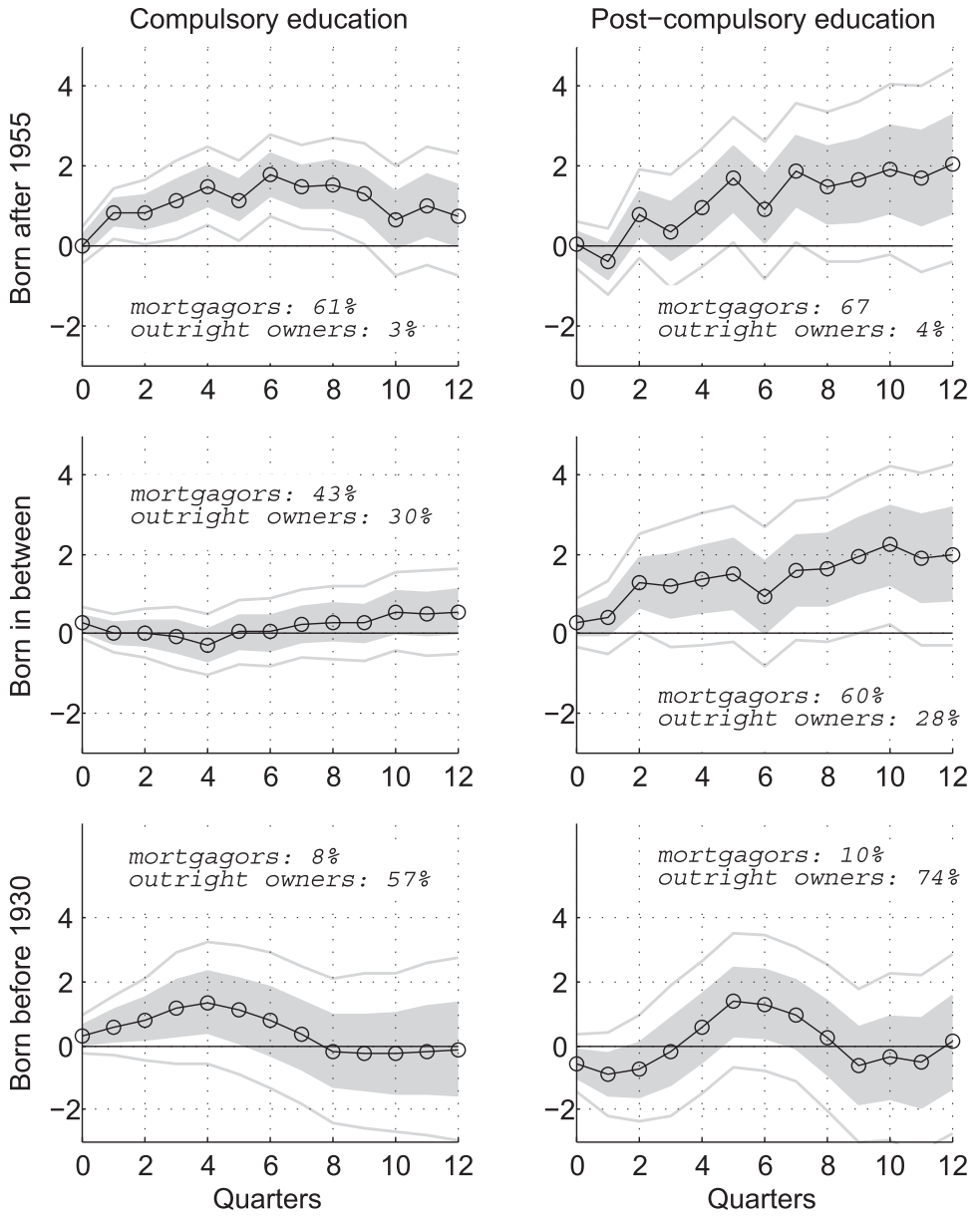

FIGURE 7

Dynamic effects of a per-taxpayer liability change in the allowance and basic rate of income tax on the change in per capita non-durable goods and services consumption across birth cohorts and education levels using a VAR in non-durable consumption per capita change, real GDP per capita change, real government spending per capita change, and Bank Rate. Shaded areas (grey lines) represent 68% (95%) confidence bands over 10,000 bootstrap repetitions. Sample: 1978–2009. Percentages refer to the share of mortgagors and the share of outright owners within each birth year/education cohort

5.3. *Impatience and risk aversion*

Impatience and risk aversion may play a role in explaining why mortgagors change their consumption significantly following a tax change. However, these explanations do not seem to easily explain why mortgagors respond more to smaller income tax changes than to larger tax changes. As a result, these hypotheses seem to square less well with our results than housing tenure.

5.4. *Compositional changes*

Even if one can construct a consistent time series of consumption changes for each cohort (as we have argued in Section 3), a change in the composition of each tenure group may still affect the interpretation of our estimates. This could be the case, for instance, if a significant sub-set of social renters took advantage of the "Right-to-Buy" scheme launched by the Conservative Party during the 1980s, which allowed those living in accommodation rented from local authorities or housing associations to purchase their house at a subsidized price.

To explore this compositional change interpretation of our evidence, we perform two additional exercises. First, in the Online Appendix we ask how fast and large such a compositional change would need to have been to account for the heterogeneity documented in this article. The bottom line of this thought-experiment is that it is very hard to come up with a compositional change that could rationalize the response of mortgagors without generating predictions for the other groups that are largely at odds with the rest of our evidence.

In the second exercise, we restrict our focus to mortgagors with income above the median value of their group in each quarter. This sub-set of households with debt is the most likely to be populated by "genuine" mortgagors (as opposed to social renters who became mortgagors because of the government incentives). The results in the Figures 18 of the Appendix confirms the finding in the top panel of Figure 4, consistent with the view that compositional changes seem unlikely to account for our findings.

6. CONCLUDING REMARKS

Recent years have witnessed a renewed interest in the role of household debt in the transmission of macroeconomic shocks. Theoretical studies have formalized the idea that some agents may become liquidity constrained by purchasing a large illiquid asset such as housing. A main implication of this ongoing research effort is that, following an exogenous change in taxes, households with mortgage debt could increase their consumption by more than those without.

Our results are strongly suggestive of the notion that tax cuts affect consumption mostly by relaxing liquidity constraints for indebted households. In particular, we are able to document that the data line up with a number of qualitative and quantitative predictions of models stressing illiquidity problems faced by households who have significant positive net wealth. However, because mortgage status is not randomly assigned, there are potential selection concerns that imply our analysis may not be fully conclusive. Identifying, or engineering, an empirical setting where mortgagor versus outright owner status is randomly assigned appears to be a formidable challenge, but definitive proof that mortgage status is the cause of households' response to tax cuts may have to await for such a feat to be accomplished. Nevertheless, the result that a household's debt position is a strong predictor of its consumption response to tax cuts has potentially far-reaching policy implications.

APPENDIX

A. THE U.K. INCOME TAX SYSTEM

In the U.K., income tax accounts for around 25–30% of total government revenue and consists of a set of allowances, bands of income and marginal tax rates that apply to each income band. Each individual has a personal allowance which is deducted from their income to calculate their "taxable income". An income taxpayer is, therefore, someone who earns more than their allowance (although there is a system of tax credits that lowers the tax liability of the poorest taxpayers). In the tax year 2012–3, for instance, the first 34,370 pounds above the allowance was subject to a 20% cent rate, any further earnings up to 150,000 pounds were subject to a 40% rate and then 50% was charged on all earnings over £150,000. These bands and allowances are increased each year in line with inflation unless the U.K. Parliament decides otherwise. We do not treat automatic inflationary increases as tax shocks in our data set.

B. DATA DESCRIPTION

B.1. *Aggregate data*

- *GDP and government spending data*: U.K. Office for National Statistics codes ABMI, BKTL, and NMRY. *U.K. Total Population* data are from Eurostat. *FTSE* data are the FTSE All Share Index from Bloomberg.
- *Price index*: Retail Prices Index excluding mortgage interest payments (RPIX), ONS codes CHMK and CDKQ (extended back from 1976 using the growth in the Retail Prices Index, CZBH).
- We scale our tax measure by the total number of taxpayers, available from Her Majesty's Revenue and Customs' website (www.hmrc.gov.uk).
- Data on income tax thresholds used for constructing our final micro data set are from the Institute for Fiscal Studies (www.ifs.org.uk). See the text for more discussion.
- Data sources for the construction of our income tax shocks can be found in the Online Appendix. The tax series also makes use of tax shock data from Cloyne (2013).

B.2. *Household micro data*

We use the FES (later called the Expenditure and Food Survey and, recently, the Living Costs and Food Survey) from 1978 to 2009. These data are available from the U.K. Data Archive.

B.2.1. Household consumption expenditure.

- *Non-durable goods and services expenditure*: 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.
- *Durable expenditure*: durable household goods, motor vehicles, and durable leisure goods. This includes expenditure such as furniture and furnishings, electrical appliances, and audio-visual equipment.
- *Total non-housing expenditure*: total expenditure minus housing expenditures (including rents, rates, and water charges).
- We gross-up using household weights and divide by the number of household members to construct a per capita measure. Where the weights are not directly available in the FES, we use household weights constructed by the Institute for Fiscal Studies.

B.2.2. Housing tenure. The FES records the tenure status of households. Social renters are defined as those living in local authority housing or accommodation provided by housing associations. Mortgagors and owners are taken directly from the FES. The private renters category is dropped due to the limited number of observations.

B.2.3. Demographics and other loans. The demographic variables are taken from the FES: age, education, and employment status of the head of household. The data on non-mortgage loans is a binary indicator for whether the household also has another type of loan other than a mortgage.

B.2.4. BHPS. The BHPS is available from the U.K. Data Archive.

C. DEMOGRAPHICS OVER TIME

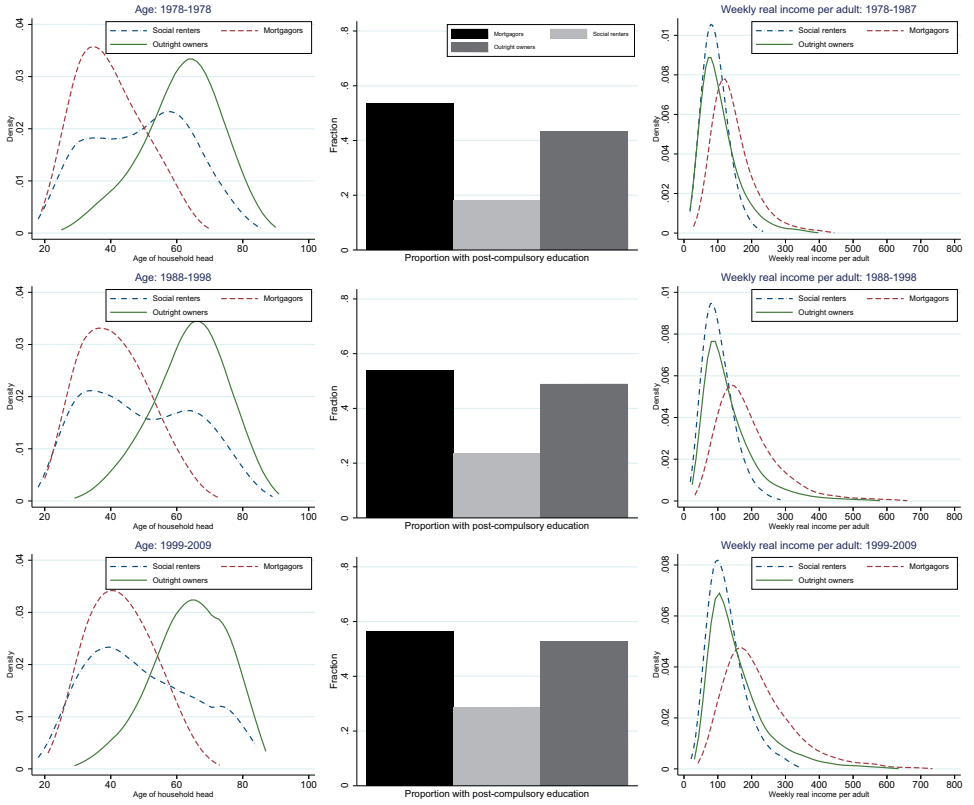

FIGURE 8

Evolution of age, education, and income distributions across housing tenures.

D. SENSITIVITY ANALYSIS

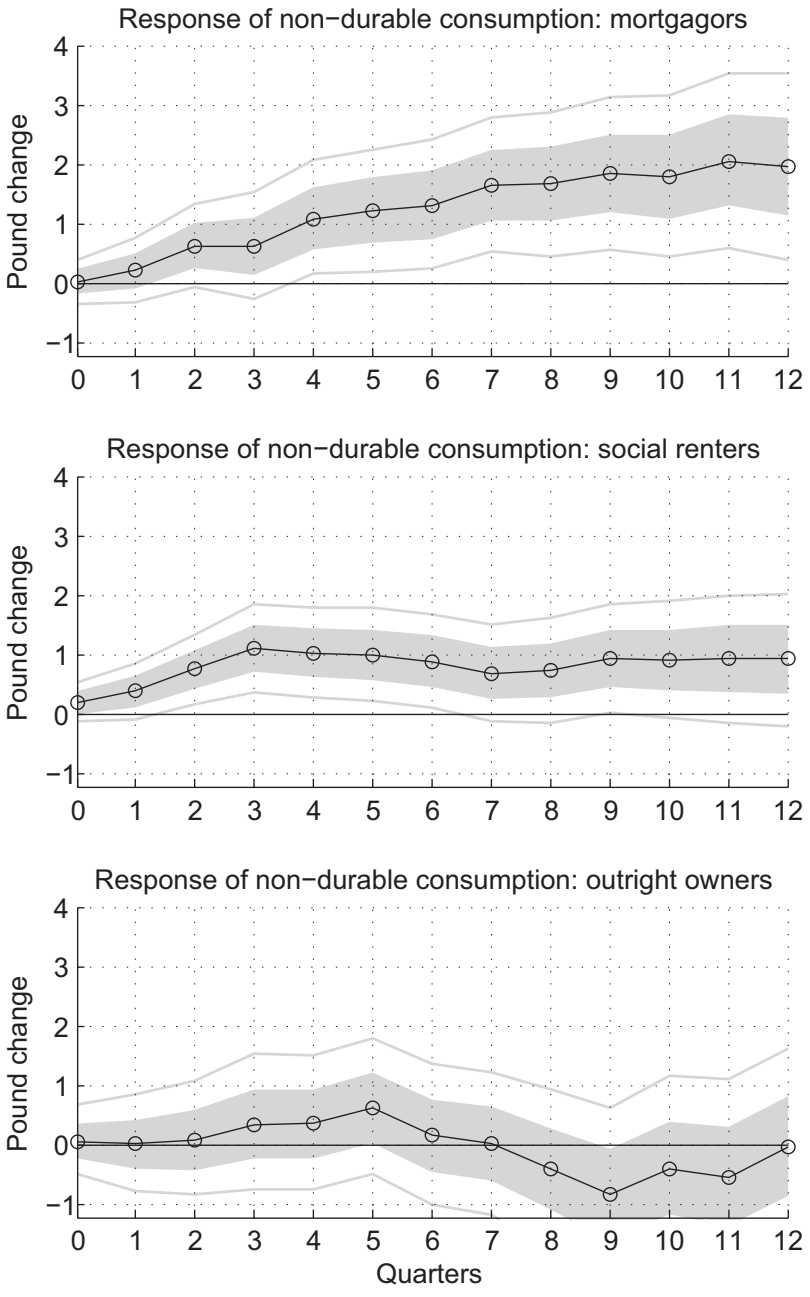

FIGURE 9

Dynamic effects of a per-taxpayer liability change in the allowance and basic rate of income tax on per capita non-durable goods and services consumption across housing tenures using a VAR in non-durable consumption per capita change, real GDP per capita change, real government spending per capita change, Bank Rate, and other exogenous tax changes. Shaded areas (grey lines) represent 68% (95%) confidence bands over 10,000 bootstrap repetitions. Sample: 1978–2009

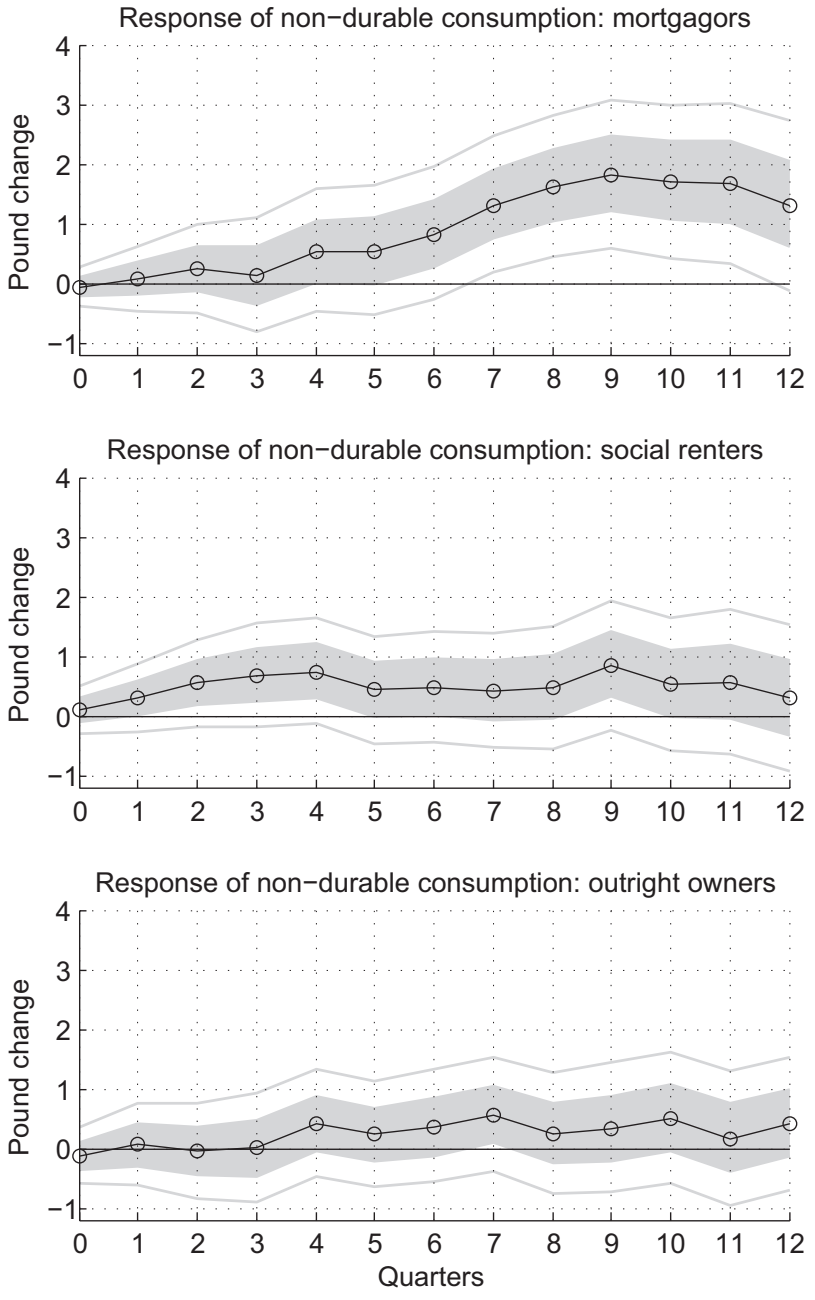

FIGURE 10

Dynamic effects of an "unanticipated" per-taxpayer liability change in the allowance and basic rate of income tax on per capita non-durable goods and services consumption across housing tenures using a VAR in non-durable consumption per capita change, real GDP per capita change, real government spending per capita change, and Bank Rate. "Unanticipated" changes refer to exogenous tax changes that were implemented within 90 days of announcement. Shaded areas (grey lines) represent 68% (95%) confidence bands over 10,000 bootstrap repetitions. Sample: 1978–2009

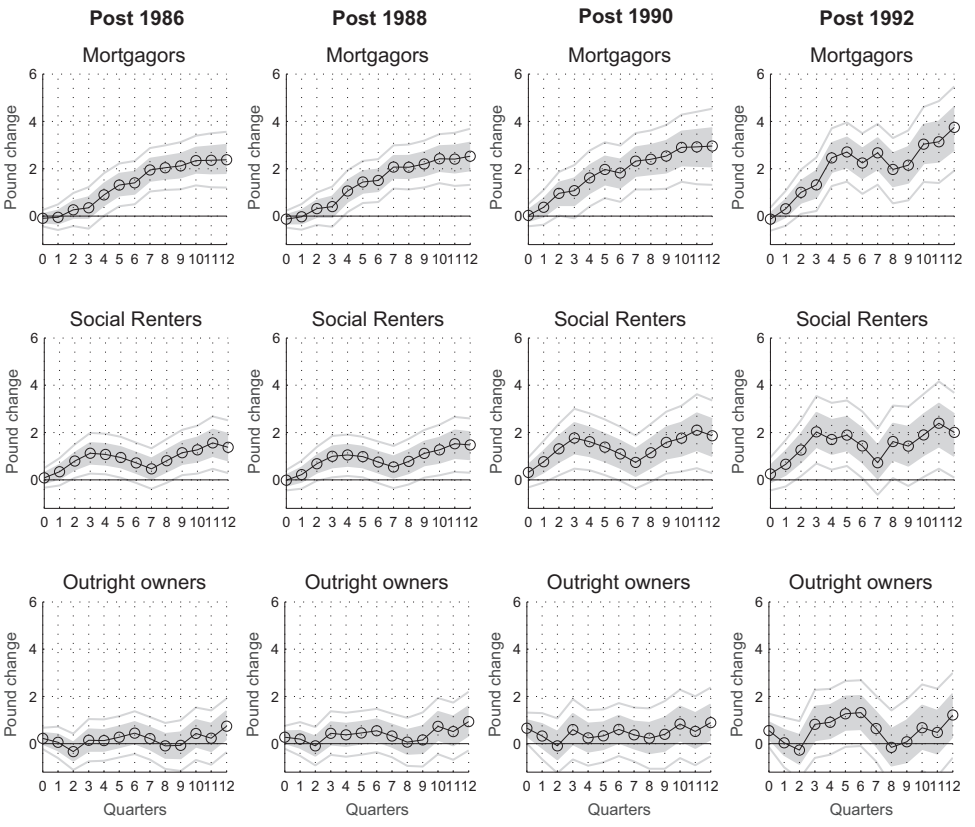

FIGURE 11

Sample stability. Dynamic effects of a per-taxpayer liability change in the allowance and basic rate of income tax on per capita non-durable goods and services consumption across housing tenures over different sub-samples. First column: 1986–2009; second column: 1988–2009; third column: 1990–2009; fourth column: 1992–2009. Shaded areas (grey lines) represent 68% (95%) confidence bands over 10,000 bootstrap repetitions

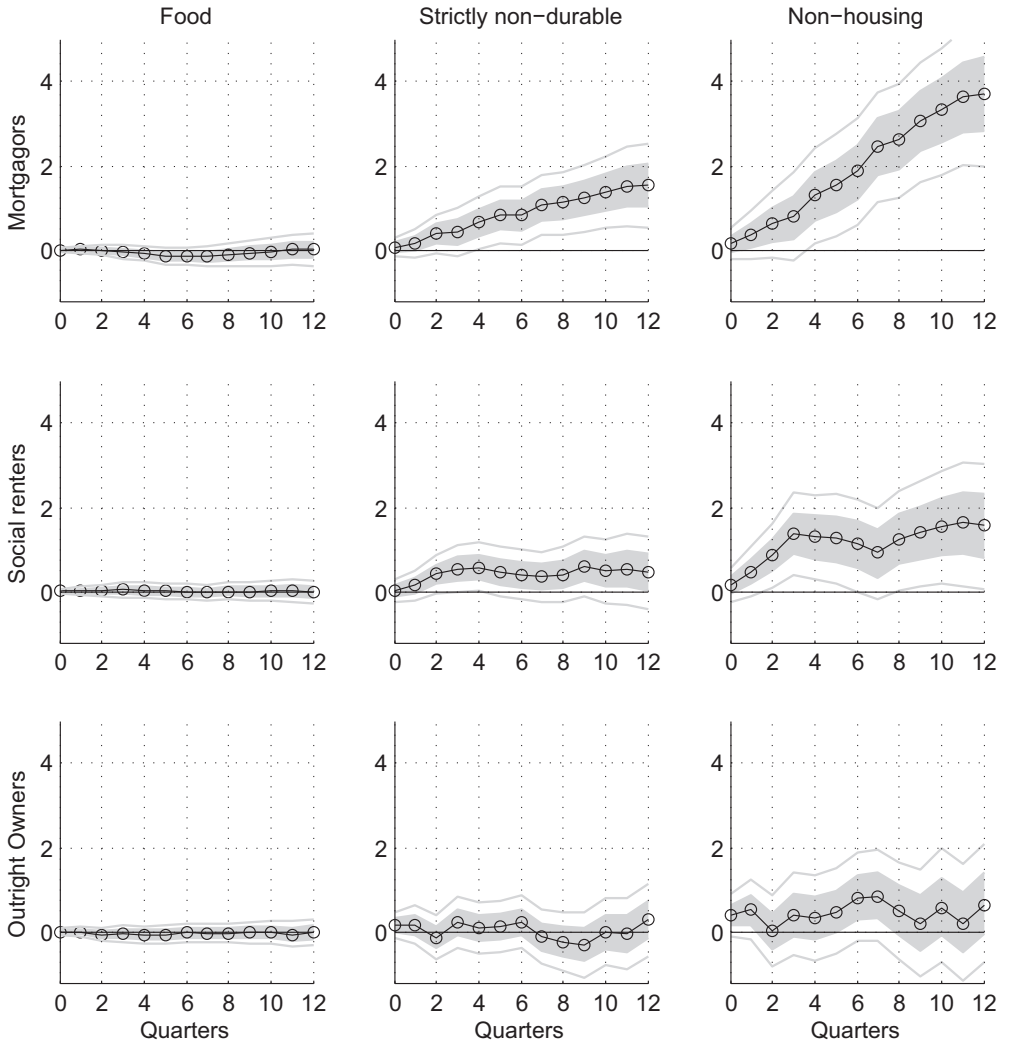

FIGURE 12

Dynamic effects of a per-taxpayer liability change in the allowance and basic rate of income tax on per capita expenditure on food, strictly non-durable and non-housing goods, and services across housing tenures using a VAR in non-durable consumption per capita change, real GDP per capita change, real government spending per capita change, and Bank Rate. Shaded areas (grey lines) represent 68% (95%) confidence bands over 10,000 bootstrap repetitions. Sample: 1978–2009

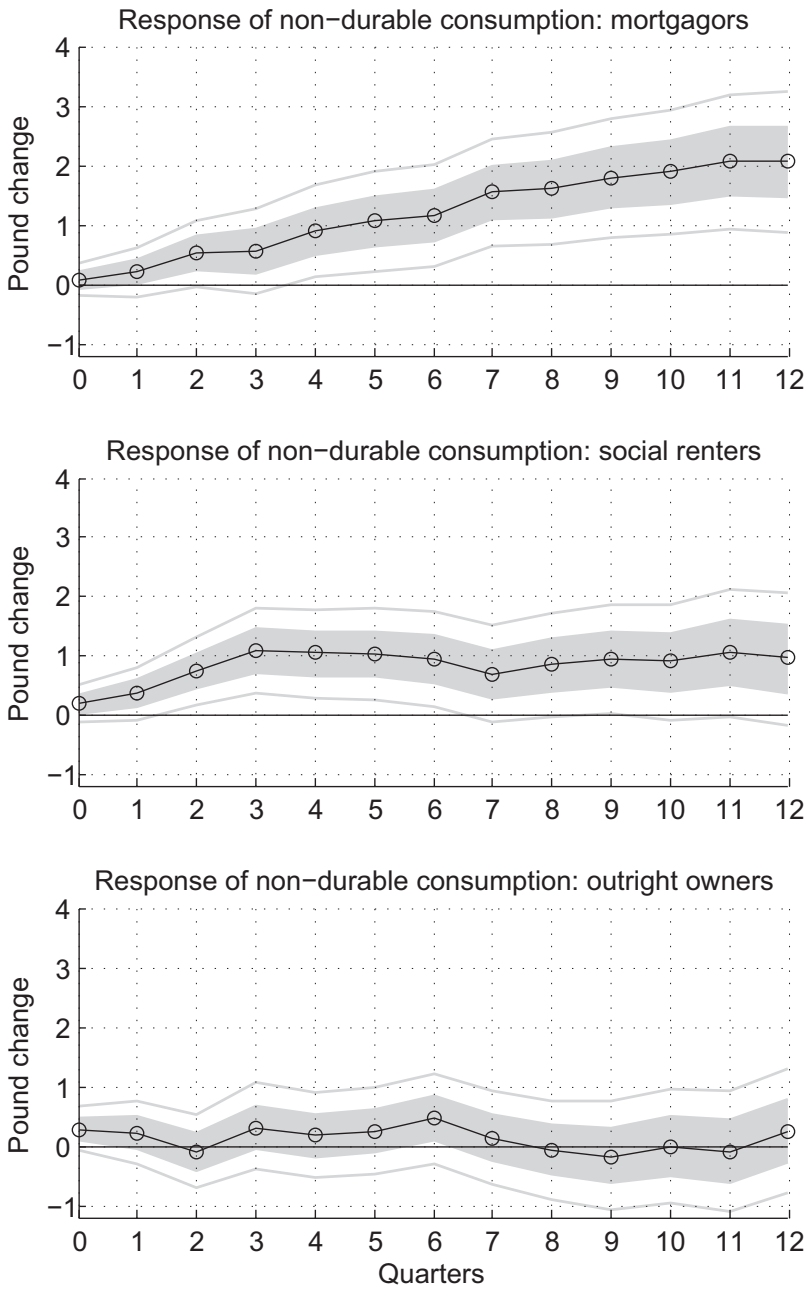

FIGURE 13

Dynamic effects of a per-taxpayer liability change in the allowance and basic rate of income tax on per capita non-durable goods and services consumption across housing tenure using a VAR in non-durable consumption per capita change, real GDP per capita change, real government spending per capita change, and Bank Rate, controlling for the amount of the tax change. Shaded areas (grey lines) represent 68% (95%) confidence bands over 10,000 bootstrap repetitions. Sample: 1978–2009

E. INCOME RESPONSES

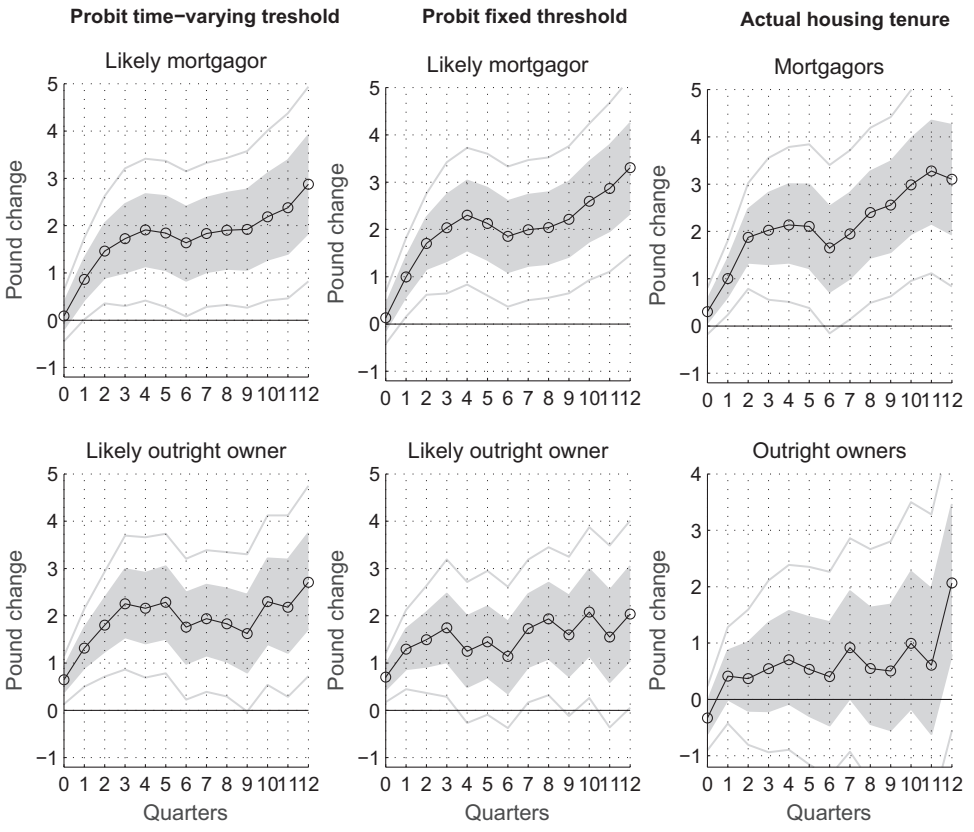

FIGURE 14

Dynamic effects of a per-taxpayer liability change in the allowance and basic rate of income tax on per capita income across housing tenures using a VAR in non-durable consumption per capita change, real per capita income change, real government spending per capita change, and Bank Rate. Shaded areas (grey lines) represent 68% (95%) confidence bands over 10,000 bootstrap repetitions. Sample: 1978–2009

F. FURTHER RESULTS ON DEMOGRAPHICS

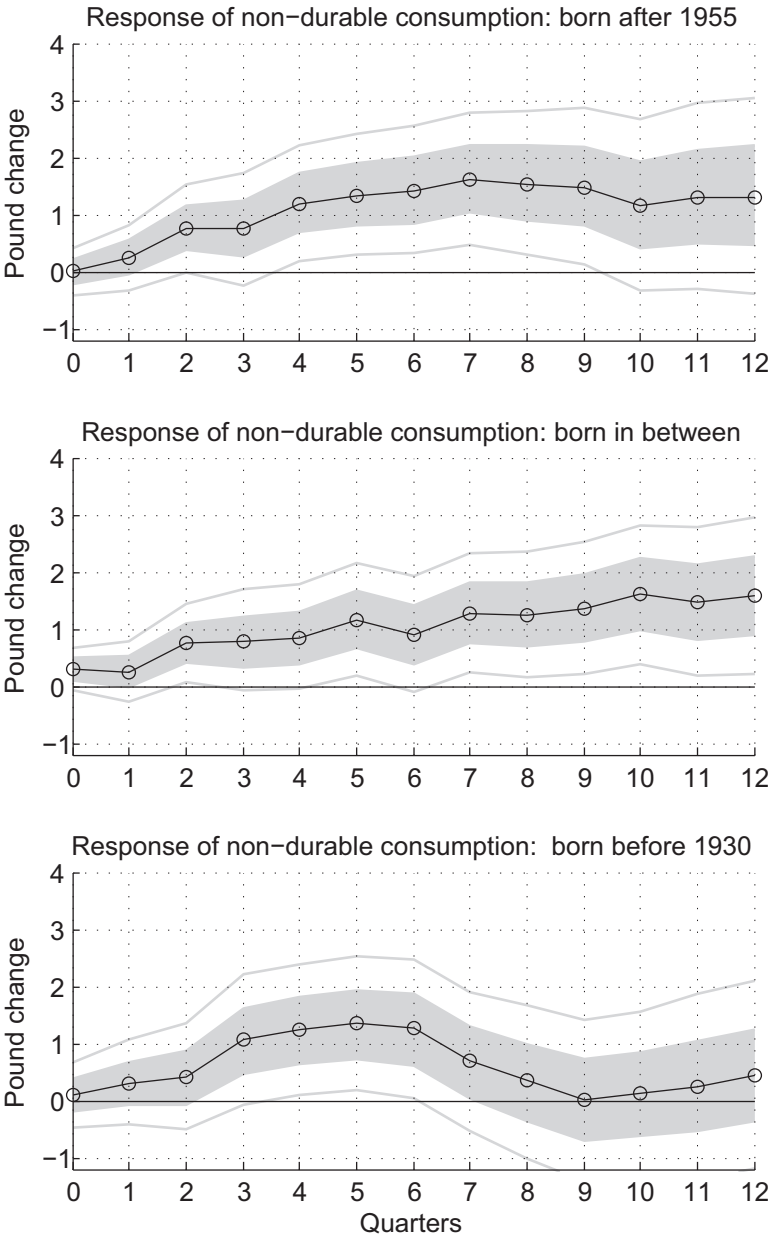

FIGURE 15

Dynamic effects of a per-taxpayer liability change in the allowance and basic rate of income tax on per capita non-durable goods, and services consumption across birth cohorts. Shaded areas (grey lines) represent 68% (95%) confidence bands over 10,000 bootstrap repetitions. Sample: 1978–2009

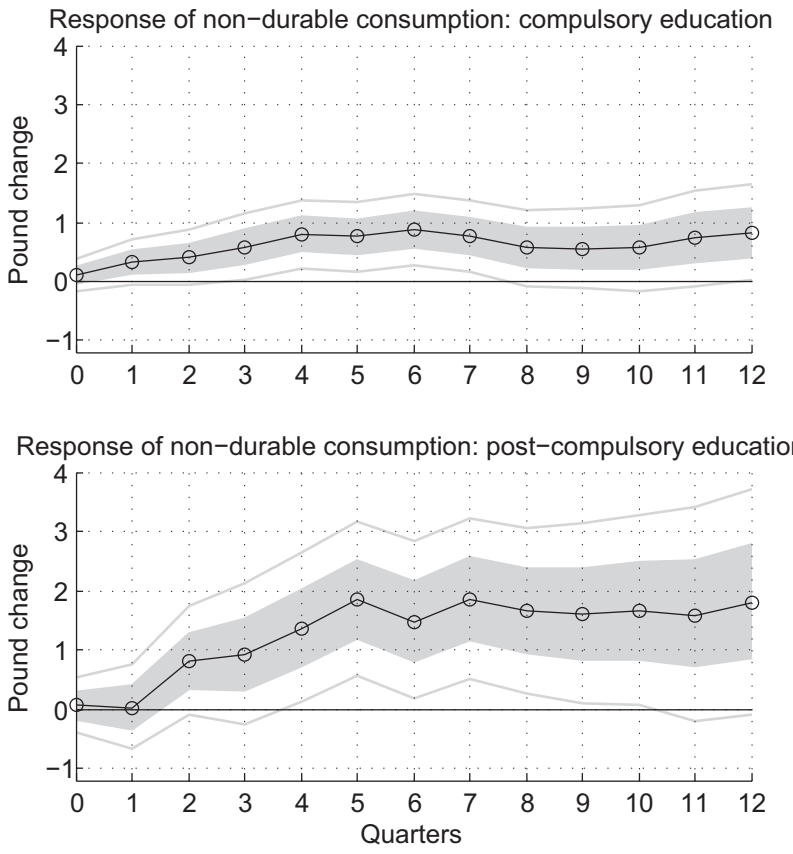

FIGURE 16

Dynamic effects of a per-taxpayer liability change in the allowance and basic rate of income tax on per capita non-durable goods and services consumption across education levels. Shaded areas (grey lines) represent 68% (95%) confidence bands over 10,000 bootstrap repetitions. Sample: 1978–2009

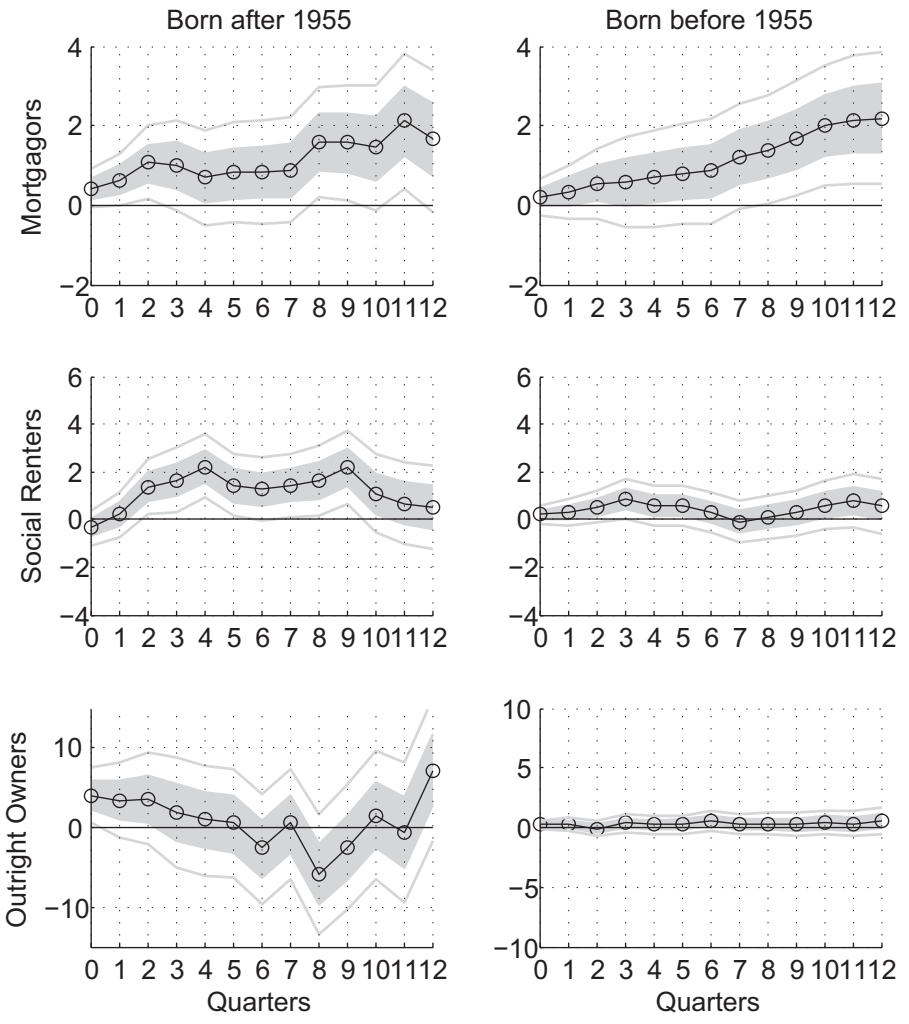

FIGURE 17

Dynamic effects of a per-taxpayer liability change in the allowance and basic rate of income tax on per capita non-durable goods and services consumption across housing tenure and birth cohorts. Shaded areas (grey lines) represent 68% (95%) confidence bands over 10,000 bootstrap repetitions. Significantly fewer observations per cell are available in the pseudo-cohort in the bottom left panel. Sample: 1978–2009

G. ABOVE AND BELOW MEDIAN INCOME MORTGAGORS

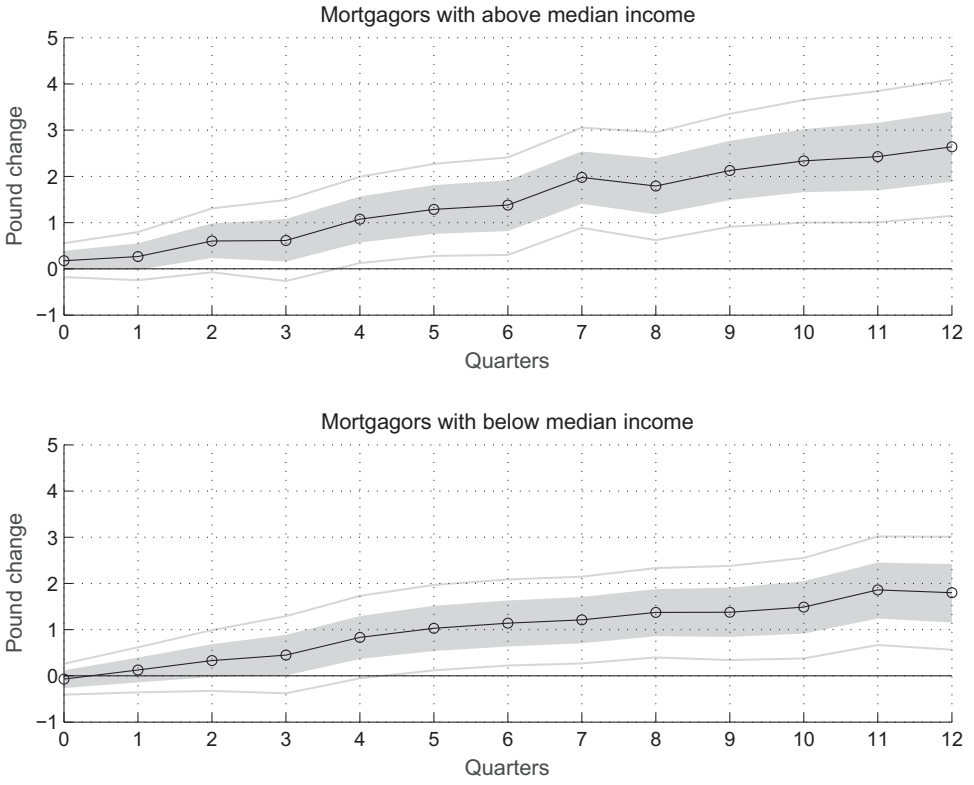

FIGURE 18

Dynamic effects of a per-taxpayer liability change in the allowance and basic rate of income tax on per capita non-durable goods and services consumption for households with mortgage debt and head with above (top panel) and below mortgagors' median income (bottom panel) in each quarter using a VAR in non-durable consumption per capita change, income per capita change, real government spending per capita change, and Bank Rate. Shaded areas (grey lines) represent 68% (95%) confidence bands over 10,000 bootstrap repetitions.

Sample: 1978–2009

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Supplementary Data

Supplementary data are available at *Review of Economic Studies* online.

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