Household Debt and the Dynamic Effects of Income Tax Changes*

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Abstract

This paper investigates a new channel in the transmission of fiscal policy: household debt. Using a long span of expenditure survey data and a new narrative measure of exogenous income tax changes for the UK, mortgagors exhibit large and persistent consumption responses to tax changes. Home owners without a mortgage, in contrast, do not appear to react, with responses not statistically different from zero at all horizons. Social renters increase their consumption, but by less than mortgagors. Households with non-mortgage debt also tend to adjust their expenditure by more than non-borrowers. Splitting the sample by age and education yields only limited evidence of heterogeneity as the distributions of these demographics overlap across housing tenures. Our findings highlight the role of household debt in evaluating the effectiveness of fiscal policy both in the aggregate and across different groups in society.


Key words: household debt, fiscal policy, narrative identification.

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1 Introduction

The persistent rise in household leverage over the years prior to the recent financial crisis has drawn considerable attention to the role of private debt in the transmission of economic shocks. On the empirical side, Mian et al. (2012), Dynan (2012) and the IMF (2012) report that household leverage is likely to have amplified and prolonged the Great Recession of 2007–09. On the theoretical side, Kaplan and Violante (2012), Eggertsson and Krugman (2012) and Andres et al. (2012) lay out models with heterogeneous agents in which fiscal policy is more effective the larger the proportion of debt-constrained households in the economy.

A common presumption behind these contributions is that the household response to changing macroeconomic conditions depends on their balance sheet position. If wealth is mostly illiquid (for instance, because of a large durable purchase) then, following an exogenous income change, households with debt may adjust their consumption by more than households without. Conversely, if borrowers are not liquidity constrained they may use the additional income to pay down debt and thus change their consumption by less than households without debt.

Despite the clear relevance of this transmission channel for policy and academic research, little is known empirically about whether the dynamic effects of income changes on consumption vary with a household’s balance sheet position and whether the response of debtors tends to be larger or smaller than the response of non-debtors. Three considerations make this task particularly difficult. First, consumption and income changes are jointly determined. Second, survey data with good expenditure coverage typically lack equally detailed and reliable information on the household finance position over a sufficiently long period of time. Third, whether a household holds debt or not is the outcome of a selection into group.

To address the endogeneity of income changes, we exploit exogenous variation in aggregate income taxes identified using the narrative approach pioneered by Romer and Romer (2010) and applied to the UK by Cloyne (2013). The UK is a natural
choice for our purposes because there have been a large number of income tax changes in the last forty years. Furthermore, detailed information from official documents allow us to identify individual tax measures and their motivation. Tax changes that were introduced for reasons unrelated to the business cycle can then be used to identify exogenous variation in household income.

Good quality household survey data are available for the UK over a long period of time from the Family Expenditure Survey (FES). Unfortunately, the FES is a repeated cross-section rather than a panel and therefore, following Browning et al. (1985), a grouping estimator is needed to aggregate individual observations into pseudo-cohorts. To elicit households’ debt positions — and motivated by renewed interest in the relationship between expenditure, household balance sheets and the transmission of fiscal policy — we group households by housing tenure. The reason for this choice is twofold. First, mortgages are the most prominent form of household debt, in both incidence and value. Second, the extensive margin of whether a household has a (mortgage) loan or not is likely to be less prone to measurement error than the intensive margin of its outstanding value.

A potential drawback of grouping by housing tenure is the selection effect associated with possible transitions from one tenure status to another over time. However, the very gradual rate at which ownership has risen in the UK suggests this may be less of a concern. Indeed, our results are robust to using the grouping strategy proposed by Attanasio et al. (2002), which directly addresses the possibility of compositional changes. Using this method, we also show that the results based on housing tenure carry over to the household’s non-mortgage debt position.¹

Using a long span of household survey data and a new narrative measure of fiscal shocks, we report significant aggregate effects of exogenous income tax changes on private consumption. More importantly, our disaggregated approach allows us to identify the expenditure response of different groups of households to an income change. We find that the estimated effects are highly heterogenous across housing

¹For which we only have a shorter span of survey data.
tenures, in a way that is understated — if not missed — when households are grouped by age and/or education.

We establish a number of specific results. First, mortgagors exhibit the largest and most significant response, followed by those renting from local authorities or housing associations (which, for short, we call “social renters”). In contrast, outright owners hardly respond and the effect is never statistically different from zero. Over a three years horizon, mortgagors, social renters and outright owners respond to a tax cut of one pound by raising their average expenditure by respectively 170, 80 and 35 pence. We also show that households with non-mortgage debt tend to adjust their consumption by more than non-borrowers. Finally, the extent of heterogeneity appears larger for durable goods and services purchases.

This paper contributes to a growing empirical literature on heterogeneity in consumption behaviour. Recent studies, including Anderson et al. (2012), De Giorgi and Gambetti (2012a,b), Ercolani and Pavoni (2012), Giavazzi and McMahon (2012) and Misra and Surico (2013) have focused on the role played by age, liquidity and income to account for the diverse responses of household expenditure to a fiscal shock.\(^2\) The findings from earlier contributions have been convincingly interpreted as supportive of theories based on precautionary saving, partial insurance and limited participation in financial markets. The results presented here highlight the role of an additional channel for the transmission of structural shocks: household debt. This is consistent with theories where the decision of some households to purchase a large durable good makes them liquidity-constrained.

Our results are also of interest for the vast body of research in empirical macroeconomics on the effects of fiscal policy on real activity. While our estimates for the whole economy are consistent with those reported by Mountford and Uhlig (2009), Romer and Romer (2010), Mertens and Ravn (2012) and Cloyne (2013), among oth-

\(^2\)Campbell and Mankiw (1989), Attanasio and Weber (1993) are earlier contributions in the vast empirical literature on the correlation between consumption and income changes. Recent examples include Jappelli and Pistaferri (2010), Acconcia et al. (2011) and Pistaferri and Saporta (2012).
ers, 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. These appear important dimensions along which to evaluate the effects of tax interventions both in the aggregate and across different groups in society.

2 Data and empirical specification

In this section, we describe the different data sets we employ. We first discuss the narrative data on UK tax changes and the way we exploit these to construct an exogenous income tax measure. We then discuss the household survey data and the grouping strategy used to construct time series of consumption for cohorts based on housing tenure status. The final part of this section explains the estimation strategy, which brings together the tax and expenditure data.

2.1 Narrative evidence on UK income tax changes

Estimating the effect of tax changes on consumption requires a suitable measure of taxes. Tax changes may affect consumption and other macroeconomic variables but common measures of taxes, such as total tax revenues, are also affected by the state of the economy. Furthermore, even if one could construct a measure free of these cyclical effects, tax instruments are changed by policymakers in response to macroeconomic conditions. As our household groups represent large shares of the population, the simultaneity between fiscal policy and consumption prevents consistent estimation of the dynamic effects of tax changes by regressing consumption on, for example, tax revenues.\(^3\)

To identify the component of tax changes uncorrelated with other macroeconomic fluctuations, we employ the narrative approach introduced by Romer and Romer (2010) for the United States and applied by Cloyne (2013) to the United Kingdom.

\(^3\)Even the construction of cyclically adjusted revenue can be problematic as this still requires an estimate of the output elasticity of revenue. See, for example, Caldara and Kamps (2012).
This method uses detailed documentation from historical sources to identify ‘exogenous’ legislated policy changes from the motivations given by policymakers at the time. The narrative approach has found large macroeconomic effects of aggregate tax changes on aggregate real activity. However, aggregate effects could mask considerable heterogeneity. In this paper we examine whether different groups in society react differently to a similar fiscal shock.

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. However, by going through detailed official UK budget documents, we are able to isolate specific changes in income tax (see Cloyne (2012)). Specifically, we focus on tax reforms that are more likely to affect all income taxpayers. Compared to the macro literature on the effects of tax changes, our innovation is to construct measures of the changes in the main rate/thresholds of income tax (the “basic rate”) and the changes in the amount of tax-free income (the income tax “allowances”).

By employing a narrative approach, our income tax series is designed to be uncorrelated with macroeconomic fluctuations. A simple regression, such as

\[
\Delta x_t = \alpha_0 + \sum_{j=0}^{\infty} \beta_j \tau_{t-j} + \nu_t, \tag{1}
\]

should then consistently uncover the dynamic effects of the tax shock on variable \(x\) (the \(\beta\) coefficients). \(\tau_t\) is the ‘exogenous’ tax series. Note that the key identifying assumption is \(E(\nu_t \mid \tau_t, \tau_{t-1}, ...) = 0\), where \(\nu_t\) may well be a function of current and past structural shocks.

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4 The idea has also been used to identify government spending shocks (Ramey and Shapiro (1998); Ramey (2011)), monetary policy shocks (Romer and Romer (1989, 2004)) and fiscal consolidations (Guajardo et al. (2011)).

5 For example, a change in Stamp Duty affects home-buyers and Vehicle Excise Duty would affect those with a motor vehicle.

6 Mertens and Ravn (2013) split the Romer and Romer dataset 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.

7 This also implies that the exogenous tax changes need to be uncorrelated with prospective macroeconomic fluctuations.
To construct our new tax measure, we first collect data on all the discretionary income tax changes from narrative sources such as UK Budget documents. Specifically, we isolate around 150 changes in the tax-free allowances and the basic rate and thresholds of income tax. For the quantitative magnitude, we follow Romer and Romer (2010) and use the revenue forecasts from the Budget documents. However, the focus is on the change in tax liabilities rather than any short-run revenue effect due to the timing of revenues reaching the Exchequer. We therefore use the ‘full year’ revenue estimate, which is the projected on-going annualised revenue effect. This value is then assigned to the implementation date of the policy change. As explained by Mertens and Ravn (2012), it is possible that this implementation date is anticipated if it is announced some time earlier. We address this possibility later and show our findings are robust to considering only those tax changes that were more likely to be unanticipated.

The source for the policy changes and revenue estimates is the Financial Statement and Budget Report (FSBR) which is published alongside the Budget speech. For actions between Budgets (not already covered in the FSBR), we use the estimates given by the Chancellor of the Exchequer to Parliament. The source is the official parliamentary record, Hansard. The tax changes are then categorised by their given motivation (using a variety of UK government, parliamentary and historical documents and speeches), isolating decisions that were taken for reasons uncorrelated with macroeconomic fluctuations. Following Romer and Romer (2010), we refer to these changes as ‘exogenous’.\footnote{The authors refer to these as tax changes “not taken to offset factors pushing growth away from normal” (Romer and Romer (2010), 770). Detailed information on the classification of individual measures and supporting evidence for the each classification can be found in Cloyne (2012).}

The classification of tax changes keeps as close as possible to the motivation given in the narrative documents (although the history literature was also used to frame and cross reference the interpretation of the given statements). The ‘exogenous’ category contains actions to improve long-run economic performance, ideological changes
related to party political or social causes, rulings from external bodies such as courts, and fiscal consolidation measures based on long-run considerations. Actions that do not satisfy the ‘exogenous’ definition are classified as ‘endogenous’ and are not used. These changes are policy actions to manage demand, stimulate production, offset a debt crisis and fund spending decisions. For illustrative purposes, the Appendix provides an example of tax changes in each camp.

Individual exogenous income tax changes are assigned to quarters and aggregated. Figure 1 shows, as the solid line, our newly constructed tax series scaled by nominal GDP, together with the aggregate tax change series in Cloyne (2013) which is depicted as the dashed line. There have been a sizable 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 supply-side reforms designed to encourage long-run economic performance, sharpening incentives and lowering the burden of taxation.

The exogeneity of both the narrative tax changes and their composition is the key identifying assumption. One way to evaluate this is to examine whether movements in our tax series are predictable using past information at either the aggregate or cohort-specific level. Specifically, we conduct Granger causality tests based on a VAR which contains the change in consumption per capita for each household group (our dependent variables in the next section), the change in real GDP per capita and the central bank’s policy rate. Reassuringly, we could not reject the hypothesis that these variables do not Granger cause our income tax series: the p-values using various lag lengths were high, over 0.6, for 4, 6 and 8 lags.\(^9\) The lack of Granger causality adds weight to the narrative classification of these tax changes as exogenous.

### 2.2 Household consumption data

To examine whether the heterogeneous effects of income tax changes differs across groups in society, we use household survey data on non-durable goods and services

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\(^9\)Similar results are obtained using the change in real gross income per capita for each group.
consumption from the Living Costs and Food Survey. This survey has had a variety of names over the years and is most commonly known in the literature as the FES. We use 32 waves of the survey from 1978 to 2009. Although the survey has run annually from 1968, a consistent measure of educational attainment is only available from 1978. Each wave of the survey contains around 7,000 households generating over 200,000 observations in total.

Unfortunately, the FES is a not a panel dataset, with each household surveyed only once. Faced with this challenge, it is common to use a grouping estimator along the lines proposed by Browning et al. (1985). We therefore aggregate households into pseudo-cohorts and use the narrative tax changes discussed above to identify the differential response of consumption.

Motivated by the renewed interest in the consequences of leverage and mortgage debt, we group households by their housing tenure status. We focus on three groups: those living in accommodation rented from local authorities or housing associations, for want of a better term we call these ‘social renters’; those with mortgages; and, those who own their home outright.\textsuperscript{10}

While the main focus of the econometric analysis is on secured debt, housing tenure groups may also differ significantly in the degree of exposure to unsecured debt. Figure 2, based on the Bank of England NMG surveys from 2007 to 2012, illustrates this point.\textsuperscript{11} The top panel shows that mortgagors are also the most leveraged with respect to unsecured debt, followed by social renters and then outright owners. The bottom panel shows that after including savings, the leverage ratios of mortgagors and social renters are remarkably similar but both are very different from the leverage ratio of outright owners. We return to non-mortgage debt in Section 5.

\textsuperscript{10}Mortgagors represent on average about 50\% of the full sample whereas social renters and owners outright cover another 20\% each. Unfortunately, private renters account for about 10\% of observations (with even lower shares in particular years) and therefore they are excluded from the analysis below. Preliminary attempts to run a specification based on private renters revealed that the point estimates for this group are similar to the point estimates for mortgagors, but the standard errors are so wide (probably due to the fewer number of observations) that they cannot be discriminated statistically from the point estimates of any other group.

\textsuperscript{11}As discussed below, the FES lacks a readily available measure of non-mortgage leverage.
The empirical analysis focuses on non-durable goods and services consumption, in keeping with earlier contributions such as Attanasio and Weber (1993), Attanasio et al. (2002) and Campbell and Cocco (2007). The data are discussed in more detail in the Appendix. In Section 6, we show that the results are robust to using non-housing expenditure. For each group, we gross-up the individual household responses using household weights, 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). We construct a quarterly series by assigning households to quarters based on their interview date. To address seasonality, we use the annual change in quarterly expenditure for each housing tenure group.

One issue using this grouping approach is that the dimensions along which aggregation is performed need to be constant or fully predictable over time. This explains why birth cohort has proved so popular in the literature. 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 will still have the same tenure status in the next period. Figure 3 shows that there have been changes in the shares of the tenure groups over time, although these changes have been relatively slow moving: it seems unlikely that individuals fluctuate between groups at a quarterly or even annual frequency. However, to ensure robustness of our findings, we also consider grouping households according to their predicted probabilities of being a mortgagor, following Attanasio et al. (2002). We discuss this approach later.

Prominent studies in the literature have constructed pseudo-cohorts for consumption based on birth year and/or education of the household head. We show that, in the context of fiscal policy, these ‘traditional’ sample splits produce limited evidence of heterogeneity. We will argue that this finding become more easily understood when we look at the data through the lens of household debt.

Before turning to our empirical results, it is useful to examine the demographic properties of the three housing tenures. For each group, Figure 4 shows kernel density estimates of age and weekly household real income per adult, as well as the share of
households with different education levels and non-mortgage debt. Mortgagors are on average younger, more educated and relatively richer but it is worth noting that the distributions of the three tenure groups overlap significantly. In particular, the estimated densities for both groups of home owners are characterised 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. Finally, the mortgagor group has the largest share of households with non-mortgage debt. Outright owners have the lowest share of households with non-mortgage debt. These finding are in line with Figure 2 from the more recent Bank of England NMG surveys.

2.3 Empirical specification

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, 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 RPIX and the total number of individual income taxpayers.

Two features of this specific transformation make it particularly useful for our analysis. First, the size of the estimated coefficients on tax changes are readily interpretable as the pound change in consumption following a one pound change in taxes. Second, the imputed windfall does not vary systematically with household income, guarding against the concern that household consumption and income are jointly determined within the same time period.
In keeping with the discussion above, we estimate a variant of equation (1):

$$\Delta C_{i,t} = \alpha_i + \sum_{j=0}^{12} \beta_{ij}^t \tau_{t-j} + \gamma^i \Delta Z_{i,t} + u_{i,t} \quad \text{with} \quad i = M, S, O.$$  

(2)

where $\Delta C_{i,t}$ is the pound change in real per capital non-durable consumption, $\tau_t$ is our proposed measure of income tax change, which is the prospective annual change in real tax liabilities per taxpayer, and $\Delta Z_{i,t}$ is a vector containing the change in the mean of the demographic variables age and education as well as the proportion of employed households and retired households per group. The index $i$ refers to the cohorts: mortgagors (M), social renters (S) and outright owners (O).

By construction, $\tau_t$ are uncorrelated with other shocks to consumption $u_{i,t}$. For a £1 change in taxes, the sum of the $\beta$ coefficients in equation (2) gives the total four quarter consumption relative to trend at a point in time. For example, after four quarters the sum of the first four $\beta$ coefficients gives the total additional consumption (in pounds) over the first year after the tax reform. In the next section, we show that the aggregate effect of a change in exogenous income taxes is the same when using expenditure data from the FES and the UK National Statistics. For this exercise, the dimension of $i$ reduces to one and the vector $Z_t$ is excluded.

Given the relatively long time dimension but small cross-section dimension of our data, the system of equations (2) is estimated using the method of Seemingly Unrelated Regressions. The rationale for this choice (as opposed to equation-by-equation OLS) is twofold. First, each of the three tenure groups cover a large share of aggregate consumption and we cannot exclude a priori that the errors terms might be correlated across the equations. Omitted correlation in the covariance matrix of the disturbances does not pose a challenge for consistency but it would reduce efficiency. Second, the SUR method provides a natural framework in which to test formally for heterogenous responses across groups.

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3 Aggregate results

Before presenting evidence on the response of different housing tenure groups, it is important to verify that the aggregate effects found using the household survey data are consistent with results found using national accounts data.

Figure 5 reports the dynamic effects of an exogenous income tax liability cut on two measures of aggregate expenditure on non-durable goods and services. The first measure comes from the Office for National Statistics (ONS) and it represents the official aggregate series on non-durable consumption for the UK economy. The second measure comes from the FES and it is constructed by aggregating household-level expenditure in each quarter using household-specific weights that ensure representativeness of the FES sample in the UK population.

The point estimates for the impulse responses based on aggregate FES data are shown as the continuous black line with circles and results using the ONS data are shown in the blue dotted line. The shaded area represents the 68% confidence intervals based on 20,000 bootstrap repetitions; the grey lines show the 95% intervals. Under a normal distribution, the 68% band corresponds to adding one standard deviation either side of the point estimates and it is commonly reported in the empirical literature on fiscal policy (see, for example, Blanchard and Perotti (2002) and Romer and Romer (2010) among many others).

Three findings are worth noting in Figure 5. First, the point estimates of the impulse response based on FES data are very close to those based on ONS data, with the latter always well within the 68% confidence bands of the former. The two impulse responses virtually overlap during the first year and, after that, the discrepancy is smaller than 5% on average. Second, both sets of impulse responses are associated with real effects of fiscal policy that peak just below 1.5 after three years. Third, these real effects are statistically larger than zero at the 5% significance level for all horizons after the second quarter. The results in this section lead us to conclude that the FES appears a reliable source for conducting a disaggregated analysis of the effect
of fiscal changes on different groups in society.

The aggregate effects estimated using a narrative approach are typically larger than found using specific episodes of fiscal stimulus, such as the 2001 and 2008 income tax rebates in the US. However, it is important to note the effects reported in that literature refer to temporary payments aimed at stimulating aggregate demand. In this paper, as in the vast majority of empirical macro studies on fiscal policy, the focus is on persistent tax changes, which are likely to have both a longer-term supply-side impact and larger general equilibrium effects.

Finally, it is worth emphasizing that the size of the estimated responses in Figure 5 is consistent with the findings reported in the macro literature, where a one percentage change in aggregate taxes to GDP is shown to trigger large and persistent increases in total consumption and GDP, with peak effects above 2 and 3 percent respectively (see Romer and Romer (2010) and Cloyne (2013)). Using aggregated FES data, we also find evidence of large and persistent effects of the narrative tax changes on household pre-tax income, which is consistent with a supply side interpretation of the magnitude of the consumption responses reported in this paper.

4 The heterogeneous effects of tax changes

In this section, we demonstrate significant heterogeneity in the dynamic effects of fiscal policy on household consumption across housing tenures. We begin with the estimates for the three groups: mortgagors, social renters and outright owners. We then contrast our main results with those from more ‘traditional’ group splits based on birth year and education, and find that the heterogeneity is far more pronounced (and significant) when the sample is divided according to housing tenure status. Finally, we show that our results are not sensitive to employing a VAR using group-specific consumption, aggregate output and the short-term interest rate.

\footnote{See, for example, Parker et al. (2013), Misra and Surico (2013) and the references therein.}
4.1 Housing tenure

The dynamic effects of a one-pound exogenous tax liability change on the non-durable expenditure of each household group are displayed in Figure 6. Recall that these charts show the four-quarter response. For example, after four quarters this is the cumulative effect on consumption over the first year.

The first row shows that the consumption of mortgagors (M) always responds significantly at the 5% level beyond the first quarter and it reaches a peak above 2 pounds after three years. The point estimates in the second row suggest that social renters (S) adjust their non-durable expenditure by less than one pound with responses that are significant at the 32% level beyond quarter 1 and at 5% between quarters 3 and 6. In contrast, the response of owners without mortgage (O) in the last row is never statistically different from zero at the 32% significance level and peaks below 0.8 or 80 pence five quarters after the shock. Over the entire forecast horizon, mortgagors, social renters and outright owners increase their expenditure by about 1.7, 0.8 and 0.35 pounds on average, following a one-pound tax windfall, with the differences across groups being more marked during the second and third year. As owners without a mortgage have a significantly higher gross income than social renters on average, it seems unlikely that the heterogeneity in Figure 6 is driven by possible heterogeneity in the tax windfall.\footnote{We return to this issue in Section 6.3.}

In the first column of Table 1, we formally test the extent of heterogeneity shown in Figure 6. For each tenure group, we report the effect of the tax cut after one, two and three years. For each yearly horizon $f$, we report p-values for three null-hypotheses: (i) homogeneity among the responses of all three groups, (ii) homogeneity between the responses of mortgagors and social renters and, (iii) homogeneity between the responses of mortgagors and owners without mortgage.

A number of salient points emerge from Table 1. On the one hand, the point estimates of the responses of the outright owners at each year horizon are outside the
95% confidence bands of the corresponding responses for mortgagors. On the other hand, the null hypothesis of homogenous responses across the three tenure groups is rejected at the 32% significance level at two and three year horizons but not at the 5% significance level. However, the p-values for the bilateral tests suggest that the findings of the joint hypothesis testing may be driven by the large standard errors associated with the response of the outright owners.

To examine the extent to which imprecise estimates — as opposed to weak heterogeneity — are responsible for the relatively high p-values of the joint hypothesis test, we conduct tests for homogeneous responses based on estimation of equation (2) setting insignificant coefficients to zero. These results are shown in the second column of Table 1. The point estimates from this restricted specification are not significantly different from the unrestricted specification but the evidence for heterogeneity is much stronger, now significant at the 5% level in most tests. We conclude that imprecise estimates are, at least partially, responsible for some of the higher p-values in the first column. We postpone the discussion of the last two columns of Table 1 to the sensitivity analysis of Section 6.

In summary, our estimates suggest that housing tenure is highly correlated with unobserved characteristics driving the heterogenous response of household consumption to an exogenous income tax change. Specifically, whether a household has debt seems an important dimension through which tax changes affect consumption. We will show this result carries over to the non-mortgage debt position in Section 5.

4.2 Comparison with ‘traditional’ sample splits

In this section, we examine the degree of heterogeneity across two alternative groupings that have been used extensively in the micro literature: age and education.\textsuperscript{18}

As these variables are either exogenous or fully predictable over time, they are ideal

\textsuperscript{18}A further advantage of grouping households by housing tenure, relative to using age, 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.
dimensions along which to construct pseudo-cohorts.

We re-estimate equation (2) over these different groupings. The first column of Figure 7 refers to a specification where households are grouped depending on whether the head is born before 1920 (first row), between 1920 and 1939 (second row) and between 1940 and 1959 (third row). The second and third columns then split these groups further by education, depending on whether the household head has only compulsory or also post-compulsory education.

In line with the descriptive estimates in Figure 4, heterogeneity across groups is more muted (and far less precise) along these ‘traditional’ dimensions. Taken at face value, the point estimates in Figure 7 suggest that, if anything, younger and more educated households tend to adjust their non-durable consumption by a relatively larger amount following a tax change.

The evidence of heterogeneity based on the traditional characteristics appears far less compelling than the evidence based on housing tenure. While Figure 4 shows that mortgagors tend to be younger and more educated, it is important to realize that, crucially, not all young and educated households are mortgagors or liquidity-constrained. The housing tenure grouping therefore has a considerable advantage for the purpose of identifying heterogeneity in consumption responses.

4.3 Controlling for business cycle conditions

Household consumption is likely to be influenced by macroeconomic conditions as well as group-specific dynamics. The narrative identification procedure, as well as the formal Granger causality tests in Section 2, suggest that our tax changes are uncorrelated with macroeconomic fluctuations. However, to control for the possibility of residual correlation between the tax measures and the dynamics of the business cycle we control for lagged values of GDP and the interest rate. Furthermore, to

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19 This is consistent with the finding in Bottazzi et al. (2010) of considerable variation in the rate at which different birth cohorts transition to home ownership.

20 We also showed the tax series was not Granger caused by consumption of the household groups.
control for the dynamics of consumption we add the lagged values of non-durable consumption of the relevant group.

The dynamics of consumption, GDP and the short rate are jointly modeled using a Vector Autoregression (VAR) for each tenure group. We use four lags of the change in non-durable per-capita consumption (as above), the change in real GDP per-capita and the central bank’s policy rate. As above, we use twelve lags of the narrative measure of tax changes and include this as exogenous variable. The VAR results are shown in Figure 8: the responses of the three groups are largely unaffected and, if anything, the confidence bands for the outright owners appear smaller. More importantly, the point estimates for the owners are still outside the 95% confidence bands around the response of mortgagors for horizons beyond five quarters.

5 Non-mortgage debt

In this section, we assess the extent to which the finding of heterogeneity across housing tenures carries over to non-mortgage debt. In the FES dataset, we observe the value of non-mortgage loans at origination but the outstanding amount is not reported. Furthermore, these loan data are only consistently available since 1986, which forces us to use a restricted sample.

In keeping with the previous analysis, a possible grouping strategy would be to split the sample according to whether a households hold non-mortgage debt or not. One problem with this strategy is the potential compositional change of the two groups. A grouping estimator on repeated cross-sectional data produces consistent pseudo-cohorts if the dimension along which households are pooled together is either constant over time or fully predictable. This is unlikely to be the case for non-mortgage loans — many of which are likely to be unsecured — as a household observed in a particular year may well have paid off the loan by the following year. As discussed earlier, we believe housing tenure is less likely to be subject to compositional changes

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21 Results are robust to using shorter and longer lag lengths.
22 The results of the previous section remain when the sample is restricted to 1986-2009.
from one period to another.

To control for compositional changes and selection effects, we adapt the methodology proposed by Attanasio et al. (2002) to generate individual predicted probabilities of owning non-mortgage debt. In the robustness section below, we also apply this method to housing tenure and show that the results based on actual status are confirmed using predicted probabilities.

Specifically, we run a probit regression over the full sample to generate individual predicted probabilities of having a non-mortgage loan based on a high order polynomial in age, education, a time trend and their interactions. For households observed in period \( t \), we compute the probability that they had a non-mortgage loan four quarters earlier. For these two periods, we classify a household as a ‘likely borrower’ based on whether the probability in the first of the two periods is greater than the share of non-mortgage debt borrowers in that period. We then take the difference in consumption across these two periods for each group.\(^{23}\)

The consumption response of the ‘likely’ borrowers in Figure 9 is significant at the 5% level beyond the fifth quarter following the tax shock and it peaks at around 3.2 pounds after three years. The response of the ‘unlikely’ borrowers, in contrast, is never statistically different from zero at the 32% level and the maximum impact of the tax shock is much lower, at around 80 pence.\(^{24}\) Relative to the housing tenure split, the evidence of heterogeneous responses is possibly less decisive, though the point estimates of the unlikely borrowers are still outside the 95% confidence bands for the likely borrowers at horizons beyond six quarters. It should be noted, however, that we are using a shorter sample, characterised by fewer tax changes and a lower number of time periods. Furthermore, the number of borrowers with non-mortgage loans is, on average, about sixty percent of the number of mortgagors. All these

\(^{23}\)Running separate probit regressions for each year or using a fixed threshold produces very similar results. The estimates of the probit regression for the whole sample are reported in Appendix B.

\(^{24}\)These charts are produced running a VAR for each group, as in the previous section. To preserve degrees of freedom using a shorter sample, the results are based on a VAR(1). Similar findings are obtained using a VAR(4), although the estimates are less precise.
factors may explain the relatively less precise estimates in Figure 9.

6 Sensitivity analysis

We now examine the robustness of our findings to a range of exercises. First, we control for other exogenous changes in tax liabilities by also including the distributed lags of the remaining exogenous tax changes. Second, we consider only the unanticipated component of our baseline measure of exogenous income tax changes. Third, we show that our results carry over to a broader measure of household expenditure. Fourth, we examine whether the heterogeneity we find might be due to heterogeneity in the windfall. Finally, we address the possibility of changing composition and selection effects in the housing tenure groups, applying the method outlined above.

6.1 Other tax changes and anticipation effects

In this subsection, we confirm the robustness of our baseline results to two possibilities: that our allowance and basic rate income tax changes were offset by other tax changes; and, that our results reflect anticipation effects.

In considering a subset of tax changes, one possible concern is that these are correlated with other tax changes. For example, increases in the higher income rate may be correlated with a reduction in the lower rates, inducing spurious heterogeneity if these other tax changes are omitted. To address this possibility, the third column of Table 1 builds on the empirical specification in the second column (discussed in Section 4) by adding the significant coefficients on contemporaneous and (up to twelve quarters) lagged values of any other exogenous tax changes.\(^{25}\)

The estimates reveal that our baseline results are not altered by controlling for these other tax changes. The null hypothesis of homogeneous responses among the three tenure groups is overwhelmingly rejected at any of the yearly horizon with more than 95% confidence. Mortgagors again respond the most, with responses that are

\(^{25}\)This is our new series subtracted from the aggregate exogenous tax changes in Cloyne (2013).
always significant at the 5% level. The converse is true for the outright owners. Social renters adjust consumption by significantly less than mortgagors but the effects are still statistically larger than zero.

In the last column of Table 1, we perform a similar exercise except that we only 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 estimates and hypothesis tests broadly confirm our earlier findings.

### 6.2 Other expenditure categories

So far our analysis has focused on non-durable consumption. We now explore the extent to which the heterogeneous responses documented in Section 4 depend on the composition of expenditure and, more specifically, on the purchase of durable goods and services. To this end, we consider three alternative categories: (i) ‘food’, (ii) ‘strictly non-durable’ and (iii) ‘non-housing’ expenditure. Following Lusardi (1996), strictly non-durable consumption excludes semi-durable categories such as ‘apparel’, ‘health’ and ‘reading’ from non-durable goods and services. Following Attanasio et al. (2011), non-housing consumption excludes items such as ‘rents’, ‘mortgage payments’ and ‘water bills’ from total consumption and therefore includes durable categories such as ‘vehicles’ and ‘electrical appliances’.

The rows of figure 10 report the point estimates of the dynamic effects of a one pound income tax cut on food (dashed line), strictly non-durable (solid line) and non-housing expenditure (dotted line) for each housing tenure group. Following the tax shock, the expenditure on food is characterised by small adjustments and virtually no heterogeneity. The responses of the strictly non-durable categories are more pronounced and more diverse across housing tenures. But it is only when we focus on non-housing expenditure that the dynamic effects for mortgagors and

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26The results in this section are based on the method of Seemingly Unrelated Regressions but are robust to using the VAR model employed earlier.

27In the Appendix, we also report confidence bands for each set of impulse responses.
social renters become large and (as shown in the Appendix) mostly significant. This contrasts with the behaviour of owners whose response in the third row tends to be smaller than those for the other groups and it is never (rarely) statistically different from zero at the 5% (32%) significance level.

Compared with the non-durable results in earlier sections, the response of non-housing expenditure for each tenure group tends to be larger and the response of strictly non-durable expenditure tends to be smaller. Furthermore, the differences across expenditure categories appear largest for mortgagors and smallest for outright owners at most horizons. The degree of heterogeneity in the response of household consumption to income tax changes therefore seems to increase with the degree of ‘durability’ of the goods and services purchased.

6.3 Accounting for heterogeneity in the amount of windfall

We now examine whether the heterogeneous responses in Figure 6 might reflect (omitted) heterogeneity in the tax windfalls. To this end, we run two further robustness checks. Initially, we only consider tax changes in the allowance, which should largely generate the same windfall across tax-payers independently from their income. Using only these tax changes the heterogeneity found between mortgagors, social renters and owners remains.

In the second exercise, we use cohort-specific average income levels to construct a measure of exogenous tax liability changes that varies across housing tenure groups according to group-level income.\textsuperscript{28} The drawback of using such as measure is that it makes the tax variable depend on income, raising the concern that the variation in the amount of the windfall across groups becomes endogenous. Specifically, to the extent that the amount of tax change increases with income and that higher tax rates

\textsuperscript{28}Specifically we multiply the change in projected liabilities scaled by GDP — proportional to the average income tax rate — by the cohort-specific average income in the previous period. We further rescale the revenue to GDP figure by the inverse of the average share of household income in GDP and the average share of revenue raised by the basic rate of income tax in total income tax revenue. This should then proxy the average tax rate for income subject to the basic rate.
reduce consumption, using a cohort-specific tax series is likely to bias our estimates downward. Furthermore, the bias would be more pronounced for groups that react the most. Bearing these caveats in mind, Figure 11 presents the results, which broadly confirm our previous findings. The responses of social renters and owners are largely unaffected. Furthermore, mortgagors still respond the most, although the peak effect is reduced to around 1.5 pounds (consistent with the sign of the bias discussed before). It therefore remains the case that households with debt respond the most and those without — the outright owners — hardly respond at all.

6.4 Compositional changes and selection effects

Whilst the shares of social renters, mortgagors and outright owners have varied slowly over time (see Figure 3), both compositional changes and selection into group might bias our estimates. To explore this possibility, we again apply the method in Attanasio et al. (2002) to divide our sample in ‘likely’ and ‘unlikely’ mortgagors. We focus on mortgagors because the evidence in Section 4 suggests that this is the group with the largest and most heterogeneous consumption response.\textsuperscript{29} We categorise a household as ‘likely’ mortgagor if their predicted probability is greater than the share of mortgagors in that particular period.

The results for this sample split are shown in Figure 12. Consistent with the evidence based on actual housing tenure, the response of households who are unlikely to hold a mortgage is largely never statistical significant at the 32% level. In contrast, the dynamic effects of the exogenous tax change on the non-durable consumption of the ‘likely’ mortgagors is always significant at the 5% level and, in line with the estimates in Figure 6, peak at values above 2 pounds after three 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 at horizons beyond six quarters. We conclude that the potential bias associated with compositional changes and selection effects is

\textsuperscript{29}As before, a probit regression is run over the full sample and predicted probabilities for being a mortgagor are constructed using the same high order polynomial in age, education, a time trend and their interactions as in the previous section.
likely to be negligible when grouping households by their housing tenure status.

7 Concluding remarks

Recent years have witnessed a renewed interest in the role of household debt in the transmission of macroeconomic shocks. Theoretical studies have formalised the idea that some agents may become debt-constrained by making a large durable purchase. These theories predict heterogenous responses to income variation suggesting that, following an exogenous change in taxes, households with debt should increase their consumption by more than those without.

This paper finds empirical support for this hypothesis. Using a new narrative measure of exogenous income tax changes for the United Kingdom and a long span of household survey data, we find that the consumption response of mortgagors is significantly larger than the response of outright owners. Similarly, households with non-mortgage debt adjust their expenditure by more than households without. As the demographics of these groups are similar in at least some dimensions, we argue that traditional sample splits (based on age and education) may understate the full extent of heterogeneity in the data.

As for policy implications, our results suggest that income tax cuts are more likely to foster economic growth the larger the proportion of indebted households in the economy. Furthermore, their dynamic effects are probably larger and more heterogeneous for durable goods and services.

Combining narrative policy measures and household expenditure survey data may also prove fruitful for studying heterogeneity in the transmission of monetary policy. One would expect that, following an unanticipated change in the central bank interest rate, households with mortgages and other loans adjust their consumption by more than households with no debt. We leave this promising avenue for future research.
Figure 1: Tax liability changes over GDP: income tax measure (red) vs. all exogenous tax changes (black dashed)
Figure 2: Household leverage by housing tenure. Source: Bank of England NMG Survey, 2007-2012.
Figure 3: Shares of social renters, mortgagors and outright home owners.
Figure 4: Distributions of age and income and proportions of households with post-compulsory education and non-mortgage loans across housing tenures.
Figure 5: 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: aggregated Family Expenditure Survey (FES) data vs. Office for National Statistics (ONS). Shaded areas (grey lines) represent 68% (95%) confidence bands.
Figure 6: 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 housing tenures. Shaded areas (grey lines) represent 68% (95%) confidence bands.
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. Shaded areas (grey lines) represent 68% (95%) confidence bands.
Figure 8: 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 housing tenures using a VAR(4) in non-durable consumption per-capita change, real GDP per-capita change and the Bank rate. Shaded areas (grey lines) represent 68% (95%) confidence bands.
Figure 9: 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 non-mortgage loan ownership likelihoods predicted by probit estimation. Shaded areas (grey lines) represent 68% (95%) confidence bands.
Figure 10: Point estimates of the dynamic effects of a per-taxpayer liability change in the allowance and basic rate of income tax on the change in per-capita expenditure on food, strictly non-durable and non-housing goods and services across housing tenures. Shaded areas (grey lines) represent 68% (95%) confidence bands.
Figure 11: 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 housing tenure controlling for the amount of the tax windfall. Shaded areas (grey lines) represent 68% (95%) confidence bands.
Figure 12: 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 housing tenure likelihoods predicted by probit estimation. Shaded areas (grey lines) represent 68% (95%) confidence bands.
TABLE 1: Testing for heterogeneity in non-durable expenditure allowance & ABR all tax changes unanticipated ABR

<table>
<thead>
<tr>
<th>response by tenure</th>
<th>effect at the one year horizon ($\beta_{yi1}^1$)</th>
<th>effect at the two year horizon ($\beta_{yi2}^2$)</th>
<th>effect at the three year horizon ($\beta_{yi3}^3$)</th>
</tr>
</thead>
<tbody>
<tr>
<td>mortgagors (M)</td>
<td>basic: ABR 1.50** (0.50)</td>
<td>basic: ABR 1.88** (0.65)</td>
<td>basic: ABR 2.34** (0.81)</td>
</tr>
<tr>
<td></td>
<td>ABR restricted 1.16** (0.36)</td>
<td>ABR restricted 1.55** (0.42)</td>
<td>ABR restricted 1.72** (0.46)</td>
</tr>
<tr>
<td></td>
<td>all tax changes exl. ABR 1.45** (0.35)</td>
<td>all tax changes exl. ABR 1.84** (0.44)</td>
<td>all tax changes exl. ABR 1.96** (0.44)</td>
</tr>
<tr>
<td></td>
<td>unanticipated ABR 1.69** (0.48)</td>
<td>unanticipated ABR 2.08** (0.54)</td>
<td>unanticipated ABR 2.14** (0.56)</td>
</tr>
<tr>
<td>social renters (S)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>owners outright (O)</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

p-values for H0:

$\beta_{yi1}^1 = \beta_{yi2}^1 = \beta_{yi3}^1$

$\beta_{yi1}^1=\beta_{yi2}^1, \beta_{yi1}^1=\beta_{yi3}^1$ 0.33 0.05 0.01 0.02

$\beta_{yi2}^2=\beta_{yi3}^2, \beta_{yi1}^2=\beta_{yi3}^2$ 0.12 0.01 0.01 0.01

$\beta_{yi3}^3=\beta_{yi3}^3, \beta_{yi1}^3=\beta_{yi3}^3$ 0.23 0.05 0.02 0.03

Note: based on the specification $\Delta c_i = \alpha + \sum_{j=0}^{12} \beta_j^i \Delta T_{i-j} + \gamma^i Z_i + u_i^i$ with $i=S, M$ and $O$ where $i$ stands for tenants of social housing (S), owners with (M) or without a mortgage (O).

$\beta_{yi}^j = \sum_{j=0}^{12} \beta_j^i$ with $j=1,..,3$. $c_t$ is per-capita non-durable expenditure on goods and services. $Z_i$ include $age_i^t$, $emp_i^t$, $educ_i^t$ and $retire_i^t$ where age refers to the household head, emp (retire) is a dummy that takes value of one if the household head is employed (retired). educ is 0, 1 or 2 for compulsory education, A level and degree level, respectively. In columns 1 and 2, $\Delta T_i$ refers to the change in our Allowance and Basic Rate measure of income tax liability divided by RPIX and total numbers of tax payers (ABR). In columns 2-4, we drop coefficients $j$s on $\Delta T_{i-j}$ that are not statistically significant at 32% level. In columns 3 and 4, we add all per-payer liability changes other than ABR as further control. In column 4, $\Delta T_{i-j}$'s become the unanticipated part of ABR. *(**) denotes 32(10)% significance level. Standard errors in parenthesis.
A Data description

A.1 Aggregate data

- **Non-housing, non-durable goods and services expenditure**: UK Office for National Statistics categories Non-durable goods plus Services, Semi-Durable Goods (such as clothing and household maintenance) minus housing and water (codes: utiq+utii+utim-adft-adfu-adfw).

- **Price index**: Retail Prices Index excluding mortgage interest payments (RPIX), ONS code chmk.

- We scale our tax measure by the total number of taxpayers, available from Her Majesty’s Revenue and Customs’ website (www.hmrc.gov.uk)

A.2 Household micro data

We use the Family Expenditure Survey (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 UK Data Archive.

**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.

**Housing tenure and non-mortgage loans**

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 renter category is dropped due to the limited number of observations. The FES also asks a household for any loans they have. We use this variable to define non-mortgage borrowers and non-borrowers.

**Demographics**

The demographic variables are taken from the FES: age, education and employment status of the head of household.
B Demographics over time

Figure 13: Evolution of age, education and income distributions across housing tenures.
## Probit regressions results

Table A.1: Probit estimation results for the likelihood of being a borrower

<table>
<thead>
<tr>
<th>Variable</th>
<th>Panel A: mortgages</th>
<th>Panel B: other loans</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Coefficient</td>
<td>Std. Err.</td>
</tr>
<tr>
<td>Age of head</td>
<td>0.100</td>
<td>(0.002)</td>
</tr>
<tr>
<td>(Age of head)$^2$</td>
<td>-0.002</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Head has A levels</td>
<td>1.172</td>
<td>(0.031)</td>
</tr>
<tr>
<td>College education</td>
<td>2.116</td>
<td>(0.047)</td>
</tr>
<tr>
<td>Age $\times$ A levels</td>
<td>-0.012</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Age $\times$ College</td>
<td>-0.023</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Trend</td>
<td>-0.064</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Trend$^2$</td>
<td>0.002</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Trend$^3$</td>
<td>0.000</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Trend$^4$</td>
<td>0.000</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Trend$^5$</td>
<td>0.000</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Age $\times$ Trend$^2$</td>
<td>0.000</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Age $\times$ Trend</td>
<td>0.000</td>
<td>(0.000)</td>
</tr>
<tr>
<td>A levels $\times$ Trend</td>
<td>-0.003</td>
<td>(0.000)</td>
</tr>
<tr>
<td>College $\times$ Trend</td>
<td>-0.005</td>
<td>(0.000)</td>
</tr>
<tr>
<td>Constant</td>
<td>-0.885</td>
<td>(0.059)</td>
</tr>
</tbody>
</table>

Note: Panel A (B) is based on the full-sample 1978-2009 (restricted sample 1986-2009).
D Tax change examples

In this section we illustrate how the exogenous tax change series is constructed. As noted above, the Financial Statement and Budget Report (FSBR) categorises all the individual tax changes in each budget, together with their projected revenue effects and implementation dates.

Table A.2 illustrates two examples of the identification approach. The first row considers one of the larger changes in income tax announced in 1986. The FSBR records a “Reduction of 1p in the basic rate”, which the Treasury projected to cost 830 million each year. The measure came into effect from April 6th 1986 and is assigned to 1986 quarter 2.

The second step is to determine whether the policy change was reacting to other macroeconomic fluctuations. The table shows the motivation given by Chancellor Nigel Lawson in the budget speech. There is no mention of this tax reform reflecting business cycle movements and was a purely supply-side measure designed to improve long-run economic performance. Providing a one sentence illustration is limiting but gives the essential idea behind the classification. In practice, we use a richer variety of statements, commentaries and contexts to construct our specific income tax change series, building on the classification strategy in Cloyne (2012).

The second row illustrates a tax change that would be regarded as endogenous — reacting to other macroeconomic fluctuations. Consider the stimulus package announced in 2008 as a response to the deepening recession. Chancellor Alistair Darling announced in November 2008 that an earlier temporary increase in the personal allowance would be made permanent. As the quote shows, this was part of a package of measures designed to stimulate the economy. This income tax change (and the other tax changes announced) is endogenous and excluded from our series.

\[\text{For more information on how the UK Treasury calculates these revenue projections see, for example, H.M. Treasury (2005) page 205.}\]
<table>
<thead>
<tr>
<th>Policy change</th>
<th>Announced</th>
<th>Implemented</th>
<th>Treasury projected revenue effect (£m)</th>
<th>Motivation given</th>
<th>Classification</th>
</tr>
</thead>
</table>
| 1pp reduction in basic rate of tax   | 18/3/1986    | 6/4/1986    | -830                                 | “It is no accident that the two most successful economies in the world, both overall and specifically in terms of job creation those of the United States and Japan - have the lowest level of tax as a proportion of GDP. Reductions in taxation motivate new businesses and improve incentives at work. They are a principal engine of the enterprise culture, on which our future prosperity and employment opportunities depend”.
|                                      |              |             |                                      | (a) Hansard, HC Deb 18 March 1986 vol 94 c182.                                                                 | Exogenous       |
| Increase in personal allowance      | 24/11/2008   | 6/4/2008    | -3370                                 | “To prevent the recession from deepening, we also need to take action to put money into the economy immediately”. “The Government has already taken action to help people through the current global economic difficulties. Building on this, the Government announces further packages of targeted support, providing additional help to those who need it most now”.
|                                      |              |             |                                      | (b) Hansard, HC Deb 24 November 2008, c495. (c) HM Treasury (2008): Pre-Budget Report.                                                                                                                                         | Endogenous      |
E Other expenditure categories: confidence bands

Figure 14: Dynamic effects of a per-taxpayer liability change in the allowance and basic rate of income tax on the change in per-capita expenditure on food, strictly non-durable and non-housing goods and services across housing tenures. Shaded areas (grey lines) represent 68% (95%) confidence bands.
References


