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# Do households with debt cut back their consumption more? New evidence from the United Kingdom

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**Abstract**

We investigate whether the debt position of UK households affects the response of nondurable consumption to income and wealth changes. We construct a novel estimate of nondurable consumption to track the same individual households over time for an extended period ranging from 1993 to 2017. Using this series, we explore how household indebtedness propagates negative and positive income and wealth changes to consumption responses. We assess whether negative and positive shocks imply the same consumption adjustments and whether such mechanism is crisis specific. Our evidence reveals that falls in income trigger substantially larger adjustments in consumption than income rises for households with debt, while the findings for wealth are less conclusive. The results also point to a macro-financial link between a debt overhang and consumer spending, which carries implications for macroprudential policy makers aiming to ensure household resilience. These effects are not specific to the financial crisis period.

KEYWORDS
consumption, debt, income, wealth

**JEL CLASSIFICATION**
D14, D31, E21, H31

# 1 INTRODUCTION

Is the consumption of indebted households more sensitive to income shocks? The answer to this question informs our understanding of how indebtedness impacts macroeconomic dynamics. It is important for quantifying the effects of monetary and fiscal policy and understanding household resilience to shocks.

We address this question by investigating the link between adjustments in consumption after income or wealth shocks, while controlling for the level of household indebtedness prior to the shock. We use UK household data from 1993 (or 1994 in changes) to 2017, a period that encompasses rising and falling household indebtedness before and after the great financial crisis (*GFC*). Recent studies—in the aftermath of the Great Recession—suggest that high levels of household indebtedness can lead to deeper recessions.[1] A key channel for debt effects on consumption is that heavily indebted households find it harder to smooth their expenditure patterns in the event of an income shock, either because they have fixed debt–service obligations, fewer savings to draw on, or because they are unable to borrow more due to financial constraints and limited collateral capacity. These effects can also drag on the economic recovery as indebted households save more to rebuild balance sheets.

We contribute to the literature on consumption responses to income shocks for indebted households. By combining several household surveys in a panel, we document changes in household indebtedness and leverage over almost a quarter of century. We are able to look at the response of *individual* households to income changes, controlling for ex ante indebtedness, including debt–service, debt-to-income, and debt-to-asset (leverage) ratios. While other studies have used UK data to look at aggregate consumption and debt dynamics using synthetic panel methods, such as Cloyne and Surico (2017), there is less empirical analysis on household-level consumption–income dynamics, conditional on debt. Tracking individual household behavior is important, as synthetic panels are not a substitute for genuine household panel data and are prone to different sources of bias (see Khan, 2021; Verbeek, 2008, for example). Furthermore, studies that look at individual household responses to income shocks by debt or wealth, such as Bunn et al. (2018), Christelis et al. (2019), and Drescher et al. (2020), often use answers to hypothetical questions that ask households how they would respond to a temporary income shock. These studies are usually limited to short sample periods or are prone to biases associated with online surveys.

Another contribution of our study is the exploration of asymmetric consumption responses to shocks, that is, depending on whether the income or wealth change is positive or negative.[2] Our data allow us to check the presence of asymmetry for indebted households for a

[1] See, for example, Jarmuzek and Rozenov (2019), Rogoff and Reinhart (2010), and Dynan (2012). Heterogeneities among households due to their debt positions make these channels also relevant for heterogenous agent models, for example, Kaplan et al. (2018).

[2] The intuition is that in a life-cycle model with financial constraints, the adjustment is sharper under negative shocks, as financial constraints become binding and the household cannot draw on its liquid wealth or borrow to smooth consumption (Carroll & Kimball, 1996; Christelis et al., 2019). Other reasons of asymmetry reported in the literature include precautionary savings and loss aversion (Caballero, 1990; Kahneman & Tversky, 2013).

substantially larger period compared to previous studies that mainly focus on the postcrisis period. This is important for the generalizability of the debt channels outside periods when credit supply may also be constrained by lenders' own balance sheet problems. Investigating asymmetric responses carries also useful policy implications. For example, if asymmetry exists, expansionary monetary and fiscal shocks may yield a smaller impact on consumption than contractionary shocks of the same size. It follows that expansionary policies should be larger than contractionary policies to generate equivalent effects on household expenditure (Bunn et al., 2018).

Our estimation results show that household indebtedness exerts a significant impact on how consumption responds to shocks, and negative income shocks in particular. The effects are largest for the debt–service ratio: our estimate suggests that households with higher debt–service ratios—that is, in the top quartile of indebted households by debt–service—are almost three times more sensitive to negative income shocks, when compared to low or medium debt–service households. This asymmetry points to the prevalence of a liquidity risk channel for indebted households, who cut back their consumption more in response to income shocks than debt free households. We evaluate the indebtedness levels of households explicitly as propagators of income and wealth shocks, by exploring the interactions of the indebtedness metrics with transitions to unemployment (by members of the household), as well as changes in gross housing wealth. Importantly, we show that the debt effects are present throughout the entire period of investigation (1993–2017) rather than only in the period following the global financial crisis, as reported in recent UK-related papers. This finding implies that policies related to household indebtedness—be it fiscal, monetary, or macrofinancial—should not be focused solely on financial crisis periods but also apply more generally when unemployment or income risk for (indebted) households is elevated.

The paper proceeds as follows. Section 2 reviews the literature on consumption responses to shocks, focusing on the role of household debt burden. Section 3 describes the data, including how we combine several UK panel and cross-sectional surveys to create our longitudinal data covering nondurable spending, income, and indebtedness. Section 4 presents our regression results. Section 5 concludes.

## 2 | INDEBTEDNESS AND CONSUMPTION DYNAMICS: A REVIEW

In the life-cycle framework, the response of consumption to income changes depends on the permanence and predictability of shocks, and the ability to smooth. With no uncertainty and no liquidity or borrowing constraints, a *permanent* income shock reduces spending one-for-one. In the case of a transitory income shock—again, assuming no liquidity or credit constraints—households adjust their consumption by only a small fraction, as they aim to smooth it across their life cycle (Friedman, 1957).

Households facing credit constraints, or with limited savings, may find it harder to smooth consumption in response to variable income. This is one channel whereby indebtedness can matter for the consumption response to income changes (Baker & Yannelis, 2017; Deaton, 1991; Jappelli & Pagano, 1994; Le Blanc & Lydon, 2020; Zeldes, 1989). Credit constraints can affect consumption behavior even when they are not currently binding. For example, if increased uncertainty about future earnings raises the prospect of binding constraints in the future, precautionary savings can increase (Crossley & Low, 2014).

Changes in wealth can also affect consumption smoothing. More indebted households, which might already have high leverage ratios, are more at risk if housing equity is a potential source of borrowing (de Roiste et al., 2021; Hurst & Stafford, 2004; Zhu et al., 2019). This is also related to the idea of "debt deflation" in Fisher (1933), whereby households facing declining asset values save more out of their income to repay their debts and increase net wealth. Dynan (2012) also cites "target-leverage" motivations as one reason for the larger consumption fall of indebted US households during the Great Recession, echoing the findings in Bunn and Rostom (2014) and Albuquerque and Krustev (2018).

Indebtedness can also lead to asymmetric consumption responses. That is, spending changes differ depending on whether the income change is positive or negative. Jappelli and Pistaferri (2010) provide a rationale for asymmetry as follows: under a negative income shock, credit-constrained households are unable to bring future consumption forward, leading to a larger adjustment, whereas under positive income shocks, households are able to save and smooth consumption in the future, and, therefore, there is no need for temporary adjustment to be that high.

Several other papers have looked also at asymmetry in the consumption response of UK households. Cloyne and Surico (2017) construct a semiaggregated pseudo-panel dataset to show that the consumption response of indebted homeowners to negative income changes is larger than that of outright homeowners. Exogenous income shocks are identified using a narrative approach based on legislative tax changes. Bunn et al. (2018) use the Bank of England Internet/NMG Consulting Survey to estimate the marginal propensity to consumer (MPC) out of hypothetical unanticipated income shock.[3] They show that borrowers' responses to income changes are systematically larger than those for savers, and that households with low liquid wealth or more debt also have larger MPCs. They also find large asymmetric effects, that is, significantly larger MPCs out of negative income shocks compared to positive shocks. Kovacs et al. (2018) also construct a synthetic panel from different household surveys to look at the interaction of debt with income shocks. They find that highly leveraged households reduced their consumer spending by more during the financial crisis. Furthermore, the debt vulnerabilities vary with the type of debt instruments held by households.

This paper, while building on the earlier work on asymmetric consumption responses of indebted households, differs in three important ways. First, we use actual panel data instead of pseudo-panel or synthetic panel data to study this relationship. As pointed by Verbeek (2008), synthetic panels are not a substitute for genuine household panel data, especially when an endogenous variable is used in the design panel cohorts, for example, household's region or tenure status. A drawback of grouping households by their housing tenure status to construct synthetic panels is the likelihood of endogenous transitions from one tenure status to another over time because of changes in the dependent variable (i.e., moves induced by income shocks/tax changes). Additionally, synthetic panels are prone to aggregation and sampling error biases (Khan, 2021), while Windsor et al. (2015) show that they tend to underplay the significance of age controls and inflate consumption responses.

Second, our dataset allows us to estimate the responses different responses to positive and negative income shocks, while controlling for ex ante indebtedness and changes in wealth. This subsampling is not possible in smaller surveys, nor in synthetic panels, which are largely limited to exploring the differences in the responses of outright homeowners and mortgagors. Importantly, in synthetic panels, asymmetries can only be identified across cohort and not within, that is whether the average income of a given cohort rises or falls. This type of aggregation bias can

[3] The NMG survey has been documented to suffer from the typical issues related to Internet surveys, namely nonresponse bias and extreme outliers. If no substantial adjustments are made, it is assumed that households that choose not to respond are similar to those who do respond. A detailed discussion about the characteristics of the NMG survey can be found in Barwell et al. (2006).

limit the identification of asymmetric behavior only during times when a recession affects cohorts defined by region in an imbalanced way, with positive and negative shocks taking place.[4] By contrast, in an actual panel, we can identify idiosyncratic income changes regardless of the state of the business cycle.

Third, we apply the Blundell et al. (2004, 2008) imputation approach for household spending on nondurables to household panel data for the period from 1992 to 2017, a substantially longer time period than all of the existing studies for the United Kingdom. Etheridge (2015) applies a similar procedure to analyze consumption dynamics, but only uses the BHPS dataset from 1991 to 2006, therefore missing out on the financial crisis period, when consumption fell sharply, as well as the period after the crisis.

# 3 | DATA USED IN THE ANALYSIS

## 3.1 | Survey data

As there is no UK survey data that track the spending of the same household across time, most related studies use synthetic panel estimation, as outlined above. As the British Household Panel Survey (BHPS) and, its successor, Understanding Society (USoc) contain all the key conditioning variables we are interested in—that is, income, indebtedness, and wealth—we adopt a different approach, imputing nondurable spending in these data from expenditure surveys.

We use data from five UK household surveys. Data on incomes, housing wealth, and indebtedness comes from the BHPS (1993–2008) and USoc (2009–2017) survey.[5] As they are two different samples, there is a gap in 2009 when looking at year-on-year changes. Unless stated otherwise, the dataset used in our baseline estimations is a pooled sample of two unbalanced panels accounting for all households appearing in at least two consecutive waves of each survey. All estimates are performed by applying longitudinal population weights that mitigate for differential nonresponse and attrition across waves.

As we explain next, we impute total spending on *nondurables* in the panel data (BHPS/USoc) from three expenditure diary surveys, where the key variable for the imputation and linking between surveys is food spending.[6] These surveys are the Family Expenditure Survey (FES, 1991–2000), Expenditure and Food Survey (EFS, 2001–2007), and the Living Cost and Food Survey (LCF, 2008–2017). These are repeated cross-sectional annual surveys, designed to measure household expenditure on goods and services. The structure and coverage of the surveys has evolved over time. We therefore draw on the derived variables in Oldfield et al. (2020) who harmonize the expenditure and demographic data in all three surveys.

As we impute total nondurable spending from food spending, we drop a small number of households in the BHPS/USoc that report zero or incomplete spending on food. We also drop households that report zero net income or have missing values for region, age, and household composition. All analyses are at the household level, except characteristics such as age are of the household representative person, defined as the owner or renter of the accommodation in which the

[4] See, for example, de Roiste et al. (2021) who explore asymmetry for the case in New Zealand for the period following the global financial crisis.

[5] The BHPS starts earlier in 1991. However, total mortgage debt, which we need to construct indebtedness measures, is only available from 1993.

[6] Variable definitions, including what exactly constitutes nondurables, are provided in the Appendix A.

**TABLE 1** Comparison of means between BHPS/USoc and FES/EFS/LCF

| | 1992–2000 | | 2001–2008 | | 2009–2017 | |
|---|---|---|---|---|---|---|
| | BHPS | FES | BHPS | EFS | USoc | LCF |
| Age of household head | 52.4 | 46.7 | 54.6 | 47.1 | 53.9 | 47.5 |
| Gender | | | | | | |
| Male | 0.7 | 0.7 | 0.6 | 0.7 | 0.5 | 0.7 |
| Region | | | | | | |
| England | 0.9 | 0.8 | 0.9 | 0.8 | 0.8 | 0.8 |
| Wales | 0.1 | 0.0 | 0.1 | 0.0 | 0.1 | 0.0 |
| Scotland | 0.1 | 0.1 | 0.1 | 0.1 | 0.1 | 0.1 |
| Northern Ireland | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 |
| Household type | | | | | | |
| Single adult | 0.3 | 0.2 | 0.3 | 0.2 | 0.3 | 0.2 |
| Single adult, kids | 0.1 | 0.1 | 0.1 | 0.1 | 0.1 | 0.1 |
| Single adult, no kids | 0.3 | 0.2 | 0.3 | 0.2 | 0.3 | 0.2 |
| Two adults, kids | 0.3 | 0.3 | 0.3 | 0.3 | 0.2 | 0.3 |
| More than two adults, kids | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 |
| More than two adults, no kids | 0.0 | 0.1 | 0.0 | 0.1 | 0.1 | 0.1 |
| Household size | 2.3 | 2.6 | 2.3 | 2.6 | 2.4 | 2.6 |
| Tenure status | | | | | | |
| Owner | 0.3 | 0.3 | 0.3 | 0.3 | 0.3 | 0.3 |
| Mortgagor | 0.4 | 0.4 | 0.4 | 0.4 | 0.3 | 0.3 |
| Renter | 0.3 | 0.3 | 0.3 | 0.3 | 0.3 | 0.3 |
| Food expenditure[a] | 1841.6 | 2074.2 | 2379.5 | 2443.1 | 2842.1 | 3024.7 |
| Net household income[a] | 10,592.5 | 12,816.1 | 15,509.7 | 17,277.8 | 21,222.7 | 21,204.6 |
| Observations | 40,472 | 69,056 | 59,473 | 69,195 | 185,264 | 48,910 |

Abbreviations: BHPS, British Household Panel Dataset; USoc, Understanding Society; FES, Family Expenditure Survey; EFS, Expenditure and Food Survey; LCF, Living Cost and Food Dataset.
[a]Equivalized using OECD equivalence scales.

household lives. If there are multiple owners or renters, the eldest of them is the Household Reference Person (HRP). Additionally, food expenditure, which is crucial for the imputation, is missing from the first wave of BHPS and mortgage debt outstanding is not available in waves 1 and 2 of BHPS; we drop the first wave from our entire analysis and keep the second wave only in the imputation but not in the regressions. Mortgage debt outstanding was also not reported in waves 2, 3, and 4 of USoc. However, we can impute it by adding the variable of "amount of additional mortgage on home" to the initial mortgage outstanding after subtracting in-between repayments.

Table 1 shows the summary statistics of the household panel (BHPS/USoc) and expenditure (FES/EFS/LCF) datasets for three subperiods corresponding to each of the expenditure surveys outlined above. Beyond the fact that BHPS/USoc household heads appear to be older on average, the data sources are remarkably similar across all subperiods, including across income and food expenditure. As food expenditure is the key linking variable for the imputation of total nondurable spending, Figure 1 compares the trends in food spending across time and characteristics in the panel and expenditure datasets. In all cases, we find that food expenditure has a very


(a) Total food expenditure across time in USoc and LCF

(b) Total food expenditure by age in USoc and LCF

(c) Total food expenditure by household structure in USoc and LCF

(d) Total food expenditure by region of residence in USoc and LCF

**FIGURE 1** Comparing food expenditure levels between BHPS/USoc and FES/EFS/LCF [Colour figure can be viewed at wileyonlinelibrary.com]

similar pattern across the various sources. Nevertheless, as is shown in the next section, we take care to correct for any discrepancies in food consumption attributed to the design of the two surveys.

## 3.2 | Imputing consumption in the BHPS/USoc

With a dearth of panel data on household spending in many countries, imputation has a long history in economic analysis of consumer spending. Earlier papers used food consumption directly to *proxy* total consumption expenditure (see, e.g., Altonji & Siow, 1987; Hall & Mishkin, 1982; Zeldes, 1989). However, divergent, and often not well-understood, results led many to question this approach.

The "accounting identity" approach estimates consumption as a residual, by subtracting the flows of saving from flows of income across time (see, e.g., Andersen et al., 2016; Browning et al., 2013; Disney et al., 2010; Ziliak, 1998). A common limitation of the approach is that it requires accurate data on savings flows, which is not straightforward to collect, even in dedicated wealth surveys (see, e.g., Cussen et al., 2018). A further complication arises if the specified model uses income or wealth as explanatory variables, which are also used in the imputation of consumption, the results may be biased (Browning et al., 2014).

The third approach is to use additional covariates, on top of food expenditure, as well as information from other surveys to impute consumption (see, e.g., Lamarche, 2017; Skinner, 1987). However, this ignores the impact of relative prices on consumption, which may lead to imprecise estimates (Ziliak, 1998). Blundell et al. (2008) address these issues by specifying an Engel curve of nondurable expenditure, which depends on prices, the overall household budget, household sociodemographic characteristics, and their time interactions.

Following Blundell et al. (2008), we impute nondurable expenditure in a panel dataset (BHPS/USoc), using information drawn from consumer diaries (FES/EFS/LCF).[7] The imputation method includes the following steps: First, we estimate demand for food in the FES/EFS/LCF sample, using nondurable consumption, socioeconomic characteristics, time interactions, and relative prices as covariates.[8] Next, we store the estimated coefficients from all the regressors in the estimation. Second, we specify a similar food demand equation in BHPS/USoc, which we invert by solving for nondurable consumption. Third, we feed the estimated coefficients from FES/EFS/LCF to the same regressors on the inverted food demand equation in BHPS/USoc. Consequently, we can predict nondurable consumption for the same individual households across time.

Etheridge (2015) also uses consumer diaries to impute consumption in the United Kingdom. The approach in this paper differs in two respects. First, he looks at the BHPS until 2006, while we also draw on USoc. USoc is not only a much larger sample than BHPS but also food consumption is treated as a *continuous* variable that reduces measurement error bias.[9] Additionally, Etheridge (2015) relies on the estimation of income elasticities. As income elasticities out of food consumption are typically very small, and at times almost zero, it is harder to invert a food expenditure curve that includes individual income, so we follow Blundell et al. (2008) and omit it in our specification. Furthermore, given our aim in this paper to look at the relationship between income changes and nondurable spending, it would be potentially spurious to include individual income in the estimation.

The specification for estimating the Engle curve for food demand (i.e., the log of total food expenditure for the period 1992–2017) is as follows:

$$f_{i,t} = W'_{i,t}\mu + p'_t\theta + \beta(B_{i,t})c_{i,t} + e_{i,t}, \tag{1}$$

where $f_{i,t}$ is the log of real food expenditure (equivalized), $W'_{i,t}$ is a vector of sociodemographic characteristics including the age of household head, 5-year birth cohorts, the number of children, and region. This vector is available not only in the FES/EFS/LCF but also BHPS/USoc. $p'_t$ is a vector of monthly price indices from the ONS. Total spending on nondurables is captured by $c_{i,t}$, and includes the following spending categories: total food and nonalcoholic beverage, beverages and tobacco products, total clothing and footwear, total housing, water, and electricity (excluding furniture and restoration expenses), total health expenditure, total transport costs, total communication expenditure, and total amount spent on recreation activities. The budget elasticity $\beta$ is

[7] Our analysis focuses on nondurable consumption because the long-lived nature of durable goods provides the household with a flow of utility for multiple periods, which is hard to translate into consumption services for the time period associated to the income/wealth effect.

[8] Following Engel curves literature (Cox & Wohlgenant, 1986), we control for relative prices, by drawing on monthly food, transport and fuel price indices, from the Office of National Statistics.

[9] For the BHPS, we use the midpoint of expenditures reported between each band.

**TABLE 2** Instrumental variable regression of total food expenditure

| | Total food consumption (logged) | |
|---|---|---|
| | Coefficient | SE |
| Nondurable consumption (ln) | 0.73*** | (0.01) |
| Age of household head | 0.00 | (0.00) |
| Age of household head squared | −0.00 | (0.00) |
| Food prices UK, monthly (ln) | −0.60*** | (0.15) |
| Fuel prices UK, monthly (ln) | −0.13* | (0.06) |
| Transport prices UK, monthly (ln) | −0.32*** | (0.09) |
| England | −0.02 | (0.01) |
| Wales | −0.05*** | (0.00) |
| Scotland | −0.04*** | (0.01) |
| Two children | 0.22* | (0.10) |
| Three children | 0.27* | (0.10) |
| Four children | 0.58*** | (0.16) |
| Household size | 0.12*** | (0.00) |
| Observations | 201,206 | |
| $R^2$ | 0.63 | |

*Note*: FES/EFS/LCF (1991–2017): includes additional controls for 5-year birth cohorts and number of children. The natural logarithm of total consumption is interacted with year dummies and number of children in the household. Price indices are CPIH components from the Office of National Statistics. Standard errors are in parentheses. (*), (**), (***) significant at the 10%, 5%, and 1% levels, respectively.

allowed to vary with time and the number of children in the household, represented by the vector $B_{i,t}$. Last, $e_{i,t}$ captures unobserved heterogeneity and measurement error in food demand.

When estimating Engel curves of food consumption, Attanasio et al. (2012) highlight two potential sources of endogeneity. First, in a multiple-stage system with consumption allocation across time, different agents might have different intertemporal preferences on their consumption decisions. For example, impatient agents with strong preference for food might have a higher level of consumption in the first period, as well as a higher share of food consumption. These preferences are not observed in the model, and therefore captured by the error term. Second, endogeneity might be due to measurement error. To address this, we follow the relevant literature and use clusters of hourly wages by birth cohort, occupational status, and survey year for both household head and spouse as instruments for total consumption (see, also Blundell et al., 2008).[10]

Table 2 presents the instrumental variable regression results estimated in the FES/EFS/LCF sample. The dependent variable is the natural logarithm of total food consumption. The nondurable spending elasticity is 0.73 and the price elasticity is −0.60. The Hansen test for overidentification of instruments for consumption of nondurables fails to reject the null hypothesis that all instruments are uncorrelated with the error term with a $p$-value of 0.16. Using these coefficients on our BHPS/USoc household panel data, we invert the Engel curve to impute a series of nondurable consumption as follows:

$$\hat{c}_{i,t} = \beta B_{i,t}{}^{-1}(f_{i,t} - W'_{i,t}\mu + p'_t\theta). \tag{2}$$

[10] Our results remain robust to other clustering choices, for example, the inclusion of income percentiles instead of the occupational status.

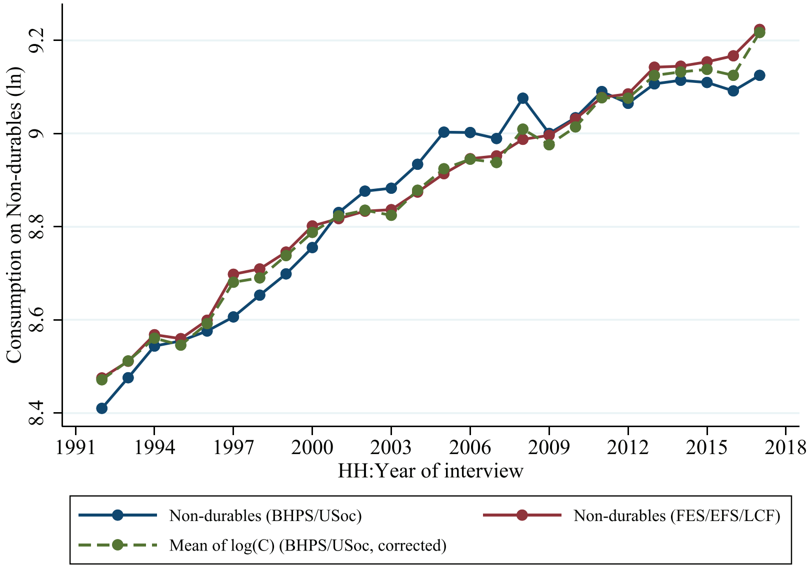



**FIGURE 2** Equivalised spending on nondurables [Colour figure can be viewed at wileyonlinelibrary.com]

Figure 2 shows that the level and trend of mean-imputed nondurable consumption in BHPS/USoc (blue line) are very similar to those in the FES/EFS/LCF (red line). The imputed BHPS/USoc level is lower than the FES/EFS/LCF level at times, consistent with the lower levels of food consumption in the former, as shown in Table 1. To check that this is indeed the case and not a problem of misspecification, we also estimate a "corrected" measure of imputed nondurable spending in BHPS/USoc (the green line in Figure 2), based on Blundell et al. (2004). The correction involves dividing the differences in food expenditures between the two datasets with the coefficients of the Engel specification and subtracting them from the imputed consumption estimate (i.e., $M(c_u) - \frac{M(f_u)-M(f_f)}{\hat{\beta}}$). The mean of the corrected imputed series is practically identical the FES/EFS/LCF series, implying that our model specification provides a precise estimate of nondurable consumption in the BHPS/USoc, and any difference should be attributed to deviations in food expenditure levels between the two surveys.

# 4 | NONDURABLE SPENDING, INCOME CHANGES, AND DEBT

## 4.1 | Baseline results

We now turn to the main research question of this study, which is to explore how households adjusted their nondurable consumption expenditure in response to changes in income and wealth. First, we estimate a baseline regression relating annual changes nondurable spending to changes in income and wealth over the period 1992–2017. We then add controls for indebtedness and sign of the income shock to test for debt and asymmetry effects. The baseline specification is as follows:

$$\Delta C_{i,t} = \alpha + \beta_Y \Delta Y_{i,t} + \beta_w \Delta W_{i,t} + \beta_u \Delta U_{i,t} + \beta_r \Delta r_{i,t} + \gamma_1 X_{i,t} + \gamma_2 Z_i + u_{i,t}, \tag{3}$$

**TABLE 3** Baseline regression for change in nondurable spending (1994–2017)

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Annual change in spending on nondurables | | | |
| | All households | Owners | Mortgage | Renters |
| Change in income | 0.066*** | 0.032*** | 0.079*** | 0.0898*** |
| | (0.004) | (0.007) | (0.007) | (0.00888) |
| Change in house value | 0.050*** | 0.042*** | 0.047*** | 0.0147 |
| | (0.011) | (0.015) | (0.015) | (0.0834) |
| Enter unemployment | −0.049*** | −0.024 | −0.026* | −0.0783*** |
| | (0.010) | (0.023) | (0.015) | (0.0164) |
| Change in mortgage interest rate | −0.005 | 0.002 | −0.008* | −0.00795 |
| | (0.003) | (0.006) | (0.004) | (0.00718) |
| Change in household size | 0.033*** | 0.043*** | 0.019*** | 0.0338*** |
| | (0.004) | (0.010) | (0.006) | (0.00784) |
| Constant | 0.0342 | −0.0784 | 0.0187 | 0.0739 |
| | (0.0274) | (0.137) | (0.0302) | (0.0578) |
| Constant | 0.054** | −0.050 | 0.035 | 0.0940* |
| | (0.026) | (0.132) | (0.029) | (0.0564) |
| Observations | 171,452 | 56,400 | 64,787 | 50,265 |
| $R^2$ | 0.004 | 0.002 | 0.004 | 0.006 |
| Number of observations | 38,878 | 13,322 | 15,503 | 14,213 |

*Note*: Standard errors are in parentheses. (*), (**), (***) significant at the 10%, 5%, and 1% levels, respectively. House value for renters is regional mean within year. Sample is 1994–2017. Age and education controls for head of household are also included. The change in the mortgage interest rate and household size is semielasticities per unit change, where the mortgage interest rate is measured in percentage.

where $\Delta C_{i,t}, \Delta Y_{i,t}, \Delta W_{i,t}$ refer to annual changes in the log of nondurable consumption, total net household income, and housing wealth, respectively. $\Delta U_{i,t}$ captures the change that takes place when at least one member of the household has entered a state of unemployment between $t$ and $t-1$. $\Delta r_{i,t}$ is the change in the mortgage interest rate.[11] $X_{i,t}$ is a $1 \times K$ vector of time varying household characteristics including mainly changes in family size. $Z_i$ is a $1 \times G$ vector of time invariant characteristics including educational qualifications and the age of household head. We apply the inverse hyperbolic sine transformation on consumption, income, and wealth changes that allows us to retain zero or negative values for natural logarithm and reduce high influence of extreme value observations.[12]

Table 3 presents the baseline regression estimates, both for all households and by tenure status.[13] We split tenure because in the next stage of the estimation, when we condition on indebtedness, we focus on mortgaged households only. The estimated income and wealth elasticities are

[11] Monthly interest rate statistics from the Bank of England, averaged to annual frequency.

[12] The inverse hyperbolic sine transformation of a variable $x_{it}$ is written as $\log(x_{it} + (x_{it}^2 + 1)^{1/2})$, and behaves similarly to a logarithmic variable (Dynan, 2012; Pence, 2006). For more details on the advantages of the transformation, see (Burbidge et al., 1988).

[13] As we use an estimated dependent variable (EDV) in our model, for example, imputed nondurable consumption, it is important to consider potential issues with EDVs reported in the relevant literature. In a renowned article, Lewis and Linzer (2005) report that in models fitting EDVs, variation in the sampling variance of the observations on the dependent

similar to those in the literature that uses longitudinal household-level data (see, e.g., Dynan, 2012; Disney et al., 2010), but generally lower than estimates produced using aggregate time-time series or synthetic panel approaches (see Baker & Yannelis, 2017; Bunn & Rostom, 2014; Campbell & Cocco, 2007). Renters present the highest income elasticities (0.09), followed by mortgagors (0.08) and outright owners (0.03). For renters, the coefficient for housing wealth, measured by changes in regional house prices, is found insignificant. This is consistent with the results in Campbell and Cocco (2007) who include an identical "housing wealth" control for renters in order to rule out the possibility that changes in wealth are really picking up the correlation between house prices and income expectations.

The dummy variable, which equals 1 when a member of the household becomes unemployed, is negatively correlated with nondurable spending. The coefficient is only statistically significant for mortgaged and renter households, ranging from 3% to 8%, respectively, which is similar in scale to the results in Christelis et al. (2015). Sharper adjustments for mortgagors and renters may imply the presence of financial constraints, which limit consumption smoothing in the event of unemployment. Income changes naturally correlate with the unemployment variable and, when we exclude the latter, the income elasticity increases. However, we include it in our specification as it captures both changes in the current level of income resulting from the transition to unemployment and potential uncertainty about future income. As we explain next, the average household income shock associated with a member of the household becoming unemployed is large (over −15%), and income remains below preunemployment levels for 4 years on average. Changes in household size (a semielasticity) are positively correlated with changes in spending for all tenure groups, but especially for outright homeowners and renters.

## 4.2 | Consumption responses to income shocks conditional on debt

In this section, we assess whether the debt position of the household matters for consumption adjustments to income or wealth shocks. We focus on owner–occupier households with a mortgage only. In addition to changes in income and wealth, we employ three indebtedness indicators:

- The debt–service burden (*DSR*): This is the ratio of mortgage debt repayments to net disposable household income on a monthly basis.
- The debt-to-income (*DTI*) ratio: This is the ratio of the stock of outstanding owner–occupier mortgage debt to net disposable income.
- The debt-to-assets (*LEV*) ratio (also called leverage or loan-value ratio in the literature): This is the ratio of the stock of outstanding owner–occupier mortgage debt to the gross value (owner-occupied) property, as reported by the householder in BHPS/USoc.

Each indicator implies different mechanisms on how indebtedness influences household consumption (Kukk, 2016). *DSR* is a financial distress indicator as it reflects the way both interest rate

variable is likely to induce heteroscedasticity. To assess the variance of our residuals in all specifications fitting a dependent variable we (i) ran Breusch–Pagan/Cook–Weisberg tests for heteroscedasticity and (ii) visually assessed whether our residuals present an obvious pattern against the fitted values in our models. Our formal test for specification 3 yields a test statistic of 1.21. When compared to a Chi-squared distribution with 1 degree of freedom, the $p$-value is estimated at around 0.2, which falls well above the standard 0.05 level. Thus, we failed reject the null hypothesis of homoscedasticity. Likewise, our visual inspection of the residuals provided us no clue of potential heteroscedasticity in our EDV. The figures presenting residuals against fitted values can be provided upon request from the authors.

and income shocks influence a household's repayment capacity. *DTI* is a liquidity risk indicator and reflects the vulnerability of households to changes in their capacity to reimburse mortgage debt following income shocks. Lastly, *LEV* is a solvency risk indicator as it tracks households' ability to pay back their mortgages if their property were to be sold at the market price. *LEV* is therefore mostly associated with housing wealth shocks.

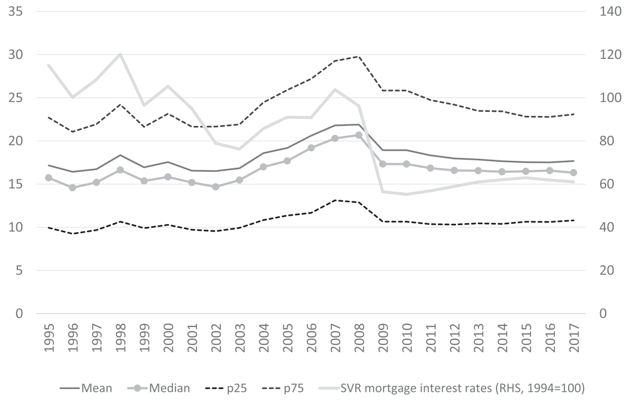
Figure 3 shows, for the estimation sample 1994–2017, the trends in each of the indebtedness metrics for the mean, median, and interquartile range. The large increase in household indebtedness and house prices in the early 2000s, up to the eve of the financial crisis, is apparent in all three charts. The increase in indebtedness is particularly acute in the right tail of the distribution, shown by the 75th percentile cutoff in the charts. After little in the way of change for the first decade or so of the sample, for the top 25% of indebted households by debt–service (first chart) debt–service increased by over eight percentage points between 2003 and 2008, to almost 30% of net income. This is partly driven by rising interest rates during that period (as shown by the gray line in the chart). However, as the more than 100-point increase (from 2.5 to $> 3.5$) in the p75 debt-to-income ratio shows, this is not just driven by rising interest rates but also households taking on more debt over time.

Next, we visually inspect the sensitivity of changes in spending to changes in income, according to the level of indebtedness for each metric. Figure 4 shows the mean change in nondurable spending for a given change in income (5% buckets). We also condition on high, medium, or low values for each of the indebtedness metrics. The thresholds for each of these categories is based on the average cut-offs for the interquartile ranges across the sample period, as shown in Figure 3. For example, a "high" debt–service household is one where at least 25% of disposable income goes toward servicing mortgage debt; a "low" debt–service household is where no more than 10% of income services mortgage debt. The "medium" category is all households in between the 10%–25% range. Similar definitions apply to high, medium, and low debt-to-income and leverage groups, as explained in the table notes.

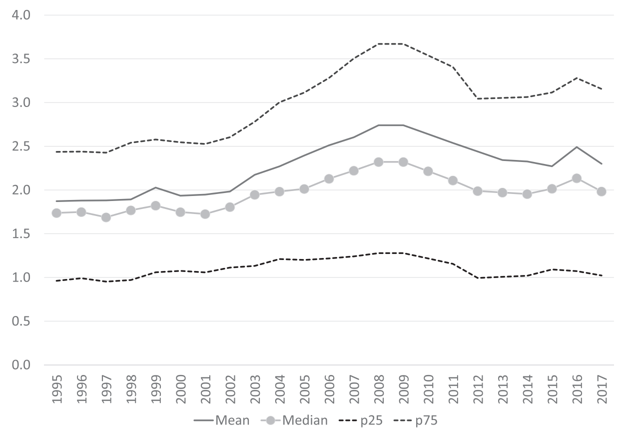
For positive income changes, there is little discernible difference between high-, medium-, and low-debt households, for any of the debt metrics. This is confirmed in Table 4, which tests for differences in the mean consumption change conditional on income changes and indebtedness. We do see a difference for negative income changes ($\Delta C|\Delta Y < 0$ in Table 4), where more indebted households reduce spending by significantly more, especially for large income shocks. The most significant difference is for households with high debt–service burdens and income shocks in excess of −15%. On average, spending for these households falls by almost 11%, compared to 2% for low-debt households facing the same income drop. The almost 9 percentage point difference is statistically significant. We see a similar difference for large income shocks when we compare high-leverage with low-leverage households, albeit borderline statistically significant. High-debt households by debt-to-income are also more sensitive to negative income shocks, although the pattern here is less consistent, with *smaller* negative income shocks appearing to matter more.

The effects of indebtedness on the income elasticity appear to kick-in in some cases when the income shocks are larger, in excess of −15%. Notably, negative income shocks of this size are not uncommon in the data. For example, in our sample period from 1994 to 2017, over a quarter of households experience a *negative* year-on-year fall in disposable income of over 8%. Furthermore, for households where a at least one member enters unemployment during the survey year—on average, 2.7% of households per year, rising to 3.5% during the Great Recession—the *average* fall in disposable income in the first year of the shock is −15.4%. It is only by the fourth year do we see average income for these households return to preunemployment levels.

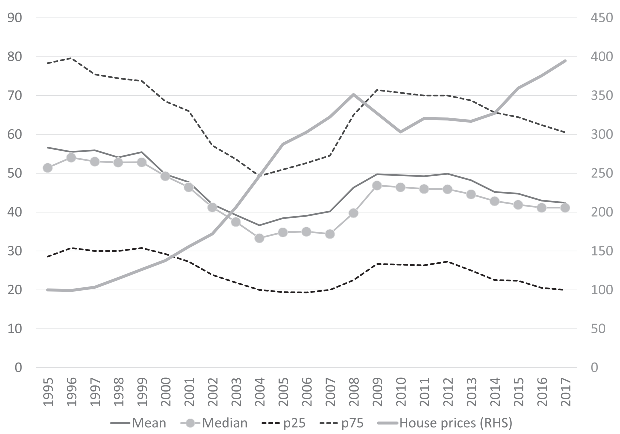
**FIGURE 3** Indebtedness trends (owner–occupier mortgages)



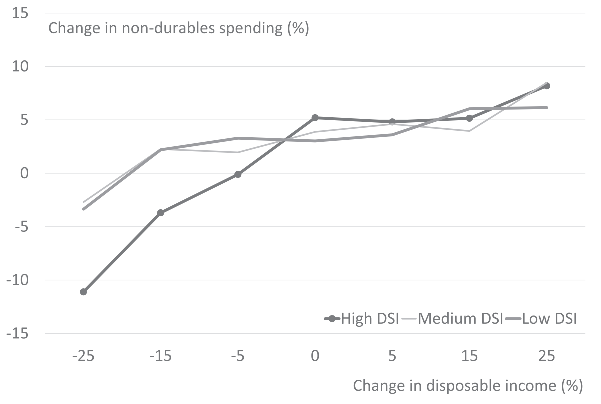

(a) Debt-service ratio (per cent)



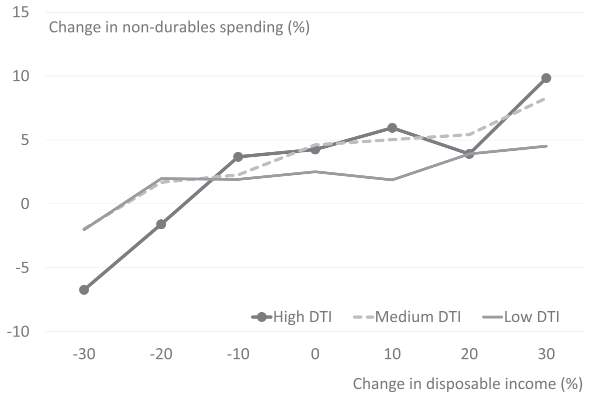

(b) Debt-to-income ratio (per cent)



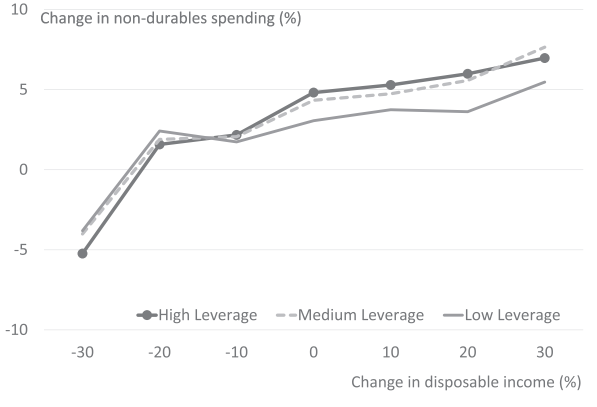

(c) Debt-to-asset (LTV) ratio (per cent)




(a) Income and expenditure changes by debt-service ratio (per cent)




(b) Income and expenditure changes by debt-to-income ratio (per cent)




(c) Income and expenditure changes by leverage ratio (per cent)

**FIGURE 4** Income and expenditure changes by indebtedness

*Note*: (*) Top-coded at 15+ for cell-size reasons. "High" *DSR*/*DTI*/*LEV* thresholds are 25/3/70; "low" thresholds for *DSR*/*DTI*/*LEV* are 10/1/2; and "medium" are all values in between. *X*-axis categories are buckets of changes in income, for example, "5" is for a 5%–9% change.

**TABLE 4** Testing for differences in $\Delta C|\Delta Y$, by indebtedness

| | High debt (A) | Medium/low debt (B) | Diff. (B)–(A) | $H_0$: Diff. = 0 *t*-stat |
|---|---|---|---|---|
| Debt–service | | | | |
| $\Delta C\|\Delta Y < 0$ | −0. 0264 | 0.0127 | **0.0391** | **4.90** |
| | (0.0069) | (0.0039) | (0.008) | |
| $\Delta C\|\Delta Y \leq -15\%$ | −0.1048 | −0.0194 | **0.0853** | **4.73** |
| | (0.0134) | (0.0119) | (0.0179) | |
| $\Delta C\|\Delta Y \geq 0$ | 0. 0539 | 0.0511 | −0.003 | −0.39 |
| | (0.0067) | (0.0028) | (0.0073) | |
| Debt-to-income | | | | |
| $\Delta C\|\Delta Y < 0$ | −0.0091 | 0.0058 | **0.0149** | **1.81** |
| | (0.0046) | (0.0068) | (0.0083) | |
| $\Delta C\|\Delta Y \leq -15\%$ | −0. 0737 | −0.0577 | 0.0853 | 0.79 |
| | (0.0142) | (0.0146) | (0.0203) | |
| $\Delta C\|\Delta Y \geq 0$ | 0. 0446 | 0.0496 | 0.0050 | 0.71 |
| | (0.0032) | (0.0062) | (0.0070) | |
| Leverage | | | | |
| $\Delta C\|\Delta Y < 0$ | 0.0061 | 0.0003 | −0.0058 | −0.60 |
| | (0.0043) | (0.0087) | (0.0097) | |
| $\Delta C\|\Delta Y \leq -15\%$ | −0.0914 | −0.0500 | **0.0414** | **1.60** |
| | (0.0234) | (0.0112) | (0.0260) | |
| $\Delta C\|\Delta Y \geq 0$ | 0.0508 | 0.0469 | −0.0040 | −0.58 |
| | (0.0062) | (0.0032) | (0.0069) | |

*Note*: Standard errors are in parentheses. The sample is owner–occupier households with a mortgage in the United Kingdom from 1994 to 2017. Each row shows the annual change in spending on nondurables ($\Delta C$) for a given change in income, that is positive (or zero) ($\Delta Y \geq 0$), negative ($\Delta Y < 0$), or large negative falls ($\Delta Y < -15\%$). Columns (A) and (B) condition on the level of mortgage indebtedness. The "High" *DSR*/*DTI*/*LEV* thresholds are 25/3/70, as outlined in the text. The column "Diff (B)–(A)" is the difference in the two group ("High" debt vs. all other mortgaged households) means. The final column reports the *t*-statistic from the null hypothesis $H_0$ : $\mathrm{mean}(B) - \mathrm{mean}(A) = 0$, assuming unequal variances in the two groups.

We next estimate a regression to formally quantify how indebtedness affects the sensitivity of households to shocks. We build on the baseline specification in Equation (3) by adding incrementally the following variables. First, we add the lagged indebtedness metrics, namely $DSR_{t-1}$, $DTI_{t-1}$, and $Lev_{t-1}$, and interact them with changes in income and wealth, one at a time. The lag aims to capture the debt position before the income or wealth shock takes place. This is close to the approach of Baker (2018) who looks at how indebtedness impacts total consumption–income elasticities in the United States. The main difference of our approach to Baker (2018) is that, in addition to including lagged leverage and *DTI*, we also look at debt–service and differentiate between positive and negative income shocks. We include the interaction with gross housing wealth changes to capture credit constraint or balance sheet rebuilding channels, as in Dynan (2012) and Mian and Sufi (2009). The specification is similar to the one in Equation (3), though a *second* interaction term is added to the income and wealth variables to control for asymmetric responses following positive or negative changes. Lastly, we add a *third* interaction term (*GFC*) for the period 2008–2012 to test whether our results are specific to the financial crisis period. Not only does the inclusion of the dummy allow us to compare our results with a wide number of papers that focus only on the crisis or its early aftermath (see, e.g., Christelis et al., 2015; Kovacs et al., 2018), but it is also an important test of the generalizability of our hypothesis. If debt matters only for how households react to shocks during the financial crisis, then it is hard to separate aggregate credit supply effects relating to financial institutions' balance sheets, or the severity of income and wealth shocks (business cycle effects[14]), from demand side issues relating to household overindebtedness.

We present separate results for each of debt–service (Table 5), debt-to-income (Table 6), and leverage (or debt-to-assets, Table 7), focusing on the coefficients of the various income or wealth changes.[15] With regard to the income elasticities conditional on debt, the results confirm what we see in the graphical analysis in Figure 4: spending of more indebted households is more sensitive to changes in their income, but only for negative income changes. The regression shows that not only are these debt effects statistically significant but they are also economically large. To illustrate, in specification (2) of the debt–service regression (Table 5), the coefficient on the negative income change interacted with *DSR* is estimated at about 0.4, implying that going from a debt–service ratio of 10%–30% —approximately the interquartile range—increases the sensitivity to negative income shocks almost threefold, from an income elasticity of around 0.05 to 0.13. Higher debt-to-income and leverage households are also more sensitive to negative income shocks. However, the incremental impact on the income elasticity when income falls is largest for the debt–service ratio, reflecting the importance of debt payments, as opposed to the size of debt outstanding, for indebted households' consumption adjustment to income shocks.

The third specification (column 3) tests whether the debt effects we observe are specific to the financial crisis period. For all three debt metrics, the coefficient on the negative income shock interacted with indebtedness is larger during the financial crisis. However, the debt effects for negative income changes remain both statistically and economically significant for periods outside of the financial crisis, indicating that this is not simply a crisis effect. The increased magnitude of the coefficients during the *GFC* can be attributed to business cycle effects magnifying the severity of shocks and tighter credit constraints due to limited supply of bank lending throughout this period. However, a Wald test for equality of non-*GFC* and *GFC* coefficients on the negative income change–indebtedness interaction is not rejected, returning an $p$-values of 0.20, 0.32, 0.41 in the *DSR*, *DTI*, and leverage regressions, respectively.

The interaction of housing wealth effects with the debt measures are not as consistent as the income results. The debt–service ratio has no incremental effect on the consumption-wealth elasticity. We do find incremental wealth effects for both debt-to-income and leverage, similar to the results in Mian et al. (2013), albeit only when housing wealth falls. Furthermore, we do not observe these channels playing a significant role during the financial crisis. The results on wealth should be read with some care as they indicate rather large standard errors. When compared to Disney et al. (2010), which also use the BHPS data for the period between 1994 and 2003 but changes in savings to back-out changes in consumption, we find slightly stronger income effects

[14] Although it is tricky to completely rule out business cycle effects, having a larger sample should moderate their impact. A similar point is made in Christelis et al. (2019).

[15] For some BHPS/USoc households, there are missing values for overall mortgage debt. A small number of households ($<$ 1.5%) also report excessive debt–service ratios, i.e., $DSR > 100\%$. A small number of owner-occupier mortgaged households with missing values for overall mortgage debt and/or debt service (less than 1.5%) is dropped from the estimated sample at this stage. Furthermore, as the basic results—such as income and wealth elasticities are almost identical to those for mortgaged households in the baseline regression in Table 3—this should not carry significant implications for the analysis in this section; our baseline results remain robust to the exclusion of these observations.

**TABLE 5** Debt–service ratio and sensitivity of households to changes in income

| Variables | (1) | (2) | (3) |
|---|---|---|---|
| | Annual change in spending on nondurables | | |
| Change in income | 0.067*** | 0.050*** | 0.051*** |
| | (0.009) | (0.010) | (0.010) |
| $DSR_{t-1}$ | −0.033 | 0.018 | −0.004 |
| | (0.020) | (0.023) | (0.022) |
| Change in income$\times DSR_{t-1}$ | 0.099*** | | |
| | (0.035) | | |
| Change in income $\geq 0 \times DSR_{t-1}$ | | 0.054 | |
| | | (0.036) | |
| Change in income $< 0 \times DSR_{t-1}$ | | 0.398*** | |
| | | (0.071) | |
| Change in income $\geq 0 \times DSR_{t-1} \times GFC = 0$ | | | 0.054 |
| | | | (0.038) |
| Change in income $\geq 0 \times DSR_{t-1} \times GFC = 1$ | | | 0.041 |
| | | | (0.049) |
| Change in income $< 0 \times DSR_{t-1} \times GFC = 0$ | | | 0.290*** |
| | | | (0.076) |
| Change in income $< 0 \times DSR_{t-1} \times GFC = 1$ | | | 0.428*** |
| | | | (0.102) |
| Change in house value | 0.072*** | 0.075*** | 0.058*** |
| | (0.022) | (0.022) | (0.022) |
| Change in house value$\times DSR_{t-1}$ | −0.122 | | |
| | (0.104) | | |
| Change in house value $\geq 0 \times DSR_{t-1}$ | | −0.200 | |
| | | (0.122) | |
| Change in house value $< 0 \times DSR_{t-1}$ | | −0.013 | |
| | | (0.138) | |
| Change in house value $\geq 0 \times DSR_{t-1} \times GFC = 0$ | | | −0.105 |
| | | | (0.122) |
| Change in house value $\geq 0 \times DSR_{t-1} \times GFC = 1$ | | | −0.040 |
| | | | (0.206) |
| Change in house value $< 0 \times DSR_{t-1} \times GFC = 0$ | | | 0.201 |
| | | | (0.165) |
| Change in house value $< 0 \times DSR_{t-1} \times GFC = 1$ | | | −0.047 |
| | | | (0.190) |
| Enter unemployment | 0.011 | 0.010 | 0.010 |
| | (0.021) | (0.021) | (0.021) |
| Enter unemployment $\times DSR_{t-1}$ | −0.251** | −0.209** | −0.165* |
| | (0.098) | (0.098) | (0.094) |
| Constant | 0.066*** | 0.063*** | 0.065*** |
| | (0.010) | (0.010) | (0.010) |
| Observations | 61,678 | 61,678 | 61,678 |
| $R^2$ | 0.006 | 0.007 | 0.007 |

*Note*: Sample is owner–occupier households with a mortgage, 1994–2017. *DSR* is the debt–service ratio, as defined in the text. *GFC* is a dummy variable equal to 1 for the financial crisis period (2008–2012). Standard errors are in parentheses. (*), (**), (***) significant at the 10%, 5%, and 1% levels, respectively. For variable definitions, see notes to Table 3.

**TABLE 6** Debt-to-income ratio and sensitivity of households to changes in income

| Variables | (1) | (2) | (3) |
|---|---|---|---|
| | Annual change in spending on nondurables | | |
| Change in income | 0.075*** | 0.057*** | 0.058*** |
| | (0.008) | (0.009) | (0.008) |
| $DTI_{t-1}$ | −0.001 | 0.002* | 0.001 |
| | (0.001) | (0.001) | (0.001) |
| Change in income$\times DTI_{t-1}$ | 0.002 | | |
| | (0.001) | | |
| Change in income $\geq 0 \times DTI_{t-1}$ | | −0.000 | |
| | | (0.001) | |
| Change in income $< 0 \times DTI_{t-1}$ | | 0.021*** | |
| | | (0.004) | |
| Change in income $\geq 0 \times DTI_{t-1} \times GFC = 0$ | | | −0.000 |
| | | | (0.001) |
| Change in income $\geq 0 \times DTI_{t-1} \times GFC = 1$ | | | 0.001 |
| | | | (0.002) |
| Change in income $< 0 \times DTI_{t-1} \times GFC = 0$ | | | 0.015*** |
| | | | (0.005) |
| Change in income $< 0 \times DTI_{t-1} \times GFC = 1$ | | | 0.022*** |
| | | | (0.006) |
| Change in house value | 0.045** | 0.050*** | 0.047** |
| | (0.019) | (0.019) | (0.019) |
| Change in house value$\times DTI_{t-1}$ | 0.004 | | |
| | (0.005) | | |
| Change in house value $\geq 0 \times DTI_{t-1}$ | | −0.005 | |
| | | (0.007) | |
| Change in house value $< 0 \times DTI_{t-1}$ | | 0.016** | |
| | | (0.008) | |
| Change in house value $\geq 0 \times DTI_{t-1} \times GFC = 0$ | | | −0.001 |
| | | | (0.007) |
| Change in house value $\geq 0 \times DTI_{t-1} \times GFC = 1$ | | | −0.000 |
| | | | (0.012) |
| Change in house value $< 0 \times DTI_{t-1} \times GFC = 0$ | | | 0.025*** |
| | | | (0.009) |
| Change in house value $< 0 \times DTI_{t-1} \times GFC = 1$ | | | −0.002 |
| | | | (0.012) |
| Enter unemployment | 0.002 | −0.001 | −0.009 |
| | (0.018) | (0.019) | (0.018) |
| Enter unemployment $\times DTI_{t-1}$ | −0.014*** | −0.011** | −0.009* |
| | (0.005) | (0.005) | (0.005) |
| Constant | 0.058*** | 0.055*** | 0.057*** |
| | (0.010) | (0.010) | (0.010) |
| Observations | 51,582 | 51,582 | 51,582 |
| $R^2$ | 0.006 | 0.006 | 0.007 |

*Note*: Sample is owner–occupier households with a mortgage, 1994–2017. *DTI* is debt-to-income, as defined in the text. *GFC* is a dummy variable equal to 1 for the financial crisis period (2008–2012). Standard errors are in parentheses. (*), (**), (***) significant at the 10%, 5%, and 1% levels, respectively. For variable definitions, see notes to Table 3.

**TABLE 7** Leverage ratio and sensitivity of households to changes in income

| Variables | (1) | (2) | (3) |
|---|---|---|---|
| | Annual change in spending on nondurables | | |
| Change in income | 0.075*** | 0.076*** | 0.072*** |
| | (0.011) | (0.011) | (0.010) |
| $LEV_{t-1}$ | −0.001 | 0.012 | 0.010 |
| | (0.007) | (0.008) | (0.008) |
| Change in income×$LEV_{t-1}$ | 0.005 | | |
| | (0.018) | | |
| Change in income $\geq 0 \times LEV_{t-1}$ | | −0.040* | |
| | | (0.022) | |
| Change in income $< 0 \times LEV_{t-1}$ | | 0.052** | |
| | | (0.022) | |
| Change in income $\geq 0 \times LEV_{t-1} \times GFC = 0$ | | | −0.035 |
| | | | (0.022) |
| Change in income $\geq 0 \times LEV_{t-1} \times GFC = 1$ | | | −0.023 |
| | | | (0.035) |
| Change in income $< 0 \times LEV_{t-1} \times GFC = 0$ | | | 0.040* |
| | | | (0.023) |
| Change in income $< 0 \times LEV_{t-1} \times GFC = 1$ | | | 0.070** |
| | | | (0.035) |
| Change in house value | 0.062*** | 0.056*** | 0.060*** |
| | (0.020) | (0.021) | (0.020) |
| Change in house value×$LEV_{t-1}$ | 0.004 | | |
| | (0.005) | | |
| Change in house value $\geq 0 \times LEV_{t-1}$ | | −0.022 | |
| | | (0.029) | |
| Change in house value $< 0 \times LEV_{t-1}$ | | 0.069 | |
| | | (0.059) | |
| Change in house value $\geq 0 \times LEV_{t-1} \times GFC = 0$ | | | −0.026 |
| | | | (0.031) |
| Change in house value $\geq 0 \times LEV_{t-1} \times GFC = 1$ | | | 0.010 |
| | | | (0.042) |
| Change in house value $< 0 \times LEV_{t-1} \times GFC = 0$ | | | 0.114* |
| | | | (0.061) |
| Change in house value $< 0 \times LEV_{t-1} \times GFC = 1$ | | | −0.162 |
| | | | (0.106) |
| Enter unemployment | −0.007 | −0.005 | −0.015 |
| | (0.022) | (0.022) | (0.021) |
| Enter unemployment ×$LEV_{t-1}$ | −0.058 | −0.054 | −0.040 |
| | (0.037) | (0.037) | (0.035) |
| Constant | 0.056*** | 0.055*** | 0.056*** |
| | (0.011) | (0.011) | (0.011) |
| Observations | 51,963 | 51,963 | 51,963 |
| $R^2$ | 0.006 | 0.006 | 0.006 |

*Note*: Sample is owner–occupier households with a mortgage, 1994–2017. *LEV* is the leverage (debt-to-asset) ratio, as defined in the text. *GFC* is a dummy variable equal to 1 for the financial crisis period (2008–2012). Standard errors are in parentheses. (*), (**),(***) significant at the 10%, 5%, and 1% levels, respectively. For variable definitions, see notes to Table 3.

but more moderate wealth effects. This divergence could be attributed to their identification of wealth effects as "unpredictable changes in house prices" (a variable not available in USoc). As far as indebtedness measures are concerned, they find stronger elasticities for households in negative equity experiencing wealth gains. Although we do not control for negative equity explicitly, and find no significant consumption-wealth-debt effects for increases in housing wealth (Table 7). We do, however, observe a larger wealth elasticity for high leverage households when housing wealth falls (0.11), which could be viewed as a being broadly in line with their results, assuming higher leverage households are more likely to be in negative equity after a fall in house prices.

The results in this section show that household indebtedness affects how household spending responds to income and wealth shocks, pointing to the crucial importance of income falls. Our findings confirms evidence from the recent literature for the United Kingdom, the Netherlands, and other countries (Bunn et al., 2018; Christelis et al., 2019). Much of this literature focuses on the financial crisis period, which naturally raises the question of whether the results apply outside of this period. Our analysis using 23 years of data suggests that these debt-channel effects are not specific to the financial crisis. This means that, even outside periods when system-wide issues maybe impinging credit supply, negative income shocks drag more heavily on the spending of more heavily indebted households. Importantly, our use of longitudinal data, revealing individual variations in income and wealth for indebted households, points to the role of idiosyncratic shocks affecting consumption changes. This is important evidence in support of asymmetric expansionary and contractionary policies to support demand, as suggested in Bunn et al. (2018).

## 5 | CONCLUDING REMARKS

This paper explored the sensitivity of UK households spending on nondurables to changes in their income and wealth, conditional on their level of indebtedness. To track individual households over time, we constructed a demand equation framework to combine panel data on income from the BHPS and the USoc surveys, with consumption data from repeated cross-sectional surveys. Using this panel series estimation of nondurable expenditure, we estimated the consumption elasticities out of changes in income and housing wealth, whilst also controlling for households level of indebtedness. All our panel estimations controlled for unemployment transitions and time-invariant household characteristics. We studied how indebtedness, measured by the debt service ratio, the debt-to-income ratio and the loan-to-house-value ratio affected consumption adjustments. We assessed whether negative and positive shocks imply the same adjustments, and also controlled for the Global Financial crisis, aiming to uncover whether business cycle effects affect the relationship. Our evidence reveals that consumption growth is significantly positively correlated with income and wealth. Consumption is negatively correlated with transitions to unemployment, picking up both short-term and longer-lasting income changes. We found that the elasticity of non-durables consumption with respect to income is higher for mortgagors and renters than households owning a dwelling. We found that falls in income triggered larger adjustments in household consumption than income rises, particularly for more heavily indebted households. These amplifications of income shocks via indebtedness are not unique to the financial crisis period. This finding reinforces and generalizes the asymmetric shocks hypothesis, evidenced also in previous analyses for the UK and other countries. Our results have several policy implications. One is that expansionary (or contractionary) monetary policy or counter-cyclical fiscal policy can have larger effects in terms of stabilising output, employment, and demand during a downturn than during an upturn. Furthermore, the impact of these policies can depend on the level of household indebtedness in the economy. The results also point to a macro-financial link between high levels of indebtedness and consumer spending, which should be of interest to macro-prudential policy makers that aim to promote household resilience.

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