Consumption spending, housing investments and the role of leverage

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Abstract

In this paper we estimate the degree to which leverage amplifies the effects of house price shocks on consumer spending. We do by using instrumental variable methods that are able to combine the information in two datasets. One is a panel with information on household balance sheets which does not include consumption spending. The other is a survey with detailed consumption data that does not include information on wealth. We find strong responses of household spending to leveraged housing returns. Spending responses are roughly three times larger for housing investments than they are for nondurable consumption spending. We show how these results can be explained if households treat housing as a financial asset in a model of portfolio choice.

Keywords: House prices, leverage, consumption

1 Introduction

Two factors make movements in house prices especially important for consumption spending and the wider economy. The first is that it represents an significant proportion of households' wealth. In the UK, property accounts for 59% of households aggregate net wealth.[1] The second factor is leverage.

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[1]See Table 2 in ONS (2015). This figure excludes private pension wealth.

The vast majority of UK households purchase their first home with a substantial credit, leading to a large difference between households gross and net asset positions. Under standard mortgage contracts households liabilities do not change as house prices rises and fall, meaning that for leveraged households price movements can lead to significant changes in a households' net housing wealth. As a result, leverage can magnify the effects of economic shocks on households' overall balance sheets, with potentially important implications for their willingness to spend as well as their access to credit.

A number of channels have been proposed through which leverage may amplify the effects of house price shocks on consumer spending. These are firstly: a *portfolio* or wealth channel (that more leverage households will see a greater change in housing wealth for a given change in house prices); a *collateral* channel (that high leverage is an indicator of credit constraints); and a debt-overhang channel (that households with higher leverage might reduce spending so as to meet some target level of leverage). This latter channel is particularly associated with Dynan (2012).

In this paper we make two contributions to the literature on consumption and housing leverage. The first is to suggest a micro-foundation for the debt-overhang channel. In a life-cycle model of household portfolios (e.g. Merton (1969)), desired leverage should evolve smoothly over time. House price increases will leave households over-leveraged relative to this target, inducing a desire to reduce consumption spending and increase savings. We emphasise an important corollary to this effect: in periods where house prices increase, consumers will find themselves *under*-leveraged. This will have the effect of encouraging consumers to borrow and invest in housing stock (or other risky assets). Such portfolio adjustments can be thought of as increasing the *size* of household balance sheets in order to bring them back to their desired rate of return following a price increase (and decrease in their leverage).

Our second contribution is empirical. We investigate how leverage mediates the effects of house price increases on households' consumption and housing investment. To do so we require detailed data on consumer spending that will allow us to break down consumer spending into its consumption and investment components. However, datasets that do this typically do not include information on household's wealth or leverage. We therefore combine information from two surveys. The first contains information on households balance sheets but which does not have detailed consumption data. The second is a national budget survey which, in common with many surveys of consumer spending, contains information on households expenditures but not on their wealth. Our approach is to apply of two-sample IV methods (Angrist & Krueger, 1992) using a source of variation in leverage that is common to both samples. The instrument we use is the a measure of credit conditions when households moved into their first homes (the average loan-to-income ratio on new house purchases).

We show empirically that higher leveraged returns on housing are associated with higher consumer spending and higher rates of secured borrowing. Leverage increases the size of these returns for a given house price increase and therefore the size of these responses. We also find that responses are proportionally much larger for residential investment spending which points to households simultaneously increasing the size of their balance sheets by re-leveraging and investing in the value of their home. We find that effects are smaller for nondurable (consumption) spending than for total spending. The former is arguably a much better estimate of the marginal propensity to *consume* (MPC) which is often the target of micro-studies on consumer responses to house price changes. Our results highlight the importance of looking at consumption responses to house price increases directly rather than relying on consumption proxies such as borrowing or total spending estimated using the dynamic budget constraint. The reliance on these latter measures may result in overestimates of the propensity to consume out of housing wealth.

The remainder of this paper is structured as follows. In Section 2 we discuss conceptual issues around the question of why leverage may matter. Section 3 describes our empirical approach and in particular two-sample and three-sample instrumental variables and how we make use of them in our empirical application. In Section 4 we present our results. Section 5 provides some concluding thoughts.

2 Why does leverage matter?

A number of mechanisms or 'channels' have been put forward to explain why more leveraged households should be more sensitive to wealth shocks.

1. A *portfolio/wealth* channel: Households who have higher leverage will see a greater change in net wealth for a given change in asset prices. Consider for instance two households, A and B, with the same net housing wealth (£20,000) but different gross housing wealth (£100,000 for A and £200,000 for B). Of the two households B is clearly more leveraged. A 10% increase in house prices will represent a 50% increase in housing wealth for A but a 100% increase for B.

2. A *collateral* channel: High leverage can be an indicator that households are credit constrained. In theory, credit constrained households should have a marginal propensity to consume out of wealth gains of 100%, and so we should observe much greater responsiveness of more leveraged households to shocks. The channel is referred to as the collateral channel as households who see house price gains can offer these gains as collateral in order to increase borrowing.

3. A '*debt-overhang*' effect: Households with higher leverage than they are comfortable with may reduce spending so as to meet some leverage target.[2]

[2] Evidence for a debt-overhang effect is presented in Dynan (2012) who uses the PSID from 2005 onwards, and examines how consumption changes differed for more leveraged households from 2005-2007 and 2007-2009. She finds

A number of papers have now attempted to estimate the size of leverage effects and to identify which of these channels is most important. The typical empirical approach to the question of how and why leverage affects household responses to housing wealth shocks, is to run a regression of the form

$$\Delta C_{it} = \beta_0 + \beta_1 \Delta P_{it} + \beta_2 \Delta P_{it} \times f(L_{it-1}) + \epsilon \tag{1}$$

Here C_{it} is some *measure* or indicator of consumption spending, saving or borrowing, P_{it} is the individual's house price and $L_{i,t-1}$ is the individual's lagged leverage (sometimes measured as the loan to value - LTV - ratio on the individual's home). The function $f(.)$ if often a piece-wise linear spline allowing the effects of leverage on house price changes to vary in a nonlinear way. C_{it} is often only an indicator of consumption (or related measure such as borrowing) as comprehensive consumption measures are rarely included in surveys that contain panel data on wealth. This is a point we shall return to in what follows.

The most common finding is that β_2 is positive, but that the amplifying effects of leverage tend to be concentrated among those households who have the very highest leverage levels. Disney, Gathergood, and Henley (2010) find that savings respond very little on average to house price shocks but that the response is five times greater for households emerging from a situation of negative equity than for households with initially positive equity values. In addition, it is often found that the house price effects tend to be larger for households who otherwise appear more likely to be credit constrained. Cooper (2013) for example finds that the consumption of households with high debt service ratios, low liquid wealth and high expected future income growth saw the greatest sensitivity to wealth changes (all characteristics indicative of credit constraints) .

Other studies looking directly at borrowing behaviour often also find that response are largest for those with high leverage and high unsecured debt. Looking at the UK for the years 1995-2005, Disney, Bridges, and Gathergood (2010) find that households' propensity to remortgage does not increase with household's LTV ratios but is higher for those with high LTV *and* high unsecured debt. In a related paper that using the US Panel Study of Income Dynamics, (PSID) Disney and Gathergood (2011) again look at changes in household indebtedness by initial LTV ratios (again allowing these to be interacted with levels of unsecured debt). As before, households with high leverage - particularly those with high leverage and high unsecured debt - were seen to be much more responsive. Mian and Sufi (2011) examine the link between growth in debt and regional variation in home prices in the United States over the period 2002-2006. They instrument for changes in house prices using estimates of the

that those with higher leverage in previous periods not only saw greater consumption declines in the latter period (which covered the recession), but also smaller consumption increases in the years 2005-07 when wealth tended to be increasing. The asymmetric nature of leverage effects point to it having an independent role rather than amplifying wealth shocks. Effects of leverage were also found conditional on household's net wealth changes.

elasticity of housing supply in different regions (reasoning that regions with more elastic supply should see smaller price increases for a given demand shock). Their estimates suggest very strong effects of house price growth on borrowing. Each 1% gain in house prices is estimated to increase borrowing by 0.52 percent. The heterogeneity in this elasticity across consumers is substantial, and they also find that effects are strongest for those who appear credit constrained. For consumers with credit scores one standard deviation below the 1997 mean, the estimated elasticity is 0.35. For those with credit scores one standard deviation above the mean it is 0.75. They find no evidence of house price effects for those in the top quartile of the credit score distribution. Similar differences are observed across consumers with higher and lower credit card utilisation rates. Responses are also in general larger for younger households.

The apparent tendency for homeowners with high unsecured debts to borrow more when house prices increase could either be because households in these circumstances increase their consumption more than other households or because they use second mortgages to substitute secured debt for costly unsecured debt. Without direct data on expenditure it is not possible to directly separate these two potential explanations - potentially a problem for studies that do not observe expenditure in their data. Circumstantial evidence can sometimes be provided however. Mian and Sufi (2011) infer that increased borrowing is used to finance greater spending, since house price increases are not associated with declines in credit card borrowing or the opening of new mortgage accounts in their sample. Arguing that consumption responses may be small, Disney, Bridges, and Gathergood (2010) cite survey evidence pointing to "paying off debt" as an important motivation for refinancing. However, over half of households in the same survey reported using extracted equity to fund home improvements (partly a form of consumption expenditure), and 15% reported using it to finance household purchases. Surveys in the United States also point to increasing expenditures as an important motivation for home equity extraction even if paying off debts were a more commonly cited reason (Brady, Canner, & Maki, 2000).

The fact that evidence of leverage effects seems to be strongest among those with very high leverage levels or high unsecured debt tends to be interpreted as indicating that of the mechanisms listed above the collateral channel is most important.[3] The fact that house price effects are typically small for households who are less likely to be credit constrained by contrast is thought to suggest a limited role for the portfolio channel. This is perfectly possible if traditional housing wealth effects on consumption are small in general. However, there are reasons to view this conclusion as premature. Importantly, the portfolio channel has quite similar empirical implications to the collateral channel, as we now demonstrate with a simple model.

[3] An exception to all these findings is Andersen, Duus, and Jensen (2014) who also find effects of leverage at lower down the leverage distribution as well.

2.1 A stylised model

Consider a stylised world with two assets: a risky asset (housing, h_t) with price p_t, and a risk-free asset which household can short (bond, b_t) with price 1, and interest rate r. This latter asset can be thought of as representing a mortgage. As an empirical matter, housing makes a up a large share of marketable wealth, especially for younger homeowners. For now we assume that housing is neither associated with transaction costs nor with its own independent flow of utility. Moreover, the consumer faces no credit constraints. This latter assumption means that we will be examining a case where the collateral channel is not operative. Nonetheless we will show that returns to house price increases will increase according to the consumer's leverage in a highly non-linear way.

In this setting, households face the following budget constraint

$$x_t = y_t + p_t h_{t-1} + (1+r)\, b_{t-1} \tag{2}$$

where y_t is income, x_t is start of period resources and c_t is consumption spending. The stochastic variables in this model are y_t and p_t. In this model, a homeowner with a typical mortgage will have gross assets of $p_t h_t$ and debt equal to b_t.

Leverage is typically defined as the consumer's loan-to-value ratio which in this case is given by

$$L_t = -\frac{b_t}{p_t h_t} \tag{3}$$

which implies that the consumer's portfolio share in the risky asset (housing) is

$$w_t = \frac{p_t h_t}{p_t h_t + b_t} = \left(\frac{p_t h_t + b_t}{p_t h_t}\right)^{-1} = (1 - L_t)^{-1} \tag{4}$$

Notice here that this implies that the the portfolio share of housing is a very nonlinear transformation of leverage. Moreover, the portfolio share will typically exceed one. Consider for example a young household who has a 5% deposit on a home and thus a 95% loan-to-value ratio. This household will have a portfolio share in housing of 20. A portfolio share of exactly one here implies that the household is an outright owner (and holds no other financial assets).

Now define the return on housing as $r_t^* = \frac{p_t}{p_{t-1}} - 1$. This means that available resources will evolve according to

$$x_t = y_t + \left(1 + r + \frac{1}{1 - L_{t-1}} \left(r_{t-1}^* - r\right)\right) * (x_{t-1} - c_{t-1}) \tag{5}$$

Equation shows (5) shows that leverage acts as a risky portfolio share that magnifies the effects of house price shocks in a non-linear way. The effect on a house price increase on a consumer's net wealth would therefore therefore highly non-linear in leverage even in a setting where credit constraints were not present.

2.2 Leverage as a portfolio choice

At time t, consumers will have a choice over c_t, b_t and w_t (or equivalently L_t).

In a wide-class of models, consumers desire to smooth c_t over the life-cycle. Similarly, in a wide-class of models consumer's *desired* leverage will evolve smoothly over time. This includes seminal models of portfolio choice such as Merton (1969) and Samuelson (1969) in which (given CRRA preferences, perfectly capitalisable labour income and other assumptions) the share of a risky asset in consumer's portfolios is constant over the life-cycle. Other models have extended these cases to consider situations where labour income is not perfectly capitalisable Cocco, Gomes, and Maenhout (2005). However in these models, desired risky asset shares are still expected to evolve in a smooth way.

Now consider the implications of price shocks if consumers desired portfolio shares (and hence leverage) evolves slowly. Recall that leverage is given by

$$L_{t-1} = -\frac{b_{t-1}}{p_{t-1}h_{t-1}}$$

This means that if house prices *rise* then, absent a portfolio adjustment, leverage would fall. As a result, the consumer's risky portfolio share would fall, and so their expected portfolio risk and return fall. In order to maintain their portfolio's desired risk and return, we would therefore expect the consumer to releverage: that is to *borrow more and invest more in housing.* This results from the fact that the portfolio share for housing is typically greater than one. The common intuition that one should sell assets whose prices have risen is only true for assets with portfolio shares between zero and one. In the presence of transaction costs of housing, we might expect consumers to do this by investing in their own home through extensions, rennovations and so on. We look for empirical evidence of these sorts of effects below.

Consider also the case where house prices fall. In this case, again absent a portfolio adjustment, the consumer will find that their leverage has increased. As a consequence the risk and return on the consumer's portfolio would rise. To respond to this the consumer would be expected to deleverage by paying down debt. This could be achieved by selling housing, or if this is again associated with transaction costs, reducing consumption spending and saving. Such reductions in consumption spending would result in debt over-hang effects observed in for example Dynan (2012). Portfolio adjustments therefore provide a micro-foundations for such effects.

2.3 Life-cycle model

[to come]

In a life-cycle model of portfolio choice, desired leverage evolves smoothly (drawing on Merton (1969)).

$$u=\frac{c_t^{1-\gamma}}{1-\gamma}+v\left(h_t\right)$$

$$x_t=y_t+p_t h_{t-1}+(1+r)\,b_{t-1}$$

$$x_t=c_t+p_t h_t+b_t$$

Define leverage as debt divided by gross equity (loan to value ratio)

$$L_t=-\frac{b_t}{p_t h_t}$$

$$b_t=-L_t*p_t h_t$$

$$x_t=y_t+\left(1+r+\frac{1}{1-L_{t-1}}\left(r_{t-1}^*-r\right)\right)*\left(x_{t-1}-c_{t-1}\right)$$

$$x_t-c_t=\left(1-L_t\right)p_t h_t$$

$$V_t\left(x_t\right)=\max_{c_t,L_t}\frac{c_t^{1-\gamma}}{1-\gamma}+v\left(\frac{x_t-c_t}{\left(1-L_t\right)p_t}\right)+\beta E_t\left[V_{t+1}\left(x_{t+1}\right)\right]$$

$$V_t\left(x_t\right)=\max_{c_t,L_t}\left\{\frac{c_t^{1-\gamma}}{1-\gamma}+v\left(\frac{x_t-c_t}{\left(1-L_t\right)p_t}\right)+\beta E_t\left[V_{t+1}\left(y_{t+1}+\left(1+r+\frac{1}{1-L_t}\left(r_t^*-r\right)\right)*\left(x_t-c_t\right)\right)\right]\right\}$$

$$c_t^{-\gamma}-v'\left(\frac{x_t-c_t}{\left(1-L_t\right)p_t}\right)\frac{1}{\left(1-L_t\right)p_t}=\beta E_t\left[V_{t+1}'\left(x_{t+1}\right)\left(1+r+\frac{1}{1-L_t}\left(r_t^*-r\right)\right)\right] \tag{6}$$

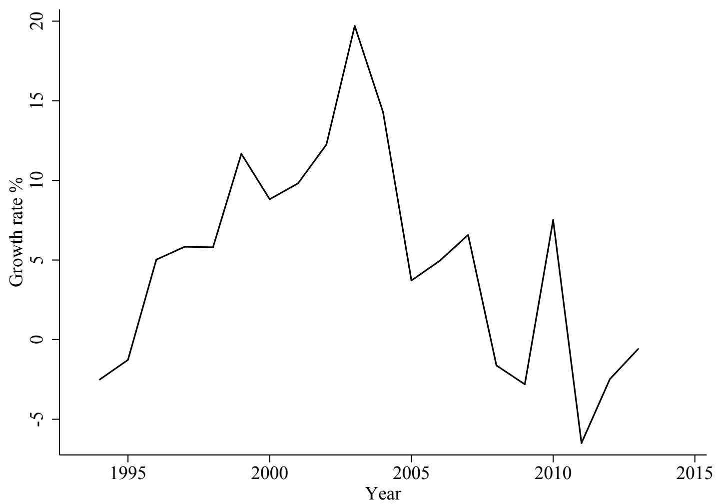
note that in period t there is uncertainty over the future return on equity held at the end of period t. This return is denoted r_t

$$v'\left(\frac{x_t-c_t}{\left(1-L_t\right)p_t}\right)\frac{x_t-c_t}{\left(1-L_t\right)^2 p_t}+\beta E_t\left[V_{t+1}'\left(x_{t+1}\right)\left(r_t^*-r\right)*\left(x_t-c_t\right)\frac{1}{\left(1-L_t\right)^2}\right]=0$$

$$v'\left(\frac{x_t-c_t}{\left(1-L_t\right)p_t}\right)\frac{1}{p_t}+\beta E_t\left[V_{t+1}'\left(x_{t+1}\right)\left(r_t^*-r\right)\right]=0 \tag{7}$$

$$\frac{\partial V_t\left(x_t\right)}{\partial x}=v'\left(\frac{x_t-c_t}{\left(1-L_t\right)p_t}\right)\frac{1}{\left(1-L_t\right)p_t}+\beta E_t\left[\frac{\partial V_{t+1}\left(x_{t+1}\right)}{\partial x_{t+1}}\left(1+r+\frac{1}{1-L_t}\left(r_t^*-r\right)\right)\right]$$

Figure 1: House price growth rates, 1994-2013



Source: Office for National Statistics

Note: House prices are UK averages deflated using the Consumer Price Index.

$$c_t^{-\gamma} = \frac{\partial V_t(x_t)}{\partial x}$$

$$c_t^{-\gamma} - v'\left(\frac{x_t - c_t}{(1-L_t)\,p_t}\right)\frac{1}{(1-L_t)\,p_t} = \beta E_t\left[c_{t+1}^{-\gamma}\left(1 + r + \frac{1}{1-L_t}\left(r_t^* - r\right)\right)\right] \tag{8}$$

3 Empirical evidence for re-leveraging

Managing the evolution of household leverage over the business cycle is a key concern for macro-prudential policymakers. In this section we describe how the leverage of UK households has evolved over time and how it has responded to increases in house prices.

To start with Figure 1 shows how average real house price growth varied from year to year over the period 1994-2013. We deflate house prices using the Consumer Price Index (CPI). For most of this period, house prices were increasing, with annual falls only observed in 1994-1995 and 2007-2009. In real terms the decline in UK house prices over the period of financial crisis were modest compared to those in other countries. In the period in between these years, house prices grew rapidly. Annual price increases peaked in 2003 at a rate of almost 20%.

These price changes are reflected in consumers average loan to value ratios as Figure 2 shows. Average loan to value ratios fell from 54% to under 40% from 1994-2007, before rising again as prices declined in the wake of the financial crisis. As Figure 3 shows however, households do not passively allow leverage to fall as prices rose. The proportion of households aged 25-45 who were taking out additional mortgage debt increased to exceed 10% in the period of most rapid house price growth. Older households responded much less to these developments.

Figure 2: Average loan-to-value ratios, 1993-2011

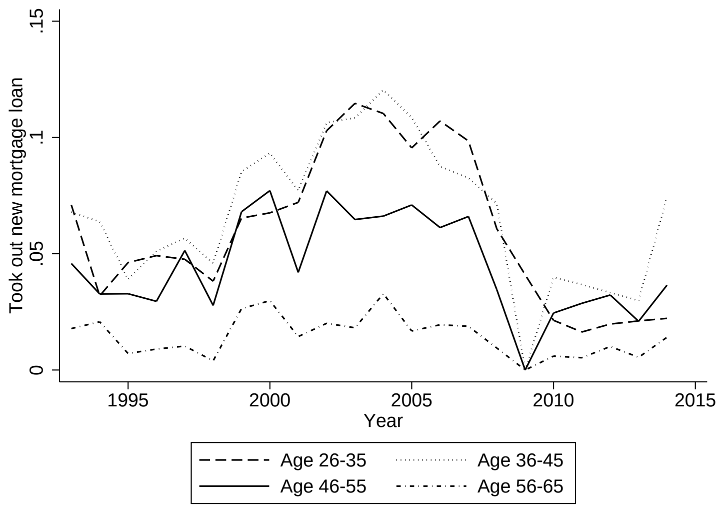
Source: British Household Panel Survey/Understanding Society

In Table **??** we more formally examine the extent to which households adjusted their leverage as prices rose. Here we regress changes in log loan-to-value ratios on changes in households' self-reported home values. Since by definition

$$LTV = \frac{\text{mortgage debt}}{\text{house value}} \tag{9}$$

if mortgage debt did not adjust as house prices increased, then we would expect the coefficient on log house values to equal -1. In column (1) we obtain a coefficient of -0.88. This is significantly different to -1 at the 1% level. This may partly reflect increases in home values due to home improvements or other adjustments to the value of housing rather than changes in mortgage debt. To account for this possibility, in column (2) we instrument the change in self-reported house values with changes in regional house prices. Doing this reduces the value of the coefficient further to -0.56. In column (3) we add a control for age to account for natural developments in leverage over the life-cycle. This does not significantly affect the results.

Figure 3: Proportion of home-owners taking out additional mortgages, 1993-2013



Source: British Household Panel Survey/Understanding Society

Table 1: Changes in leverage and changes in house prices, 1993-2013

$\Delta logLTV$	OLS	IV	IV
	(1)	(2)	(3)
$\Delta \log HValue$	-0.881***	-0.562***	-0.606***
	(0.017)	(0.030)	(0.029)
Age			-0.003
			(0.0002)
R^2	0.240	0.216	0.213
N	26,829	26,694	26,694
Clusters	7,411	7,403	7,403

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The above results show evidence of re-leveraging behaviour in the face of price increases, as we would expect in a model where consumers treated leverage as a portfolio choice.

Figure 4: Proportion with positive spending on housing extensions, 1993-2013

Source: Living Costs and Food Survey and predecessors

4 Spending regressions

With CRRA preferences, consumption should evolve according to

$$\ln C_t - \ln C_{t-1} = \alpha_0 + \underbrace{\alpha_1 r + \alpha_2 w_{t-1} E_{t-1}[r_t^*]}_{\text{anticipated returns}} + \underbrace{\alpha_3 (w_{t-1} \times (r_t^* - E_{t-1}[r_t^*]))}_{\text{unanticipated returns}} + u_{t+1} \tag{10}$$

Now suppose that house prices follow a random walk (and hence that returns r_t^* are i.i.d). This implies that the expectation of future house prices in each period is constant (that is $E_{t-1}[r_t^*] = k$). Hence we can rewrite (10) as

$$\ln C_t - \ln C_{t-1} = \alpha_0 + \alpha_1 r + (\alpha_2 - \alpha_3) k w_{t-1} + \alpha_3 w_{t-1} r_t^* + u_{t+1} \tag{11}$$

This equation gives log consumption growth as a function lagged leverage and of any leveraged asset returns. Higher leverage has the effect of raising the expected return to housing wealth (and hence, other things equal, decreasing the wealth effect of a given realised return to housing).

This equation gives consumption in terms of growth rates. We cannot estimate this on our data however as we do not have access to a consumption panel. Note however that using the first order conditions of the consumer's problem we can exploit the fact that $\ln C_{t-1} = f(\lambda_{t-1})$ to get

$$\ln C_t = \alpha_0 + \alpha_1 r + (\alpha_2 - \alpha_3) k w_{t-1} + \alpha_3 w_{t-1} r_t^* + f(\lambda_{t-1}) + u_{t+1} \tag{12}$$

where λ_{t-1} is the consumer's marginal utility of wealth. While this of course not something we can directly observe, we may proxy for this term using variables in the consumer's current state space. We do this using a rich set of controls including cohort-year interactions, dummies for the consumer's current region, tenure, house-type, lagged log regional house prices, sex and education.

This implies the following empirical model

$$\ln C_t = \beta_0 + \gamma_{ct} + \beta_1 w_{t-1} + \beta_2 \left\{ w_{t-1} \times \left(\frac{p_{rt}}{p_{rt-1}} - 1 \right) \right\} + X\beta_3 + e_t \tag{13}$$

Here γ_{ct} are a set of cohort-year interactions (with cohorts defined using 10 year birth intervals). The inclusion of these dummies means that we identify leverage effects through comparisons *within* rather than across different cohorts. The dummies can be thought of capturing shocks that differ in their effects across young and old. One such shock is to future income expectations which would be expected to boost the consumption of younger (and so more leveraged) cohorts by more. If effect such as these are not controlled for they could lead to spuriously large estimates of house price wealth effects for younger households (Attanasio, Blow, Hamilton, & Leicester, 2009). We run our model on home-owners who have lived in their home for at least one year (and thus exclude those who moved in the current period).

Two issues prevent us from estimating such a model directly. The first is that, as we alluded to above, panel surveys that contain balance sheet data on wealth and leverage rarely contain comprehensive consumption measures. A panel survey is required in order to know the consumer's lagged leverage position w_{it-1}. It would also allow us to represent $f(\lambda_{t-1})$ with a household fixed effect (or similar) in place of our current set of proxies. While some well-known panel surveys (such as the PSID, and Health and Retirement Survey as used to address similar questions in Christelis, Georgarakos, and Japelli (2015)) now include both, they remain relatively short. No such dataset exists for the country we are considering (the UK). Previous studies have either used available proxies for consumption (such as borrowing, (Mian & Sufi, 2011)), subsets of consumption that are observed (e.g. (Lehnert, 2004)) or measures backed out from the consumer's budget constraint (using the difference between observed income and wealth changes, as in Cooper (2013)). Each of these approaches has drawbacks. Changes in particular categories of consumption, or variables related to consumption need not give the full spending response to shocks. They also do not allow us to investigate how the composition of spending varies as house prices change. In addition, the use of budget constraint identity to impute consumption can in general lead to biased estimates of wealth effects in the presence of measurement error (Browning, Crossley, & Winter, 2014). If reported wealth in the previous period is smaller than actual wealth, then leverage as observed by the researcher in that period will be too high and consumption in the current period be too large, biasing estimates upward. Given that house price values and so leverage are typically self-reported such measurement errors may be a significant concern.

A second issue is that leverage is potentially endogenous (that is $cov(L_t, e_t) \neq 0$). The conventional strategy is to use lagged leverage as the leverage measure in equations such as (1). As we have seen however, lagged leverage is itself a choice variable that will depend on households expectations of house prices at the time when consumption decisions are made. As we document in what follows lagged leverage is also correlated with non-housing assets and gross housing wealth. In order for our empirical application to identify the effects of independently varying leverage, these other variables ought to be held constant. We attempt to address both of these issues using two-sample IV methods (Angrist & Krueger, 1992) which allows us to simultaneously impute and instrument for leverage in a (cross-sectional) dataset that contains consumption data using wealth data taken from a household panel survey. Details of our approach are given in Appendix A.

4.1 Data and instrument

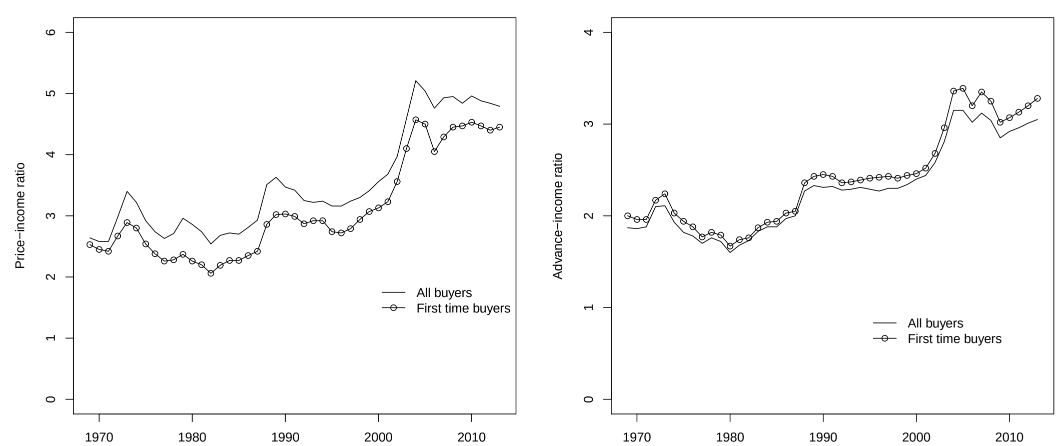
To investigate the relationship between consumption and leverage, we make use of two datasets. The first is the Living Costs and Food Survey and it's previous incarnations the Expenditure and Food Survey and Family Expenditure Survey (which we shall refer to collectively as the LCFS). The LCFS is a comprehensive, long-running survey of consumer expenditures involving between 5,000-8,000 households per year. Households are asked to record high-frequency expenditures in spending diaries over a two week period. Recall interviews are used to obtain spending on information on big ticket items (such as holidays or large durables) as well as standing costs on items such energy and water, internet bills and magazine subscriptions. The survey also collects information on incomes, demographic characteristics and, since 1992, on the value of households' mortgages (but not on other aspects of household balance sheets). The second dataset we use is the British Household Panel Survey and its successor Understanding Society (both of which we shall refer to as the BHPS). The BHPS is available in 18 waves from 1991 to 2008. Understanding Society began in 2009 and incorporated the original BHPS sample members from 2010 onwards. Both surveys include limited information on household spending on food and drink as well as self-reported house values. The BHPS contains data on mortgage values in all years, while Understanding Society dropped these variables in its second wave in 2010. In the remaining years, we continue to observe whether households own their homes outright, and details on the length and type of their mortgage if they have one. We use these along with past information on mortgages values to calculate mortgages in years following 2010. Loan to value ratios are calculated by dividing the value of mortgages by the (self-reported) value of homes.

For house prices we use regional data on the prices of transacted houses published by the Office for National Statistics.

Figure 5: Credit conditions, 1969-2013

(a) Price-income ratios

(b) Advance-income ratios



Consumption spending is observed in the LCFS but leverage is not. At the same time the BHPS includes information on leverage but not on consumer spending.

For our proposed method we require a source of variation in leverage that explains why some households took out larger loans than others that is common to both datasets. For this purpose we exploit variation in the average price to income ratios for new loans at the time households moved into their current residences (denoted P/Y_{-T}). This variable is often used as a measure of the cost of credit (*loan* to income ratios for example included in the credit conditions index of (Fernandez-Corugedo & Muellbauer, 2006)). In our case it indicates the cost of borrowing in the years house prices were made, and so the degree to which households would have been able to leverage their housing purchases at the time they moved.

The solid line in Figure 5 (panel a) shows how this instrument varies over time. There is a gradual upward trend in the price to income ratio suggesting that credit has become looser over time. In 2013, average loans were almost five times greater than the incomes of buyers. This compares to a ratio of 2.5 in 1969. This provides one source of identification. Importantly however, there is also cyclical variation in this variable, with for example evidence of credit tightening following the 2008 financial crisis. Movements in other measures of credit conditions such as the average deposit on new homes (Figure 5, panel b) show similar patterns.

This instrument is credibly related to households' loan-to-value ratios. Figure 6 shows the evolution

Figure 6: Loan to value ratios by age and cohort

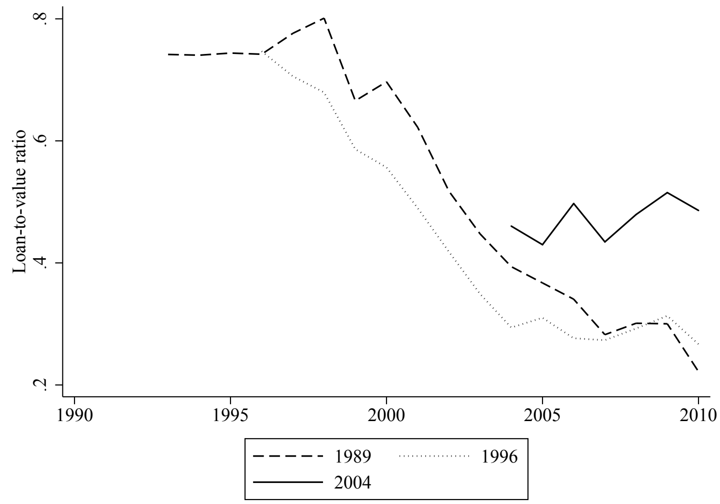
Source: British Household Panel Survey/Understanding Society

of loan-to-value ratios among homeowners for households with heads born within 10-year birth cohorts between 1940 and 1979. Focusing on those aged 45 and under, it is clear that those born in later cohorts tend to have higher loan-to-value ratios than those born earlier. This difference is especially pronounced between the two youngest cohorts (those born in the sixties and those born in the seventies). Those born in the mid-year of these two cohorts would have reached aged 30 in 1995 and 2005 respectively. These are precisely the years between which the average loan-to-income ratios on new loans shown in Figure 5 were increasing most rapidly. The differences that we see in initial leverage however tend to fall over the course of the life-cycle. From around age 45 onwards, the loan-to-value ratios across cohorts are very similar.

Our instrument is only available from 1969 onwards, and so in what follows we drop households who moved into their homes before this. This constitutes 0.5% of the total number of observations in our LCFS sample.

We also drop households where the head is aged under 25 or over 45. This latter restriction is due to the fact that, owing to the limited variation in the leverage of older households, are instrument tends to be much weaker for this group. Finally, to avoid problems of measurement error, when estimating our first stage we drop households who have a lagged leverage portfolio share in the top 1% of the distribution.

Figure 7: Loan to value ratios by age and year moved in (1960s cohort)



Source: British Household Panel Survey/Understanding Society

4.2 Instrument relevance and validity

Two requirements must be satisfied in order for our instrument to be considered appropriate. The first is that the instrument must be relevant (that it is indeed correlated with the endogenous variables it is replacing). Figure 7 shows how our instrument relates to loan-to-value ratios for a given cohort (those born in the 1960s). This is the only ten-year birth cohort that we observe for almost our entire sample period. We plot loan-to-value ratios for households who moved into their homes in three different years: 1989,1996, and 2004. These three years represent peaks and troughs in price-to-income ratios on new housing purchases from panel a in Figure 5. Price to income ratios reached a temporary high of 3.7 in 1989 before falling to a low of 3.2 in 1996. Thereafter they increased to a peak of 5.2 in 2004. As Figure 7 shows, households that moved when price-to-income ratios were relatively high in 1989 tended to have higher leverage than those in the same cohort who moved in in 1996. This is true not only at the point they moved in to their current homes but also long-afterward. Loan-to-value ratios are also persistently higher for those who moved in when credit conditions were even looser in 2004.

This relevance of our instruments can be more formally tested by looking at the results of first stage regressions. We do this in Table 2.

We have two first stage regressions, one for leverage and one for leverage interacted with house pricses. In both cases, the F-statistics are greater than the value of 10 suggested as a rule of thumb by Staiger and Stock (1997) for IV estimated using a single sample. Two sample IV methods may suffer less of a bias than standard 2SLS estimators, as errors in the first stage estimation will be unrelated to errors in the second stage equation. This is indeed the rationale for estimators that run first and second stages in split samples (Angrist & Krueger, 1995)). Nonetheless weak instruments may still result in coefficients being biased towards zero in finite samples. The relatively strong first stage we obtain is therefore reassuring. Kleibergen-Paap statistics for the first stage also heavily reject the hypothesis of underidentification.

A further 'first stage' check we can conduct is to look for a positive association between our instrument and total mortgage debt in the LCFS. This would us that the association between our instrument and leverage is not limited to our first sample. Regressing mortgage debt on (P/Y_{-T}) and out controls yields a positive coefficient with a t-statistic of 21.03.

Table 2: First stage results

	w_{it-1}	$w_{it-1} \times (\frac{p_{rt}}{p_{rt-1}} - 1)$
P/Y_{-T}	0.691	-0.009
	(0.010)	(0.007)
$P/Y_{-T} \times \ln(P_{rt})$	-1.102	0.846
	(0.248)	(0.030)
Shea partial R^2	0.010	0.122
F-value (p-value)	34.36	427.98
	(<0.001)	(<0.001)
Kleibergen-Paap (p-value)	39.964	
	(<0.001)	
N	14,912	
Clusters	4,396	

The second requirement for a suitable instrument is that it is itself uncorrelated with the error term. This is normally an assumption which cannot be verified. Omitted variables are typically omitted because they are unobserved, and so it is usually impossible to test for an association between them and our instruments (conditional on the exogenous elements of $\mathbf{M}$). However, when using a two sample approach, such tests are possible. Some variables may be observed in the sample in which we run our first stage regressions even if they are not present in our main sample.

In our case, there may be concerns that those who move home in years with higher price-income ratios will have spending patterns that are different to those who moved in other years for reasons other than the degree of their leverage. The most obvious challenge is that since price-income ratios have tended to increase over time, those households with higher values of our instrument will tend to have moved more recently. They may therefore be younger, or be more likely to furnishing a new home. We address these concerns of this nature directly by including cohort-year interactions, and years of tenure in the home as controls. Questions about endogeneity may remain however. For example, households may have been more likely to move when house prices were high because greater unobservable wealth made them less price sensitive. This would create a spurious association between our instrument and consumption. Households who moved at times when credit was loose may also be more likely to move in response to economic shocks and drop out of our sample introducing a selection bias.

To address these concerns we look for an association between our instruments and household labour income, asset incomes and the probability of being a mover in the BHPS and Understanding Society panels conditional on our covariates. Table 3 reports results from regressions of these potential sources of endogeneity on our instruments and the variables included in $\mathbf{X}$. The instruments are both jointly and individually insignificant in all models suggesting that they are plausibly orthogonal to these omitted variables. In addition to the results shown in Table 3, we also regress unsecured debt to income ratios and an indicator for whether households have positive debts on our instruments. Debts are only observed in 3 of the 18 waves of the BHPS survey, and so these tests are necessarily conducted on a much smaller sample. The instruments are again individually and jointly insignificant (with F-statistics 0.98 and 0.50 respectively).

A further exercise we can do is test to how our instrument compares to leverage positions in the previous period. This is the source of variation used in a number of previous studies (e.g. Disney et al. (2010), Dynan (2012)). The results of this comparison are shown in Table 4. While our instrument remains uncorrelated with each of these variables, there is strong evidence that those with higher lagged leverage have fewer financial assets and lower home values.

5 Results

Table 5 shows results for using our estimation equation (13) for different forms of spending. These include log total spending, log nondurable spending (consumption), log total spending less residential investment, and the log of household mortgage debt. For residential investment itself, we use the inverse hyperbolic sine transformation rather than the log of spending as many households record zero spending on this category. This variable approximates log values at high values of spending but remains defined at zero.

The regressions show that leveraged returns are positively associated with both increased total and

Table 3: Exogeneity of instruments

Dependent var.	ln(HH income)	Invest inc. > 1000	Invest inc.$= 0$	Mover$_{t+1}$
P/Y_{-T}	0.011	-0.003	-0.009	-0.014
	(0.018)	(0.008)	(0.019)	(0.007)
$P/Y_{-T} \times (\frac{p_{rt}}{p_{rt-1}} - 1)$	0.016	0.009	0.024	-0.004
	(0.049)	(0.023)	(0.047)	(0.022)
P-values				
P/Y_{-T}	0.55	0.72	0.64	0.06
$P/Y_{-T} \times (\frac{p_{rt}}{p_{rt-1}} - 1)$	0.75	0.69	0.61	0.85
F-test	0.79	0.85	0.77	0.15
N	13,990	14,527	14,527	11,804
Clusters	4,359	4,352	4,352	3,625

Note: Controls for education, cohort-year dummies, sex, region, house type, number of rooms, number of adults, number of children, years at address, and lagged log regional house prices. Standard errors clustered at the individual level.

consumption spending. Our results imply that for a 10% increase in house prices, total spending would be 1.04% higher for outright owners or 2.08% higher for those with a loan-to-value ratio of 50% (i.e w_{it}=2). The equivalent nondurable consumption increases would 0.75% and 1.5% respectively. These are substantial responses, though the implied elasticities are lower than those found in some US-based studies (such as Mian and Sufi (2011)).

Consumption responses are however not as great as those for residential investment spending. The implied spending elasticity for this category of spending is over three times as large as that for nondurable consumption spending.

The final column of Table 5 shows how these increases in expenditure might be financed. Each 10% increase in house prices for an individual with a 50% loan to value ratio is associated with a 41.4% increase in mortgage debt. We therefore see substantial evidence of equity release in the face of price rises. The increase in spending on residential investment that we see at the same time points towards the releveraging of consumer portfolios in response to price increases that we discussed above.

Table 4: Credit conditions vs. lagged leverage

	P/Y_{-T}	L_{t-1}
Mover_{t+1}	-0.028*	0.000
	(0.016)	(0.010)
1<Invest inc.<100	0.003	-0.025***
	(0.015)	(0.008)
100<Invest inc.<1000	0.002	-0.060***
	(0.016)	(0.009)
Invest inc.> 1000	-0.012	-0.125***
	(0.026)	(0.019)
ln(House value)	0.006	-0.089***
	(0.017)	(0.012)
F-test (p-value)	0.58	0.00
R^2	0.742	0.306
N	11,464	11,509
Clusters	3,563	3,574

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Controls for education, cohort-year dummies, sex, region, house type, number of rooms, number of adults, number of children, years at address, and lagged log regional house prices. Standard errors clustered at the individual level.

Table 5: Log spending regressions

	Total	Nondurables	Res inv. (IHS)	Total - Res	Mort. debt
	(1)	(2)	(3)	(4)	(5)
w_{it-1}	-0.007	-0.002	-0.044	0.000	0.171***
	(0.013)	(0.012)	(0.010)	(0.012)	(0.038)
$w_{it-1} \times (\frac{p_{rt}}{p_{rt-1}} - 1)$	0.104***	0.075**	0.284**	0.096***	0.207**
	(0.036)	(0.032)	(0.144)	(0.034)	(0.103)
R^2	0.305	0.309	0.073	0.299	0.356
N	29,557	29,557	29,557	29,557	27,060

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Marginal propensity to consume/borrow is calculated at average UK house prices in 2013 and at median consumption/debt values. Figures are for each £1000 house price increase.Controls for education, cohort-year dummies, sex, region, house type, number of rooms, number of adults, number of children, years at address, and lagged log regional house prices. Standard errors clustered at the individual level.

6 Conclusion

In this paper, we show that viewing leverage as a portfolio choice over risky assets can help to explain non-linear effects of leverage in amplifying wealth shocks on consumption, deleveraging and releveraging behaviour over the business cycle and also residential investment over the house price cycle.

Empirically we find evidence that households view housing as a financial asset. Leveraged households see greater consumption gains in response to price increases and they invest more in housing as house prices rise in order to rebalance their portfolios.

Our results suggest that leverage works to amplify the effects of house price shocks. Moreover house price rises themselves lead households to releverage and so to increase the 'size' of their balance sheets. These effects would be expected to in turn make households more sensitive to future shocks to their housing wealth.

These findings suggest that macro-prudential policy interventions aimed at limiting releveraging behaviour could have an important role in stablising consumption over the business cycle.

Appendix A Two-sample IV

Two sample instrumental variables (TSIV) is best explained by first considering a standard two-stage least squares (2SLS) approach.

Let $\mathbf{M} = [\ X \quad \ln P \quad L \quad L \times \ln(P)\ ]$ denote the $n \times (k+p)$ matrix of right-hand side variables (p of which are endogenous). Suppose we face the problem of consistently estimating the $1 \times (k+p)$ vector of coefficients δ in the model

$$C = \mathbf{M}\delta + u$$

where L and u are correlated. It is well known that the coefficients estimated using a naive OLS regression of C on $\mathbf{M}$ will be biased. To solve this problem, instrumental variable methods make use of an $n \times (k+q)$ matrix of instruments $\mathbf{Z}$ where the p endogenous variables in $\mathbf{M}$ are replaced with $q \geq p$ variables that are assumed to be exogenous. This assumption implies that $E[u|\mathbf{Z}] = 0$ and means that δ can be consistently estimated using the 2SLS estimator

$$\hat{\delta}_{2SLS} = (\hat{\mathbf{M}}'\hat{\mathbf{M}})^{-1}\hat{\mathbf{M}}'C \tag{14}$$

where $\hat{\mathbf{M}} = \mathbf{Z}(\mathbf{Z}'\mathbf{Z})^{-1}\mathbf{Z}'\mathbf{M}$, or the fitted values from the set of reduced form regressions of the columns of $\mathbf{M}$ on $\mathbf{Z}$

$$\mathbf{M} = \mathbf{Z}\Pi + v$$

Notice here that while this estimator requires knowledge of both the cross-products $\mathbf{Z}'\mathbf{M}$ and $\mathbf{Z}'C$ we do not require the cross product $\mathbf{M}'C$. This insight was the basis for two sample IV proposed in Angrist and Krueger (1992).[4] They show that under certain conditions, it is possible to estimate δ even if no sample can be found that contains data on $\mathbf{M}$, C and $\mathbf{Z}$ simultaneously. All that is required is a sample that includes both C and $\mathbf{Z}$ (but not necessarily the endogenous components of $\mathbf{M}$) and another which includes $\mathbf{Z}$ and $\mathbf{M}$ (but not necessarily C) . This allows us to calculate a two sample 2SLS estimator (TS2SLS) that is analagous to (14)

$$\hat{\delta}_{TS2SLS} = (\hat{\mathbf{M}}_1{}'\hat{\mathbf{M}}_1)^{-1}\hat{\mathbf{M}}_1{}'C_1 \tag{15}$$

[4]In their orginal article, Angrist and Krueger (1992) in fact proposed orginally an alternative GMM estimator $\hat{\delta}_{IV} = \left(\mathbf{Z}_2'\mathbf{M}_2/n_2\right)^{-1}\left(\mathbf{Z}_1'C_1/n_1\right)$. Asymptotically this gives identical results to the TS2SLS estimator. However, Inoue and Solon (2010) show these two approaches will in general give different answers in finite samples, and that the TS2SLS is more efficient. This gain in efficiency arises because the latter estimator corrects for differences in the two samples in the distribution of $\mathbf{Z}$

where $\hat{\mathbf{M}}_{\mathbf{1}} = \mathbf{Z_1}(\mathbf{Z_2'}\mathbf{Z_2})^{-1}\mathbf{Z_2'}\mathbf{M_2} = \mathbf{Z}_1\hat{\Pi}_2$. Here C_1 and $\mathbf{M}_1$ contain n_1 observations from the first sample while $\mathbf{M}_2$ and $\mathbf{Z}_2$ contain n_2 observations from the second. $\hat{\Pi}_2$ is the coefficient matrix formed from a regression of $\mathbf{M}_2$ on $\mathbf{Z}_2$.

This estimator can be implemented using a simple two step procedure:

1. Run a first stage regression in sample 2 and using the recovered coefficients to impute $\mathbf{M}$ in sample 1.

2. In sample 1, regress C_1 on the imputed values of $\mathbf{M}$ to recover $\hat{\delta}_{TS2SLS}$.

The standard errors in this second regression can then be corrected using a formula provided in Inoue and Solon (2010).

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