House Price Rises and Borrowing to Invest

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Abstract

Household borrowing and spending rise with house prices, particularly for leveraged households, but household spending is not consumption. We propose an alternative borrow-to-invest channel by which house price gains affect household spending on residential investment. We show that rational, leveraged households have an incentive to make additional residential investments when house prices rise. Our empirical application compares responses in different kinds of spending across more and less leveraged households. We find strong evidence of the borrow-to-invest channel in UK data. Credit constraints matter through reducing access to leveraged returns and so reducing lifetime resources, rather than through consumption smoothing.

Keywords: House prices, leverage, consumption, home investment

JEL Codes: E21, D14, D15, G51

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1 Introduction

The housing boom followed by the financial crisis has focused attention on how households respond to house price changes, and how those responses are affected by household indebtedness. Much of the existing literature focuses on how total spending and borrowing adjusts to house prices without addressing the question of what spending is used for, and concludes that borrowing constraints that limit consumption spending are the main mechanism driving spending responses to changes in housing wealth. We depart from this literature in two ways. First, we distinguish between consumption spending and residential investment spending, and emphasise that these concepts are likely to respond differently to house price changes. Second, we emphasise that household leverage is a portfolio choice, and so leveraged households, who hold gross housing wealth in excess of net wealth, will invest in housing (rather than selling) to rebalance their portfolios after a house price increase. This generates a investment motive for borrowing. The desire to borrow to reinvest exists even if households believe returns are i.i.d.

This borrow-to-invest channel is an important and overlooked mechanism by which house price increases affect household spending. It can produce a strong response of household spending to house price increases, but it is largely an investment spending response. The borrow-to-invest channel generates substantial nonlinearities in the scale of response to house price changes by leverage: highly leveraged households respond far more than less leveraged households. A house price shock takes leveraged households below their target leverage, and leads to increased borrowing, compared to less leveraged households. This is firstly because a given house price change leads to a greater wealth shock for more leveraged households; and secondly because households with higher target leverage will want to borrow more in response to a given wealth shock. Conversely, when house prices fall, the housing portfolio share for leveraged households will rise, and they will want to pay-down debt to rebalance their portfolios. This behaviour provides a mechanism for the 'debt-overhang' effects reported by Dynan (2012).

The borrow-to-invest channel is distinct from the much emphasized channel in which consumption spending is limited by borrowing constraints, which are then relaxed by house price increases. Both channels generate total spending responses that are nonlinear in leverage. However, responses in the borrow-to-invest channel are in investment rather than consumption spending, and moreover these responses do not rely on consumption spending being constrained prior to the house price increase. Borrowing constraints can play a role in moderating investment responses. In this scenario, credit constraints affect households welfare primarily through constraining wealth building rather than through constraining consumption smoothing.

To model the borrow-to-invest channel, we setup a life-cycle framework of portfolio choice with transactions costs and two assets: housing and bonds, with both rate-or-return and income uncertainty. Households can borrow (short the bond) to increase their gross housing wealth. These leveraged households have a portfolio share of housing that is greater than one. We show the implications of this model first, by making specific assumptions to generate closed form solutions; and second, by solving and simulating the complete model.

We then document empirically the extent to which households increase mortgage borrowing in response to house price increases using panel data from both the US and the UK. Household mortgage debt among existing home-owners, who do not move home, increases by roughly 9 cents with each $1 increase in home values in the US and by 7 cents in the UK. In this respect, households increased the size of their balance sheets in the years prior to the financial crisis in a similar way to investment banks (Adrian and Shin (2010)).

We test for the borrow-to-invest channel by examining how different categories of household spending - consumption spending and investment spending - respond to house price changes and how those responses vary with household leverage. To do so, we use detailed household-level data on borrowing, consumption and investment decisions from the UK. We link data on households balance sheets from a panel survey with spending data in a household budget survey using two-sample IV methods (Angrist and Krueger (1992)). This IV strategy also accounts for the fact that leverage is endogenous in our framework. We consider a number of instruments based on past credit and housing market conditions which have a persistent effect on leverage.

We find strong evidence of large differences in the responses of residential investment by households according to their initial leverage. Our results imply that 10% increase in house prices results in a 7.3% greater increase in residential investment for a household with an LTV of 66% relative to a household with an LTV of 50% (in our empirical model the effects scales linearly with each further doubling of households' debt to equity ratios). We also show that households that have greater initial leverage are more likely to make second home purchases in response to rising local prices over longer time horizons. However, we do not find evidence that more leveraged households disproportionately increase either their total, non-durable or durable consumption spending.

The borrow-to-invest channel provides a reinterpretation of the empirical findings of a large literature on spending response to house price changes. Papers that show that borrowing responses to house price changes are larger for leveraged households include Cloyne, Huber, Ilzetzki, and Kleven (2019), Aladangady (2017), Cooper (2013), Mian and Sufi (2011), Disney, Gathergood, and Henley (2010). The most common interpretation in these papers is that consumption spending is constrained and this constraint is relaxed by house price increases. Berger, Guerrieri, Lorenzoni, and Vavra (2017) show that households at greater risk of facing a binding credit constraint would be expected to accumulate precautionary savings, which they would then decumulate faster in response to a rise in house prices.

These papers mainly focus on the response of total spending and debt, and interpret the total spending response as a consumption response.

Our work relates to a literature on housing investments and portfolio choices over the life-cycle. Cocco (2004) and Chetty, Sandor, and Szeidl (2017) consider how portfolio decisions and stock purchases are affected by the presence of housing and shocks to house prices. Melzer (2017) shows that highly leveraged households (at increased risk of negative equity and default) strategically make fewer housing investments. Our work differs from these papers by explicitly considering how housing investments respond to shocks to housing wealth.

In the years since the financial crisis, policy makers have been increasingly interested in macro-prudential measures that use credit constraints to limit leverage growth among households during asset booms. Our borrow-to-invest model makes salient key points about credit constraints and macro-prudential policies. First, loan-to-value constraints are relaxed by house price increases and therefore loan-to-value restrictions impose less restraint on borrowing in house price booms. By contrast, loan-to-income constraints are not affected by the current state of house prices and so continue to act to constrain leverage. Second, in the borrow-to-invest framework, investment rather than consumption is constrained. This means that rather than hindering consumption smoothing, credit constraints limit returns and so reduce life-time wealth. Further, there may be unintended distributional consequences of macro-prudential policies depending on who is constrained by the policies.

The remainder of this paper is structured as follows. Section 2 sets out a theoretical framework of leverage over the life-cycle. Section 3 describes the data and provides descriptive evidence that households re-leverage by increasing borrowing when house prices rise. Section 4 tests the borrow-to-invest channel by comparing consumption and investment responses to house price changes at different degrees of leverage. Section 5 concludes.

2 Life-Cycle Portfolio Choice

We set up a life-cycle model of housing as a portfolio choice. We use this model to show the effects on consumption, on housing investment and on borrowing of house price realisations. We begin by setting up the general framework, and then show the effects of house price realisations first in a simplified version of the model, and then in a calibrated version of the full model.

Consider a unitary household, i, with two assets available to hold in its portfolio, that each period chooses consumption, $c_{i,t}$, and the amount of housing, $h_{i,t}$, to maximise expected lifetime utility.[1]

[1] In principle households could alter the risk and return of their portfolios by adjusting holdings of other, financial assets (such as stocks). In practice, for the vast majority of households in the UK, housing wealth is by far the most important asset that households hold. It is also unique in having historically offered a mix of both high returns with a

$$U_{i,t} = \max_{c,h} E_{i,t} \left[\sum_{\tau=0}^{T-t} \beta^{\tau} \frac{(c_{i,t+\tau})^{1-\gamma}}{1-\gamma} \right] \tag{1}$$

To illustrate the borrow-to-invest channel, we assume that housing is just an investment good and so does not yield a flow of utility. If housing provides a flow of utility, this would be like a dividend from the asset. Households receive income, $y_{i,t}$ each period:

$$\ln y_{i,t} = \ln y_{i,t}^{P} + u_{i,t}, \qquad u_{i,t} \sim N(0, \sigma_u^2) \tag{2}$$

where $y_{i,t}^{P}$ is permanent income:

$$\ln y_{i,t}^{P} = \ln y_{i,t-1}^{P} + f_i(t) + \eta_{i,t} \qquad \eta_{i,t} \sim N(0, \sigma_\eta^2)$$

where $f_i(t)$ captures the deterministic age-trend. We assume there is no labour supply choice and exogenous retirement.

Households can hold a risk-free asset (a bond) denoted $b_{i,t}$ with price 1 and interest rate r. Housing is a risky asset with price p_t, and return:

$$r_t^* = \frac{p_t}{p_{t-1}} - 1. \tag{3}$$

The excess return of housing over the risk-free rate is i.i.d.:

$$r_t^* - r = \mu + \varepsilon_t \qquad \varepsilon_t \sim N(0, \sigma_\varepsilon^2) \tag{4}$$

The return on housing is common across individuals within a group, and so is not indexed by i. By assuming returns are i.i.d., we show how house price increases may affect investment decisions even if shocks to housing returns have no persistence. If there is persistence in housing returns or if households believe there is persistence, this would provide an additional reason to expect house price increases to affect residential investment but our point is that we can rationalise investment behaviour without recourse to persistence or over-optimism.

Households can short the bond (that is, take a mortgage loan), but cannot short housing. We define debt as $d_{i,t} = -b_{i,t}$. Further, there are two additional credit constraints. First, a loan-to-income constraint:

$$d_{i,t} \leq \lambda_y y_{i,t} \tag{5}$$

Second, a loan-to-value constraint:

$$d_{i,t} \leq \lambda_h p_t h_{i,t} \tag{6}$$

where $h_{i,t}$ is the quantity of housing chosen in period t.

relatively low variance (Jordá, Knoll, Kuvshinov, Schularick, and Taylor (2019)).

We assume that it is costly to adjust housing: the household must pay:

$$\kappa * \left| h_{i,t} p_t - \bar{h}_{i,t} p_t \right| \tag{7}$$

where $\bar{h}_{i,t}$ is the quantity of housing owned at the start of the period. In other words, the adjustment cost is proportional to the size of adjustment.

We define the leverage position of the household (the loan-to-value ratio) as:

$$L_{i,t} = \frac{\text{debt}}{\text{gross housing wealth}} = \frac{d_{i,t}}{p_t h_{i,t}} \tag{8}$$

and the portfolio share of housing as:

$$\omega_{i,t} = \frac{\text{gross housing wealth}}{\text{net wealth}} = \frac{p_t h_{i,t}}{p_t h_{i,t} - d_{i,t}} = \frac{1}{(1 - L_{i,t})} \tag{9}$$

Leverage $0 < L_{i,t} < 1$ implies $\omega_{i,t} > 1$. For example, a household with a 95% "mortgage" ($L_{i,t} = 0.95$) has a housing portfolio share of $\omega_{i,t} = 20$, while for outright owners $\omega_{i,t} = 1$ if they hold no bonds.

The intertemporal budget constraint describing the evolution of net wealth, $x_{i,t}$ is:

$$x_{i,t} = (1 + r + \omega_{i,t-1} (r_t^* - r)) * (x_{i,t-1} - c_{i,t-1}) + y_{i,t} \tag{10}$$

or equivalently,

$$x_{i,t} = \left(1 + r + \frac{1}{1 - L_{i,t-1}} (r_t^* - r)\right) * (x_{i,t-1} - c_{i,t-1}) + y_{i,t} \tag{11}$$

This highlights the way that leverage magnifies risk and return.

For a particular house price realisation, we can use equation (10) to show the impact on wealth:

$$x_{i,t} - E_{i,t-1}[x_{i,t}] = \omega_{i,t-1} (r_t^* - E_{i,t-1} [r^*]) * (x_{i,t-1} - c_{i,t-1}) \tag{12}$$

Equation (12) shows that the effect on net wealth of a given house price realisation will be greater when the portfolio share is greater: leveraged households have a greater increase in their wealth for a given house price realisation, and these effects are highly nonlinear in leverage. This is the first channel through which leverage impacts behaviour: it determines the size of the change in wealth. Of course, the variance of portfolio returns are also higher for leveraged households. We can also express the change in wealth directly in terms of house prices:

$$x_{i,t} - E_{i,t-1}[x_{i,t}] = (p_t - E_{i,t-1} [p_t]) * h_{i,t-1}. \tag{13}$$

We show the implications of house price realisations in this model first, by making specific assumptions to generate closed form solutions; and second, by numerically solving and simulating the complete model.

2.1 Simplified Model

We make some extreme assumptions to fix ideas. We assume that the household has no labour income ($y_{i,t} = 0$), there are no adjustment costs associated with housing and no borrowing constraints other than the inability to short housing and a no-bankruptcy condition, and an infinite horizon. This leads to the consumption and portfolio choice model of Merton (1969)), and the policy functions are well known. There is a linear consumption function:

$$c_{i,t} = \alpha x_{i,t} \tag{14}$$

and there is a constant target portfolio share for the risky asset:

$$\omega_{i,t} = \omega^* \tag{15}$$

In the Merton model, the portfolio share of the risky asset depends only on moments of the return distribution. As leverage is just a transformation of the housing portfolio share, this implies there is a constant target leverage that delivers the household's desired combination of risk and return.

In this model, the change to wealth due to a particular house price realisation (as shown in equation (13)) is partly consumed:

$$c_{i,t} - E_{i,t-1}[c_{i,t}] = \alpha \left(x_{i,t} - E_{i,t-1}[x_{i,t}] \right) \tag{16}$$

and partly saved ($s_{i,t}$) according to the consumption function:

$$s_{i,t} - E_{i,t-1}[s_{i,t}] = (1 - \alpha) \left(x_{i,t} - E_{i,t-1}[x_{i,t}] \right). \tag{17}$$

The point about these two equations is that ω only enters into these equations to the extent that ω affects the change in net wealth: there is no additional effect of leverage on consumption over and above the net wealth effect.

By contrast, when we consider the impact of house price changes on investment, the portfolio choice rule implies that the additional saving in equation (17) is leveraged by ω^* to generate an increase in housing wealth:

$$p_t * h_{i,t} - E_{i,t-1}[p_t * h_{i,t}] = \omega^* (1 - \alpha) \left(x_{i,t} - E_{i,t-1}[x_{i,t}] \right). \tag{18}$$

This means that ω has an additional effect on the portfolio decision and enters into the portfolio decision over and above the direct effect that ω has on net wealth that is shown in equation (12). The greater effect of ω on investment spending forms the heart of our empirical test of the borrow-to-invest channel that we perform in Section 4.

Using equation (13), equation (18) implies extra active investment in housing of:

$$\begin{aligned} &(p_t * h_{i,t} - E_{i,t-1}[p_t * h_{i,t}]) - (p_t * h_{i,t-1} - E_{i,t-1}[p_t] * h_{i,t-1}) \\ &\qquad = \left(\omega^* (1 - \alpha) - 1 \right) (p_t - E_{i,t-1}[p_t]) \, h_{i,t-1} \end{aligned} \tag{19}$$

The first term on the left hand side of equation (19) is the change in desired gross housing wealth. The second term is the additional housing wealth that comes mechanically from the unexpected price increase. The difference between the two is the additional active investment in housing (funded by debt) to return the housing portfolio share to w^*. The key conclusion from this model is that, if there is an unexpected house price increase, a leveraged household will increase investment in housing and borrow to do so (even if the household believes that housing returns are i.i.d.), whereas the consumption will change very little. Conversely, an unexpected house price fall will increase the leverage of the portfolio and the household will want to sell housing and retire debt to return to ω^*. In other words, the key margin of adjustment is investment in housing.

For example, suppose that the household owns a $\pounds 600,000$ house with $\alpha = 0.05$ and $\omega = 3$ (so that the household has 33% equity in the home.) If the house value unexpectedly goes up by 5% ($\pounds 30,000$), the consumption function implies that net wealth increases by $\pounds 28,500$ and the constant portfolio rule implies that the household then desires gross housing wealth of $\pounds 685,500$. As the house value is now $\pounds 630,000$, the households makes new investment in housing of $\pounds 55,500$, financed by new debt. Note that the extra investment spending ($\pounds 55,500$) is much larger than the extra consumption spending ($\pounds 1,500$). The marginal propensity to invest ($\omega(1-\alpha)-1$) is 1.85, and the marginal propensity to consume (α) is 0.05. Clearly in this example the balance sheet of the household has expanded quickly, and we show in the solution to the complete model how the presence of credit constraints and frictions moderate households' desire and ability to do this.

The set of additional assumptions to reach this conclusion are very stark, and the mechanism described will be moderated in a number of ways in the simulations of the complete model. Nevertheless, equations (17) and (18) suggest that the borrow-to-invest channel that we describe will operate so long as the policy functions for consumption and for the risky asset portfolio share are sufficiently flat in net wealth. A gently sloped consumption function implies that a significant fraction of a wealth shock is saved. A fairly flat portfolio rule implies that the household will not want to change its portfolio shares dramatically in response to a wealth shock. A wide range of life-cycle consumption and portfolio-choice models share these features.

In the next subsection we assess the wider validity of this conclusion by solving numerically the more general model. In the empirical part of the paper, we test the empirical relevance of this mechanism.

2.2 Housing and Consumption Choices with Transaction Costs

We return to the general model outlined above and use calibration and simulation to show the effects of house price realisations on housing investment, analogously to equation (19). Compared to the Merton model, the general model allows for income risk; transactions cost of adjusting housing; and for loan-to-income and loan-to-value borrowing constraints; and a finite horizon.[2]

The purpose of analysing the general model is to illustrate when the borrow-to-invest mechanism is likely to be relevant. We solve the model numerically using parameters specified in Table 1.[3] We do not estimate the parameters for the simulations, rather we take parameter values from external sources and simulate the model with these values to illustrate the mechanisms at play. The expected return on housing and its standard deviation are estimated from aggregate UK house price data imposing a unit root on house prices, following Attanasio, Bottazzi, Low, Nesheim, and Wakefield (2012).[4]

Table 1: Calibration Parameters

Parameter		**Value**
Expected Return on Housing	μ	0.025
Standard Deviation of Return on Housing	σ_ε	0.076
Deterministic Return on Bonds	r	0.015
Standard Deviation of Income	σ_u	0.1
Discount Factor	β	$\frac{1}{1.025}$
Coefficient of Relative Risk Aversion	γ	1.5
Length of Life (years)	T	50
Max loan-to-value	λ_h	0.85
Max loan-to-income	λ_y	3.5
Transactions cost	κ	{0,0.02}

We consider various values of κ and report the decisions when $\kappa = 0.02$ on both buying and selling, compared to when $\kappa = 0$. In Figure 1, the graphs on the left-hand side are when there is no transaction cost, and on the right-hand side when there is a 2% transaction cost, κ. The x-axis in all four graphs is the deviation in the house price at the start of the period from its expected value. The graphs report the choice rules that households follow in response to house price realisations, given particular values

[2]When non-insurable labour income risk is included, households effectively treat their remaining human wealth as another asset in their portfolio (Cocco, Gomes, and Maenhout (2005)), reducing the effective portfolio share of housing and inducing households to de-lever as they age.

[3]The numerical solution is a standard application of stochastic dynamic programming. The only complication is because of kinks in the policy functions induced by the transactions costs.

[4]In this model we do not allow for housing in the utility function. As noted above if housing provides a flow of utility, this is like a dividend. However, if there is is diminishing marginal utility from housing (and extra housing cannot be rented efficiently), the total return to housing falls with gross housing wealth held, and this will temper some of the re-leveraging motive.

for the initial quantity of housing $h_{i,t-1}$, the start-of-period debt and wage rate.

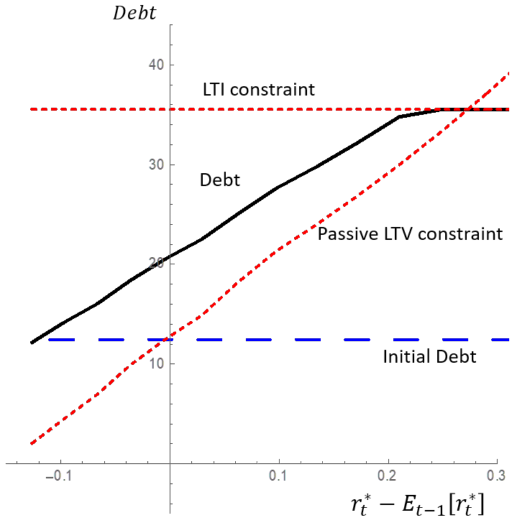
The top row shows, in the dark solid line, the impact on end-of-period debt of house price realisations. This is compared to the start-of-period debt shown by the horizontal dashed line. The horizontal dotted line (labelled "LTI constraint") shows the level of debt implied by the loan-to-income constraint binding. The upward sloping dotted line (labelled "Passive LTV constraint") shows the level of debt implied by the loan-to-value constraint binding if there were no additional saving: this is calculated as the LTV if the quantity of housing did not change in response to the house price realisation.

The basic point is straightforward: the greater the house price increase, the greater the levels of debt taken on, meaning that households increase their leverage in response to a house price increase. In the absence of transactions costs, the household adjusts their debt levels to whichever borrowing constraint is binding. Further, households save part of their earnings to increase their net wealth and then leverage this extra wealth. This extra net wealth and resulting leverage, relaxes the loan-to-value constraint and increases the level of debt above the passive loan-to-value constraint that would have been binding with the start of period housing wealth. This highlights that debt, housing and consumption are all co-determined. The desire to borrow and leverage net assets in the model comes from the return on housing. As households age, this incentive diminishes somewhat.

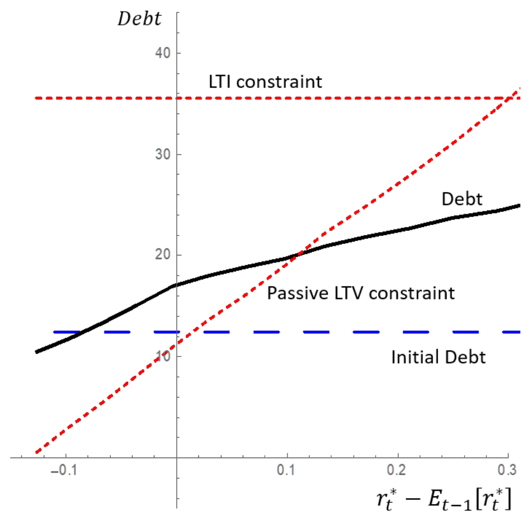
By contrast, when transactions costs are present, the desire to releverage or deleverage is more muted: households do not adjust their debt levels as sharply in response to the house price shock. Further, credit constraints mean that the response to the house price increase is dampened relative to the Merton case.

These decisions about the response of choices about debt following house price shocks are mirrored in the decisions about housing and consumption, shown in the bottom row of Figure 1. We plot the level of consumption, the value of housing at the end of the period, $p_t h_{i,t}$, as well as the active purchases of housing (or housing investment) following the price change, corresponding to equation (19) in the simplified model. The key comparison is between how much consumption changes following the house price change and how much active housing investment changes. The increased debt levels shown in the top row translate into extra housing investment, with very little change in consumption levels, similar to the implications of equation (16) in the Merton model. At its heart, this result is driven by the marginal propensity to consume out of changes to housing wealth being small, and the portfolio allocation decision pushing leveraged households to relever following a house price increase. The housing investment decision is more muted in the presence of the transaction cost because of the cost of reversing a house purchase or investment decision. Even in this case, however, the more muted active purchase response reflects a reduced amount of new borrowing rather than the same amount of borrowing being used to boost consumption. Further, if households are already at a binding loan-to-income constraint when the house price shock occurs, and so housing purchases cannot adjust, there is

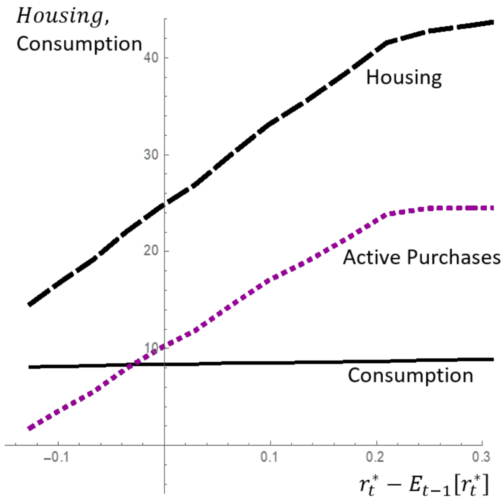
Figure 1: Debt, Consumption and House Purchases Following a House Price Shock



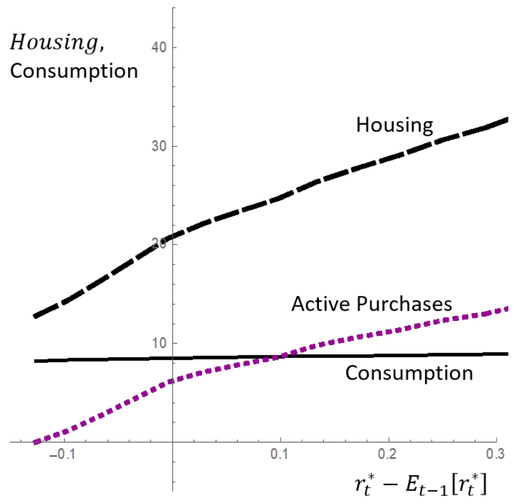
No Transactions Cost



2% Transactions Cost



No Transactions Cost



2% Transactions Cost

no response in consumption either. In this particular scenario, portfolio shares will adjust substantially in response to the house price change but this is simply because debt cannot adjust upwards.

This analysis highlights that the distinction between the consumption spending response and the housing investment response is an important distinction whether or not debt levels are constrained and whether or not housing is subject to transactions costs. Figure 1 shows the optimal policy rules for a household with a particular amount of leverage at the start of the period. Households that are less leveraged will respond less to the house price realisation: a given house price change will lead to less of a wealth increase, and that wealth increase will be transmitted into a lower increase in debt because of the lower desired leverage. For an outright owner, the house price change does not change the portfolio share and so only has a direct wealth effect and no additional releveraging motive.

The empirical work that follows tests for the borrow-to-invest channel via a double comparison of spending responses between categories of spending (consumption versus residential investment) and between more and less leveraged households. The model predicts a much larger response of investment spending than consumption, but particularly for more leveraged households.

Further, our framework highlights that credit constraints affect households primarily through constraining wealth building rather than through constraining consumption smoothing. This point can be seen clearly in the bottom panels of Figure 1: the consumption response to a wealth shock is little affected by whether the LTV or LTI constraints are binding. By contrast, the decision to invest in housing is clearly affected by the borrowing constraints.

3 Data and Descriptive Evidence

In this section, we describe the three datasets that we use for our empirical work. We then provide two sorts of descriptive evidence from these micro datasets: first, we show average profiles by age and time of leverage, borrowing and the incidence of spending on residential investment. Second, we use the panel data to show the household level association between house price realisations and releveraging. In section 4, we use the UK data to test the borrow-to-invest channel. We focus on the UK for our test because we have a plausible instrument for leverage in the UK (as we elaborate below).

3.1 Data

The first dataset we use is the Living Costs and Food Survey and its previous incarnations the Expenditure and Food Survey and Family Expenditure Survey (which we shall refer to collectively as the LCFS) (Department for Environment, Food and Rural Affairs, Office for National Statistics, 2016). 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 such as home values).

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) (University of Essex. Institute for Social and Economic Research (2010); University of Essex. Institute for Social and Economic Research. (2016)). 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 total mortgage debt from 1993 onwards, 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 impute mortgages in years following 2010 (see Appendix B for details). Loan to value ratios are calculated by dividing the value of mortgages by the (self-reported) value of homes. The BHPS and Understanding Society also contain information on whether households own a second home.

We need to use two UK surveys because consumption spending is observed in the LCFS but leverage is not, whereas the BHPS includes information on leverage but not on consumer spending. Hence we use two-sample methods that combine the information contained in both datasets, as we describe below.

The third dataset we use is the Panel Study of Income Dynamics (PSID). The PSID is a US-based panel of households that includes information on home ownership, household balance sheets, income and spending decisions. Since 1997, the survey has been biennial. The PSID has included questions on the value of households home equity and mortgages in each wave from 1999 onwards. Prior to 1999, these were only asked every 5 years. In terms of spending data, the survey consistently included only spending on food and rental payments until 1999. In that year, coverage was extended to include other non-durable expenditures including health, utilities, education and childcare. Other expenditures such as clothing and entertainment were added in 2005. Since 2001, households have been asked whether they have undertaken home improvements worth $10,000 or more since January of the year two years prior to the interview. If they answer in the affirmative, they are then asked to give the exact amount spent.[5]

[5] We annualise this figure using individuals' month of interview to determine the exact length of the period covered by this question.

For house prices we use regional/state-level data on the prices of transacted houses published by the Office for National Statistics (for the UK) and the Federal Housing Finance Agency (for the US).

In all of what follows, we drop households where the head is aged under 25 or over 65. To avoid problems of measurement error, we also drop households who have a lagged housing portfolio share in the top 1% of the distribution and those who have negative equity. We also drop households resident in Northern Ireland from both the BHPS and the LCFS samples as these were only introduced into the BHPS sample in later years. Finally, for most of our analysis, we drop households who have lived in their home for less than one year. Appendix A provides some descriptive statistics for our three samples.

In the UK, non-durable spending is the largest component of expenditure (accounting for 76%). Residential investment spending, which includes extensions, renovations, household repairs, large furniture, carpets, and large household appliances accounts for roughly 9.7% of total spending. The remainder is accounted for by spending on non-residential durables. The spending questions included in the PSID are not as detailed as in the LCFS, and so we are forced to define categories differently for the US. We measure residential investment in the PSID as the sum of responses to the question "how much did your family spend altogether on household furnishings and equipment, including household textiles, furniture, floor coverings, major appliances, small appliances and miscellaneous housewares?" and responses to questions regarding home improvement spending (which are censored from below at \$10,000). Since we are unable to exclude spending on small furnishings and smaller electrical appliances from this value, this definition is somewhat broader than the one used in the UK. As measured, it accounts for 6.9% of total spending. Non-residential durable spending in the PSID is essentially restricted to cars. Relative to our definition for the LCFS, it therefore excludes audio-visual equipment, as well appliances such as vacuum cleaners and microwaves (which may be included as durable household furnishings). This category accounts for 10.6% of expenditures. The remaining 82.5% of measured spending (including clothing, utilities, entertainment, vacations, motor fuel, healthcare and child care) is classified as going on non-durables.[6]

For the borrow-to-invest mechanism to operate on the intensive margin of home improvements, a substantial share of the costs of home improvement spending must be recouped through increased home values. *Realtor* magazine conducts an annual survey of the costs and value added associated with different home improvement projects in different US housing markets to estimate of the proportion of costs of different projects that homeowners can expect to recoup through higher re-sale values.[7]

[6] In all of these categories, non-responses to individual questions are treated as implying zero expenditures.

[7] Real estate agents are asked to the expected value different projects are expected to add to a home's sale price, while professionals in the remodelling industry are asked to provide estimates of their likely cost. http://www.remodeling.hw.net/cost-vs-value/2016/

In 2016, the average value-cost ratio of investments made on properties sold within a year was 64%. Investments in attic insulation had the most cost effective effects on resale values, with 117% of costs recouped through higher home values. Bathroom additions had the lowest returns with 56% of costs being recouped.[8]

The fact that homeowners can expect to recoup a significant fraction of the costs of home improvement means that investment motives are likely to play an important role in households' decisions to make such expenditures. Moreover, the returns to investments in one's own home appears to increase along with local home values, suggesting that this is indeed a way that households can increase the importance of housing in their overall portfolios. Gyourko and Saiz (2004) find that home improvement spending responds strongly to the ratio of local house values to construction costs, which is consistent with a rational investment motive for such projects that responds to house price growth. Choi, Hong, and Scheinkman (2014) investigate the impact of local house price growth on the average ratio of costs recouped as measured by the *Realtor* survey, controlling for other factors such as local unemployment and income growth. They also find that the investment value of home improvement projects is positively associated with local house price growth.

3.2 Age and Time Profiles

Figure 2 shows how leverage evolves over time across four different 10-year birth cohorts (born between the years 1940 and 1970). In both the UK and the US there is a steady and reasonably smooth decline in leverage by age. In the UK there are pronounced differences in leverage between cohorts at younger ages. However, the different cohorts largely converge to similar leverage by around age 45. As we discuss further below, the differences in initial leverage across UK cohorts are likely to be explained by the differing credit conditions and house prices the different cohorts were exposed to at the point they became home-owners. This is the source of variation in leverage which we exploit to test the borrow-to-invest channel. In the US, there is much less evidence of cohort effects, and the decline in leverage by age is much less steep than in the UK.[9]

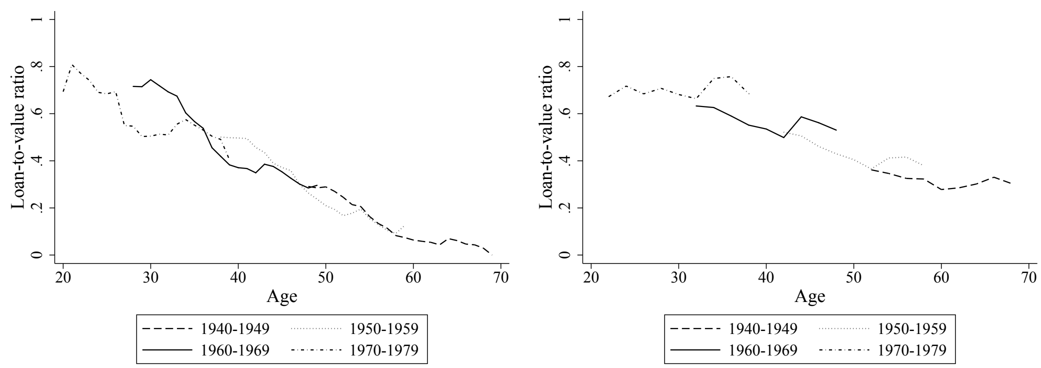
The top two panels in Figure 3 shows average real house price growth in the US and the UK over time. The bottom two panels correspondingly shows how leverage varies over the same time period among both younger (aged 25-45) and older (46-65) households in the two countries.

[8] Similar surveys exist in the UK, for example the insurance company GoCompare provides a property investment calculator which provides estimates of the costs and returns associated with different projects. This suggests greater returns to home improvement spending in the UK, although the methodology behind the calculator has not been published. As in the US, Energy-saving investments have the highest returns, while net bathrooms have negative returns (https://www.gocompare.com/home-insurance/property-investment-calculator/).

[9] The differences in the age path of leverage in the UK and the US may reflect differences in the tax treatment of interest payments or in other institutional details, but disentangling these effects is beyond the scope of this paper.

Figure 2: LTV ratios by age and cohort

(a) UK (1993-2013) (b) US (1999-2013)



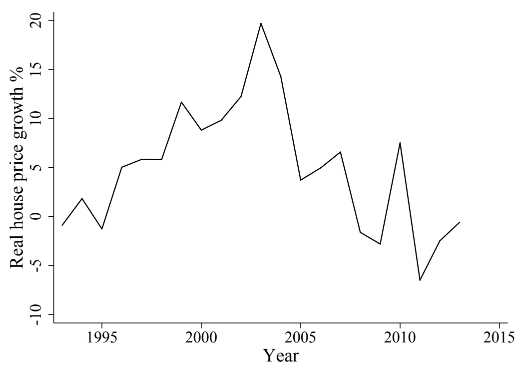
Source: Authors' calculations using BHPS/Understanding Society and PSID

In the US, loan-to-value ratios among both younger and older households remained strikingly stable throughout the period of house price growth that continued until 2006. House price growth peaked at a national rate of 8% in 2005, when loan-to-value ratios were essentially unchanged from the previous year (at around 60% for those aged 25-45 and 40% for those aged 46-65). When real house prices started to decline from 2007 onwards however, loan-to-value ratios rose rapidly. House prices fell by 2% in 2007 and between 7-8% in each of the years from 2008-2011. Over this period, the average LTV among younger households increased from 62 to 71%, while for older households it increased from 37 to 44%. This suggests an asymmetry in the ease of re-leveraging and de-leveraging in response to house price changes; we examine this hypothesis with panel data on individual homeowners below.

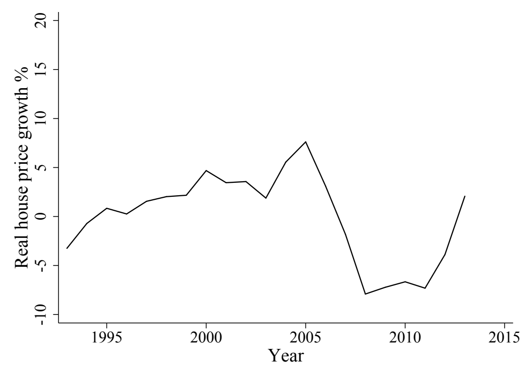
Such price declines did not occur in the UK, where the fall in prices was modest relative to both previous UK house price slumps and to the declines observed in the US (Bénétrix, Eichengreen, & O'Rourke, 2012). We plot UK house price and loan-to-value data from 1993-2013. For most of this period, UK house prices were increasing, with annual falls only observed in 1994-1995 and 2007-2009. In the period in between these years, house prices grew rapidly. There is more evidence of a fall in average loan-to-value ratios as house prices rose in the UK than in the US. However, UK households were also borrowing more over this period, even if the scale was not as great as it was in the US. Annual price increases peaked in 2003 at a rate of almost 20%. If UK homeowners had responded passively to this increase, and the set of homeowners had been fixed, average loan-to-value ratios should have fallen by the same percentage. Instead they fell by just 7% in that year. Over the whole of the period of greatest house price growth, LTVs among the under 45s fell from 62% in 1995 to 43% in 2004 before climbing again as house price growth moderated (the over 45s saw smaller changes in their average

Figure 3: LTV ratios and house price growth rates

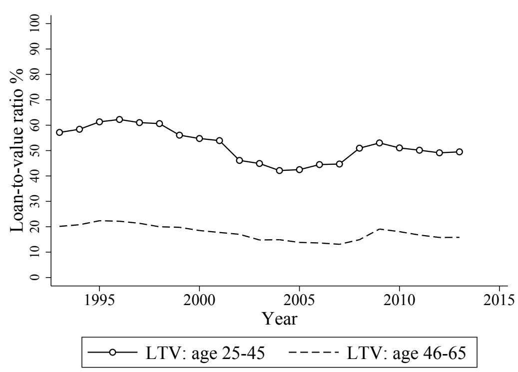
(a) House price growth (UK), 1993-2013



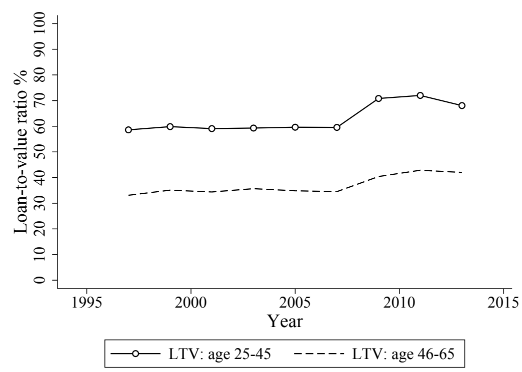
(b) House price growth (US), 1994-2013



(c) Loan-to-value ratios (UK), 1993-2013



(d) Loan-to-value ratios (US), 1994-2013



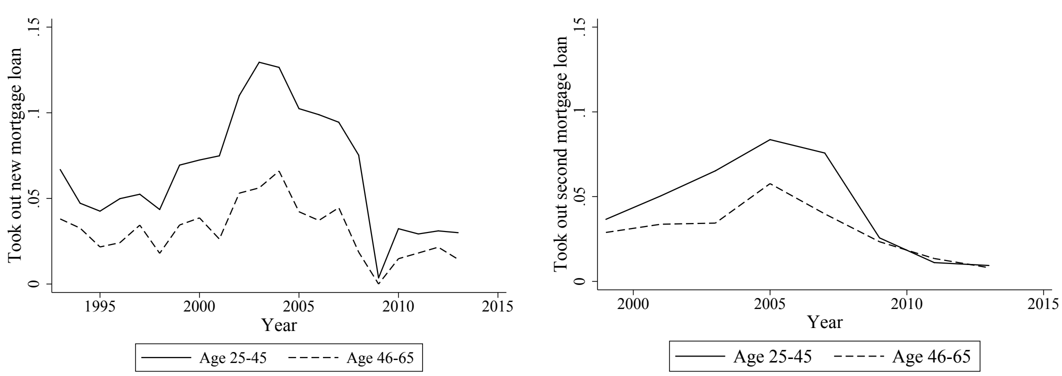
Notes: House prices for the UK are national averages taken from the Office for National Statistics HPI deflated using the UK CPI. House prices in the US are national averages taken from the Federal Housing Finance Agency and are deflated with the US CPI. Loan-to-value ratios are taken calculated for the UK using data from BHPS and Understanding Society and for the US using the PSID.

leverage).

Changes in mortgage debt could be driven by changes in the amount of borrowing used to purchase new homes or through new borrowing by those remaining in their current homes. Panel (a) in Figure 4 confirms the presence of this latter margin by showing that existing homeowners were actively engaged in new mortgage borrowing as prices rose. The proportion of home-owning households aged 25-45 observed taking out additional mortgage debt in the UK increased to exceed 10% in the period of most rapid house price growth. Panel (b) in Figure 4 shows trends the proportion of home-owners engaged in new mortgage borrowing for the US (since the previous wave - i.e in the previous two years). As in the UK, younger households in particular were more likely to take out new mortgage loans in periods of high house price growth.

Figure 4: New mortgage loans by homeowners in the UK and the US

(a) UK, 1993-2013 (b) US, 1999-2013

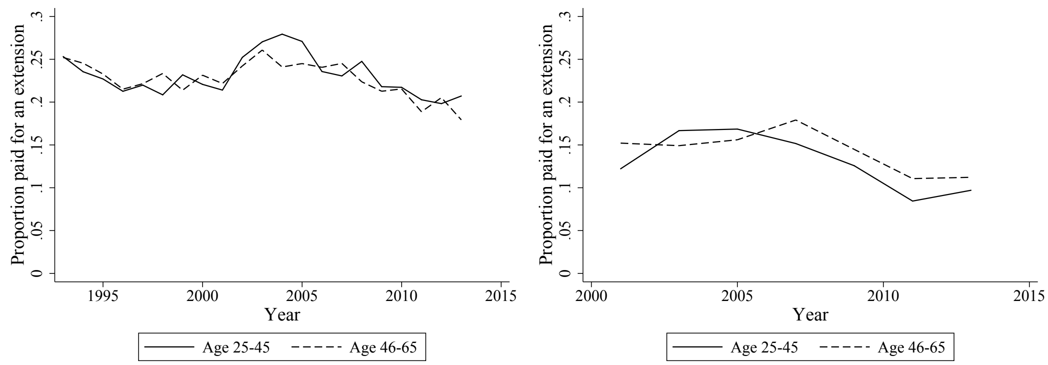


Source: British Household Panel Survey/Understanding Society and Panel Study of Income Dynamics.

Figure 5 shows that the periods of new borrowing also coincided with growth in the proportion of households with positive spending on housing extensions among homeowners. This activity was also focused on younger households. As we saw in Figure 3, older households responded much less to these developments. The right hand panel reports there is also evidence of increases in home improvement spending in the US when house prices growth was highest.

Figure 5: Housing extensions by homeowners in the UK and US

(a) UK, 1993-2013 (b) US, 1999-2013



Source: Living Costs and Food Survey and Panel Study of Income Dynamics

3.3 Household Level Dynamics of House Prices and Borrowing

The average changes in loan-to-value ratios displayed graphically above confound individuals responses with compositional changes as households enter and leave homeownership. We now turn to examining household level responses in panel data. In Table 2 we report results from a regression of changes in mortgage debt on changes in regional home values. That is, we estimate the regression:

$$\Delta d_{i,t} = \delta \Delta p_{r,t} + \epsilon_{i,t} \tag{20}$$

on a the panel samples of home-owners. As before, $d_{i,t}$ is the mortgage debt of household i in period t. $p_{r,t}$ are average house prices in region r and period t.[10] We use regional home values rather self-reported home values, which may be subject to measurement error.[11] If mortgage debt did not adjust as house prices increased, leaving LTV ratios to fall passively with house prices, then we would expect the coefficient on house values to equal 0.

We report results from both the UK and the US. Since the PSID has been biennial since 1999, we consider changes over the previous two years in both surveys. For this analysis, we use US data for the period 1994-2013, while the UK results cover the period 1993-2009.[12] Panel (a) of Table 2 presents

[10] In the US we calculate average regional house prices by uprating median house prices for each state in the year 2000 using state level house price indices. Median house prices are taken from the United States Census Bureau Historical Census of Housing Tables.

[11] An alternative approach is to instrument self-reported home values with regional house values. This yields very similar results.

[12] We do not include US data before 1994 as prior to this date the PSID did not include data on 2nd mortgages. We do not include UK data after 2010 when mortgage debt is imputed.

these results for the US; Panel (b) presents them for the UK.

Column (1) shows results for households who have not moved in the previous 2 years. Household mortgage debt and house price changes are positively correlated. Each dollar increase in regional home values is associated with in an additional 9 cents of borrowing for US households over 2 years and 7 cents of borrowing for UK households.

In columns (2) and (3) we look for evidence in asymmetries of responses when households are re-leveraging versus de-leveraging by splitting the sample according to whether regional house prices rose or fell relative to the previous wave. In both countries, the coefficient on the effect of house price falls is insignificantly different from zero (this coefficient is particularly imprecisely estimated for the UK, for which our sample only includes a few years of falling house prices). House price increases are associated with much larger changes in debt. This again suggests that households find it easier to releverage than deleverage in response to changing house prices.

Column (4) shows results when we include households who may have moved in the previous 2 years. The average change in debt associated with each dollar increase in house prices rises to 17 cents in the US and 10 cents in the UK. This indicates that up-sizing and down-sizing are important means by which households adjust their leverage as house prices change.

Table 2: Panel Correlations between Debt and Average Regional House Prices

$\Delta Debt$	(1)	(2)	(3)	(4)
Panel (a): US 1994-2013				
Δp_{rt}	0.093***	-0.034	0.154***	0.170***
	(0.015)	(0.033)	(0.022)	(0.023)
N	21,425	8,999	12,426	25,181
Clusters	5,662	4,106	4,620	6,247
Panel (b): UK 1993-2009				
Δp_{rt}	0.065***	-0.027	0.072***	0.099***
	(0.019)	(0.461)	(0.022)	(0.029)
N	27,543	1,652	25,891	30,291
Clusters	5,056	1,143	4,933	5,222
Restrictions				
Including movers				X
House price growth< 0		X		
House price growth> 0			X	

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Standard errors clustered at the individual level. The correlations are defined over two year periods. House prices are average house price within the region.

A number of factors might explain why the re-leveraging responses we estimates in the US are larger than they do in the UK. One set of reasons relates to the differing institutions in the two countries. In some US states, mortgage loans are non-recourse, meaning that lenders cannot pursue debts that are not covered through sales of foreclosed properties. This may make borrowers more comfortable with the risk of negative equity, since the costs of default in this situation are smaller.[13] In the UK, mortgage loans are recourse loans. Secondly, in the US, mortgage interest is tax deductible, creating more of an incentive to both pay-off mortgages less quickly and to increase mortgage debt when prices

[13]Ghent and Kudlyak (2011) for instance find that the monthly probability of default for borrowers in a state of negative equity is 32% higher in states where the is no threat of recourse.

rise. This was only true to a very limited extent in the UK during the period we consider, and the tax deduction was eliminated in 1999.[14]

Another potential factor is the faster pace of house price increases in the UK. This would mean that UK households would need to make much larger adjustments to their mortgage debt in order to maintain constant loan-to-value ratios. Such large adjustments may have been infeasible given loan-to-income constraints.

4 Testing the Borrow-to-Invest Channel

In this section, we test explicitly the borrow-to-invest channel. We focus on the prediction that the investment spending response to a house price realisation will vary more with leverage than the consumption spending responses. As discussed in Section 2, this prediction follows from equations (16) and (18).

4.1 Empirical Strategy

To test the specific hypothesis that more leveraged households will disproportionately increase housing investment in response to house price increases, we estimate the equation

$$\begin{aligned} IHS(C_{i,t}) = \theta_0 + \gamma_c + \psi_t + \rho_r + \theta_1(\omega_{i,t-1} - 1) + \theta_2\left(\frac{p_{r,t}}{p_{r,t-1}} - 1\right) \\ +\theta_3\left\{(\omega_{i,t-1} - 1) \times \left(\frac{p_{r,t}}{p_{r,t-1}} - 1\right)\right\} + \theta_4 X_{i,t} + e_{i,t} \end{aligned} \tag{21}$$

where C_{it} are expenditures by household i in period t (either consumption or investment); $\omega_{i,t}$ is as before the household portfolio share in housing (we subtract one so that the interaction term is zero for an outright owner); $X_{i,t}$ is a set of control variables including education, family size, characteristics of the home and years spent at the current address; γ_c, ψ_t and ρ_r are fixed effects for cohort, time, and region respectively. Below we report a series of specifications with increasingly rich fixed effects. In our preferred specification we fully interact cohort, time and region effects.

By including these fixed effects (and their interactions), we control for any region or cohort specific trends in income growth that may be correlated with house price changes. These fixed effects can be thought of capturing shocks that are potentially correlated with house price movements but differ in their effects across young and old or across different regional labour markets. 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

[14] Henderschott, Pryce, and White (2003) show that the tax deductibility of mortgage interest can have substantial effects on households' initial loan-to-value ratios.

to spuriously large estimates of house price wealth effects for younger households (Attanasio, Blow, Hamilton, & Leicester, 2009).

We transform expenditure using the inverse hyperbolic sine (IHS) transformation rather than the log as a significant fraction of households have zero investment spending. It is defined as

$$IHS(y) = \log(y + \sqrt{(y^2 + 1)}) \tag{22}$$

The IHS transformation approximates log values at high values of spending but remains defined at zero (Burbidge, Magee, and Robb (1988)).

One concern about directly estimating equation (21) is that leverage (portfolio choice) is a choice variable and so endogenous. The conventional approach to estimating leverage effects is to use individuals' once-lagged leverage (uninstrumented), but this is unlikely to be adequate with a choice variable. As we document below, once-lagged leverage is correlated with gross house values and income from non-housing assets. In order for our empirical application to identify the effects of independently varying leverage, these other variables ought to be held constant.

A second issue concerns data availability. Long-running surveys that contain balance sheet data on wealth and leverage rarely contain comprehensive spending measures. A panel survey is also required in order to know the consumer's lagged leverage position $\omega_{i,t-1}$. Previous studies have addressed this problem by either using available proxies for spending (such as borrowing, Mian and Sufi (2011)), subsets of spending 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)). These approaches do not decompose total spending into consumption spending and investment spending.[15] Using total spending may lead to the misinterpretation of an investment spending response as being a consumption response. Distinguishing between the two is crucial for testing the importance of the borrow-to-invest channel.

For these reasons, in the UK we use a two-sample IV approach (Angrist & Krueger, 1992) to combine spending data in the LCFS with data on leverage in the BHPS. This approach allows us to simultaneously impute and instrument for leverage in our (cross-sectional) UK expenditure dataset using balance sheet data taken from the BHPS. The instrument we use is the credit conditions households faced at the time they moved into their current residences. In theory, the use of this instrument requires financial frictions or transaction costs, of the kind we discussed in Section 2, that prevent households

[15] There are a few other potential drawbacks to these approaches. Credit card borrowing, which is used as proxy in Mian, Rao, and Sufi (2013), may also be more cyclical than other forms of spending. This point was made in Aladangady (2017). The use of the budget constraint identity to impute consumption can also 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.

from reaching their optimal leverage for some time after they move. We discuss actual the strength and validity of our instrument further below. We provide additional details on the implementation of our approach in Appendix C.

In principle, in the US we could investigate these questions using the PSID, which in its later years contains information on both spending and leverage. However, the number of waves in which the PSID includes comprehensive consumption data is relatively short, as are other US panel surveys, such as the HRS, used by Christelis, Georgarakos, and Japelli (2015) to study questions around leverage. In addition, as we saw in Figure 3 and in the analysis in Table 2, US households tend to re-leverage rapidly in response to house price increases.[16] As a result, the leverage of US households is far less dependent on past circumstances than it is for UK households, and so our instrument does not have power in the US. In what follows, we therefore focus on UK results.

4.2 Instrument Relevance and Validity

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 the BHPS and the LCFS. 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 (for example, *loan* to income ratios are included in the credit conditions index of Fernandez-Corugedo and 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. We discuss results using alternative instruments in Appendix D.

The solid line in Figure 6 (Panel (a)) shows how this instrument varies over time in the UK. 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 6, Panel (b)) show similar patterns.

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 roughly 0.5% of the total number of observations in our LCFS sample.

As our regression model includes cohort fixed effects, what matters is within-cohort variation in households' leverage. Figure 7 shows how our instrument relates to loan-to-value ratios within a given

[16] In a similar design for the US we obtain F-statistics of 0.58 for $\omega_{i,t-1} - 1$ and 0.64 for $(\omega_{i,t-1} - 1) \times (\frac{p_{rt}}{p_{rt-1}} - 1)$.

Figure 6: Credit conditions, 1969-2013

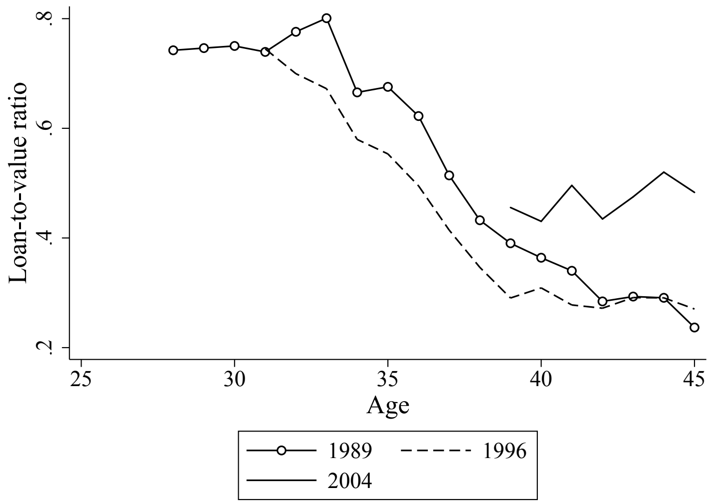
(a) Price-income ratios

(b) Advance-income ratios

Source: Office for National Statistics

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 6. 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.

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



Source: British Household Panel Survey/Understanding Society

This relevance of our instruments can be more formally tested by looking at the results of first stage regressions. We do this in Table 3. To match our preferred specification, we report first stage results including fully interacted cohort, region and time effects.

We have two first stage regressions, one for leverage and one for leverage interacted with house prises. 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.[17]

[17] A further 'first stage' check we can conduct is to test for a positive association between our instrument and total mortgage debt in the LCFS. This would demonstrate that the association between our instrument and leverage is not limited to our first sample. Regressing mortgage debt on (P/Y_{-T}) and our controls yields a positive coefficient with a t-statistic of 24.07.

Table 3: First stage results

	$(\omega_{i,t-1}-1)$	$(\omega_{i,t-1}-1)\times(\frac{p_{rt}}{p_{rt-1}}-1)$
P/Y_{-T}	0.403***	-0.009***
	(0.041)	(0.002)
$P/Y_{-T}\times(\frac{p_{rt}}{p_{rt-1}}-1)$	0.754**	0.704***
	(0.335)	(0.049)
Shea partial R^2	0.008	0.030
F-stat (p-value)	48.89	135.74
	(<0.001)	(<0.001)
Kleibergen-Paap (p-value)	97.40	
	(<0.001)	
N	30,947	
Clusters	8,250	

Note: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Standard errors are clustered at the individual level.

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 directly by including a control for years households have spent in their current address (in addition to a dummy variable for households having moved in in the last year to account for first year 'setting up' expenses).

As we discussed above we also control for rich set of fixed effects to control for other sources of endogeniety. The use of our instrument in combination with these controls means we effectively compare the spending responses of house price changes between two households in the same region and same cohort but who moved into their homes at different times (when credit was either looser or tighter).[18]

[18]The inclusion of cohort-region-year fixed effects means that we will only identify the *relative* effects of house price changes across different households within each region-cohort-year cell. Common effects of house prices changes affecting

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. They may also have moved into larger houses. 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. The assumption that such omitted factors do not induce a correlation between instruments and the error term is usually something which cannot be verified. Omitted variables are typically omitted because they are unobserved. 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.

To address additional endogeneity concerns we therefore look for an association between our instruments and gross house values, asset incomes and the probability of being a mover in the BHPS and Understanding Society panels conditional on our covariates. The two-sample instrumental variable approach allows for this kind of exogeniety testing where potential ommitted variables are observed in the second data set. Panel (a) of Table 4 reports results from regressions of these potential sources of endogeneity on our instruments and our other covariates. The instruments are both jointly and individually insignificant in all models suggesting that they are plausibly orthogonal to these omitted variables.[19]

An alternative source of variation used by a number of previous studies (e.g. Disney et al. (2010), Dynan (2012)) is household leverage lagged one period. We report in Panel (b) of Table 4 correlations between the potential omitted variables assessed in Panel (a) and households' lagged LTV ratios (leverage). There is strong evidence that those with higher lagged leverage have fewer financial assets and tend to live in less valuable homes, which invalidates its use as an instrument. The point of Table 4 is to show that our instrument (which is both a grouping instrument and further back in time) does much better than once-lagged household leverage on these exogeneity tests.

4.3 Main Results

Having established the relevance and exogeneity of our instrument, we now show in Table 5 the results of estimating equation (21) for residential investment, and total (non-durable and durable) consumption spending.

all households (and any general equilibrium effects on either national or regional housing markets) will be absorbed by our fixed effects.

[19] In additional unreported results, 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 in these regressions.

Table 4: Exogeneity of Instruments

Dependent var.	$\log(HValue)$	Invest inc. > 1000	Invest inc.$= 0$	Mover$_{t+1}$
Panel (a)		Instrument: Credit Conditions		
P/Y_{-T}	0.012	-0.001	0.0002	-0.0004
	(0.012)	(0.007)	(0.012)	(0.003)
$P/Y_{-T} \times (\frac{p_{rt}}{p_{rt-1}} - 1)$	-0.004	-0.039	0.061	0.014
	(0.106)	(0.062)	(0.102)	(0.033)
F-test: p-values	0.493	0.764	0.832	0.906
N	30,626	28,282	28,282	23,531
Clusters	8,116	7,735	7,735	6,618
Panel (b)		Instrument: Lagged Leverage		
LTV_{t-1}	-0.192***	-0.164***	0.320***	0.004
	(0.0183)	(0.012)	(0.020)	(0.007)
$LTV_{t-1} \times (\frac{p_{rt}}{p_{rt-1}} - 1)$	-0.596***	-0.045	0.110	0.002
	(0.196)	(0.116)	(0.192)	(0.076)
F-test: p-values	< 0.001	< 0.001	< 0.001	0.851
N	30,626	28,282	28,282	23,531
Clusters	8,116	7,735	7,735	6,618

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Controls for education, cohort-region-year dummies, sex, house type, number of rooms, number of adults, number of children, years at address, and a dummy variable for having moved in in the previous year. Standard errors are clustered at the individual level.

We consider three versions of equation (21). The first includes regional house price changes and controls for region and cohort fixed effects (but not time effects). In this specification, house price growth is positively associated with consumption growth and negatively associated with residential investment for outright owners. The residential investment behaviour of more leveraged households however rapidly increases in response to house price gains, while their consumption spending is no more sensitive than that of other homeowners. These findings are consistent with the predictions of our model; homeowners who are not net borrowers have a desire to reduce their exposure to housing as prices rise, while more leveraged net borrowers disproportionately increase their housing investments.

Our second regression model (columns (3) and (4)) shows results when we additionally control for time effects. These removes the effects of common shocks that may simultaneously drive house price growth and consumer spending (such as aggregate productivity changes). The main effect of house prices on consumption, which is now identified by differences in regional house price growth, is small and no longer significant. We once again find that the residential investment spending of more leveraged households is much more responsive to house price increases, while consumption spending is not.

Columns (5) and (6) present our preferred specification which includes a full set of time-cohort-region interactions, controlling for shocks that may vary in their impacts across locations and age groups. With this specification, the direct effect of prices is no longer identified, but the interaction between house prices and leverage is identified and this is the basis of the test of our mechanism. Our results imply that 10% increase in house prices results in a 7.3% greater increase in residential investment for a household with an LTV of 66% relative to a household with an LTV of 50% (ie, a housing portfolio share, ω, of 3 relative to 2).

Table 5: Log spending responses

	(1) Res inv.	(2) Cons.	(3) Res inv.	(4) Cons.	(5) Res inv.	(6) Cons.
$(\omega_{i,t-1} - 1) \times (\frac{p_{rt}}{p_{rt-1}} - 1)$	1.680***	-0.038	0.729***	0.032	0.728***	0.003
	(0.230)	(0.048)	(0.169)	(0.042)	(0.269)	(0.067)
$(\omega_{i,t-1} - 1)$	0.367***	-0.051***	-0.054	-0.021	-0.047	-0.020*
	(0.078)	(0.014)	(0.062)	(0.016)	(0.046)	(0.012)
$\frac{p_{rt}}{p_{rt-1}} - 1$	-0.596**	0.554***	-1.276***	-0.054	-	-
	(0.280)	(0.060)	(0.325)	(0.081)	-	-
Controls						
Region effects	X	X	X	X		
Cohort effects	X	X	X	X		
Year effects			X	X		
Cohort × region × year					X	X
R^2	0.063	0.341	0.065	0.346	0.082	0.360
N	60,342	60,342	60,342	60,342	60,342	60,342

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Additional controls for education, sex, house type, number of rooms, number of adults, number of children, years at address, and a dummy variable for having moved in in the previous year.

One concern with our interpretation of these results may be that the residential investment response reflects the durability or luxuriousness of housing, rather than an investment motive. In Table 6 we examine how responses to house price increases vary for subcategories of total consumption spending. First, we run regressions separately for non-durable and durable spending. We do not find evidence that leveraged households' spending on either of these subcategories is more sensitive to house price increases than other households'. Second, we report spending effects for 'luxuries' (a subset of non-durables, defined as spending on recreation and food out). We do not find evidence of strong spending responses for these goods, lending additional support to our hypothesis that the increase in spending on residential investment reflects a desire to rebalance consumers' investment portfolios rather than a consumption motive.

Table 6: Log spending responses

	Res inv. (1)	Non-durables (2)	Durables (3)	Luxuries (4)
$(\omega_{i,t-1} - 1) \times (\frac{p_{rt}}{p_{rt-1}} - 1)$	0.728***	-0.017	-0.168	-0.064
	(0.269)	(0.063)	(0.255)	(0.136)
$(\omega_{i,t-1} - 1)$	-0.047	-0.016	-0.039	-0.046*
	(0.046)	(0.011)	(0.044)	(0.024)
Cohort × region × year	X	X	X	X
R^2	0.082	0.377	0.112	0.211
N	60,342	60,342	60,342	60,342

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Column (1) reproduces column (5) in Table 5. Controls for education, cohort-region-year dummies, sex, house type, number of rooms, number of adults, number of children, years at address, and a dummy variable for having moved in in the previous year.

4.4 Robustness Checks and Additional Evidence

Robustness Checks

We carry out a range of robustness checks of our baseline results. For reasons of space, we discuss the results of these briefly here, reporting the full set of results in Appendix D.

1. *Alternative definitions of residential investment.* The definition of residential investment we use above is relatively broad compared to what would for example be used in the national accounts. In particular, we include other fixtures and durable investments (such as for example, kitchen equipment) that we consider likely to be capitalised into the value of the property but which may be excluded in other definitions. In the Appendix, we also consider a narrow definition that is restricted to spending on changes to the structure of the property as well as household repairs and maintenance.[20] The results we obtain are very similar to our main results. We also find a

[20] This definition is more in the spirit of national accounts. For example in the US National Income and Product Accounts, "private fixed investment" by owner-occupiers includes spending such as "construction of new nonresidential and residential buildings.", "improvements (additions, alterations, and major structural replacements) to nonresidential and residential buildings." and "certain types of equipment (such as plumbing and heating systems and elevators) that are considered an integral part of the structure." (see Chapter 6 of U.S. Bureau of Economic Analysis (BEA) (2016))

positive effect when we use an indicator of whether households made investments in household extensions as our dependent variable.

2. *Alternative instruments.* We also consider results using three alternative instruments. The first is the Credit Conditions Index used in Fernandez-Corugedo and Muellbauer (2006); the second is the average house price in each region at the time individuals moved into their current homes (as used as an instrument for mortgage debt in Chetty et al. (2017)); and the third is to use credit condition as the time household heads turned 25 (rather than at the time of their last move). This latter strategy means our we do not rely on possibly non-random variation in the timing of moves; however, it also means we cannot separately control for cohort effects. The use of these alternative instruments give very similar results.

3. *Sample definition.* We exclude households who moved within the previous year from our analysis, but concerns may remain that our spending effects are driven by more recent movers (who are likely to be the most leveraged, and possibly at credit a constraint). We therefore consider results from an alternative sample where we exclude those who moved into their homes within the previous five years. Results are similar to those in Table 5.

   We also separately consider results for a younger subsample of households (those with heads aged 25-45). If the relaxation of credit constraints were an important explanation for our findings, we would expect the magnitude of effects to be greater for this subsample. However, we find that the results are similar to those in Table 5. As in the case of our full sample, there is no evidence of a differential response in consumption spending between leveraged and non-leveraged households.

Extensive Margin: Other property investments

Households may also invest in housing by purchasing additional properties or by up-sizing their main residence. In this section we examine whether more leveraged households are more likely to make such investments in response to house price increases than other households, as our model would predict.

To do so we estimate the following equation using the BHPS

$$\Delta Y_{t,t+10} = X\delta_0 + \delta_1 \left(\frac{p_{rt+10}}{p_{rt}} - 1 \right) + \delta_2 \left[(\omega_{i,t-1} - 1) \times \left(\frac{p_{rt+10}}{p_{rt}} - 1 \right) \right] + u_t \tag{23}$$

where Y is some outcome of interest (second home ownership or the number of rooms in the household's main residence). We consider changes in these outcomes over a period of 10 years. This is to account for the possibility that, as a result of transaction and search costs, consumers may be slow to make new home purchases in response to increases in their housing wealth.

Table 7 shows results for the change in second home ownership. We include other controls for year, region, 10-year birth cohort, a quadratic in age and the years the household head has been living at the current address. The latter control accounts for the fact that households who have moved recently will likely be closer to their desired leverage and so less likely to need to rebalance their portfolios. As above we instrument leverage with the price to income ratio at the time households moved into their current residence. We find that the second home purchases of more leveraged households are more responsive to house price increases than the purchases of other households. Our results imply that households with LTVs of 50% are 0.4 percentage points more likely to purchase a second home than outright owners following a 10% appreciation in house prices.

Table 7 also includes results for whether more leveraged households are more likely to up-size their main residences (as measured by changes in the number of rooms in their primary residence). While the pattern of results is similar to that for second homes, the coefficient on the interaction of leverage and house price changes is not statistically significant.

Table 7: Effects of leverage on second home ownership and home size

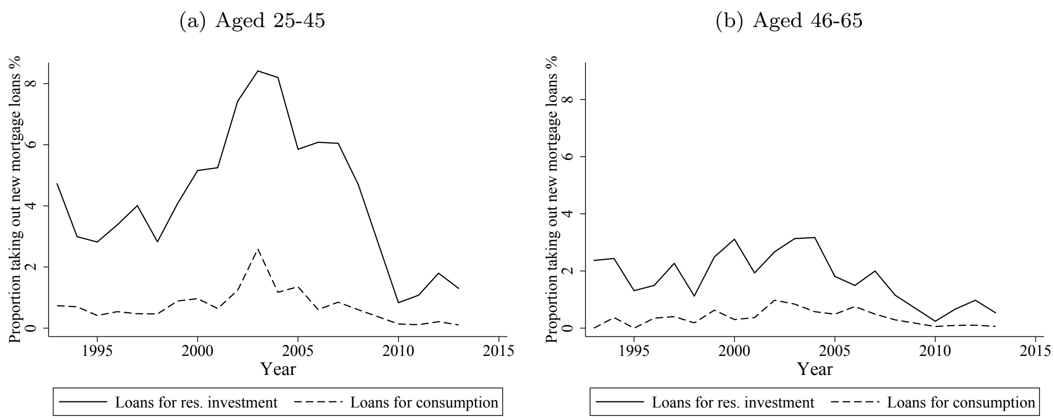
	Δ *Second home*$_{t,t+10}$	Δ *No Rooms*$_{t,t+10}$
	(1)	(2)
$(\omega_{i,t-1} - 1) \times \left(\frac{p_{rt+10}}{p_{rt}} - 1\right)$	0.041**	0.077
(Standard errors)	(0.020)	(0.075)
First stage F-stat	37.34	32.34
(p-value)	(<0.001)	(<0.001)
N	3,599	4,627
Clusters	1,393	1,440

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Controls are year dummies, dummies for 10-year birth cohorts, age, age squared, years at current address and a dummy for having just moved in. Standard errors are clustered at the individual level.

Survey Self-Reports on Spending Motives

The importance of extensions and other home improvements as a reason for new borrowing is confirmed when we consider the uses for which households report taking out additional mortgage debt. Households in the BHPS are asked whether new mortgage loans were used for extensions, home improvements, car purchases, other consumer goods, or some other reason (households may give more than one answer). We class the first two of these responses as "residential investment" and the second two as "consumption" and plot the proportions reporting each motive for household heads aged 25-45, and 46-65 in panels (a) and (b) of in Figure 8. Both younger and older households are roughly four times more likely to report taking out a loan for residential investment than for consumption spending. Overall, when we condition on taking out a new loan, 62% of new loans were taken out for a residential investment purpose compared to 11.5% for some consumption purpose in the UK.[21]

Figure 8: Purpose of new mortgage loans, 1993-2013



Source: British Household Panel Survey/Understanding Society

5 Conclusion

It is well known that households releverage and increase spending in response to house price gains. The point we stress in this paper is that spending does not mean consumption spending: spending includes investment in housing. We introduce a new "borrow-to-invest" channel whereby households want to increase their borrowing to releverage in response to house price gains, but where the borrowing is used to increase investment in housing. This is driven by a desire to maintain the risk and return of their portfolios. We model this channel in a life-cycle portfolio choice framework.

We provide an empirical test of this borrow-to-invest channel by focusing on one prediction of the model, that the investment spending response to a house price realisation will vary more with leverage

[21] There is suggestive evidence on the same lines for the US. Brady, Canner, and Maki (2000) use a "reason for loan" question in the Federal Reserve Board Survey of Consumers and find that home improvements were a more important self-reported motive for home equity withdrawal than consumption spending, as in Figure 8. Further, Cooper (2010) reports a significant association between home equity extraction and the binary indicator of residential investment in the PSID.

than the consumption spending responses. In particular, more leveraged households will respond to a greater extent to a house price increase, but this difference in response will be in their investment spending not consumption spending. We show this to be the case by regressing different categories of spending on house price realisations interacted with leverage.

Previous studies have examined the total spending or borrowing response of households to house price realisations, and concluded that access to credit drives differences in responses across more and less leveraged households. Our results suggest that leverage has an important influence on household spending responses to house price increases even when households are not at their credit constraints. Our findings have relevance for the design of macro-prudential policy interventions that restrict loan-to-value ratios and debt-to-income ratios. These interventions aim to restrict the growth of debt in the face of house price increases, however constraints on loan-to-value are themselves relaxed by house price increases and this leads to greater borrowing and greater investment in housing.

On the other hand, credit constraints on loan-to-income ratios may limit the ability of households to follow through on the borrow-to-invest motive. Even in this situation, our core point remains: increased borrowing is not used to finance consumption, rather it is used to increase investment in housing. In the context of a two-asset model, credit constraints act to limit wealth accumulation: households cannot exploit the high returns of a leveraged portfolio, and so credit constraints reduce life-time consumption levels, rather than simply changing the time path of consumption.

Our results on the impact of house price changes have further implications for the literature on consumption pass-through (which follows Blundell, Pistaferri, and Preston (2008)). That literature focuses largely on the pass-through of income shocks to non-durable consumption, but the realisations of house prices also matter, as noted by Etheridge (2019). We show that the extent of the pass-through from house price realisations will depend on the leveraged position of households, and also that it is important to distinguish between a consumption response and an investment spending response.

A final implication of our findings is that they suggest potentially important feedback mechanisms following house price increases. As house prices rise, the desire of households to re-leverage may lead to greater demand for housing. The aggregate implications of the greater demand for housing depends on whether the household desire to borrow-to-invest results in investment in new housing stock, which includes additions to existing homes and expands the supply of housing, or in purchasing existing housing stock. If the response is in purchasing existing stock, this would generate further price increases - increasing households' exposure to future house price changes and amplifying housing booms.

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Appendix A Descriptive Statistics

In Table 8 we report descriptive statistics for 1993-2013 in the UK data, and for 2005-2013 for the PSID (i.e the years when the most comprehensive spending data was available). The proportion of those owning their own homes and the average tenure among homeowners are similar across the two UK surveys, at around 70% of households. Ownership rates are somewhat lower in the PSID at around 55%. Focussing on home owners, the average loan-to-value ratio in our BHPS sample is 0.34, while US households tend to be more leveraged with an average loan-to-value ratio of 0.54.

Table 8: Descriptive statistics, BHPS and LCFS and PSID

	BHPS (1993-2013)	LCFS (1993-2013)	PSID (2005-2013)
Age	44.7	44.5	45.3
% Own home	70.6%	69.5%	58.6%
Homeowners			
Years at address	10.7	10.2	11.8
LTV ratio	0.33	-	0.44
ω_t (housing share)	2.79	-	3.59
Total spend ($ ann.)	-	42,974	65,243
Non-durable	-	32,763	53,798
Durable	-	6,039	6,919
Residential inv.	-	4,173	4,525
% Res inv. > 0	-	78.8%	73.2%

Note: UK data is for the period 1993-2013. US data is for the period 2005-2013 when more comprehensive spending measures are available in the PSID. See text for details of what is included in each spending category.

Appendix B Mortgage Imputation in Understanding Society Survey

The BHPS contains data on mortgage values from 1993 (wave 3) onwards, while Understanding Society dropped these variables in its second wave in 2010 except for households who had newly moved. However, in all years of the BHPS and Understanding Society the data contains a great deal of information on household mortgages, including whether households are outright owners, the mortgage type, the value of any additional loans, and the years left to pay on the mortgage. So as to avoid throwing data out unnecessarily, we use this information to impute mortgages for the remaining three waves of Understanding Society.

For those with interest only or 'endowment' mortgages, we assume no principal repayments. In this case, we take the current value of the mortgage to be its lagged value plus any additional loans the household may have taken out since its previous interview. For those with standard repayment mortgages we assume the loan is amortised with annual payments (which consist of both interest and principal) determined by

$$\text{Ann. Payment} = M_{t-1} \times i/(1-(1+i)^{-(\ell+1)}) \tag{24}$$

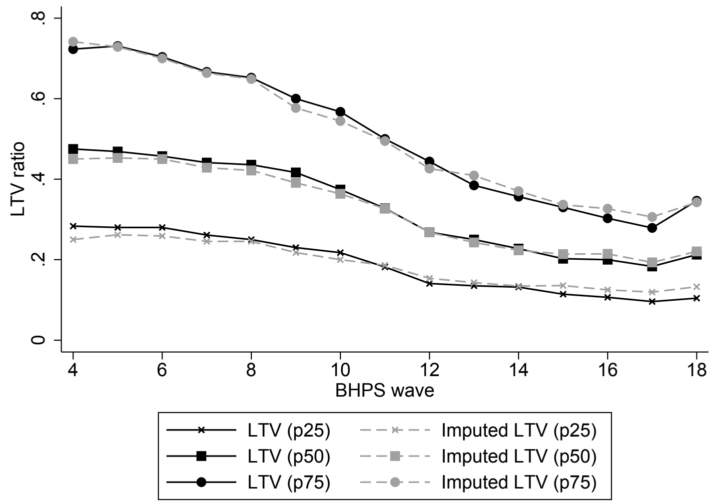
where M_{t-1} is the value of the households mortgage in the previous year, i is the interst rate and ℓ is the remaining life of the mortgage. This means that the mortgage in any given period is given by

$$M_t = M_{t-1} - \text{Ann. Payment} + iM_{t-1} + M_t^{new} \tag{25}$$

where M_t^{new} is the amount of additional mortgage we observe the household borrowing between periods t and $t-1$.

To assess the accuracy of our imputation procedure, we implemented it on waves of the BHPS for which we observe the true value of households' mortgages. That is we took a set of households observed in the 3rd wave of the BHPS, and imputed their mortgage values for all subsequent waves. We then plot the LTV ratios implied by our imputation procedure against actual values calculated from the survey for different percentiles of the LTV distribution (25th, 50th and 75th). The results of this exercise are shown in Figure 9. Our imputation procedure appears to work extremely well - accurately predicting households' LTV ratios even after 15 waves.

Figure 9: Imputed and actual LTV values, BHPS



Appendix C Two-sample IV

In this paper we make use of Two Sample Two Stage Least Squares (TS2SLS). Inoue and Solon (2010) show that this approach is more efficient than the TSIV estimator of Angrist and Krueger (1992).

TS2SLS is best explained by first considering a standard two-stage least squares (2SLS) approach. Let $\mathbf{M} = [\ X \quad \omega_{i,t-1} - 1 \quad (\omega_{i,t-1} - 1) \times (\frac{p_{rt}}{p_{rt-1}} - 1)\ ]$ 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

$$c = \mathbf{M}\delta + e$$

where ω_{t-1} and e 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 $E[e|\mathbf{Z}] = 0$ and means that δ can be consistently estimated using the 2SLS estimator

$$\hat{\delta}_{2SLS} = (\hat{\mathbf{M}}'\hat{\mathbf{M}})^{-1}\hat{\mathbf{M}}'c \tag{26}$$

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).[22] 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 (26)

$$\hat{\delta}_{TS2SLS} = (\hat{\mathbf{M}}_1{}'\hat{\mathbf{M}}_1)^{-1}\hat{\mathbf{M}}_1{}'c_1 \tag{27}$$

where $\hat{\mathbf{M}}_{\mathbf{1}} = \mathbf{Z}_{\mathbf{1}}(\mathbf{Z}'_{\mathbf{2}}\mathbf{Z}_{\mathbf{2}})^{-1}\mathbf{Z}'_{\mathbf{2}}\mathbf{M}_{\mathbf{2}} = \mathbf{Z}_1\hat{\Pi}_2$. 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 $\hat{\delta}_{TS2SLS}$.

We adjust standard errors from our second stage regression to account for the two-step nature of the procedure. Because we cluster observations from the same household in our first stage regression, we use the robust standard error correction for TS2SLS derived in Pacini and Windmeijer (2016).

Appendix D Alternative estimation approaches

D.1 Alternative definitions of residential investment

First we investigate the extent to which our results depend on our chosen measure of residential investment. The measure of residential investment that we use for our main results includes certain white goods such as cookers, refrigerators and washing machines which are often capitalised into

[22] In their orginal article, Angrist and Krueger (1992) in fact proposed orginally an alternative GMM estimator $\hat{\delta}_{IV} = (\mathbf{Z}_2'\mathbf{M}_2/n_2)^{-1}(\mathbf{Z}_1'c_1/n_1)$. 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 $\mathbf{Z}$

property values but which would not necessarily be considered residential investment spending in for instance a national accounting framework. Here we examine the extent to which our results are robust to the removal of these items by restricting out definition to goods such as electric tools, floor coverings and the costs of installing or repairing heating and air conditioning units (along with spending on household extensions).

We show results using these alternative measures in Table 9. Column (1) shows results using the inverse hyperbolic sine transformation of our narrower residential investment measure. The effects of increases in prices for more leveraged households are still large and statistically significant (and indeed very similar to those obtained in our main results). In column (2) we show results from a linear probability model in which the dependent variable takes a value of 1 if the household is observed spending a positive amount on household extensions. This is probably the purest measure of residential investment in that it only includes structural modifications to the home. Again we find that the investment spending of more leveraged households is significantly more responsive to house price changes than the spending of other home-owners. A 10% increase in local house prices is associated with a 1 percentage point increase in the probability that a household with a 50% LTV ratio builds an extension relative to an outright owner.

Table 9: Results with alternative definitions of residential investment

	Narrow Res inv.	Extensions>0
	(1)	(2)
$(\omega_{i,t-1} - 1) \times (\frac{p_{rt}}{p_{rt-1}} - 1)$	0.714***	0.100*
	(0.268)	(0.054)
$(\omega_{i,t-1} - 1)$	-0.046	-0.010
	(0.046)	(0.009)
R^2	0.081	0.039
N	60,342	60,342

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. See table 5 for list of controls.

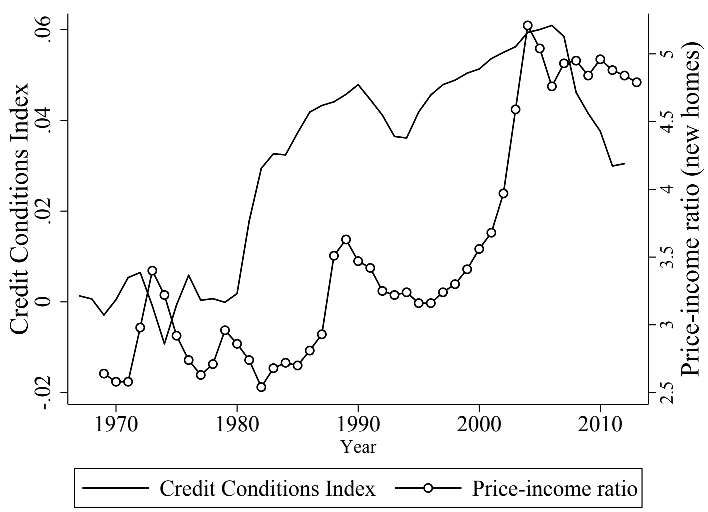
D.2 Alternative instruments

In this section, we consider how our results are affected when we use two alternative instruments in place of the price-to-income ratio at the time individuals moved into their current residences.

The first of these is the Credit Conditions Index (CCI) assembled in Fernandez-Corugedo and

Muellbauer (2006). This index contains 10 indicators of credit conditions. Two are aggregate measures of unsecured and mortgage debts. The remaining 8 are fractions of mortgages for first time buyers that are above given loan-to-value and loan-to-income ratios for different age groups and regions. The index is constructed controlling for various determinants of credit demand to ensure the index reflects credit supply conditions.[23] The series is plotted alongside our instrument in Figure 10. The CCI shows a discontinuous increase in 1981. Because this is not matched by a similarly discontinuous increase in leverage for those moving in these years in our sample, when we include households who moved before this date we find the instrument to be weak and our results imprecise. The first two columns of Table 10 present results for log total spending and residential investment (conditional on moving in 1981 or after). The results are very similar to what we obtain in our main specification, with the implied elasticity much greater for residential than other forms of spending.

Figure 10: Credit Conditions Index vs price-income ratio



The second alternative instrument we consider is the average regional price at the point homeowners moved into their homes. This makes use of interregional variation as well as intertemporal variation in house prices. We report results for this approach in Table 10. We find that they are again very similar to our main results.

Finally, we examine how our results are affected when we instrument leverage at the time household

[23]These controls are: nominal and real interest rates, a measure of interest rate expectations and of inflation and interest rate volatility, mortgage and housing return, 36 risk indicators, house prices, income, a proxy for expected income growth, the change in the unemployment rate, demography, consumer confidence, portfolio wealth components, proxies for sample selection bias and institutional features.

heads reach age 25 (around the time many households make their first purchase) rather than the date of their last move. This instrument is not dependent on the timing of moves; however, its use means we cannot separately control for cohort effects. As a result, we only include region-year interactions when using this instrument. The results using this specification are shown in columns (5) and (6) of Table 10. The elasticity of residential investment spending is still much larger for more leveraged households.

Table 10: Results with alternative instruments

	CCI		Reg. house prices		$P/Y_{age=25}$	
	Res inv.	Cons.	Res inv.	Cons.	Res inv.	Cons.
	(1)	(2)	(3)	(4)	(5)	(6)
$(\omega_{i,t-1} - 1) \times (\frac{p_{rt}}{p_{rt-1}} - 1)$	0.772**	0.017	0.716**	0.072	0.498***	0.092***
	(0.354)	(0.086)	(0.287)	(0.070)	(0.147)	(0.035)
$(\omega_{i,t-1} - 1)$	-0.134	0.003	0.093*	0.018	-0.148***	0.007
	(0.118)	(0.027)	(0.053)	(0.013)	(0.022)	(0.005)
Instruments:						
$CCI_{-T},\ CCI_{-T} \times (\frac{p_{rt}}{p_{rt-1}} - 1)$	x	x				
$P_{-rT},\ P_{-rT} \times (\frac{p_{rt}}{p_{rt-1}} - 1)$			x	x		
$P/Y_{age=25},\ P/Y_{age=25} \times (\frac{p_{rt}}{p_{rt-1}} - 1)$					x	x
R^2	0.087	0.357	0.082	0.360	0.068	0.338
N	52,143	52,143	60,342	60,342	52,722	52,722

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. See Table 5 for list of controls. Columns (5) and (6) include region-year interactions, but not cohort effects.

D.3 Alternative samples

A further concern might that our results for more leveraged households are driven entirely by households who have just moved into their homes (and are thus more likely to be at a credit constraint). Since price-to-income ratios have tended to increase over time, our first stage regressions will tend to predict higher rates of leverage for more recent movers.

To account for this, in our main results we exclude households who moved into their homes in the previous year and control for the number of years at current address. In Table 11 we consider how our results are affected when we exclude households who moved into their homes within the previous five years. The results from this exercise are remarkably similar to our main set of results.

Table 11: Results excluding those who moved in in last 5 years

	Res inv.	Cons.	Non-durables	Durables	Luxuries
	(1)	(2)	(3)	(4)	(5)
$(\omega_{i,t-1} - 1) \times (\frac{p_{rt}}{p_{rt-1}} - 1)$	0.901**	0.004	0.010	-0.264	0.148
	(0.431)	(0.108)	(0.100)	(0.415)	(0.219)
$(\omega_{i,t-1} - 1)$	-0.062	-0.021*	-0.014	-0.047	-0.030
	(0.045)	(0.012)	(0.011)	(0.044)	(0.023)
R^2	0.079	0.386	0.405	0.122	0.235
N	42,276	42,276	42,276	42,276	42,276

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. See table 5 for list of controls.

In Tables 12 and 13 we report results for a younger subsample of homeowners (those with heads aged 25-45). These are very similar to our main results (which cover households with heads aged 25-65).

Table 12: Log spending responses (age 25-45)

	(1) Res inv.	(2) Cons.	(3) Res inv.	(4) Cons.	(5) Res inv.	(6) Cons.
$(\omega_{i,t-1}-1)\times(\frac{p_{rt}}{p_{rt-1}}-1)$	3.131***	-0.640***	0.978***	-0.049	0.833***	-0.002
	(0.413)	(0.113)	(0.208)	(0.047)	(0.203)	(0.046)
$(\omega_{i,t-1}-1)$	0.374***	-0.119***	-0.075*	-0.006	-0.109***	-0.001
	(0.142)	(0.038)	(0.040)	(0.009)	(0.040)	(0.009)
$\frac{p_{rt}}{p_{rt-1}}-1$	-3.392***	1.612***	-1.923***	0.253**	-	-
	(0.812)	(0.214)	(0.502)	(0.115)	-	-
Controls						
Region effects	X	X	X	X		
Cohort effects	X	X	X	X		
Year effects			X	X		
Cohort × region × year					X	X
R^2	0.071	0.289	0.074	0.298	0.092	0.314
N	29,553	29,553	29,553	29,553	29,553	29,553

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Additional controls for education, sex, house type, number of rooms, number of adults, number of children, years at address, and a dummy variable for having moved in in the previous year.

Table 13: Log spending responses (age 25-45)

	(1) Non-durables	(2) Durables	(3) Luxuries
$(\omega_{i,t-1} - 1) \times (\frac{p_{rt}}{p_{rt-1}} - 1)$	-0.007	-0.437**	0.008
	(0.043)	(0.184)	(0.092)
$(\omega_{i,t-1} - 1)$	-0.002	0.005	-0.049***
	(0.009)	(0.036)	(0.019)
R^2	0.324	0.106	0.184
N	29,553	29,553	29,553

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Controls for education, cohort-region-year dummies, sex, house type, number of rooms, number of adults, number of children, years at address, and a dummy variable for having moved in in the previous year.