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Are Housing Wealth Effects Asymmetric in Booms and Busts?

Evidence from New Zealand

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Abstract This paper investigates the effects of household indebtedness and housing wealth on consumption. To identify exogenous movements of housing wealth and leverage, we estimate housing supply elasticities for New Zealand urban centers. We construct synthetic panel series by using household survey data to estimate the marginal propensity to consume out of exogenous changes in housing wealth, while controlling for the household leverage ratio. Our empirical results show that, on average, the marginal propensity to consume out of housing wealth is about 3 cents out of one dollar. But it is larger, about 4 cents, in response to falling house wealth than to increasing housing wealth, about 2 cents. We further investigate the role of household indebtedness in accounting for the asymmetric effect. Our findings suggest that household leverage reinforces the housing wealth effect in a housing bust, but dampens the housing wealth effect in a boom.

Keywords Household debt · Housing wealth effects · Leverage · Marginal propensity to consume.

JEL classification D12 · D14 · E21 · R2

1 Introduction

Are housing wealth effects asymmetric? This paper estimates the effect of house prices and leverage on consumption in New Zealand and assesses if the wealth effect is symmetric in housing boom and bust cycles. Building on the existing empirical literature which documents a strong marginal propensity to consume (MPC) during housing downturns (See: Mian et al., 2013, Christelis et al., 2015, and Kaplan et al., 2019), we provide evidence of asymmetric housing wealth effects, being larger in the bust than in the boom. We also

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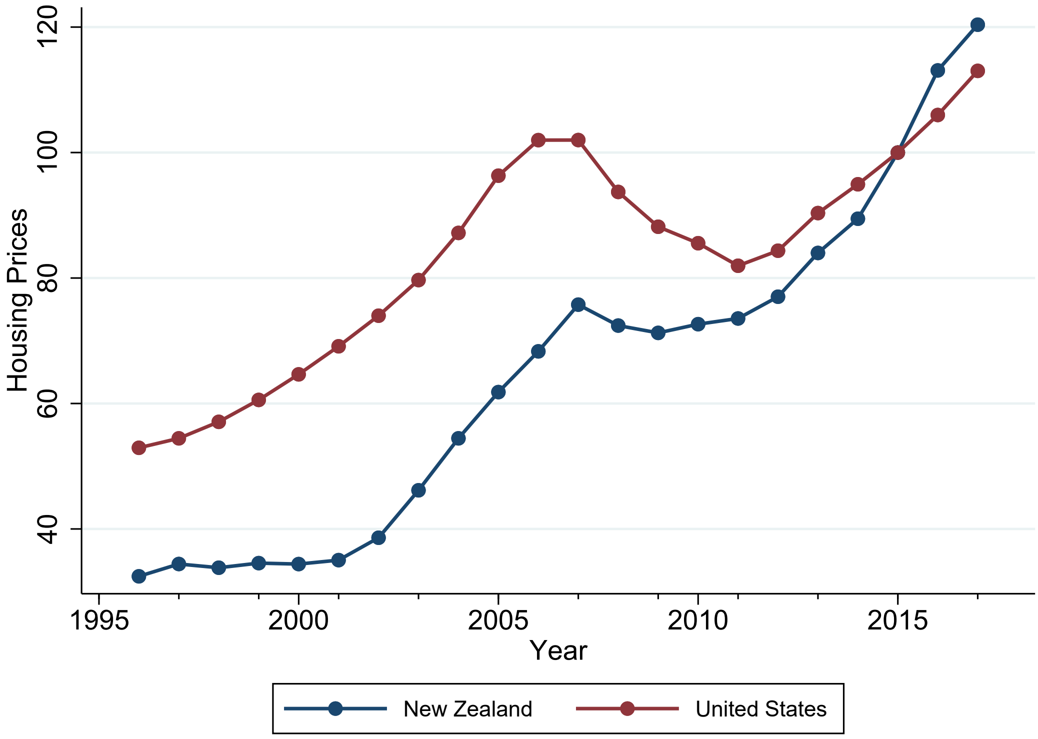
Fig. 1: Nominal house prices in New Zealand and the US (2015=100)



OECD iLibrary: Main Economic Indicators.

emphasize the role of household leverage in this asymmetric response of consumption spending to housing wealth swings.[1]

Experience from the Great Financial Crisis (GFC) led to serious concerns about the large impact of housing downturns and leverage on household expenditure (Mian et al., 2013; Cerutti et al., 2017; Berger et al., 2017). Yet, in the presence of asymmetry, linear models may produce misleading inferences for policy-makers and lead to misinformed reactions to future house price trends. For example, that a current boom in house prices has not fueled large increases in consumption should not be taken as evidence that a future fall in house prices will not bring a large cutting back of consumption. Thus asymmetries point to the importance of not becoming complacent during a boom.

In this paper, we focus on empirically identifying the role of leverage in determining the size of consumption responses to house price shocks in New Zealand. We use micro data from the New Zealand Household Economic Survey (HES), which provides comprehensive household-level information on highly disaggregate expenditures, income, loans, and reported house values. By linking HES with regional data on house prices and geographical estimates of housing supply elasticities, we study the housing wealth effect on consumption while assessing the role of household leverage.

Housing price dynamics in New Zealand represent an interesting case as contrasted to the US. While the US economy experienced a severe housing market downturn after the GFC in 2008, house prices and household leverage in New Zealand have been rising for the most part over the last decade (Figure 1). While house price dynamics in New Zealand and Australia have resembled other advanced economies, the GFC affected their housing markets differently in terms of timing and intensity (Murphy, 2011). In New Zealand, a mix of institutional practices, market conditions and economic policy responses acted early enough to shelter housing markets. On policy grounds, the Reserve Bank quickly decreased the policy rate to a record low level effectively decreasing mortgage interest rates, while providing extensive liquidity assistance to

[1] As we show in our brief literature review, housing wealth has been given numerous definitions depending on the identification technique employed in each study. As the main focus of this paper is exogenous variation in wealth coming from house price swings, we use the concepts ”housing wealth” and ”house price” effects on consumption interchangeably, unless otherwise specified.

the banking sector and, in turn, the housing market. Within this setting, our study contributes to the wealth effects literature in at least four respects:

First, our paper adds to the debate over the role of leverage and direction of wealth effects during periods of booms and busts. While there is by now a growing literature focusing on the wealth effect during the GFC (Dynan, 2012; Mian et al., 2013; Christelis et al., 2015; Angrisani et al., 2019), most studies focus on the US case, and are thus narrowed to the exploration of mostly negative swings in the housing market. With these samples, the exploration of asymmetric behavior in the relationship is limited, as there is little observed variability of housing price upswings. Yet, potential asymmetries in the relationship may help reveal the source of association between wealth, leverage, and consumption, by pointing at which wealth channel, if any, is responsible for the established empirical pattern of correlating house prices and household consumption (Altonji and Siow, 1987; Paiella and Pistaferri, 2017). Moreover, wealth and credit channels have been shown to differ significantly across countries and over time (Aron et al., 2012). Against this background, the housing market in New Zealand, characterized by price variability both inter-regionally and across time, is an ideal setting for testing the presence of asymmetric wealth effects and reveal information which is useful for policy as a standalone result.

Second, while asymmetric behavior has been explored in several studies investigating the correlation between housing wealth and consumption (Skinner, 1996; Engelhardt, 1996; Disney et al., 2010; Paiella and Pistaferri, 2017), very few studies associate its presence to the growing literature of wealth effects propagation through household leverage. For example, Dynan (2012) and Mian et al. (2013) focus only on leverage without exploring asymmetric responses, while Disney et al. (2010), who also find much stronger consumption responses for negative housing price swings, address the question only for households with negative equity for the period preceding the GFC. To the best of our knowledge, this is the first study that examines the presence of asymmetric housing wealth effects on consumption while controlling for the three main measures of indebtedness (loan-to-value ratio, debt-to-income ratio, and debt-servicing-ratio) and, thereby, shedding light to which underlying economic mechanism prevails.

Third, a major challenge for studies exploring wealth effects on consumption is to find exogenous variation in house prices which can be used to separate direct house price effects on consumption from confounding factors. To identify the causal relationship between house prices and consumption, we construct housing supply elasticities for New Zealand urban centers (Saiz, 2010), estimating the percentage of land available for development in each region. Additionally, we take into account reservations on this instrument raised for the cases of the US and England (Hilber and Vermeulen, 2015; Davidoff, 2016), and apply demand shifters that help test the robustness of our results. With the exception of studies focusing on the US and UK, this is the only study that applies housing supply elasticities to measure wealth effects. The application of this methodology strengthens the credibility of our results.

Fourth, an advantage of our dataset, over those used in similar studies, is that it provides at the same time information for repeated survey waves in all key variables of interest, namely consumption, housing wealth, and indebtedness. Very few other studies in the wealth effects literature use directly elicited information on housing wealth swings without the need to impute them or combine multiple data sources.[2] This is because most surveys and administrative sources, typically focus on a single aspect of household finances, either it is consumption or household balance sheets, or provide information on both aspects for a limited number of years. The data-set provided by Statistics New Zealand represents an integration of multiple surveys, combining regional and demographic characteristics with household finances and expenditure records for the same households.

Two sets of results emerge from our analysis:

First, the elasticity of the consumption to housing price changes is estimated at 0.3, which is slightly larger than existing empirical studies with micro-data focusing on countries or periods of no severe housing market downturns (Attanasio et al., 2009; Gan, 2010; Windsor et al., 2015). This corresponds to an average marginal propensity to consume of 3 cents of consumption out of one dollar change in housing price. Our elasticities are significantly stronger for older-homeowners than younger home-owners, implying the dominance of a genuine wealth effect over the collateral channel. No evidence of third factors affecting the relationship is found. More importantly, we find that the housing wealth effect is asymmetric with respect to positive and negative housing price swings across the economy's regions. The housing wealth elasticity is 0.37 for house prices drops, as compared to 0.21 in response to housing price upswings. To address the Davidoff (2016) critique regarding ”steep slopes”, productivity, and dwellings demand, we take extra care

[2] Exceptions include Dynan (2012); Christelis et al. (2015), and other studies focusing on the US using only the recent waves of PSID.

to assess the robustness of our supply elasticity instrument in predicting house prices, by including demand and productivity controls, such as education and regional migration flows, in our Instrumented Variable regression. Our results remain robust. Additionally, by decomposing non-housing consumption, we find that elasticities for the case of durables are twice as strong as in the case of non-durables.

Second, household indebtedness exerts a significant impact on consumption spending. The estimated wealth effects asymmetry in tandem with the prevalence of liquidity risk indicators over leverage point to the parallel presence of a liquidity constraints channel under certain qualifications. To investigate this channel deeper, we evaluate the indebtedness levels of households explicitly as propagators of the housing wealth effect by looking at their interactions (Carroll and Kimball, 1996; Disney et al., 2010; Mian et al., 2013). Our evidence suggest that household leverage reinforces the housing wealth effect in a bust, but dampens the housing wealth effect in a boom. We find that all three measures of indebtedness negatively impact on the consumption growth, with the debt-servicing-ratio showing the stronger effect. The interaction coefficient between leverage and housing wealth, as highlighted by Mian et al. (2013), is positive, but not statistically significant in New Zealand data. After separating between positive and negative housing wealth shocks, we find evidence that the interaction coefficient is significant only under negative shocks, while the level coefficient of leverage is significant only under positive housing wealth shocks. This empirical finding seems to suggest that the genuine wealth effect and the liquidity constraints effect work in the different phases of housing cycles. In a housing boom the genuine wealth effect dominates and high leverage weakens the reaction of consumption spending to housing wealth increases. In a housing downturn the liquidity constraints channel dominates and high leverage strengthens the reaction of consumption to housing wealth decreases. In other words, household leverage reinforces the liquidity constraints effect in a bust, but dampens it in a boom.

Our paper is organized as follows. In Section 2, we provide a literature review of key empirical works on housing wealth effects on consumption using micro data during the last thirty years. In Section 3, we present our data sources, provide summary statistics, and discuss the methodology behind the construction of synthetic panels. Section 4 discusses the instrumental variable approach on predicting exogenous estimates for housing wealth and leverage ratios and presents our empirical findings. We conclude in Section 5. The estimation of housing supply elasticities for New Zealand is discussed in Section D of the Appendix and robustness checks of our results are provided in Section E.

2 Housing wealth effects in the empirical literature

Traditional consumption theories (Modigliani and Brumberg, 1954; Friedman, 1957) predict small responses to housing price swings. According to the Life-cycle theory, households adjust spending over their life-span, in response to realized innovations on lifetime wealth. In this context, changes in house prices which inflate or deflate real estate assets, can, in turn, influence household expenditure via a wealth effect. The associated empirical literature focuses on the exploration of the consumption sensitivity to wealth changes and the underlying economic mechanisms behind the relationship.

The channels: wealth effects vs liquidity constraints vs confounding factors

The link between house prices and consumption is a long established empirical finding (Muellbauer and Murphy, 1990, 2008; Aron et al., 2012). Three explanations for this link emerge from the literature. The first explanation is the genuine wealth effect, which follows the life-cycle hypothesis and implies that households respond to innovations on housing wealth changes by adjusting their consumption plans over their lifetime horizon. Households perceiving appreciating home values as permanent changes in their accumulated wealth end up increasing their spending (see, for example, Muellbauer and Murphy (1990); Case et al. (2005); Campbell and Cocco (2007); Carroll et al. (2011) among others.)

A second explanation is the collateral or liquidity constraints channel, which suggests that changes in housing prices improves access to mortgage borrowing allowing households to borrow against their housing collateral (Home Equity Withdrawal), and in turn, use this borrowing to finance their spending (see, for example, Iacoviello (2005); Muellbauer and Murphy (2008); Aron et al. (2012); Cooper and Dynan (2016); Aladangady (2017); Zhu et al. (2019); Andersen et al. (2019), among others). In a similar manner, this channel eases the liquidity-constrained status or the buffer-stock levels of (typically younger) households with precautionary saving motives allowing them to adjust their spending (Gourinchas and Parker, 2001).

The third explanation, also known as the common factor hypothesis, suggests that positive income expectations or perceptions of increased future credit availability, drive both house prices and consumption in the same direction (see, for example, Attanasio et al. (2009); Windsor et al. (2015)). For example, in the UK the financial liberalization process of the 1980s and 1990s, which eased liquidity constraints for most households, has been viewed as a confounding factor behind the co-movement in house prices and consumption (Attanasio and Weber, 1994; Aron et al., 2012).

The corresponding empirical literature tests validity of these explanations focusing on different countries, periods, and identification techniques. The literature can be divided in three large strands: i) Studies using time-series aggregate data to assess the relationship between consumption and wealth in the long-run. ii) Studies using micro-econometric data that empirically assess the presence of wealth effects with a variety of techniques to identify the source of the correlation. This strand emerges with studies focusing on the 1970s US consumption boom and grows substantially with advances in the quality of micro data and which allowed authors to perform more precise model estimations. iii) Studies emerging with the eruption of the GFC which assign a central role to leverage as a key factor in explaining the impact of the housing collapse on household consumption and other key macroeconomic variables. Explorations of asymmetry are few but present in all three strands, while the identification of wealth variations receives growing attention as the literature advances.

Table 7 in Appendix A outlines a non-exhaustive list of the key works in the wealth effects literature using micro-econometric data during the last thirty years. Time-series analyses often fail to identify the underlying mechanisms or capture household heterogeneity effects, as they rely on aggregate data which rarely do allow decomposition between demographic, regional, and economic characteristics. The latter are crucial for identifying the potential transmission mechanism from wealth to consumption and estimating the corresponding MPCs, while the nature of the data makes regressions vulnerable to endogeneity issues.[3] For this reason, the focus of this paper is on studies using micro data.[4]

The first wave of literature on wealth effects attempted to address the consumption boom and savings decline that took place in the US economy during the late '70s and '80s. The bulk of these studies employed the Panel Study of Income Dynamics (PSID), to investigate a potential association between housing wealth and household consumption or saving (Skinner, 1989, 1996; Engelhardt, 1996; Juster et al., 2006). They typically estimated tiny elasticities in the range of 0.03 to 0.06. Owing to data limitations, these studies proxied total consumption with expenditure in food, narrow definitions of non-durable consumption, or imputed measures of active saving using differences in wealth over periods, thus being vulnerable to measurement error. In spite of the identification problems, this early wave of studies triggered the debate over the presence of a wealth effect and its sources and provided intuitions that are highly relevant today.

As improved data became available, the literature expanded to a small number of other countries using different techniques. Campbell and Cocco (2007) constructs synthetic panel data for the U.K. Family Expenditure Survey and show that the elasticity of consumption to house prices depends on age and tenure status. The authors report large wealth effects for older homeowners, and small effects for younger renters. As older home-owners have lower probabilities of moving house, do not face tight liquidity constraints, and are protected against rent price variability, the authors interpret their excess sensitivity to housing price changes as evidence of a genuine wealth effect. By contrast, Disney et al. (2010), using the British Household Panel Survey also for UK households, report negligible wealth effects and no heterogeneous responses across age groups. Attanasio et al. (2009) improving on Attanasio and Weber (1994), use the UK FES dataset for an expanded period, and report highest consumption responses by younger cohorts (0.2) as opposed to older ones (0.04), a result contradicting Campbell and Cocco (2007). The interpretation given to this result is that mostly confounding factors drive consumption changes and less so collateral and liquidity effects, while the possibility of a genuine wealth effect is ruled out by the authors. Similarly, Browning et al. (2013) using administrative data for Danish households report a small housing wealth effect (0.13). Using a dataset that allows them to identify credit constraints better, the authors suggest that young home-owners with liquidity constraints react to financial liberalization reforms which enable home equity extraction, thereby implying that house prices changes impact expenditure through the collateral channel. Windsor et al. (2015) draw similar conclusions for the case of Australia.

Other data sources allowed for more sophisticated dis-aggregation on both variables of interest. For example, Bostic et al. (2009), using the US Survey of Consumer Finances (SCF) for the period 1989-2001,

[3] See, Carroll (2012), for a related discussion on the limitations of time-series data for studying wealth effects. Renowned exceptions include Aron et al. (2012) and Duca and Muellbauer (2014) who perform elaborated breaks between liquid and non-liquid wealth over time and Lettau and Ludvigson (2004) who decompose between permanent and transitory innovations in wealth.

[4] For readers interested in the time series literature, examples include Bhatia (1987); Tan and Voss (2003); Case et al. (2005); Cerutti et al. (2017). Cooper and Dynan (2016) provide an excellent literature review on wealth effects and macroeconomic dynamics using time-series data.

estimated the impact of both real and financial wealth on consumption for both durables and non-durables. Christelis et al. (2015) decompose between both permanent and transitory shocks and estimate consumption adjustments of 0.56 and 0.9 in housing and financial wealth respectively. In a similar vein, Paiella and Pistaferri (2017) decompose between anticipated and unanticipated wealth effects for Italian households and show that the unanticipated wealth effect on consumption is about 3 cents per euro increase in housing wealth. More recently, Zhu et al. (2019) focusing on home equity withdrawal and combining the SCF with regional loan refinancing data, find that the consumption elasticity under the collateral channel is double the size of the genuine wealth effect one.

The housing collapse of the GFC and the role of leverage

The housing market collapse experienced by several advanced economies in the GFC revived the study of wealth effects, by shifting the focus to the role of household leverage in shaping the housing wealth effect on consumption. In line with the collateral and liquidity constraints channel, Mian and Sufi (2011) show that US households increased their debt in response to changes in their home equity over the period 1997–2008 and used it to finance real housing consumption goods or home improvements. The housing collapse of 2008 reduced the collateral value and led leveraged households to reduce overall consumption, implying a strong link between household spending and housing wealth. This literature is theoretically backed by models with imperfect credit markets and precautionary savings motives (Carroll and Kimball, 1996) and, more recently, by heterogeneous agent models that incorporate credit conditions, beliefs, and tenure choices, predicting large consumption responses to house price shocks for liquidity constrained households (Kaplan et al., 2019). The incorporation of leverage in wealth effect models increases the predicted elasticities substantially, as the implicit rental costs, considered in the previous strands of the empirical literature for home-owners implying lower adjustments, are now offset by the rises in debts (Sinai and Souleles, 2005).

Two influential papers in the empirical literature are Dynan (2012) and Mian et al. (2013). Dynan (2012), using the Survey of Consumer Finances, shows that high household debt holds back consumption. Her results suggest that a 10% increase in leverage levels, reduce the consumption growth by 0.3 percentage point per year. In a similar vein, Mian et al. (2013) use county level data to estimate MPC to housing equity shocks. They obtain estimates of MPC in the range of 5 to 7 cents for every dollar change in housing net worth. They also show that consumption responses to negative wealth shocks are stronger in poor and/or highly indebted regions during the Great Recession. Strong debt-overhang effects are also found in Bunn and Rostom (2014) for the UK, Yao et al. (2015) for Norway, Le Blanc and Lydon (2019) for Ireland, and Price et al. (2019) for Australia.

Another key feature of this strand of literature is the use of housing supply elasticities as an exogenous instrument of house price variation, at least in studies focusing in the US and the UK (Mian et al., 2013; Baker, 2018). A reservation raised against this instrument, is that areas with steep slopes and proximity to water have low supply elasticities, but at the same time, those areas attract highly skilled people to work, effectively raising the demand for both expensive housing and consumption goods (Davidoff, 2016). This critique led the literature to consider more sophisticated techniques for instrumenting regional house price growth with housing supply elasticities (see, e.g., Guren et al. (2018)).

Asymmetries

The assessment of potential presence of asymmetry is key in the literature focusing consumption sensitivity to income shocks (Jappelli and Pistaferri, 2010; Christelis et al., 2019), yet it is largely overlooked in the wealth effects literature (exceptions include, Skinner (1996); Engelhardt (1996); Disney et al. (2010); Paiella and Pistaferri (2017); Hviid and Kuchler (2017)). The idea that households experiencing negative wealth shocks may adjust their consumption more strongly than do households experiencing positive shocks is consistent with inter-temporal choice models with precautionary saving motives and borrowing constraints (Altonji and Siow, 1987). In these models household's ability to buffer the consequences of a negative shock is limited and therefore the consumption is adjusted more sharply than under a positive shock (Christelis et al., 2019). On these grounds, Guerrieri and Iacoviello (2017) estimate a bayesian DSGE model on US aggregate data and find evidence of asymmetry.

Returning to the empirical wealth effects literature, Engelhardt (1996) and Skinner (1996) provided evidence of asymmetric wealth shocks for the US as early as in the 1990s. Engelhardt (1996) provided much stronger elasticities of saving offsets to house price falls (0.04 to 0.13) than to house price gains (0.004 to 0.008). Skinner (1996) found a response of 0.1 to housing value falls but no significant response to increases. He attributed this finding to precautionary saving motives, as younger households need to save more as an insurance against future shocks. Similarly, for the UK, Disney et al. (2010) report much stronger elasticities for households in negative equity, which are interpreted as the result of increased buffer stocks for households at this state, as access to credit worsens and the probability of income risk increases. More recently, Paiella and Pistaferri (2017) found significant elasticities only in the case of positive expected returns to housing wealth (0.035), implying the presence of binding liquidity constraints. Hviid and Kuchler (2017) using a large Danish household panel dataset, document asymmetric MPC out of positive and negative house wealth shocks, and Cloyne et al. (2019) focusing on borrowing for the UK using product sales data provide evidence of large adjustments if the collateral constraint is relaxed. Berger et al. (2017) suggest that during boom periods, higher house price growth substantially lowers the effective cost of housing, leading to large increases in housing holdings, leverage, and therefore, consumption responses. Their empirical section provides evidence of time-varying wealth effects for the US, however, no evidence of boom-and-bust asymmetry is found. By contrast, Liebersohn (2017) using housing supply elasticities to control for housing supply across regions, provides evidence of large asymmetries for durable consumption, with estimated elasticities ranging from 0.08 for the boom period to 0.31 for the post-crisis period.

Complementary to this literature review, Table 7 in the appendix describes, separately for each paper: the framework/focus, the key results, how consumption and housing wealth are measured, and the identification procedure used. In the next sections of this paper, we build on this literature by providing new empirical evidence using a novel dataset for New Zealand and a different identification strategy regarding the role of leverage on propagating wealth effects.

3 Data

The main source of our data is the New Zealand Household Economic Survey (HES), which provides cross-section information from 2006 to 2016 with around 8,000 household observations, around 60% of which are home-owners. HES collects comprehensive data on household residents living in permanent dwellings, which covers multiple aspects of household financial characteristics including highly disaggregate expenditures, income, loans, and reported house value for each individual in the household. The survey also covers detailed demographics, such as home ownership status, age, and household structure. The data are stratified by different population benchmarks including age, sex, population per region, two adults and non-two adult households, and people of Māori ethnicity. This guarantees proper weighting of households and a high degree of comparability across time since the data are cross-sectional rather than longitudinal. Data are collected along one-year waves extending from July to June. In the empirical section of this paper, we directly draw certain variables from HES, including household disposable income, total debt, primarily mortgage loans, as they are originally reported in the HES sample. Other variables of interest are constructed directly from HES variables, including household expenditure, housing wealth, and leverage measures.

In line with most of the wealth effects literature, our study mainly focuses on non-housing expenditures.[5] The excluded housing expenditures are expenses on house maintenance, improvements, and mortgage repayment. Our choice of non-housing consumption is justified as follows: (i) The long-lived nature of housing goods provides the household with a flow of utility for multiple periods, which is hard to translate into consumption services for the time period associated to the wealth effects. Accordingly, we confine our analysis to short-lived non-housing expenditure which mirrors the time points of our sample more adequately. (ii) Housing related durable goods including maintenance and improvements, may increase the value of the property, leading to an endogenous relationship between the wealth effect and durables expenditure. This is because the household's capacity to finance its consumption with borrowing may depend on the value of housing goods themselves. Thus, a justification of the focus on non-housing consumption is to avoid potential endogeneity issues between household debt and expenditures in the case of mortgagors, as housing durable goods tend to be largely debt financed. (iii) Excluding housing goods helps avoiding potential reverse causality issues between housing expenditures and house prices.

The HES expenditure data allow for disaggregation between housing and non-housing components in a triennial basis. We therefore focus on the four waves that report detailed expenditure data only. These waves are 2006-2007, 2009-2010, 2012-2013, and 2015-2016, which cover almost 8,000 observations; about 2,000 households per wave. We also estimate the sensitivity of our results by decomposing non-housing expenditures into durables, and non-durables. A detailed list of HES expenditures we allocated in these two categories is presented in Tables 8 and 9 of Appendix B.

[5] Few exceptions include Bostic et al. (2009); Attanasio et al. (2009); Zhu et al. (2019) who extend the analysis to durable or total consumption.

For housing wealth, HES reports the rateable value of the primary dwelling of the household and the year it was valued.[6] The rateable value of the dwelling is estimated by city councils periodically for levying property tax. The rateable value of a property can be outdated, if the valuation date of the council takes place prior to the survey date so we use regional house price index to up-rate or down-rate the rateable value to the year when the HES interview takes place.[7]

To construct measures of household leverage, we use inflation-adjusted house prices, disposable income, and total debt. In particular, the Debt-to-Income ratio (DTI) is constructed by using total household debt divided by disposable income. Loan-to-Value (LTV) is the ratio of total household debt over the inflation-adjusted house value at the time of survey. Both measures are based on outstanding debt to capture the actual level of leverage when the household was interviewed. Lastly, the Debt-Service Ratio (DSR) is the ratio of debt payments over disposable income.

Our sample selection strategy aims to capture the household finance behavior of home-owners and mortgagors with limited regional mobility, where housing is seen primarily as a consumer good rather than as an investment asset within the household's balance sheet. Consequently, we drop from the sample: i) those born after 1985 to avoid observations with frequent relocation due to transitions to tertiary education and unstable job settings, ii) those born earlier than 1945 due to typical changes in consumption patterns as individuals reach late retirement, and, iii) those who report more than one dwelling to avoid capturing investment behaviour. As pointed by Bostic et al. (2009), asset values in other real estate properties are more volatile than owner-occupied housing, and may impact differently on consumer spending.[8] Lastly, we drop some outliers, including extreme values over the top 1% of the distribution for housing property, disposable income, and all three indebtedness indicators.

3.1 Summary statistics

Although our empirical analysis focuses on synthetic panels constructed from the cross-sectional survey data, we initially present some summary statistics of the HES dataset itself. This section provides a picture of the basic characteristics of New Zealand households.

Table 1 reports the mean or median of the main variables in our dataset. We first present the statistics for the full sample, and then break them down for each wave. As economic variables, such as income, assets and debts, are typically skewed to the right, we report the median instead of mean for those variables. We also deflate the nominal values into real values using New Zealand CPI of corresponding years. Since our empirical analysis focuses on the effects of changes in the value of housing wealth, we restrict the sample to household owning property, and so we report the statistics of homeowners, either those having a mortgage or outright owners. The age and the size of households are the means across home-owners and they are stable across waves. Similarly, the shares of home-owners, mortgagors, and renters, do not vary significantly across the four waves of the survey. Over time, real consumption expenditure remains stable, while disposable income and housing wealth increase substantially. Income growth has lagged behind the increases in debt over time as the upward trending debt-to-income ratio (DTI) suggests. The loan-to-value ratio (LTV) is low compared to the LTV at the time of origination but this is to be expected as our LTV is calculated at the time of survey.[9]

Table 2 presents the descriptive statistics for households in different age cohorts. From this table, we get a glimpse of economic conditions of New Zealand households over their life-cycle. All life-cycle patterns of

[6] An alternative subjective measure of housing prices would be the reported prices by home-owners, as those prices would potentially reveal information on the perceptions of home-owners about their housing wealth and might point to different chosen levels of consumption out of it. As we show in Section 4.1, however, both measures are vulnerable to endogeneities. Consequently, we mainly use a measure of housing wealth based on exogenous house price elasticities and use the reported value within a robustness check.

[7] The address of each household in the survey is reported at different levels of aggregation. The geographic aggregation of New Zealand is divided into 47062 mesh-blocks (MB), 2020 area units (AU), 65 territorial authorities (TA). To construct inflation-adjusted house prices, we use TA level of house price inflation reported by Real Estate Institute of New Zealand (REINZ). For some regressions we use TA dummies to control for regional fixed effects. For more information about geographic boundaries, visit the Statistics New Zealand web-page in `https://www.stats.govt.nz/regional-data-and-maps/` For the dwellings rated in years prior to the survey date, we use REINZ data on house price inflation to ensure all house values are up to date. House prices are uprated at TA level.

[8] This is done in two stages. First, we exclude any household which owns a house and receives rental income on another property. Second, we exclude houses with debt to house value ratio of higher than 0.8. Our empirical findings are not sensitive to this threshold.

[9] For context, the variances of DTI, LTV and DSR are 0.032, 0.007, 0.002, respectively.

Table 1 Descriptive statistics for home-owners over time

	All waves	2006/07	2009/10	2012/13	2015/16
Age of household head (mean)	56.3	56.2	57.0	58.0	57.7
Household size (mean)	2.3	2.4	2.3	2.3	2.4
Real non-housing expenditure (median)	44.0	44.4	45.5	43.5	42.9
Real disposable income (median)	72.1	69.2	72.4	73.2	75.6
Real housing wealth (median)	408.5	392.7	374.4	390.1	469.4
Real debt (median)	162.7	133.5	165.5	182.1	184.1
Debt-to-Income (mean)	1.9	1.8	2.0	2.0	2.1
Loan-to-Value (mean)	0.32	0.29	0.33	0.34	.33
Debt-Servicing-Ratio (mean)	0.12	0.13	0.12	0.11	.11
Sample size	4,640	1,200	1,180	1,190	1,070

All nominal values are deflated by the New Zealand CPI of corresponding years. Homeowners include mortgagors and outright owners. Throughout the study, the number of reported observations is rounded up or down randomly to a multiple of three in compliance with Statistics New Zealand rules. Owners with multiple properties are dropped from the analysis.

Table 2 Descriptive statistics by age groups

	Young (20-40)	Prime (40-60)	Old (60-80)
Household size	3.14	2.68	1.62
Non-housing expenditure	46,000	50,600	31,500
Disposable income	79,600	80,900	44,900
Housing wealth	460,400	493,100	441,900
Total debt	174,100	129,000	51,000
Debt-to-Income	2.46	1.82	1.3
Loan-to-Value	0.43	0.29	0.16
Debt-Servicing-Ratio	0.19	0.08	0.02
Sample size	610	1,702	2,183

All nominal values are rounded in New Zealand dollars.

key variables are consistent with international evidence. Expenditure goes along side with income, increasing from young age to prime age and decreasing thereafter. House wealth takes time to accumulate and it declines after 60s, because older households choose to downsize their homes. In contrast, total debt is highest in the early stage of household life, as young households choose to borrow against their high expected income growth to smooth consumption. Debt decreases substantially in the late stage of life-cycle, both due to limited access to credit and the declined willingness to borrow. As a result, measures of household leverage tend to decrease by age.[10]

From this table, we also observe that the age distribution is skewed towards older households. There are more than three times as many households with a head older than 60 year old than households with a head younger than 40. In terms of how age cohorts affect our variables of interest, older households typically have lower income, consume less, and have lower-value housing compared to households in young and prime cohorts.

3.2 Constructing synthetic panels

Identifying which shocks in housing wealth lead households to adjust their spending requires a panel setting which would allow us to gauge the relative importance of the shock across different demographic groups and households with different levels of indebtedness. Ideally, the dataset would include information of changes in household spending of the same household across time, which can be linked to *ex-post* clearly identifiable, but *ex-ante* shocks, in their housing wealth (Carroll et al., 2014). Unfortunately, such a dataset does not exist for the case of New Zealand; the HES data, although rich in information on household finances and consumption, is a repeated cross-section. Using this dataset as it stands, it is impossible to track the same household across time and capture the unobserved individual characteristics by using the standard fixed effects estimator.

To tackle this caveat, an extended literature has emerged suggesting that by grouping individuals or households with similar characteristics in cohorts, and then treating the averages within these cohorts as observations in a synthetic panel, one can achieve analytical outcomes that approximate those of an actual panel data set (see, for example, Deaton (1985); Nijman and Verbeek (1992); Browning et al. (1985); Blundell et al. (1989); Campbell and Cocco (2007); Attanasio et al. (2009); Bunn and Rostom (2014); Demyanyk et al. (2017)). Windsor et al. (2015) perform a similar analysis to this paper using both an actual panel and a synthetic one. They suggest that the evidence coming from the two models is very similar, therefore, synthetic versions can be used as partial substitutes for actual panels. Following this literature, we construct a series of synthetic panels for the New Zealand and use these to estimate the effect of changes in housing wealth on consumption across different groups of households. In particular, the following model can be defined by aggregating all observations to a cohort level :

$$\tilde{y}_{ct} = \tilde{\alpha}_{c,t} + \tilde{x}'_{c,t}\beta + \tilde{u}_{c,t}, \quad c = 1,...,C; \quad t = 1,...,T \tag{1}$$

where the dependent variable $\tilde{y}_{ct}$ (in our case, non-housing consumption expenditure) stands for the mean value across all individual y_{ct}'s in the cohort c in period t, the vector of independent variables $\tilde{x}'_{ct}$ stands for the mean value of all individual x_{ct}'s in the cohort c in period t, and so on. The resulting dataset is a synthetic panel series with repeated observations over T periods, C cohorts, and each cohort has a size of n_c observations.

When constructing synthetic cohorts we should be cautious on two strict analytical requirements (Verbeek, 2008). First, cohorts should be defined on the basis of variables that do not vary significantly over time and are observable across all individuals in the sample. Second, the cohort's construction should lead to a final model with considerable variation over time in its key variables. Identification of the model's parameters requires that there is sufficient variation over time. Otherwise the only source of variation would be the one of cohort variables themselves. The above choices when constructing the cohorts leads to a trade off between the size of the two dimensions, namely the number of cohorts C, and the number of observations per cohort n_c.

With the above considerations in mind, we construct a synthetic panel for New Zealand by defining cohorts on the basis of two-year of birth and region for each panel wave corresponding to the period from

[10] It should be noted that a caveat of cross-section analyses, including synthetic panels constructed from repeated cross-sections, is that it is not possible to disentangle age from cohort effects. Accordingly, we stress that our discussion on life-cycle behaviour rests on the assumption of no cohort effects. The restrictive assumptions on constructing the cohorts are discussed the next section.

2006 to 2016. The choice of birth cohorts is standard in the relevant literature since it is not choice is usually fully reported in the sample and does not vary over time. More importantly, the year of birth is not a choice variable and, as such, does not create endogeneity issues by correlating to other key variables examined (Attanasio et al., 2009). As opposed to single birth year cohorts, two-year ones restrict slightly the cohort size, hence respecting the first methodological requirement of birth cohorts. In order to avoid the inclusion of households with likely changes in family composition and education, as well as those with transitions into retirement, we drop those born earlier than 1945 and those born after 1985 from our sample selection and the construction of cohorts.[11]

Although several works with synthetic panels use home-ownership as a common characteristic on defining cohorts, we avoided this because the decision to become a home-owner can be endogenous on the level of income and house price dynamics (see, for example Campbell and Cocco, 2007). Instead, we selected residential location as a second defining characteristic of cohorts because it is a largely stable variable through time and allows us to capture the variation in home prices across the country. Indeed, the selection of region may be endogenous in certain applications (Verbeek, 2008). For instance, the decision to re-locate may be correlated to the level of housing prices and hence housing wealth, especially for young mobile workers (Berger and Blomquist, 1992). As clarified in Section 3, our sample selection focuses on homeowners, either outright or mortgagors, with low re-location probabilities. It is, therefore, safe to assume that the residential location variable can be treated as exogenous in defining regional cohorts. To check this assumption, we linked our HES data with the household address tables provided by the Statistics New Zealand's "Integrated Data Infrastructure" database, which records all address changes of HES households. We checked the residential mobility rate across seven regions used in our panel construction for HES homeowners. On average, the mobility rates of New Zealand homeowners is around 5.8% for three years after the HES interview date.[12] We also break this down into the age cohorts of the two-year gap. The general pattern of mobility rates over the life cycle is downward sloping. The moving probability is about $8-10\%$ when the head of the household is between 28 - 32. It drops to around $4-5\%$ when the head of household is getting older. Based on this evidence, we conclude that residential mobility across regions does not significantly undermine the construction of our cohort-region panel.

In line with the construction of the housing supply elasticities, described at a latter section of this paper, we apply a regional division of the country into 16 regions corresponding to 11 regional councils and 5 unitary authorities.[13] As will be revealed in the following section, there is substantial variation in housing wealth both across regions and across time.

Having identified the cohorts and regions, we are able to construct a synthetic panel series that tracks home-owners with similar characteristics in New Zealand for the following periods: 2006-07, 2009 -10, 2012-13, and 2015-16. Table 10 in Appendix C reports the size of cells in our synthetic panels. The mean cohort size (n_c) approximates over 50 observations per cohort, while the number of cohorts (C) amount to 950 observations which are used in our econometric analysis. Although a further break down into single birth year cohorts would yield a greater number of observations in the final sample used in the econometric analysis, fairly large cohort sizes are required to validly ignore the cohort nature of the data (Verbeek, 2008). It is also important to note that each cohort is weighted by constructing sampling weights that match those of the actual population corresponding to each birth year and region.

Table 11 in Appendix C reports the variation in the key variables of the cohort mean data used in the empirical analysis, including non-housing consumption, disposable income, housing value, and the leverage measures. These descriptives reveal considerable variation in most of the variables, both inter-regionally and across generations. In particular, younger households indicate the highest changes in non-housing consumption and leverage followed by middled-aged and older ones. By contrast, changes in disposable income are more prominent in the middle aged-cohort.

[11] Our results are robust to their inclusion.

[12] The literature reports a strong negative association between home-ownership and residential mobility (Andrews and Sánchez, 2011; Inchauste et al., 2018). A typical conjecture is that mobility is lower among home-owners than renters because the former face higher transaction costs of changing dwellings. Consequently they spend more time in their own residence to spread the costs over a longer time period.

[13] In particular, the regions comprise of the following geographical territories: Auckland, Northland, Waikato, Bay of Plenty, Gisborne, Hawke's Bay, Trananki, Manawatu-Wanganui, Wellington, Tasman District, Nelson City, Marlborough District, West Coast, Canterbury, Otago, and Southland.

4 Empirical Analysis

4.1 Instrumenting house prices and leverage

Prior to assessing the role of price and leverage changes in driving non-housing consumption we are concerned about potential endogeneity between the key variables in the specification. Endogeneity may arise due to reverse causation, e.g., high consumption leading to high leverage, but also due to the presence of confounding variables. For example, optimism about future house price trends, may be driving the changes in both consumption and leverage. For instance, De Stefani (2017) reports that when households expect the value of their properties to rise, they borrow against the anticipated increase in home equity.

We address potential endogeneity by using housing supply elasticities (HSE) as an instrument for both housing prices and leverage, one at a time. HSE reflect the level of geographical constraints for potential housing development in a given region, therefore in regions where building is relatively restricted and supply is short, growth in housing prices and leverage is expected to be higher. Additionally, in regions with inelastic supply of housing, dwellings gained value and home-equity-withdrawal lead to higher levels household leverage. The fact that HSE functions as a driver of both housing prices and leverage can be explained by the period we explore, comprising of the housing boom and bust cycle in the country, where geographically constrained regions indicated larger increases in both variables.[14]

Several influential studies including Glaeser et al. (2008), Mian, Rao and Sufi (2013), Aladangady (2017), Baker (2018), and Guren et al. (2018), employ similar identification strategies for the US and illustrate that housing supply elasticities (HSE) can serve as powerful predictors of both house price and leverage changes. Following this literature, we instrument housing prices and leverage using a newly estimated HSE index for New Zealand. To construct HSE for New Zealand, we adopt the method of Saiz (2010) that allows us constructing an index of the share of land unavailable for residential development.[15] The measure is calculated at the level of cities as the fraction of land within a 50km radius that is available for use due to being either ocean, wetlands, or too steep. Our analysis focuses on the 17 main urban areas of New Zealand as identified by Statistics New Zealand. These urban areas vary in population from 1,534,700 (Auckland) to 31,300 (Blenheim).[16] A detailed description of the index, the construction of which is a contribution of this paper, can be found in Appendix D. As shown in Figure 2, HSEs for New Zealand's urban centers correlate with average house prices for the period 1992-2016.

A potential criticism of HSE as an instrument of house prices is that the amount of developable land may entail further economic impacts than prices and leverage. For instance, it can favor the development of land-intensive activities, or affect commuting patterns and time, and thus have a direct effect on consumption. While such effects may challenge the validity of our instrument, we expect that their effect, over and above the HSEs' effect on house prices and leverage, should be limited and observed only in slow-moving socio-demographic variables, e.g., commuting patterns. By contrast, the exogenous effect of HSE on prices and leverage should be instantaneous, within the discrete time points of our analysis. Additionally, HSE has been shown to be largely exogenous to key confounding variables such as wage growth and expansion in housing-related sectors like construction (Mian and Sufi, 2014). Another criticism of using HSEs as an instrument of house price growth is that they may be correlated with demand and productivity factors (Davidoff, 2016). To address this criticism for the case of New Zealand, we include demand factors in our IV regression and assess the robustness of HSE coefficients on their inclusion to support our empirical strategy.

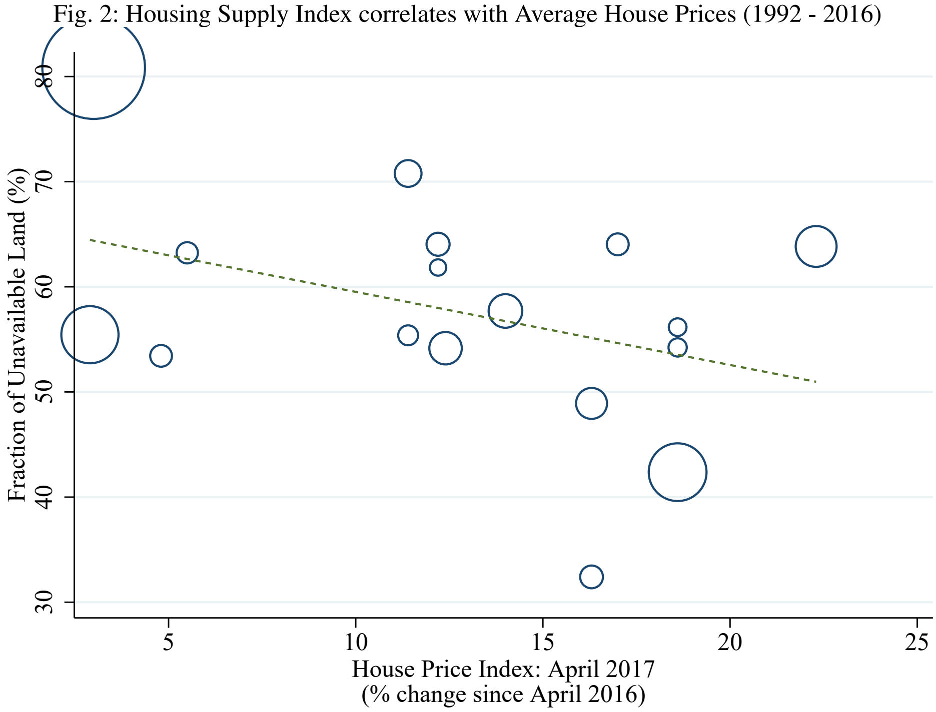
Table 3 shows the results of the first stage 2SLS specification. We regress house prices and measures of leverage on HSE, while using as control variables the number of rooms for predicting house values, and age and income quantiles for predicting leverage. Time and regional fixed effects are included in all specifications. Column (1) can be seen as a hedonic regression and reports the fitted values for the second stage of our IV. The HSE coefficient is strong and statistically significant, comparable to other studies employing it as an instrument. To respond to Davidoff's critique, we include demand shifters, similar to the ones suggested in his own paper, to account for possible correlation with demand and productivity. In (2) and (3), after including net migration flows and the share of tertiary education graduates per region, to proxy

[14] See also Baker (2018) who employs a similar identification strategy and discusses the issue more in detail with respect to the US.

[15] The main alternative approach in the literature for estimating HSE is based on using an index of local regulations, and no such index is presently available for New Zealand. Estimates by Lees (2019) provide indirect evidence that land-use regulation does matter.

[16] "These numbers are based on the "Subnational Population Estimates: At 30 June 2017 (provisional)" by Statistics New Zealand.

Fig. 2: Housing Supply Index correlates with Average House Prices (1992 - 2016)



Circle size is proportional to population of city. Our house price data are provided by Real Estate Institute of New Zealand, which covers about 95% of all house transactions in New Zealand from 1992 to 2016. To avoid the compositional bias across different housing markets, we calculate average house prices based only on 3-bedroom residential houses, which are the most common type of house transactions in New Zealand.

regional demand and productivity respectively, we observe that the HSE coefficients remain substantially robust. The migration effect confirms (Mian and Sufi, 2014) who suggest that HSEs are exogenous to internal migration and, therefore, demand. In columns (3) - (6), we consider three measures of leverage, namely, the LTV, the DTI, and the DSR. In a similar vein, HSE has a significant positive effect on leverage, though the estimated coefficients differ per leverage measure. Those results echo Baker (2018) who performed similar IV estimations.

We use this first stage regression to obtain the fitted values which represent the exogenous component of the variation in house prices.

4.2 Panel Analysis

After constructing a synthetic panel and being armed with exogenous regional housing price series, we use the fitted value of changes in house price and leverage as the exogenous source of variations to study how they affect non-housing consumption. As standard in the literature, we set up the panel regression on first-difference terms, so that time-invariant unobservable characteristics of households are offset. The baseline equation is the following:

$$\Delta C_{i,t} = \alpha + \beta_w \Delta W_{i,t} + \beta_Y \Delta Y_{i,t} + \gamma \Delta X_i + u_{i,t} \quad (2)$$

where $\Delta C_{i,t}$ is the average change in non-housing consumption of households in cohort-region combination i from $t-1$ to t. $\Delta W_{i,t}$ stands for the average change in housing wealth (reported or instrumented) of the same cohort-region combination from $t-1$ to t. $\Delta Y_{i,t}$ stands for changes in household income, while ΔX_i includes time-varying cohort characteristics, including average household size, time effects, and age.

The key coefficient of interest is β_w, which is interpreted as the elasticity of consumption (or MPC) out of housing wealth, depending on whether the natural logarithm of the variable or its level is used. As we discussed above, we use the instrumental variable (IV) approach to address the potential endogeneity issue. Housing wealth $W_{i,t}$ used in this regression is instrumented by the HSE for New Zealand major urban centers, unless specified otherwise.

Table 3 Two-Stage Least Squares: Housing Supply Elasticity as an Instrumental Variable for house prices and leverage

	(1) House Value (*Ln*)	(2) House Value (*Ln*)	(3) House Value (*Ln*)	(4) LTV	(5) DTI	(6) DSR
Housing Supply Elasticity	-1.10***	-1.07***	-1.04***	-0.07***	-1.05***	0.094***
	(0.13)	(0.40)	(0.39)	(0.04)	(0.23)	(0.02)
Number of Rooms	0.13***	0.13***	0.13***			
	(0.01)	(0.01)	(0.01)			
Net Migration (*Ln*)		0.00	0.01			
		(0.04)	(0.01)			
Tertiary Education			0.09***			
			(0.02)			
Age				-0.02***	-0.04***	-0.03***
				(0.004)	(0.02)	(0.002)
Income Quantiles				0.013***	-0.05***	-0.006***
				(0.002)	(0.01)	(0.001)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Regional fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,088	4,996	4,996	4,996	4,996	4,996
Adj-R^2	0.48	0.48	0.49	0.21	0.19	0.20

Columns (1) - (6) refer to the dependent variable in each specification while rows refer to the various instruments employed. The first three columns instrument housing prices while the latter three instrument different leverage proxies. Although, Housing Supply Elasticities (HSE) are used as instruments in all specifications, the rest of the instruments differ along house price and leverage specifications. All regressions include time and regional fixed-effects for each survey wave and TA respectively. The standard errors are reported in parentheses. The number of observations is rounded up or down randomly to a multiple of three in compliance with Statistics New Zealand rules.

4.2.1 Average marginal propensity to consume out of housing wealth

The first four columns in Table 4 show the estimate for the elasticity from which we calculate the average marginal propensity to consume out of housing wealth. In column (1) we use reported housing wealth from the HES data as described in Section 3. The estimated elasticity coefficient is 0.23 and is statistically significant. As we discussed earlier, the regression based on reported housing value might suffer from multiple endogeneity problems, which might lead to bias in different directions.[17] For these reasons, in column (2), we report the estimate based on the IV approach, where the housing wealth is instrumented by housing supply elasticity *a la* Saiz. The size of the estimate is higher than in the simple OLS regression. To interpret the estimated elasticity as MPC out of housing wealth, we use the ratio of median consumption to median housing value in our sample. We find that, on average, the MPC in dollar terms is 3 cents out of one dollar change in housing wealth. In Column 3, we run the same IV regression with additional control variables, time, and regional dummies. Our estimates remain robust.

To check if our benchmark finding is influenced by outliers, as often happens in skewed distributions such as housing assets and debts, we run further robustness test. Specifically, in column (4), we apply a robust linear specification with winsorized values and a quantile regression to the median respectively.[18] Robust linear regression (Huber, 1964) is frequently employed in the literature to control for the presence of outliers and heteroscedasticity. The process involves an iterative re-weighted least squares algorithm that minimizes the standardized residuals, multiplied by a loss function associated with the Cook distance. The robust linear regression is shown efficient when outliers are isolated from the rest of the population. In the

[17] As discussed in the previous section of this paper, higher income expectations may drive both consumption and housing value of a household. On the other hand, unobservable factors negatively correlated with housing wealth, or measurement errors in house prices can lead to attenuation bias.

[18] A similar approach using regression to the median for treating outliers in the related literature is followed by Engelhardt (1996). In contrast to the results of the present paper, the results for the mean and the median regression in Engelhardt (1996) differ substantially, implying a higher influence of outliers in the sample. For this reason, the author reports elasticities for both the mean and the median regression.

presence of clusters of outliers the method does not ensure identification of all leverage points (Rousseeuw and Van Zomeren, 1990). To reduce the influence of outliers clusters, we drop observations where the absolute standardized residuals take values above 2, corresponding to 5% of observations. Our findings yield a significant positive estimate very close to the one of the instrumented housing price variable, hence confirming the robustness of our initial hypothesis.

Our baseline elasticity estimate for wealth effects ranges at 0.3 (an average MPC of 3 cents per dollar) and is statistically significant in all specifications performed. The estimate is similar or slightly larger than studies exploring wealth effects in the pre-crisis period in other countries than the US (Attanasio et al., 2009; Disney et al., 2010; Gan, 2010; Browning et al., 2013; Windsor et al., 2015; Christelis et al., 2015). Yet, it is substantially smaller than studies focusing on the GFC period in US, when the housing bust took place. For example, Mian et al. (2013) using county level data to estimate MPC out of housing equity, provide estimates of 5 to 7 cents for every dollar change in housing net worth and Aladangady (2017), using improved housing supply elasticity measures, finds an MPC from housing wealth at 4.7 cents for every dollar rise in house prices. In the next columns, we perform several further tests that help us shed light to the economic significance of our results.

Table 4 The elasticity of consumption to housing wealth

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Survey Reported	IV (HSE)	IV (HSE)	Robust Regression	Young Owner	Negative Change	Positive Change	Non-durables Expenditure	Durables Expenditure
Δhouse value (ln)	0.23***	0.29***	0.30***	0.29***	0.28***	0.37**	0.21**	0.20***	0.44**
	(0.03)	(0.04)	(0.06)	(0.05)	(0.05)	(0.17)	(0.9)	(0.04)	(0.17)
Δ Disposable income (ln)	0.48***	0.50***	0.50***	0.49***	0.47***	0.52***	0.50***	0.41***	0.82***
	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.06)	(0.04)	(0.04)	(0.14)
Δ Household Size			0.03	0.04**	0.05**	0.07*	0.01	0.03*	-0.07
			(0.02)	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)	(0.07)
Δhouse value* Young Owner					-0.13				
					(0.08)				
Δ Age			0.002	0.001		0.002	0.001	0.001	-0.001
			(0.00)	(0.00)		(0.00)	(0.00)	(0.00)	(0.00)
Constant	0.00	-0.02	-0.17	0.09*	-0.16***	-0.33	-0.06	-0.11	0.21
	(0.01)	(0.01)	(0.13)	(0.12)	(0.04)	(0.26)	(0.13)	(0.10)	(0.46)
Time Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.375	0.367	0.370	0.41	0.372	0.401	0.298	0.344	0.117
Observations	804	804	804	768	740	212	590	733	733

The variable representing housing wealth varies in each column. Column (1) refers to the logged housing value as reported in the HES survey. Column (2) uses the logged housing value as instrumented with housing supply elasticities. Column (3) uses the instrumented variable again, but adding more control variables and time and regional dummies. Column (4) is a robustness check using a Robust Linear regression with winsorized values. Column (5) investigates heterogeneity in responses young and old home-owners by interacting house price value with a dummy for younger households born after 1965. Columns (6) and (7) decompose positive and negative house price swings. Columns (8) and (9) decompose between expenditure on non-durables and durables respectively. The number of observations is rounded up or down randomly to a multiple of three in compliance with Statistics New Zealand rules. Standard errors are presented in parentheses.

Column (5) investigates potential heterogeneity in the responses of young and older home-owners by interacting house price value with a dummy for households born after 1965.[19] This test is key in the literature identifying the source of wealth effects (Campbell and Cocco, 2007; Attanasio et al., 2009; Browning et al., 2013; Christelis et al., 2015). Older home-owners typically indicate a flatter income profile and can be considered "long" in housing as they are most likely to own their dwelling, are less likely to trade up, and have a shorter horizon in which they can utilize their accrued wealth gain. Accordingly, evidence of stronger responses for older home-owners points to a genuine wealth effect. By contrast, if the association between housing wealth and consumption is driven by confounding factors, such as productivity growth and income expectations, younger house owners should respond more to housing wealth gains since they face a longer time horizon to they have more years left to utilize their increased earnings due to higher productivity. Alternatively, a strong response for younger home-owners can signal increased (decreased) collateral capacity and tighter (eased) liquidity constraints, which become more (less) binding if house prices fall (rise) (Sinai and Souleles, 2005).

Relative to our reference group, which is older home-owners, we find a strong negative consumption response to house price shocks among younger home-homeowners. The negative sign of the coefficient for young home-owners is *prima facie* evidence of a genuine wealth effect, driven by older home-owners consumption elasticities. This finding is consistent with Campbell and Cocco (2007) and Paiella and Pistaferri (2017) for the UK and the Italian case respectively, but contradicts most of the literature pointing only to collateral/liquidity effects (Disney et al., 2010; Browning et al., 2013)) or common factors explaining the association between wealth and consumption (Attanasio and Weber, 1994; Attanasio et al., 2009). While the common factor explanation, can be possibly ruled out,[20] we cannot exclude the liquidity constraints channel as our coefficient is not statistically significant. In other words, we cannot reject the hypothesis that young home-owners with smaller collateral capacity and tighter liquidity constraints may adjust their consumption in response to house price swings, though to a lesser extent than older home-owners. We thus proceed with further tests on the underlying channel behind the association between housing wealth and consumption.

Altonji and Siow (1987) proposed an indirect way to assess the presence of liquidity constraints in income effects, based on theoretical evidence implying that under imperfect markets, liquidity constraints generate asymmetric consumer behavior; it extends naturally to wealth effects.[21] In the absence of liquidity constraints, a household optimally smoothing consumption and anticipating wealth rises, would be eager to borrow, while a household anticipating wealth falls, would be eager to save. Hence, a wealth effect should be present only in response to expected positive wealth changes, where borrowing is used to adjust the long-run consumption schedule. In the presence of binding credit constraints and unanticipated positive wealth changes, household consumption may be insensitive, as no change in the consumption smoothing process is imposed. By contrast, in the presence of liquidity constraints during an (unanticipated) housing downturn, the household is forced to cut back consumption with respect to the adjusted wealth schedule. Following this reasoning, stronger responses to negative wealth changes may signal the presence of liquidity constraints and unexpected realizations of house price falls, as those experienced during the housing market bust of the GFC. In columns (6) and (7), we test the estimated wealth effect of our baseline estimation with respect to positive and negative housing wealth shocks. We find that under negative price changes the consumption elasticity to housing wealth is substantially larger, than under positive changes. The housing wealth elasticity is 0.37 for negative swings and 0.21 in response to positive ones. In line with the theoretical channels described above, this result provides support to a liquidity constraints explanation, over and above the genuine wealth effects evidence presented above. Our results are consistent with Engelhardt (1996); Paiella and Pistaferri (2017) and Hviid and Kuchler (2017) who also document asymmetric MPC out of positive and negative wealth shocks and stronger coefficients under negative shocks.

Lastly, as an additional check, we decomposed between consumption on durables and non-durables. We then re-run the baseline estimation for each consumption category. This decomposition may help to shed light on the consumer attitude in response to wealth changes. According to Bostic et al. (2009), greater elasticities for durable than non-durable goods imply that the sensitivity in spending is partially based on unanticipated wealth changes. Instead, if durables are perceived as long term purchases by households accruing utility over

[19] A further advantage of using micro data at a synthetic panel form is that they allow us to break the sample between age groups. The use of birth-cohorts in defining our synthetic panel allows us to do this, since birth-cohorts, not being a choice variable, remain exogenous, and thus can be used as a regressor.

[20] Considering also the use of HSE as an instrument which is tested against confounding factors such as income expectations and productivity.

[21] Berger et al. (2017) incorporated this test in a heterogeneous agents model, while Paiella and Pistaferri (2017) used it to assess the presence of liquidity constraints in Italian data.

multiple periods, they may be less responsive to short-run wealth changes. Columns (8) and (9) present our results for non-durables and durables respectively. They suggest that durable consumption is more sensitive to changes in housing wealth, as the estimated coefficient for the specification on durables is more than double the size of that on non-durables. This finding points to unanticipated wealth effects which fits the liquidity constraints story described earlier, further to a genuine wealth effect. Yet, this result should be read with a degree of caution due to the problems arising from measuring wealth effects with long-term housing related goods, highlighted in Section 3 of this paper.

Considering the theoretical arguments on the important role played by borrowing in accounting for the asymmetric MPC out of housing wealth, in the next section, we explore whether household indebtedness could be a reason for having asymmetric consumption responses to negative versus positive housing wealth shocks.

4.2.2 How does household indebtedness affect the MPC out of housing wealth?

Up to this point, our analysis focused on the relationship between wealth and consumption, pointing to the presence of a genuine wealth effect and the likely presence of a liquidity constraints channel in New Zealand. However, the central role of leverage during the GFC, which took place within our sample period, makes a strong case for investigating its role independently. As far as household indebtedness amplifies liquidity constraints and future income risks due to repayment obligations, the debt component in household's net wealth, namely, the debt-overhang effect, may be crucial for the sensitivity of consumption to wealth shocks (Dynan, 2012; Price et al., 2019). In this section, we assess whether household leverage affects the MPC out of housing wealth. We allow continuous measures of household leverage to either directly affect consumption or to interact with housing wealth. Accordingly, we set up the baseline regression equations as follows:

$$\Delta C_{i,t} = \alpha + \beta_w \Delta W_{i,t} + \beta_Y \Delta Y_{i,t} + \beta_{lev} \Delta Leverage_{i,t} + \gamma X_i + u_{i,t} \tag{3}$$

$$\Delta C_{i,t} = \alpha + \beta_w \Delta W_{i,t} + \beta_Y \Delta Y_{i,t} + \beta_{lev} \Delta Leverage_{i,t} + \beta_{lev} Leverage_{i,t} \Delta W_{i,t} + \gamma \Delta X_i + u_{i,t} \tag{4}$$

where $Leverage_{i,t}$ stands for the average level of leverage measures from $t-1$ to t in cohort-region combination i.

We account for household leverage or indebtedness using three variant indicators. Each one of them reflects different mechanisms on how indebtedness can affect consumption behaviour (Kukk, 2016; Fasianos et al., 2017). First, we use the Loan-to-Value ratio (LTV) to explicitly measure household's leverage, one of the most commonly used indicators for guiding macro-prudential policy. It can be seen as a solvency risk indicator as it tracks households' ability to pay back their mortgages, given that their property can be sold at prevailing price if the household experiences repayment difficulties. Second, we use the Debt-to-Income (DTI) and the Debt-Servicing Ratio (DSR) as additional measures of the household indebtedness, which can be seen as liquidity risk indicators. DTI is key in determining the vulnerability of households to changes in their capacity to reimburse mortgage debt in cases of income shocks. As households may reduce their consumption due to high indebtedness, using this indicator we can capture the debt overhang effect. DSR hints on how interest rate and income shocks may affect households' repayment capacities. This mechanism functions through the households' financial distress position. Abrupt changes in both these indicators reveal the extend that households slip into higher, potentially unsustainable leverage territories. All three indebtedness measures are first regressed on the instrumental variables as shown in subsection (4.1) and then the fitted values are used in the variant regressions assessing the debt-overhang effect (Equations 3 and 4). As in Baker (2018), we interpret the variation in indebtedness across cohorts and regions to be mainly driven by exogenous factors, similar to the instrumental regression of housing supply elasticities.

Table (5) summarizes the empirical results from specifications (3) and (4). In columns (1), (2), and (3), we report results including only the direct effect of leverage on consumption (the debt-overhang effect), while the rest three columns report results allowing for both the direct effect and the indirect effect through housing wealth changes. In both setups, we use all three measures of household indebtedness. In Specification $(1)-(3)$, we only include the leverage ratio as a new independent variable. The estimated elasticity coefficient is negative and statistically significant. For example, in (1) a 1% increase in LTV would reduce consumption growth by 0.20% at the household level. The estimates suggest a strong debt-overhang channel implying that highly leveraged households experience weaker consumption during the last recession compared to less leveraged households. This debt-overhang effect more than offsets the increase in consumption due to the housing wealth effect if house prices and leverage both go up by the same percentage point. Our finding is

Table 5 Effect of indebtedness on the marginal propensity to consume out of housing wealth

	(1) LTV	(2) DTI	(3) DSR	(4) LTV	(5) DTI	(6) DSR
ΔHouse value (ln)	0.17***	0.18***	0.18***	0.18***	0.17***	0.18***
	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)
ΔDisposable income (ln)	0.50***	0.51***	0.48***	0.49***	0.50***	0.48***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
ΔLeverage	−0.20**	−0.05***	−0.73***	−0.19**	−0.05***	−0.81***
	(0.09)	(0.02)	(0.23)	(0.10)	(0.02)	(0.24)
Leverage*ΔW	-	-	-	-0.01	0.01	0.49
				(0.22)	(0.04)	(0.62)
Fixed effects	YES	YES	YES	YES	YES	YES
R^2	0.390	0.397	0.397	0.391	0.394	0.397
Observations	706	706	705	706	704	705

∗: ($p < 0.01$), ∗∗: ($p < 0.05$), ∗∗∗:($p < 0.01$)

Columns (1) to (6) show which leverage measure is employed in each specification. In all specifications, we employ Robust Linear regressions. The number of observations is rounded up or down randomly to a multiple of three in compliance with Statistics New Zealand rules. Fixed effects include household size, age, and time effects for each survey wave. Standard errors are presented in parentheses. Standard errors are presented in parentheses

consistent with Dynan (2012) for the US during the GFC and Price et al. (2019) for a broader sample period in Australia, both suggesting that a 10 per cent increase in debt reduces household expenditure by about 0.3%. As pointed earlier, we differentiate from these studies by employing variant indebtedness indicators. Among them, DSR indicates the largest negative impact of indebtedness on consumption. When DSR rises by 1%, household consumption growth will be dragged down by 0.73%, almost three times higher than when LTV is employed. Two implications can be drawn from this finding. First, the debt-overhang effect driving subdued consumption growth, is associated to liquidity risk to a larger extend than solvency risk is. Second, between the two liquidity risk indicators, DSR prevails over DTI, implying the importance of both interest rate and income shocks on consumer's repayment capacity. Both findings reinforce the liquidity constraints channel for New Zealand as they imply higher adjustment for constrained households with future income and repayment risks.

In specifications $(4)-(6)$, we allow the leverage ratios to interact with housing wealth, as specified in equation 4. This specification provides another important test for the liquidity constraints channel. According to Carroll and Kimball (1996, 2006) theoretical framework, low net-worth households present a higher sensitivity to wealth changes, pointing to the presence of precautionary savings or liquidity constraints. The intuition is empirically backed by Mian et al. (2013), who highlight the role of collateral capacity in consumption behaviour during a housing downturn. The authors show that, in the GFC, highly leveraged households faced binding borrowing constraints triggered by the decline in housing values and consumption responded more for over-indebted households. To assess this mechanism, we allow for the interaction between leverage levels, as proxied by LTV, DTI, and DSR, and our housing wealth measure. Our evidence now does not provide any support in favor of the liquidity constraints channel for New Zealand. While leverage continues to be negative and significant, as in the assessment of the debt-overhang channel in columns $(1)-(3)$, the estimated coefficients on the interaction term are not statistically significant in any of the three variant indebtedness indicators. Yet, they retain a positive sign as suggested by the liquidity constraint channel.

Considering the prominence of the liquidity constraints channel during a housing downturn, we aim to assess whether the interaction between leverage and the housing wealth effect is asymmetric, that is whether it depends on the direction of housing wealth dynamics. It is crucial to juxtapose the estimates for the bust with estimates for boom, otherwise the impact of wealth and leverage on consumption during the GFC may be interpreted as phenomenon exclusive to a recession (Kukk, 2016). To this end, similarly the test employed for equation 2 and following Altonji and Siow (1987), we split the sample in two sub-samples, one where housing price upswing prevail and one where house price drops prevail. A similar exercise is performed by Disney et al. (2010) for a non-financial crisis period in the UK (1994-2003). Our results are reported in Table (6). As the break down into positive and negative rates necessarily leads to smaller samples for each specification, we use robust linear regression with winsored values to ensure that our results are not influenced by the outliers. To further check the robustness of asymmetric results, we use two panel setups in this table. In particular, the upper part of the table shows empirical results using our benchmark panel, two-year of birth year cohort and 16 regions, while the lower part of table reports results based on single birth-year cohort and 7-region panel.

We find that the interaction coefficients become significantly positive when negative housing wealth shocks are used, while, with positive housing wealth shocks, the coefficients on the leverage level terms tend to be more significant. This is a very interesting result, which highlights different channels might be at work when housing wealth increases or decreases. In particular, when the housing wealth goes down, the liquidity channel highlighted by Mian et al. (2013) dominates. This is reflected by the positive estimate on the interaction terms. In the housing boom, by contrast, the housing wealth effect is positive on consumption, but it is less so for those who are more in debt. It should be noted that our dataset does not allow the identification of whether the liquidity channel is due to collateral capacity or borrowing constraints. Yet, our result comes in contrast to evidence from other countries where households increase leverage and extracted equity in times when house prices are rising. A recent example is Zhu et al. (2019) who find strong evidence of home equity extraction out of house price increases. However we interpret the finding as a result of liquidity constraints. A conjecture we can draw is that home equity withdrawal is a relatively recent development in New Zealand and its prevalence is still limited, despite the surge in housing prices. Furthermore, there is no strong evidence of a stable long-term relation between housing equity withdrawal and consumption in the country as of yet (Smith, 2010).

Table 6 Asymmetric Effect of indebtedness on the marginal propensity to consume out of housing wealth

	LTV		DTI		DSR	
	Negative	Positive	Negative	Positive	Negative	Positive
	2-year birth cohorts & 16 NZ Regions					
ΔLeverage	0.40	−0.18	0.06	-0.06^{*}	0.84	-0.82^{**}
	(0.26)	(0.16)	(0.05)	(0.03)	(0.61)	(0.41)
Leverage*ΔW	2.40^{***}	-0.07	0.53^{***}	0.02	6.95^{***}	0.27
	(0.69)	(0.31)	(0.14)	(0.06)	(2.05)	(0.83)
	Single-year birth cohorts & 7 NZ Regions					
ΔLeverage	−0.05	−0.23	−0.01	-0.07^{**}	−0.20	-0.97^{***}
	(0.24)	(0.16)	(0.05)	(0.03)	(0.57)	(0.38)
Leverage*ΔW	1.15^{*}	0.31	0.26^{*}	0.08	2.85	1.18
	(0.69)	(0.47)	(0.14)	(0.10)	(1.87)	(1.19)

Note: Standard errors in parentheses

$*$: ($p < 0.01$), $**$: ($p < 0.05$), $***$:($p < 0.01$)

The first three columns show which leverage measure is employed in each specification between LTV, DTI, and DSR. In all specifications, we employ Robust Linear regressions. The number of observations is rounded up or down randomly to a multiple of three in compliance with Statistics New Zealand rules. Fixed effects include household size, age, and time effects for each survey wave. Standard errors are presented in parentheses. Standard errors are presented in parentheses

4.3 Other Robustness Checks

In this section, we summarize a series of robustness checks for our empirical results. Table 13 in Appendix E reports the robustness results on control variables. In Column (1), we do not use panel cell weights and any other control variables except for income. With regards to Columns (2) - (5), we progressively add cell's weights and other control variables into the regression. As shown in the table, the addition of more controls does not qualitatively change our estimates. However, quantitatively, control variables make our housing wealth effect smaller, but enhance the leverage effect. Adding cells' weights seems to be quite important quantitatively. The estimated housing wealth elasticity decreases by 0.03, while the leverage coefficient rises by 0.06 in absolute value. Overall, both housing wealth and leverage effects retain their robustness in this sensitivity check.

Table 14 in Appendix E reports the results under different constructions of synthetic panels using different combinations of year of birth and regional cohorts. In particular, Column (1) shows our benchmark setting, in which the cohort of 2-year age and 16 regions of New Zealand are selected in the construction of the representative household. In the subsequent columns, we present results of different panel settings of cohort-region combinations. For example, in Column (6), we use the cohort of one-year birth cohort and 16 region geographical division for the synthetic panel construction. By doing so, we have 1038 observations in panel. However, the trade-off is that each cell in the panel data contains much small number of households, making the panel cells less representative. In overall, our results are broadly consistent with our benchmark case and changing panel construction does not affect our main findings substantially. However, when the number of cells in panel becomes small, the significance of the estimates becomes smaller.[22]

5 Concluding Remarks

The present paper investigated the interaction between housing leverage and the housing wealth effect in New Zealand. We constructed housing supply elasticities based on geographical constraints in New Zealand and a synthetic panel series using survey data for the period 2006-2016. We then estimated the MPC out of exogenous changes in housing wealth, while controlling for household's level of indebtedness. All regressions control for income, household characteristics, and regional and household fixed effects. We then focused on the role of household leverage in determining the MPC out of housing wealth. We studied how leverage, measured by the loan-to-house-value ratio (LTV), the debt-to-income ratio (DTI) and debt servicing ratio to the disposable income, affect the estimated MPC out of housing wealth.

Overall, our empirical results find an average MPC out of a one-dollar increase in exogenous housing wealth is around 3 cents. The consumption elasticities are stronger for older-homeowners than younger home-owners, implying the dominance of a genuine wealth effect over the collateral channel, with the latter still playing also a significant role. We found no evidence of confounding factors, such as income expectations or productivity gains, driving the association between wealth and consumption changes in New Zealand. Household indebtedness does exert a significant impact on consumption spending. It works both through the debt-overhang channel and the liquidity constraints/collateral channel. Our empirical results suggest that these channels may work in different phase of housing cycles. In particular, we find that household leverage weakens the consumption spending out of housing wealth in housing booms. By contrast, in a housing downturn, the liquidity constraints/collateral channel is more important, making negative housing weather effect more sensitive to high leverage.

The magnitude of the consumption response is substantially higher than in studies estimating the wealth effect in non-boom and bust periods, but lower than in studies investigating the relationship under stable periods. Our results confirm recent studies, that assign a significant role to leverage for larger consumption adjustments, however the channel appears to run from changing house prices driving changes in leverage and therefore consumption; rather than amplifying the effect of house prices on consumption directly. Importantly, the interaction between leverage and the housing wealth effect can be asymmetric, depending on whether house prices are increasing or decreasing. The policy implication of our results is that liquidity constraints might be the most important feature to be considered in the housing downturn, but in the upturn phase of the housing market, other mechanisms are at play, including the genuine wealth effect, which potentially weaken the relationship between consumption and house prices.

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A Literature Table

Table 7: A literature summary on housing wealth effects on consumption

Study	**Data, Country, Period**	**Theoretical Framework, Focus**	**Definition, Identification of wealth**	**Measurement, Definition of Consumption or Saving**	**Key results**	**Asymmetries**
			Savings decline and Consumption booms in the US			
Skinner (1989)	Panel Study of Income Dynamics, CEX; US; 1973-83 .	Life Cycle Theory: Bequest Motives.	Subjective house value is assumed to drive consumer decisions.	Non-housing consumption less vehicles imputed using food consumption coefficients from the PSID survey.	Very small wealth effects even after controlling for unpredictable changes or household heterogeneity (0.06 or less).	Not Applicable.
Skinner (1996)	Panel Study of Income Dynamics; US; 1989; Aggregate Time-series.	Life Cycle Theory / Moving costs and HEW / Bequest motives / Precautionary Saving .	Annuitized value of housing wealth.	Active saving (overall change in wealth minus capital gains).	Greater responses from younger households and smaller effect for older ones who use housing wealth as insurance against unexpected contingencies.	Asymmetries due to Precautionary Saving Motives: larger adjustment in declines than gains, especially for younger households attributed to the unanticipated nature of the decline.
Engelhardt (1996)	Panel Study of Income Dynamics; US; 1984, 1989.	Life Cycle Theory / Permanent versus Transitory housing shocks.	Real housing capital gains defined as the difference in real self-reported home less the real value of additions and repairs.	Active saving (difference in non-housing wealth plus the net additions to financial instruments, less the amount of inheritances) and changes in real non-housing wealth.	Consumption elasticity of 0.03 or higher.	Only wealth losses induce wealth effects Positive gains (0.009) negative swings (0.042).
Juster et al. (2006)	Panel Study of Income Dynamics; US; 1984, 1989, 1994.	Life Cycle Theory / Consumption based CAPM for stockholders and non-stockholders (Mankiw and Zeldes, 1991).	”Self-reported asset values to measure capital gains defined as change asset value less the active saving.”	Active saving (change in wealth minus capital gains).	The wealth effect of corporate equities gains on savings (consumption) is negative (positive) and large (19 cents per dollar over a 5-year period) while the effect of housing gains is small and non-significant.	Not Applicable.
Bostic et al. (2009)	”Survey of Consumer Finance, Consumer Expenditure Survey; US; 1989, 1992, 1995, 1998, and 2001 waves.”	Types of wealth.	Self-reported value of financial wealth, owner-occupied housing, and other reported real estate.	Durable, non-durable, and total consumption.	Housing wealth effects are positive and large in the range of 6 cents per dollar (0.04-0.06), while financial wealth are estimated in the range of 2 cents (0.02). Slightly higher wealth effects for durable consumption as opposed to total consumption.	Not Applicable.
			The channels: wealth effect vs liquidity constraints vs confounding factors			
Campbell and Cocco (2007)	Family Expenditure Survey (FES); UK; 1988–00.	Life Cycle Theory / Perception of wealth; Liquidity constraints.	Regional house price indices. Decomposition between predictable and unpredictable changes in home values.	Non-durable consumption less the purchase of vehicles.	Significant housing wealth effects (1.56 or 7.7 cents). Largest wealth effects are reported for older homeowners, and the smallest effect for younger renters.	Not Applicable.
Attanasio and Weber (1994), Attanasio et al. (2009)	Family Expenditure Survey (FES); UK; 1978–02.	Life Cycle Theory / Wealth effects, liquidity constraints, confounding factors.	Official house price statistics, controlling for regional variation. Log differences and levels considered.	Levels of total non-housing expenditure defined as the sum of durables and non-durables.	Highest consumption responses reported by younger cohorts (0.2) then old cohorts (0.04). Interpreted as changes in permanent income and productivity driving the consumption boom.	Not Applicable.
Disney et al. (2010)	British Household Panel Survey (BHPS), Family Expenditure Survey (FES); UK; 1994-03.	Life Cycle Theory / Expectations: expected versus unexpected house price changes.	Matched county-level house price data and household real financial capital gains. Self-reported changes in financial expectations..	Self-reported saving value.	Housing wealth elasticities estimated at around 0.01. No heterogeneous results between young and old.	Five times stronger elasticity in the response of households in negative equity due to precautionary savings and liquidity constraints.
Gan (2010)	Administrative data for individuals from a transactions database from six credit card issuers; Hong Kong (9 districts); 1992-04.	Life-cycle theory: liquidity constraints vs precautionary savings motive.	Housing returns at a district level. The granular level of the measure helps avoiding endogeneity problems.	Credit card spending; disaggregation between discretionary and non-discretionary spending.	Positive and significant Housing wealth effect is p (0.17). Higher for those with more than one dwelling. Strong elasticities for non-refinancing households and younger ones, implying a precautionary saving channel. Responses for discretionary spending are stronger than other goods and services.	Not Applicable.
Browning et al. (2013)	Public administrative registries (10% sample); Denmark; 1987-96.	Life Cycle Theory / Expectations: expected versus unexpected house price changes.	”AR process of average sales prices for traded single-family houses at the municipality level for the period 1985-01.”	”Non-housing consumption expenditure imputed by taking the difference of total disposable income in period t and the change in total net wealth from the previous period t-1 to the period t.”	”Small housing wealth effect (0.13). Young owners with liquidity constraints react to financial liberalization reforms which enable home equity withdrawal, thereby implying that expected changes in house prices impact expenditure through the collateral channel. No responses for older owners interpreted as no genuine wealth effect.”	Not Applicable.
Windsor et al. (2015)	”Household, Income and Labour Dynamics in Australia (HILDA); Australia; 2003-10.”	Life-cycle Theory / Wealth effects, liquidity constraints, confounding factors.	Self-reported house prices (levels). Assessment of homeowners judgment by reporting the value of their property and benchmarking it against nationwide prices.	Self-reported non-housing consumption. Expenditure on food and childcare is used to impute total consumption for certain years.	No evidence for genuine wealth effect as young homeowners indicate the strongest responses (3 cents per dollar). Young renters exhibit a positive consumption response to house prices (2.45), but smaller than young owners (6.34), implying the presence of liquidity constraints and housing collateral channels.	Not Applicable.
Christelis et al. (2015)	Internet Survey of the Health and Retirement Study (HRS) for those aged fifty and above; US; 2008-09; PSID; US; 2007,09,11.	Effects of (housing and financial) wealth and unemployment shocks on consumption during the Great Recession.	Self-reported quantitative (percentage) changes in housing and financial wealth compared to the previous year.	Self-reported qualitative (categorical) and quantitative (percentage) changes in total spending compared to the previous year.	House price falls imply an estimated wealth effect of 0.056 (associated implied MPC is 1% on a year-on-year basis).	Only consider negative housing wealth shocks.

Paiella and Pistaferri (2017)	"Italian Survey of Household Income and Wealth, Italy; 2008-10."	Life-cycle Theory, Permanent Income Hypothesis / Exogenous versus endogenous effects; expectations; liquidity constraints.	Identifying exogenous and unanticipated shocks in housing prices by comparing subjective asset price expectations with ex-post price realizations.	Self-reported household consumption on non-durables.	The estimated elasticity is 0.03 (1 to 3 cents per euro increase in wealth), driven by house price changes. The response to anticipated changes in wealth is also large (0.034), implying that households are very sensitive to both expected and unexpected changes in house prices.	Significant elasticities only in the case of positive expected returns to housing wealth (0.035).
Zhu et al. (2019)	"Panel Study of Income Dynamics; US; 1999-13; State-level refinance-loan denial ratios and refinance loan amounts from Home Mortgage Disclosure Act (HMDA). "	Life-cycle Theory / Home Equity Withdrawal	Self-reported home value, excluding mortgages; Instrumental-variable regression (IV) to identify the impact of extracted home equity on ;consumption.	Self-reported durable and non-durable goods.	The marginal propensity to consume under the home equity withdrawal channel is double the size of the genuine wealth effect channel (0.041), and the former varies depending on the level of liquidity constraints. The elasticities are stronger in the case of durables consumption for constrained households.	Not Applicable.
The housing collapse of the GFC and the role of leverage						
Dynan (2012)	Panel Study of Income Dynamics (PSID); US; 2005-09.	Life-cycle theory / Debt overhang effect.	PSID information on assets and net equity in businesses, vehicles, and second homes.	Self-reported consumption expenditure.	An increase in the LTV ratio from 1.0 to 1.1 reduces the consumption growth by 0.3 percentage point per year.	Not Applicable.
Mian and Sufi (2014)	Zip-code and county level data; US; 2006-09.	Collateral and credit constraint channels, leverage.	Financial assets using IRS Statistics of Income (SOI); housing assets using 2000 Decennial Census; Debt using Equifax Predictive Services; Housing supply elasticities (Saiz, 2010).	Auto sales and county level credit-card spending.	Elasticity of consumption with respect to housing net worth shock is 0.6 - 0.8. Average MPC out of housing wealth is 5 - 7 cents for every dollar loss. MPC is sharply higher for poor and more leveraged households.	Not Applicable.
Baker (2018)	Online financial data of 150,000 households; US, 2008 -12.	Life-cycle theory / Liquidity constraints, leverage.	Identification of firm shocks to employees' income, using SEC's 8-K filings for public firms.	Expenditure measured using electronic transactions.	The elasticity of consumption to income shocks is around 0.35, and higher leverage significantly increases the sensitivity of consumption response to income.	Smaller consumption response to positive income shocks than to negative income shocks.
Berger et al. (2017)	Panel Study of Income Dynamics (PSID); US; 1999-11.	Stylized life-cycle model	Relating MPC from housing wealth to MPC from transitory shocks. Follow Blundell et al. (2008) to identify MPC from transitory shocks.	Consumption expenditure includes food at and away from home, utilities, gasoline, car maintenance, public transportation, childcare, health, expenditures, and education.	Aggregate elasticity of consumption to housing wealth of 0.33 (Compared to 0.23 of the baseline life-cycle model).	Not Applicable.
Aladangady (2017)	CES (Public-Use and Restricted Geographic Data), house price at regional level (MSAs); US; 1986-08.	Life-cycle theory / Liquidity constraints, cost of living for renters.	Saiz-index times interest rate (10yr treasury)'. To address concerns of correlation of instrument with demand factors they show that while interest rate is highly correlated with exposure to interest rates ('potential savings from refinance'), the IV is not.	Self-reported non-housing consumption.	MPC from housing wealth estimated at 4.7 cents on a one dollar rise in house prices (slightly smaller than previous estimates). Smaller estimates than other studies using Saiz-2010 index as IV. MPCs are found near zero for renters.	Not Applicable.
Guren et al. (2018)	City level data (380 CSBAs); US; 1978-17.	Estimation of house price elasticities using city-level data. The estimates are found half those in studies using (Saiz, 2010) index as IV, and higher in 1980s, lower in 2000s (by using 10-year rolling window).	Use Bartik-style IV: 'city-level elasticity of local house price to regional house price'*'regional house-price shock'. To address concerns of Davidoff (2016) they estimate the elasticities conditional on observable demand factors.	Retail employment as a dependent variable instead of consumption as the two are correlated. The estimated elasticities are interpreted as MPC out of housing wealth.	Estimates of elasticity of consumption to house prices is estimated at 0.053 for full sample and 0.071 if drop noisy pre-1990 data). These correspond to MPC from housing of 2.4 cent on the dollar (3.3 if drop noisy pre-1990 data). [Note, these elasticities come from retail employment data.]	Provide evidence of no asymmetry between reaction to house price increases and decreases.
Price et al. (2019)	"Household, Income and Labour Dynamics in Australia (HILDA); Australia; 2001-17. (Homeowners sample)."	Life-cycle theory / Debt overhang effect.	Instrument for mortgage debt using level of outstanding debt that would exist based on standard formula for repayment schedule (schedule incorporates time of purchase, original mortgage, interest rates, duration of loan).	Self-reported consumption on non-durables.	They find that regressing consumption on debt level gives negative coefficient, implying a 0.3 % decrease in consumption for a 10% increase in debt.(debt overhang effect). Not directly interested in house prices but do find evidence for debt level having greater effect on consumption when house prices change (interaction term debt-times-house price is negative; although regression does not allow direct effect of house prices).	Not Applicable.
Cloyne et al. (2019)	Panel data on owner-occupied refinancing of mortgages, Confidential administrative data from 'Product Sales Database'; UK; 2001-15	Life-cycle theory / Effects of house price swings on borrowing, collateral effects, MEW	House value is defined as the 'appraisal value' of the bank that provides the loan. Loans refinance after 2-5 years due to rate reset. Since this is predetermined when the actual refinancing decision is made they treat the timing (and corresponding change in house price since last refinancing event) as exogenous.	Not Applicable.	Estimate an elasticity of borrowing to house prices between 0.2 to 0.3, interpreting it as evidence of a collateral channel. Also find that the size of house price change is unimportant for the elasticity.	Evidence of strong asymmetry between house prices and borrowing in positive changes. This asymmetry is only present when no controls (age, income LTV) are used, and there is no asymmetry (isoelastic) when controls are added.
Angrisani et al. (2019)	Consumption and Activities Mail Survey (CAMS); US, 2002-12.	Life-cycle theory / Housing wealth effect.	Housing wealth defined as the gross self-reported value of the primary residence. Across-state variations in housing wealth declines during the Global Financial Crisis are considered.	A complete inventory of household consumption as obtained on 39 categories of spending.	MPC to an unexpected housing wealth drop is 6 cents per dollar among households over fifty.	Only consider negative housing wealth shocks.

B The New Zealand Household Expenditure Survey

Table 8: A : classification of non-durable expenditures in HES data

Description	NZHEC code	Description	NZHEC code
Fruit and vegetables	1.1	Other recreational equipment and supplies	9.3
Meat, poultry and fish	1.2	Recreational and cultural services	9.4
Grocery food	1.3	Newspapers, books and stationery	9.5
Non-alcoholic beverages	1.4	Accommodation services	9.6
Restaurant meals and ready-to-eat food	1.5	Package holidays	9.7
Alcoholic beverages	2.1	Miscellaneous domestic holiday costs	9.8
Cigarettes and tobacco	2.2	Early childhood education	10.1
Illicit drugs	2.3	Primary, intermediate and secondary education	10.2
Clothing	3.1	Tertiary and other post school education	10.3
Footwear	3.2	Other educational fees	10.4
Actual rentals for housing	4.1	Personal care	11.1
Home ownership	4.2	Prostitution	11.2
Household energy	4.5	Personal effects not elsewhere specified	11.3
Household textiles	5.2	Insurance	11.4
Glassware, tableware and household utensils	5.4	Expenditure incurred whilst overseas	13.5
Other household supplies and services	5.6	Sales of clothing and footwear	14.1
Medical products, appliances and equipment	6.1	”Sales and trade-ins of property and materials for property improvement and maintenance”	14.2
Out-patient services	6.2	Sales and trade-ins of household contents	14.3
Hospital services	6.3	Sales, trade-ins and refunds for health (excluding insurance claims)	14.4
Private transport supplies and services	7.2	Sales and trade-ins of vehicles, vehicle parts and accessories	14.5
Passenger transport services	7.3	Sales and trade-ins for communication	14.6
Postal services	8.1	Sales, trade-ins and refunds of equipment for recreation and culture	14.7
Telecommunication equipment	8.2	Refunds for education	14.8
Telecommunication services	8.3	”Sales, trade-ins and refunds of miscellaneous goods, cash receipts from insurance claims”	14.9

Table 9: A : classification of durable expenditures in HES data

Description	NZHEC code
Furniture, furnishings and floor coverings	5.1
Household appliances	5.3
Tools and equipment for house and garden	5.5
Purchase of vehicles	7.1
Audio-visual and computing equipment	9.1
Major recreational and cultural equipment	9.2
Jewellery and watches	11.3.01

The complete New Zealand Household Economic Survey Classification (NZHEC) can be found at `http://archive.stats.govt.nz/browse_for_stats/people_and_communities/Households/household-economic-survey-classifications.aspx`

C Additional Figures and Tables

Table 10 Synthetic Panel Descriptives

Region/ Cohort	Mean cell size	Min cell size	Max cell size
Birth Cohort: (1975-1985]			
Auckland	80	32	108
Waikato	19.6	10	23
Bay of Plenty	13	2	21
Wellington	48.6	11	69
Rest of North Island	36.5	17	46
Canterbury	47.6	21	62
Rest of South Island	34.4	14	54
Birth Cohort: (1959-1975]			
Auckland	119.3	107	129
Waikato	31	19	37
Bay of Plenty	27.7	24	33
Wellington	76.2	68	81
Rest of North Island	65.5	51	74
Canterbury	73.8	60	86
Rest of South Island	65.1	43	82
Birth Cohort: (1945-1959]			
Auckland	81.7	60	112
Waikato	28.6	19	45
Bay of Plenty	26.1	22	31
Wellington	61.6	53	73
Rest of North Island	65.6	50	77
Canterbury	67.8	54	83
Rest of South Island	62.6	57	81

Table 11 Synthetic Panel Descriptives

Region/ Cohort	Δ Non-housing Expenditure (ln)	Δ Disposable Income (ln)	Δ Housing Value (pr)	LVR (pr)	DTI (pr)	DSR (pr)
Birth Cohort: (1975-1985]						
Auckland	0.08	0.11	0.11	0.49	2.59	0.20
Waikato	0.22	0.19	0.27	0.51	2.46	0.19
Bay of Plenty	0.09	0.34	0.10	0.52	2.43	0.19
Wellington	-0.03	0.04	0.15	0.55	2.54	0.20
Rest of North Island	-0.07	0.04	0.12	0.52	2.28	0.18
Canterbury	0.12	0.14	0.17	0.56	2.64	0.21
Rest of South Island	-0.14	0.21	0.20	0.50	2.24	0.18
Birth Cohort: (1959-1975]						
Auckland	0.15	0.17	0.12	0.27	1.60	0.12
Waikato	0.16	0.25	0.21	0.31	1.31	0.10
Bay of Plenty	0.06	0.06	0.17	0.23	1.14	0.09
Wellington	0.09	0.12	0.17	0.30	1.45	0.11
Rest of North Island	0.04	0.10	0.17	0.30	1.34	0.11
Canterbury	0.08	0.18	0.15	0.29	1.42	0.11
Rest of South Island	0.09	0.14	0.16	0.30	1.36	0.11
Birth Cohort: (1945-1959]						
Auckland	0.04	0.04	0.08	0.05	0.34	0.03
Waikato	0.03	0.07	0.24	0.06	0.22	0.02
Bay of Plenty	0.02	0.12	0.21	0.04	0.17	0.02
Wellington	0.01	0.02	0.13	0.07	0.29	0.03
Rest of North Island	0.08	0.13	0.19	0.07	0.24	0.02
Canterbury	0.05	0.04	0.14	0.06	0.27	0.02
Rest of South Island	0.01	0.02	0.18	0.07	0.26	0.02

D Geographical Housing Supply Elasticity Index for New Zealand

Following the methods of Saiz (2010)

House prices in New Zealand rose rapidly in in the 2010s. To assess whether these rises imply a housing boom and bust or reflect fundamental changes in supply and demand we need to be able to measure housing supply adequately. The number of new houses, or of houses-per-inhabitant capture the interplay of supply and demand, not supply directly. We introduce an index based on geographical constraints for New Zealand that provides a direct measure of (potential) housing supply.[23]

This Geographical-Contraints Housing Supply Index closely follows that introduced by Saiz (2010) for the United States, who also demostrates its empirical usefulness. The measure is calculated at the level of cities as the fraction of land within a 50km radius that is unavailable for use due to being either ocean, wetlands, or too steep.

This appendix provides the values of the index for the seventeen largest urban areas in New Zealand. We also provide the corresponding regions and mesh-blocks, to make the index easy to combine with data from other sources. The remainder of this appendix briefly details the construction of the index. We give precise definitions, listing all data sources, and provide process diagrams for the GIS (Geographical Information Systems) calculations involved. We discuss and provide justifications for any decisions that had to be made as part of the construction of the index.[24]

D.1 Summary of Methodology

We briefly describe the details of the methodology employed to create a measure of the total land unavailable for residential development in New Zealand following Saiz (2010).[25] Saiz's study explores land unavailability for 95 MSAs (Metropolitan Statistical Areas) in the United States. Saiz identified unavailable land using slopes of greater than 15%, land area (as indicated by a contour map), unsuitable land cover (not wetlands, lakes or rivers) and city centres.

Our study uses ESRI's ArcGIS 10.3.1 to replicate Saiz's methods using measures of New Zealand's slope, land cover, coastline and city centres. Where possible datasets close to 2001 were used to replicate Saiz's study. The resulting geographic areas are represented in Figure 3.

City centres: Saiz used the geographic city centres of 95 MSAs (Metropolitan Statistical Areas). These centres have populations "…over 500,000 in the 2000 Census for which I also have regulation data…" (Saiz, 2010, p1257). He does not state how these centres are determined. Our study uses the 17 main urban areas of New Zealand as identified by Statistics New Zealand based on a 2001 population. These urban areas vary in population from 1,129,700 (Auckland) to 27,300 (Blenheim). City centres in New Zealand were created using current town halls and district council buildings using with Google Maps.

City centre radius: Saiz defines a radius of 50km to calculate the amount of land unavailable for residential development using census block groups (roughly equivalent to mesh-blocks) for slope and using census tracts (roughly equivalent to Area Units) for land cover. Distance from the city centre to each meshblock centroid in New Zealand was calculated and a maximum value of 50km was used to determine the mesh-blocks within the radius. For consistency across datasets, only meshblocks were used for analysis in NZ.

Slope: Saiz identifies slope as a primary characteristic of land unavailable for development. In particular, land with a slope of greater than 15% is "…severely constrained for residential construction" due to "Architectural development guidelines..." (Saiz, 2010 p1256). Saiz calculated slope within a 50km radius from city centres using a 90m Digital Elevation Model (DEM) from the United States Geological Survey (USGS). To

[23] From the statistical perspective the important advantage of this index over many other possible measures of housing supply is that it is exogenous to housing demand. It can therefore be used as an instrumental variable for housing supply allowing causal interpretations.

[24] The values of index itself can be found in this appendix, and in the supplementary materials can be found here: `https://www.dropbox.com/s/fdnze4pzixwx4ua/HousingSupplyIndex_NZ.xlsx?dl=0HousingSupplyIndex\_NZ.xlsx`, and here: `https://www.dropbox.com/s/zqfqf71tstcd7tg/HousingSupplyIndex_NZ.csv?dl=0`.csv). Some users may also want the distance of Meshblocks ids (as defined by 2001 New Zealand Statistics definitions) from each of the urban centres. These are given in `https://www.dropbox.com/s/82gh0khiosmt2ui/DistToUrbC.txt?dl=0`DistToUrbC.txt. They allow for easy mapping of the index to the level of meshblocks. For example, you might assign the index value for an urban centre to all meshblocks within 50km of that urban centre, doing this separately for each of the seventeen urban centres.

[25] For comprehensive documentation on the index please contact either Mairead de Roiste, School of Geography, Environment & Earth Sciences, Victoria University of Wellington, *mairead.deroiste@vuw.ac.nz*. Or Robert Kirkby, School of Economics & Finance, Victoria University of Wellington, *robertdkirkby@gmail.com*

Table 12 Housing Supply Index for New Zealand

Urban Centre	X	Y	Urban Population 2001	Total area (m^2)	Fraction of unavailable land (m^2)
Auckland	1757203.1363	5919992.7928	1,082,250	35131427033	0.19
Blenheim	1679690.4291	5404026.2258	27,504	9525270120	0.38
Christchurch	1570560.718	5180600.5242	335,646	9929563061	0.45
Dunedin	1406201.5478	4917039.0324	108,231	11700242649	0.46
Gisborne	2037447.419	5708183.792	32,235	12527980762	0.44
Hamilton	1800866.483	5815238.1021	171,687	95833107448	0.36
Invercargill	1242800.7033	4849586.6766	46,536	13222844552	0.37
Kapiti Coast	1768714.2615	5468672.9598	34,176	10081330601	0.46
Napier-Hastings Center	1930265.0407	5610550.2085	116,091	12729216023	0.42
Nelson	1623761.0488	5431120.8349	54,642	12524264821	0.36
New Plymouth	1693152.6607	5676086.6429	48,735	14717596294	0.47
Palmerston North	1821662.7942	5529325.6004	73,644	8629113853	0.29
Rotorua	1885309.405	5774105.0642	53,301	6968135121	0.68
Tauranga	1879422.9653	5824611.4447	98,976	13979276716	0.51
Wanganui	1775052.9754	5577666.2171	40,719	9611930424	0.45
Wellington	1748827.0876	5427595.7843	343,062	11744215356	0.58
Whangarei	1719182.8568	6045764.2951	49,149	8979892162	0.36

Land unavailable for development within 50km of the 17 main urban areas in New Zealand. The fraction of unavailable land is area of unavailable land divided by total area. Authors' own estimations.

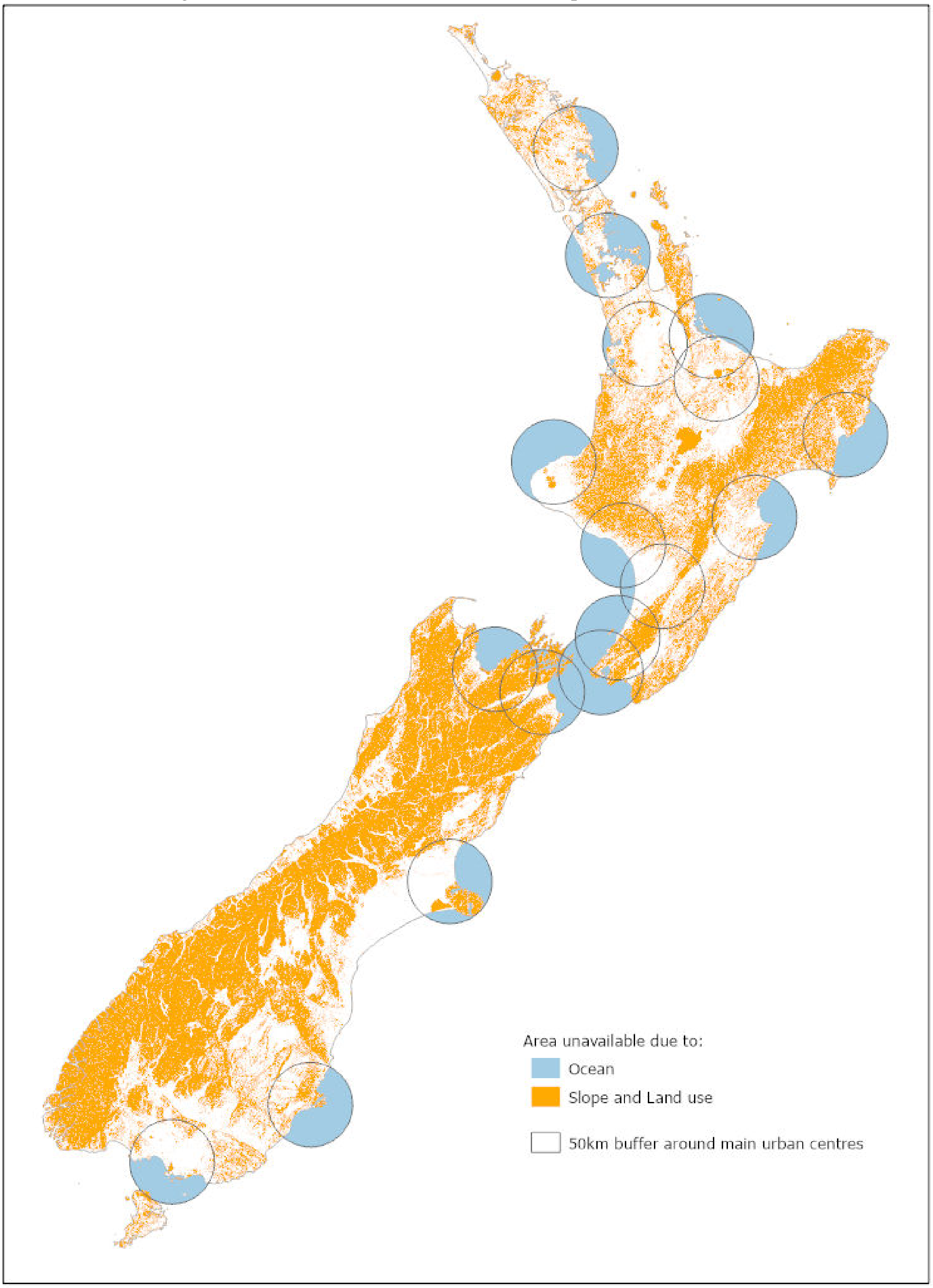
replicate Saiz's work, the University of Otago's 15m DEMs were re-sampled to 90m resolution. Slope was measured as 'PERCENT_RISE' and areas of greater than 15% were identified.

Land cover: Saiz identifies inland waterbodies and wetlands as 'undevelopable area'. New Zealand land cover was sourced from the New Zealand Land Cover Database 2 (LCDB 2) for summer 2001/2002. There is no strict definition of wetlands from LCDB 2 and relevant vegetation classes were combined. Unsuitable land cover areas were identified as 'Lake and Pond', 'River', 'Estuarine Open Water', 'Herbaceous Freshwater Vegetation', 'Herbaceous Saline Vegetation', 'Flaxland' and 'Mangrove'.

Coastlines: Saiz used digital contour maps to delineate areas made unavailable by oceans and the Great Lakes. The data source was not provided. New Zealand's coastlines and islands were sourced from the Land Information New Zealand (LINZ) data service (2015). The data comprises coastline and island outlines. The amount of area lost to oceans within a 50km radius was found using the city centres and removing overlapping wetland areas from the land cover criteria.

Combining relevant data: Using the exclusion criteria of unsuitable slope (over 15%), unsuitable land (wetlands, rivers or lakes) and ocean within 50km of the city centre, Saiz identifies undevelopable areas. The three criteria were combined for each of the 17 urban areas in New Zealand and the fraction of land unavailable for development is provided in Table D.1.

Fig. 3: Land unavailable for residential development in New Zealand



E Additional Robustness Checks

Table 13 Robustness of housing wealth and leverage effect on consumption (Robust Linear Regression)

	ΔConsumption (ln)				
	(1)	(2)	(3)	(4)	(5)
ΔW (HSE)	0.19^{***}	0.21^{***}	0.18^{***}	0.18^{***}	0.16^{***}
	(0.04)	(0.04)	(0.04)	(0.4)	(0.05)
Δ Leverage (LTV)	-0.14^{**}	-0.17^{*}	-0.19^{**}	-0.19^{**}	-0.20^{**}
	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)
Leverage (LTV)*ΔHP	−0.07	−0.09	0.01	−0.01	0.02
	(0.22)	(0.22)	(0.22)	(0.22)	(0.22)
ΔIncome	0.56^{***}	0.50^{***}	0.50^{***}	0.50^{***}	0.50^{***}
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Cell weighting	No	YES	YES	YES	YES
Household size	No	No	YES	YES	YES
Household age	No	No	No	YES	YES
Time fixed effects	No	No	No	No	YES
R^2	0.355	0.406	0.412	0.413	0.403
Observations	705	705	702	706	704

$*$: ($p < 0.01$), $**$: ($p < 0.05$), $***$:($p < 0.01$)

The standard errors are reported in parentheses. The number of observations is rounded up or down randomly to a multiple of three in compliance with Statistics New Zealand rules.

Table 14 Effect of LTV on the marginal propensity to consume out of housing wealth

	(1) 2-year birth 16 region cohort	(2) 2-year birth 7 region cohort	(3) 2-year birth 76 region cohort	(4) 1-year birth 5 region cohort	(5) 1-year birth 7 region cohort	(6) 2-year birth 16 region cohort
ΔW (HSE)	0.16***	0.23***	0.25***	0.27***	0.27***	0.17***
	(0.05)	(0.06)	(0.04)	(0.06)	(0.5)	(0.04)
ΔLeverage (LTV)	−0.19**	−0.28**	−0.17*	−0.31**	−0.26***	−0.16*
	(0.10)	(0.12)	(0.10)	(0.09)	(0.10)	(0.09)
Leverage (LTV)*ΔW	0.02	0.16	0.18	0.49	0.40	0.14
	(0.22)	(0.36)	(0.23)	(0.31)	(0.30)	(0.23)
Other controls	YES	YES	YES	YES	YES	YES
R^2	0.404	0.370	0.4516	0.425	0.413	0.423
Observations	704	427	1073	599	787	1038

Standard errors in parentheses

$*$: ($p < 0.01$), $**$: ($p < 0.05$), $***$:($p < 0.01$)

The standard errors are reported in parentheses. The number of observations is rounded up or down randomly to a multiple of three in compliance with Statistics New Zealand rules.

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