Does mortgage regulation stabilize household consumption?*

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Abstract

We use Norwegian data on household balance sheets, housing transactions and consumption, to evaluate the impact of loan-to-value (LTV) regulation. First, we show that LTV-caps lead to lower leverage. Second, we show that higher LTV-caps also lead to a persistent decline in liquidity. Third, we find evidence of larger consumption responses to negative income shocks. In contrast, we do not find any change in the reaction to negative wealth shocks. We use these estimates as inputs in a simple analytical framework to show that LTV-caps are unlikely to stabilize household consumption, at least through the impact on household balance sheets.

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1 Introduction

Following the financial crisis, several papers have documented the potential risks of rapid house price growth and household debt for macroeconomic outcomes.[1] In response to these concerns, a broad range of countries have implemented borrower-based macroprudential policies. An important component of these policies are loan-to-value (LTV) restrictions, which impose an upper bound on mortgage debt relative to the value of the property. Typically, these policies are implemented in order to increase household resilience and make households less vulnerable to adverse economic shocks.

Several papers, as discussed below, have studied the impacts of LTV-caps using aggregate data or loan level data. We contribute to this literature by being the first to utilize detailed balance sheet and income statement information, merged with comprehensive consumption data on the universe of Norwegian households, to study the impact of LTV regulation on household balance sheets and their ability to smooth consumption in response to fluctuations in income and wealth. We make two important contributions to the existing literature.

Our **first main contribution** is to document what we refer to as the "leverage-liquidity tradeoff" of mortgage regulation. Using the tax data, we show that higher LTV-caps reduce leverage as expected, but – perhaps more surprisingly – also reduce liquidity, due to higher downpayment requirements. As a result, a larger portion of households' wealth is illiquid assets.[2] This creates a tension, as lower leverage is expected to make households *less* vulnerable to certain adverse shocks such as house price declines, while lower liquidity is expected to make households *more* vulnerable to other adverse shocks such as loss of income.[3]

Our **second main contribution** is to use individual-level consumption data for the full population of Norwegian individuals to evaluate the net impact of LTV-caps on households ability to smooth consumption in response to income and wealth fluctuations, measured as *household consumption volatility*. We focus on consumption volatility, as an important purpose of LTV-caps is to stabilize demand in response to adverse economic shocks and thereby limit negative aggregate demand externalities. It is important to note, however, that LTV-caps could also impact financial stability through other mechanisms, such as enhancing household loan performance and, consequently, reducing the probability of household defaults. Such aspects are beyond the scope of this paper.[4]

We show that higher LTV-caps lead to larger consumption responses to income shocks, consis-

[1] Some prominent examples include Mian and Sufi (2011), Eggertsson and Krugman (2012), Korinek and Simsek (2016), Farhi and Werning (2016) and Mian, Sufi, and Verner (2017).

[2] The minority of mortgages (around 25 %) in Norway allow for direct home equity withdrawal.

[3] Key theoretical contributions to the literature, such as Korinek and Simsek (2016) and Farhi and Werning (2016), do not feature this distinction as these models do not include illiquid wealth.

[4] In Norway lenders have full recourse to borrowers' assets and income for life. As a result homeowners have strong incentives to avoid default and mortgage defaults have been consistently low for an extended period. Even during the 1988-1992 Norwegian banking crisis, the losses on mortgages were not severe for Norwegian banks. More generally, several papers (e.g. Ghent and Kudlyak, 2011) have shown that recourse deters default.

tent with the fact that the reform led households to reduce their liquid savings, resulting in smaller buffers to smooth income fluctuations. Most of the amplified consumption response is driven by durables (75%), while the remainder is driven by non-durables. In contrast, we do not pick up any significant change in the consumption response to wealth shocks. These findings are especially relevant for households that are typically impacted by LTV restrictions. As we show, these households are more likely to face job loss and have fewer assets available to mitigate the impact of income fluctuations. Using our estimates as inputs in a simple analytical framework, along with measures of income and wealth volatility from micro- and macro-data, we find that LTV-caps are unlikely to stabilize household consumption through changes in household balance sheets. In fact, our estimates suggest that, if anything, household consumption is likely to become *less* stable due to the detrimental impact of lower liquidity. Still, it is possible that LTV-caps have stabilizing effects through other channels, the most plausible one being through reduced house price volatility. However, we find limited support for this channel in the data.

We combine data from three separate sources, which are merged together to provide a comprehensive overview over relevant household behavior. First, the Norwegian tax data provides detailed and third-party-reported information on income, wealth and debt. Second, housing transaction data allows us to identify house buyers and observe the exact purchase price of the house. Third, to measure consumption, we rely on data on electronic payments from NETS Branch Norway. Our consumption data is available at the weekly frequency and covers debit card transactions, detailed at a wide range of consumption categories, for all Norwegian residents. Debit cards are by far the most common mean of electronic payments in Norway and accounts for approximately 80% of all card transactions. In addition to covering debit card transactions, our consumption data also covers bank wire transfers (invoice payments) from households to firms, including for instance monthly payments of credit card bills. All data is aggregated to the household level, and we restrict the sample to exclude the self-employed. The Norwegian Financial Supervisory Authority introduced a maximum LTV-level of 90 percent in mid-2010, and then lowered this to 85 percent from the beginning of 2012. We study the effect of both of these policies.

Our empirical analysis consists of three main steps. In order to estimate the impact of LTV-caps on household balance sheets in a difference in differences setup, we need a treatment indicator. The first step in our analysis is therefore to obtain a measure of how the regulation affected a given household, by predicting LTV-ratios based on pre-reform information on demographics, income and wealth. We define households as treated if they have a predicted LTV-ratio in excess of the regulatory cap. The intuition being that these are the households who would have preferred a high LTV-ratio in absence of the regulation, but who are now constrained to choose a lower LTV-ratio.

To evaluate the precision of our treatment classification, we test the predictions in non-reform years. 70% of households are correctly classified, and the majority of the households which are not correctly assigned have actual LTV-ratios which are “close” to their predicted LTV-ratios. Due to the inherent measurement error in our treatment indicator, we confirm that all our results are robust to two alternative treatment measures. The first one being simply a continuous measure of predicted LTV-ratios, and the second one being a semi-continuous measure. For the latter measure, households with predicted LTV-ratios below the cap are considered non-treated, while households with predicted LTV-ratios above the cap are assigned a treatment intensity based on distance to the cap.

As a second step, we use the treatment indicator in a difference-in-differences setup, comparing treated and non-treated households before and after the reform.[5] Starting with the extensive margin results, we find that the LTV-cap reduced house purchase probabilities by six percent in the year the regulation was introduced. Moving on to the intensive margin, we document a decrease in LTV-ratios of 2.4 percentage points or just above three percent. Non-student debt conditional on purchase falls by more than ten percent, whereas house purchase prices fall by roughly eight percent. The reduction in debt also leads to a significant reduction in interest expenses.

In addition to the reduction in household leverage, we also identify a decrease in liquid assets of ten percent. This implies that households affected by the regulation are left with smaller financial buffers following a house purchase. This response is intuitive, as the LTV-restrictions imply that a higher downpayment is needed for a given house purchase. We use an event study setup to show that the decrease in liquid savings is in fact quite persistent, showing no sign of convergence even four years after the purchase. Our results thus indicate that mortgage regulation has a beneficial impact on leverage and a detrimental impact on liquidity.

We perform several robustness checks to confirm these results. Importantly, house price growth fluctuates quite noticeably in our sample period, with a modest house price decline during the financial crisis, and a strong rebound thereafter. To make sure that our results are not driven by a differential effect of house price growth on our treatment group, we also report results in which the treatment indicator is interacted with the change in house prices. All our main results are robust to this specification. The same also holds for lending rates – which is another important macroeconomic variable likely to affect households home purchase decisions. Further, we report placebo tests for all our results, confirming that our findings are unique to reform years.

Third and finally, we evaluate the impact of LTV-caps on household consumption volatility in light of the documented declines in leverage and liquidity. This is crucial as it addresses both the main argument used for LTV-regulation by policy makers, as well as the main theoretical justification. When the Norwegian Financial Supervisory Authority implemented LTV-guidelines, their main concern was that high household debt was making households increasingly vulnerable to adverse shocks such as "interest rate increases, unemployment and income reductions" (FSA 2010). This mirrors the theoretical literature on macroprudential regulation, where an important purpose of such regulation is to reduce aggregate demand externalities due to for instance income

[5]Since our treatment indicator is estimated, we bootstrap the standard errors to account for the first stage estimation uncertainty.

shocks, wealth shocks or forced deleveraging (Farhi and Werning, 2016; Korinek and Simsek, 2016). Directly measuring the size of such externalities is outside the scope of this paper, but the effect of LTV restrictions on the size of these externalities is likely to depend crucially on how LTV-caps affect households' demand responses to adverse shocks.

Several papers document that household's marginal propensities to consume out of income shocks decrease in liquidity (Kaplan and Violante (2014), Fagereng, Holm, and Natvik (2021)). We start by testing whether this is evident also in our setting. Specifically, we compare households who purchased a home right before and right after the requirement, and who then became unemployed in the subsequent year. We find that households who purchased right after the reform, and who therefore have a reform-induced reduction in liquidity, experience larger consumption declines upon unemployment – consistent with findings from the literature. Approximately 75% of the additional consumption decline is driven by durables, defined as furnishing, utilities and vehicles, where especially the two former consumption categories show large responses. The remainder of the additional consumption decline is driven by non-durable spending, and this decline is in turn driven especially by expenses on food and restaurants.

While liquidity is important for the reaction to income shocks, lower leverage is important for the reaction to wealth shocks, see for instance Mian, Rao, and Sufi (2013). To explore the role of this in our data, we consider two important wealth changes for households: house price changes and changes in stock market wealth due to international stock market movements. We follow the same procedure and compare the consumption response of households who purchased a home right before and right after the requirement, and who experienced a wealth change in the subsequent year. We find that the two household groups in general have similar responses to wealth changes across our broad set of consumption categories, suggesting that the reduction in leverage does not have a stabilizing impact on consumption. Note however, that this comparison only captures the behavior of recent house buyers, and thus does not take the extensive margin response into account.[6]

In order to also include the effect of reduced house purchase propensities, we use a simple analytical framework to evaluate the net impact on consumption volatility. The calculations rely on i) our estimated impacts on leverage and liquidity (through both the intensive and the extensive margin), ii) the impact of changes in leverage and liquidity on marginal propensities to consume (MPCs), and iii) the volatility of income and wealth, i.e. the size of shocks to these variables. To quantify the sensitivity of MPCs to leverage and liquidity, we use two alternative approaches. First, we rely on estimates from the literature. Second, we use our own estimated consumption responses to income and wealth shocks. To quantify the volatility in income and wealth, we consider

[6]One caveat to note regarding this conclusion is that our sample does not encompass a period characterized by significant decreases in house prices. Several studies, such as those by Campbell and Cocco (2007), Aladangady (2017) and Berger et al. (2018), have indicated that MPC varies depending on borrowing constraints. This aligns with the results of a recent study on Norwegian data conducted by Aastveit et al. (2024). They examine regional disparities in house prices, stemming from the oil price shock of 2014-2015, to analyze the impact of substantial fluctuations in house prices on household consumption.

Norwegian micro and macro data, also confirming that the figures are comparable to US data.

Two broad conclusions emerge. First, consumption volatility for home buyers *increases*. This is driven by a relatively large decline in liquidity, which outweighs the relatively modest decline in leverage. Second, consumption volatility for the average household remains roughly unchanged. This is driven by a relatively large reduction in leverage for the households that do not buy a new home as a result of the LTV-caps, i.e. the extensive margin. Even for the average household however, consumption volatility does not fall - if anything it increases very slightly.

The result that household consumption does *not* become more stable as a result of LTV-caps is robust to different measurement assumptions about the volatility of house prices and income. However, it rests on the assumption that these volatilities are unaffected by the regulation. This might not hold, as LTV-caps could potentially reduce the volatility of house prices, see for instance Laufer and Tzur-Ilan (2021). To investigate whether LTV-caps affect house price volatility, we compare house price growth in municipalities with many affected households to those with few affected households, pre- and post regulation. Our difference in differences estimate is negative but not statistically significant. Still, we extend our analysis to also include this (insignificant) dampening impact on house price volatility. The results in this case are mixed, and consumption volatility may increase or decrease depending on the assumptions made. In any case however, the effect is quantitatively trivial, implying a change of maximum 0.1%. We conclude that LTV-caps are unlikely to improve consumption volatility through changes in household balance sheets, and that any positive effect must come through other channels, such as lower house price volatility.

Contribution to the literature Our paper contributes to the empirical literature on the consequences of macroprudential policies by using administrative household level data to i) document a trade-off between lower leverage and a persistent reduction in liquidity, and ii) quantitatively comparing the impact of these two effects on household's consumption responses to adverse shocks. Until recently, the literature mainly used aggregate data to evaluate the impact on house prices, household debt and bank lending.[7] Recently however, a handful of papers have used micro data – mostly in the form of loan level data – to study the effect of borrower-based macro-prudential policy. We discuss the findings from these recent papers below. Relative to the literature, we make two novel contributions. First, we document a *persistent* decline in household liquidity in response to LTV-restrictions. Second, as far as we know, we are the first to use high-quality consumption data to study whether LTV-caps make household consumption more stable – the key motivation behind this type of regulation.

Acharya, Bergant, Crosignani, Eisert, and McCann (2022) use loan level data from Ireland to study the impact of loan-to-value and loan-to-income requirements. They show that mortgage

[7] See Corbae and Quintin (2015); Greenwald (2018); Claessens, Ghosh, and Mihet (2013); Vandenbussche, Vogel, and Detragiache (2015); Kuttner and Shim (2016); Cerutti, Claessens, and Laeven (2017); Akinci and Olmstead-Rumsey (2018); Borchgrevink and Torstensen (2018); Morgan, Regis, and Salike (2019).

credit is reallocated from more constrained areas to less constrained areas, inducing a dampening effect on house price growth. Peydró, Rodriguez-Tous, Tripathy, and Uluc (2023) use loan level data from the UK and also find dampening impacts on credit and house price growth.[8] Tracey and van Horen (2022) also use loan-level data for the UK and find that a relaxation of the down-payment constraint increased access to houmeownership, especially for young households.[9] Relatedly, Tzur-Ilan (2023) use data from Israel and finds that LTV-limits affect both housing affordability and location. Bolliger et al. (2022) find similar results for Switzerland. While our findings are certainly consistent with a dampening effect on credit and house price growth, we focus on the impact on household behavior, documenting significant responses in household balance sheet items and consumption responses to shocks.

DeFusco, Johnson, and Mondragon (2020) use loan level data and a bunching design to study the impact of a US debt-to-income requirement on credit volumes and prices. They find modest price effects, combined with relatively large quantity effects along both the intensive and extensive margin. Eerola, Lyytikäinen, and Ramboer (2022) do a similar analysis on Finnish data, further documenting that the reduction in mortgage debt was accompanied by an increase in other types of debt. Consistent with these findings, we document a reduction in house purchase probabilities and a reduction in household leverage conditional on a house purchase. Further, we contribute by also evaluating the liquidity impact of the regulation, and by quantitatively comparing the beneficial leverage effect to the detrimental liquidity effect.

The paper most similar to ours is Van Bekkum, Gabarro, Irani, and Peydró (2019). In ongoing work, the authors study the impact of a Dutch LTV-cap of 106% using household level data, and find a dampening effect on LTV-ratios and debt, as well as a short-lived decline in liquidity. In their setting however, households have incentives to quickly increase their liquid asset holdings by maximizing their mortgage, due to an extraordinarily generous mortgage interest reduction.[10] We show that, in a setting without this institutional feature, the negative liquidity effect is in fact highly persistent. More fundamentally, our unique and comprehensive consumption data allows us to directly study how household consumption responses to adverse shocks are affected by LTV-caps, and to quantify the net impact on consumption volatility.

[8] Epure, Mihai, Minoiu, and Peydró (2018) also use loan level data to study the impact of LTV-regulation, but their focus is mainly on foreign exchange and local currency loans in an emerging economy setting.

[9] Fuster and Zafar (2016, 2021) have used surveys for the US to also highlight the relevance of down-payment constraints for homebuying.

[10] In fact, according to the European Commission report "Tax Reforms in EU Member States 2014", at the time of the regulation, the Netherlands had the lowest marginal cost of investing in housing among all EU-countries, due to the especially high interest rate deductibility. In addition, Dutch LTV-restrictions were set to increase year-by-year going forward, making home equity less liquid, and again increasing household's incentives to quickly rebuild their liquid assets.

2 Data

We use data from three different sources. The first major data source is Norway's administrative tax records, covering the universe of tax filers in the period 2003-2017. Since Norway levies both income and wealth taxes, the data from the tax registry provides a complete and precise account of household income and balance sheets over time. Moreover, most of the data is provided by third parties, such as employers and banks. The tax data is merged with the second major data source, housing transaction data from the Land Registry. This data allows us to precisely identify home buyers in a given year and accurately measure housing wealth at the time of purchase. Finally, we merge these two data sets with the third major data source; data on consumption expenditures. This data is provided by NETS, an international provider of payment -, card - and information services in Scandinavia, the Baltics and Switzerland. It contains information on debit card payments through BankAxept for the universe of Norwegian residents. During our sample period, BankAxept accounts for approximately 80% of total card transactions in Norway and is an open system any bank in Norway can join and to which nearly all firms are connected.[11] In addition, the consumption data also contains information about invoice payments and direct remittances. The data is at the individual x week x zip-code level for 26 different consumption categories based on COICOP.[12]

We aggregate our merged data to the household x year level, and exclude the household if the household head is self-employed. In Norway, labor and capital income is taxed at the individual level, while the wealth tax is levied at the household level. Because we do not observe mortgage debt directly – only total debt and student debt – excluding self-employed households makes it less likely that we are including business related debt in our measure. However, we still have to worry about incorrectly including other sources of debt, such as consumer credit and car loans. While we cannot separate mortgage debt from other non-student debt in the micro data, we do a simple adjustment in which we subtract average unsecured debt when calculating LTV-ratios. Specifically, we define $\text{Mortgage debt}_{it} = \text{Total debt}_{it} - \text{Student debt}_{it} - \overline{\text{Unsecured debt}}_t$. Fagereng, Guiso, Malacrino, and Pistaferri (2020) show that the fraction of unsecured debt is fairly constant among high-leveraged households. Still, while mismeasurement of mortgage debt could potentially add some noise to our LTV-ratio results, it should not directly affect our results related to house purchase probabilities, non-student debt uptake, liquid asset holdings, and the reaction to adverse income and wealth shocks.

In addition to studying the impact on LTV-ratios, non-student debt, house purchase prices, and interest expenses, we also study the impact on liquid savings. In our baseline specification,

[11] The aggregate debit card transactions data closely resembles the national accounts data for household consumption and serves as an early and reliable real-time indicator for household consumption in Norway, see Aastveit, Fastbø, Granziera, Paulsen, and Torstensen (2023).

[12] "The Classification of individual consumption by purpose, abbreviated as COICOP, is a classification developed by the United Nations Statistics Division to classify and analyze individual consumption expenditures incurred by households, non-profit institutions serving households and general government according to their purpose. It includes categories such as clothing and footwear, housing, water, electricity, and gas and other fuels." (Eurostat 2023).

liquid savings are proxied by bank deposits, although we also consider total financial assets. Bank deposits are the most common saving form in Norway, and median bank deposits in our sample are almost ten times as large as median holdings of all other financial assets in the years surrounding the regulation.[13]

For parts of our analysis, we focus exclusively on first-time buyers. First-time buyers are defined as individuals who in the year of their house purchase did not previously own any housing wealth and did not previously purchase a house. For this group, we can reduce the concern that our debt measure includes unsecured debt by considering the *change* in non-student debt from the previous year, as they are assumed to not have had any mortgage debt previous to their house purchase. However, any unsecured debt uptake in the year of the house purchase would still be included. Reassuringly, measured LTV-ratios are relatively insensitive to whether we use non-student debt or the change in non-student debt for this group.

	2009				2013			
	Mean	25th	50th	75th	Mean	25th	50th	75th
LTV (%)	88	76	90	99	85	75	85	96
DTI	3.5	2.4	3.1	4.0	3.8	2.6	3.4	4.3
Non-student debt	331,000	218,000	284,000	387,000	430,000	281,000	369,000	502,000
House purchase price	373,000	233,000	303,000	431,000	496,000	310,000	414,000	578,000
Interest expenses	11,000	5,000	9,000	14,000	13,000	6,000	11,000	17,000
Bank deposits	33,000	5,000	15,000	35,000	41,000	7,000	20,000	45,000
Other financial assets	47,000	0	2,000	7,000	54,000	0	2,000	7,000
Pre-tax income	115,000	70.000	101,000	144,000	136,000	82,000	118,000	168,000
Age (years)	36	27	33	42	36	27	33	43
First-time buyers (%)	54	0	100	100	43	0	0	100
N	36,993				47,112			

Table 1: Summary statistics - balance sheet and housing market outcomes.
Notes: Summary statistics for house buyers with $LTV \in [60, 110]$ in USD (USD/NOK=5.8) if not otherwise stated, for year 2009 and year 2013. All amounts in USD are rounded to the closest 1000.

Our analysis relies on different parts of the data. When investigating the impact on house purchase probabilities, i.e. the extensive margin, we use the full panel. The intensive margin effects are however only defined for home buyers. Thus, when investigating the intensive margin effects, we use a repeated cross-section of home buyers in any given year. Table 1 reports summary statistics on balance sheet and housing market outcomes for 2009, the year before the implementation of the first LTV-limit, and 2013, the year after the implementation of the second LTV-limit. The

[13]In recent years, house purchase saving accounts (so called ”BSU” accounts) have gained popularity. These accounts offer attractive interest rates and tax deductions for individuals aged 33 or younger, and will be included in our bank deposit measure. We consider these savings to be roughly as liquid as other forms of bank deposits. If individuals decide to spend these savings on non-house expenditures, the only cost is that the tax deductions on the amount spent on non-housing needs to be reimbursed, and that the remaining funds are transferred into a normal saving account.

institutional details of the regulation are discussed in detail in the next section. The table includes information on home-buyers balance sheets, house purchase prices, age, LTV-ratios, Debt-to-Income (DTI) ratios, as well as the fraction of first-time buyers. Values are expressed in USD, using a fixed exchange rate of Norwegian Kroner (NOK) to USD of 5.8.[14]

From 2009 to 2013, we see a decline in the median LTV-ratio from 90 to 85 percent. Likewise, LTVs for the 25th and 75th percentile also fall, suggesting that the LTV-restrictions implemented during this period had a broad impact on the distribution. Interestingly, we also observe that the fraction of first-time buyers falls from 54 to 43 percent. Furthermore, we observe an increase in house purchase prices. House price growth in Norway has generally been quite strong, with average annual growth rates exceeding six percent over the past twenty years, see Appendix Figure A2.[15] The increase in house purchase prices is accompanied by an increase in household debt. While the income distribution of households also appears to shift to the right over time, the increase is smaller than the increase in debt, resulting in higher debt-to-income levels.

	Mean	Median	Std. dev	N
Total consumption				
Total consumption	39,893	28,285	52,691	1,997,288
Non-durable consumption				
Total non-durable consumption	24,940	18,710	33,167	1,997,288
Food and restaurants	11,577	8,954	20,577	1,997,288
Clothing and footwear	2,449	1,465	4,082	1,997,288
Recreation and culture	5,574	3,060	14,107	1,997,288
Consumption, misc.	5,338	3,774	8,754	1,997,288
Durable consumption				
Total durable consumption	14,952	8,386	30,222	1,997,288
Furnishing	2,719	1,406	5,642	1,997,288
Vehicles	2,462	34	12,199	1,997,288
Utilities	9,770	5,639	23,471	1,997,288

Table 2: Summary statistics - consumption.

Notes: Summary statistics for the population in USD (USD/NOK=5.8) for 2009. Durable consumption consumption is defined as the sum of vehicle expenses, furnishing and utilities, while non-durable consumption is defined as purchases of food and restaurants, clothing and footwear.

Another key variable in our analysis is consumption. In Table 2 we show population wide summary statistics for the major consumption categories. Total annual consumption for the population

[14] 5.8 was the average exchange rate in 2012, see https://www.dnb.no/bedrift/markets/valuta-renter/valutakurser-og-renter/historiske/hovedvalutaer/2012.html. Note, however, that there have been substantial fluctuations in the exchange rate over the sample period.

[15] While house prices fell in 2008, the rebound following the financial crisis was fairly quick, with relatively high house price growth in the reform years 2010-2012.

is around 40'000 USD. Non-durable consumption makes up the largest share (65%), and includes purchases of food and restaurants, clothing and footwear, recreation and culture and miscellaneous. The remainder is durable consumption, defined as the sum of vehicle expenses, furnishing and utilities. Within the more detailed subcategories, food and restaurants and utilities are the two largest consumption groups in terms of average values.

3 Institutional background

In this section we first describe the Norwegian borrower-based mortgage regulation, with emphasis on the LTV-restrictions implemented in 2010 and 2012. We then illustrate how LTV-distributions have changed over time, and discuss the bunching which takes place at the regulatory limits. Finally, we provide some background information on how household balance sheets typically evolve around a house purchase. We show that the average house purchase to a large extent is debt-financed, and that households typically increase liquid savings prior to a house purchase, before reducing liquid assets by more than this increase once the house purchase is realized.

Following the financial crisis, several countries implemented stricter mortgage regulation in terms of maximum levels for loan-to-value ratios when purchasing a house. In fact, according to Alam, Alter, Eiseman, Gelos, Kang, Narita, Nier, and Wang (2019), LTV-regulation is the most used macroprudential policy tool, with 25 of the 36 advanced economies studied in the paper having implemented this type of regulation. In Norway, the Financial Supervisory Authority (FSA) introduced national guidelines in March 2010, stating that mortgages should normally not exceed 90 percent of the market value of the house. The guidelines further stated that the FSA expected banks to be in compliance with the new guidelines by fall the same year, and that failure to do so could result in higher capital requirements.

In December 2011, the guidelines were updated, and the maximum LTV-level was reduced from 90 to 85 percent.[16] This is comparable to other advanced economies where the LTV-limits mostly range from 80 to 95 percent.[17] This time, the FSA stated that they expected banks to adjust to the new requirements immediately, and that they would start their supervisory work with regards to the new guidelines in early 2012. As in the initial guidelines, failure to comply could lead to higher capital requirements for the given bank. The requirements specified in the original and the updated guidelines were not hard requirements, in the sense that banks were given some room to deviate. Specifically, a bank could provide a loan with an LTV-level in excess of the maximum level if i) there existed additional collateral, or ii) if the bank had undertaken an extraordinary risk assessment.

In this paper, we study the two LTV-restrictions introduced in March 2010 and December 2011. Because the tax data is annual, we define the pre- and post-periods on an annual basis as well.

[16] In addition, the guidelines imposed that interest only loans should have an LTV-ratio of 70 percent or below.
[17] One exception, however, is the Netherlands where the LTV-limit was set to 106 percent.

That is, for the first LTV-cap, we define 2010 as the first year in the post-period, although the regulation was only enforced from mid-year, implying that the full effect might not show up until 2011. In principle, we could have identified house buyers based on whether they purchased a house before or after March 2010 from the Land Registry data. However, this means that we would be selecting on individuals who purchase a house at different times of the calendar year, which might be problematic. Also, because the FSA stated that they expected banks to be in compliance with the requirement by the fall the same year, it is not clear where to draw such a monthly cut-off. For the 2011 guidelines, the definition of pre- and post-periods is more straight-forward. Banks were supposed to be in compliance with the new guidelines by January 2012, and so we consider 2012 as the first year in the post-period. Because we consider the second LTV-restriction to be cleaner, we will focus on the results from this regulation in our discussions, but note that our findings are similar across the two regulatory changes.

When interpreting our results in a broader context, some additional institutional background might be useful. First, we note that homeownership rates in Norway are close to the OECD median, and somewhat higher than in the US (Causa, Woloszko, and Leite (2019)). House prices and mortgage debt are at quite high levels, and the strong growth in recent years has been an important part of the motivation behind borrower-based macroprudential policies such as LTV-restrictions. At the time of the LTV-regulation being introduced, the mortgage rate deductibility in Norway was 28%, which is close to most other European countries with similar deduction schemes – see the *Tax Reforms in EU Member States 2014* report.

In terms of income risk, unemployment levels in Norway are generally among the lowest in the OECD-area, typically falling below four percent – at least prior to the oil price collapse of 2014 (Juelsrud and Wold, 2019). OECD data on 2015 unemployment insurance replacement rates from the *Tax and Benefit Systems: OECD Indicators* shows that out of the 40 countries included, Norway is ranked as number 18, i.e. close to the OECD median. All in all, we believe the institutional setting to be relatively similar to that in many other countries which have implemented, or are considering implementing, similar regulatory measures.

3.1 LTV-distributions

Simply plotting the raw LTV-distributions gives us a good indication that the regulatory restrictions are indeed affecting LTV-ratios. The left panel of Figure 1 depicts LTV-distributions prior to and following the initial regulation, i.e. the LTV-cap of 90% introduced in mid-2010. While there is a substantial mass of mortgages with LTV-restrictions at roughly 100% in both cases[18], there is clear

[18] There are at least three possible explanations for why there is substantial mass above the regulatory LTV-limits: i) the LTV-restrictions were not strict limits, and banks could chose to grant mortgages with higher LTV-caps as long as they did an extraordinary risk assessment, ii) because we do not observe the LTV-ratio as measured by the bank, there might be measurement error, especially related to non-mortgage debt, and iii) households might have additional collateral or financial support which we do not observe, such as parental support.

indication of reform-induced bunching at just below 90% for the 2011-distribution. The panel to the right in Figure 1 depicts LTV-distributions prior to and following the subsequent LTV-tightening, i.e. the LTV-cap of 85% introduced at the end of 2011. In this case, we see that the bunching which initially occurred right below 90% is now shifted to right below 85%. We have also included the long-term distribution, here captured by the LTV-distribution in 2017, to illustrate that the bunching seems to intensify over time. This could indicate that households and banks take several years to fully adjust to the regulation. If so, the total impacts of the reform might be larger than the ones we identify here, which mostly focus on short-run responses.[19]

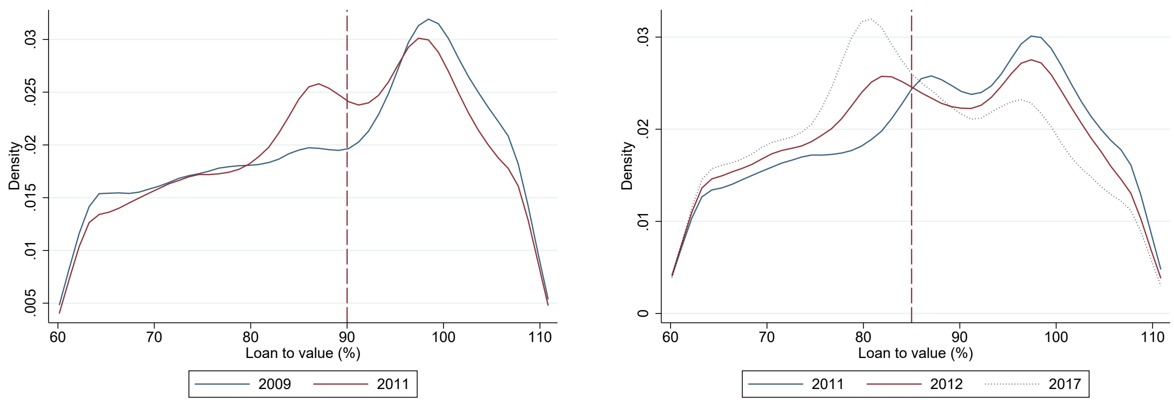

Figure 1: LTV-distributions by year.
Notes: LTV-distributions (%) for house buyers with $LTV \in [60, 110]$ by year. LTV is defined as non-student debt less average unsecured debt holdings, relative to house purchase price.

4 Effects of LTV-regulation

We now move on to estimating the impact of LTV-regulation on household balance sheets, as this is key to understanding how household consumption volatility is affected. Considering first the extensive margin, we find that affected households are significantly less likely to purchase a house once the regulation is introduced. Moreover, this negative effect on house purchase probabilities is driven entirely by households with low liquid savings, who might have a harder time meeting the new downpayment requirement.

In terms of intensive margin effects, we find that affected households respond to the regulation by reducing LTV-ratios, debt and interest expenses. This reduction in leverage is likely to make household demand more resilient toward negative wealth shocks. However, we also find a persistent reduction in liquid assets, which is likely to make household demand *less* resilient towards negative income shocks. In Section 5, we attempt to evaluate the net benefit of these two forces, arguing

[19]Note that the LTV-distributions for earlier years can be used to test for anticipatory effects. We do not find support for this in the data.

that the detrimental impact of lower liquidity outweighs the beneficial impact of lower leverage.

4.1 Methodology

Predicting LTV-ratios To estimate the causal impact of LTV-regulation we need a treatment indicator, telling us which households are likely to be affected by the regulation. Here we follow the same approach as in Van Bekkum, Gabarro, Irani, and Peydró (2019), and compare individuals with different predicted LTV ratios prior to and following the requirements in a difference-in-differences analysis. Our identification strategy thus relies on households with high predicted LTV-ratios having a similar change in outcome variables over time, i.e. similar time trends, as households with low predicted LTV-ratios *in absence* of the reform.

We start by using past data to predict which households are likely to take up a mortgage with an LTV-ratio in excess of the maximum level. That is, in the year prior to the requirement, we regress LTV-ratios on several current demographic variables, several current and lagged income variables, and several lagged wealth variables – see equation (1). The demographic variables include age, age squared, gender, zip code and household type. The current period income variables include pre-tax income and post-tax income, while the lagged income variables include pre-tax income, post-tax income and interest income. The lagged wealth variables include bank deposits, financial wealth, student debt, non-student debt and housing wealth.

$$LTV_{i,t} = \alpha + \beta_1 Demographics_{i,t} + \beta_2 Income_{i,t} + \beta_2 Income_{i,t-1} + \beta_2 Wealth_{i,t-1} + \epsilon_{i,t} \tag{1}$$

Given the predicted LTV-ratios, $L\hat{T}V_i$, we define a dummy variable $L\hat{T}V_i^{high}$, which is equal to one for households with predicted LTV-ratios above the limit and zero otherwise. We can test the precision of our predicted LTV-ratios in non-reform years, to assess the degree of measurement error.[20] Because our treatment indicator is estimated, we bootstrap the standard errors to account for the first stage estimation uncertainty. Specifically, we draw 1,000 samples with replacement of equal size as our original sample. We then use the variation in the estimated coefficients to calculate the standard errors.

In our baseline, treatment status will be defined based on the binary indicator variable $L\hat{T}V_i^{high}$. This captures the notion that only households who would optimally choose an LTV-cap above the limit are directly affected by the regulation. However, we also consider several alternative treatment measures. To allow treatment intensity to depend on distance to the regulatory limit, we consider a continuous treatment indicator $L\hat{T}V_i$. We have also tried including $L\hat{T}V_i$ in squared form, but this does not show up as statistically significant. Moreover, one could argue that distance to the regulatory cap matters only for households with high preferred LTV-ratios. To capture this idea

[20]Note that measurement error in our treatment indicator will bias our estimates towards zero. Our baseline estimates could therefore be viewed as a lower bound of the effect of the regulation.

we use a semi-continuous treatment indicator, which is zero for households with $L\hat{T}V_i$ below the cap, and given by the distance to the cap for households with $L\hat{T}V_i$ above the cap. Reassuringly, our results are robust to all of these different treatment measures.

Which households are affected? A natural question for interpreting our results is what characterizes households that are likely to be affected. In Tables 3 and 4, we report the results from testing the differences in means of different observables for those with predicted LTVs above the relevant regulatory thresholds and those below, in the year prior to the regulation. In general, affected households are more likely to be young, first-time buyers, they have higher propensities to become unemployed and they have lower income and liquid savings. Overall, this is consistent with the notion that LTV-restrictions primarily affect households that experience more risk, have limited within-household insurance opportunities and have lower asset holdings.

	$L\hat{T}V_i >= 0.9$	$L\hat{T}V_i < 0.9$	Difference	S.E.	Obs.
Single-person household	0.41	0.33	0.074***	(0.005)	36,993
Propensity to become unemployed	0.078	0.072	0.006**	(0.003)	36,993
First-time buyer	0.75	0.44	0.31***	(0.005)	36,993
Age	28.6	39.8	-11.2***	(0.11)	36,993
Pre-tax income	97,000	124,000	-27,000***	(1365)	36,993
Deposits	21,000	39,000	-18,000***	(583)	36,993

Table 3: Differences in observables according to predicted LTV.
Notes: Difference in means of characteristics for households with $L\hat{T}V_i >= 0.9$ and $L\hat{T}V_i < 0.9$. Values in in USD (USD/NOK=5.8) if not otherwise stated, for the year 2009. All amounts in USD are rounded to the closest 1,000.

	$L\hat{T}V_i >= 0.85$	$L\hat{T}V_i < 0.85$	Difference	S.E.	Obs.
Single-person household	0.40	0.313	0.085***	(0.005)	44279
Propensity to become unemployed	0.094	0.085	0.010***	(0.003)	44279
First-time buyer	0.73	0.34	0.38***	(0.005)	44279
Age	28.4	39.6	-11.2***	(0.099)	44279
Pre-tax income	100,000	136,000	-36,000***	(819)	44279
Deposits	23,000	43,000	-20,000***	(572)	44279

Table 4: Differences in observables according to predicted LTV.
Notes: Difference in means of characteristics for households with $L\hat{T}V_i >= 0.9$ and $L\hat{T}V_i < 0.9$. Values in in USD (USD/NOK=5.8) if not otherwise stated, for the year 2009. All amounts in USD are rounded to the closest 1,000.

Extensive margin Once we have the predicted LTV-ratios to use as our treatment indicator, we start by estimating the extensive margin effects of the reform according to equation (2). That is, we estimate the impact of LTV-restrictions on house purchase probabilities using a sample of all households.

$$Purchase_{i,t} = \alpha + \delta_t + \beta\, L\hat{T}V_i^{high} \times I_t^{post} + \gamma L\hat{T}V_i^{high} + \epsilon_{i,t} \tag{2}$$

The dependent variable $Purchase_{i,t}$ is a binary indicator variable equal to one if household i purchases a home in year t. Year fixed effects are captured by δ_t. The coefficient of interest β captures the relative purchase probability of households with high predicted LTV-ratios *after* the reform. Standard errors are clustered at the municipality level.

Generally, we report results using both the initial LTV-cap introduced in 2010 and the tightening of the LTV-cap in 2012. For the initial regulation, we define the post-period as year 2010 and onward. For the subsequent regulation, we define the post-period as year 2012 and onward. While we find it useful to consider both pieces of regulation, we consider the secondary tightening in 2012 to be cleaner from an econometric point of view as it was introduced in accordance with the calendar year and hence easier to identify in the tax data. Moreover, going forward, as most countries have already implemented LTV-caps, understanding how *changes* to an existing LTV-cap affects household behavior has high policy relevance.

Intensive margin We define the intensive margin effects of the reform as the balance sheet responses conditional on a house purchase. To capture these effects, we use a repeated cross-section of house buyers and estimate the impact on LTV-ratios, debt, interest expenses, house purchase prices and liquid assets in the year of purchase. The regression is specified in equation (3), where the dependent variable $y_{i,t}$ is a balance sheet outcome for a household i buying a home in year t. As before, year fixed effects are captured by δ_t. The coefficient of interest, $\hat{\beta}$, captures the relative balance sheet response of home buyers affected by the regulation.

$$y_{i,t} = \alpha + \delta_t + \beta\, L\hat{T}V_i^{high} \times I_t^{post} + \gamma L\hat{T}V_i^{high} + \epsilon_{i,t} \tag{3}$$

Identifying assumptions To interpret the key coefficients causally, we rely on a set of identifying assumptions. At the intensive margin, we assume that - absent any reform - balance sheet outcomes $y_{i,t}$ would be similar for treated vs. non-treated households, conditional on year and a treatment indicator/group fixed effect. At the extensive margin, we assume that - absent any reform - the propensity to purchase a house would be similar for treated vs. non-treated households, again conditional on time and treatment indicator/group fixed effect.

There are two key threats to the validity of these assumptions. First, there could be systematic differences in either the house purchase propensities or balance sheet outcomes for treated vs. non-treated households. To evaluate whether this is the case, we run several placebo-tests to test for significant results either prior to or following the reform. Reassuringly, we do not find similar results in non-reform years.

Second, even though the systematic evolution of the key outcomes are similar prior to and after the reform, it could be that particular macroeconomic shocks *during* the reform years explain differences across the treatment and control groups. Two potential candidates are interest rate changes and house price growth. For instance, if higher house price growth leads to more lax credit standards, which differentially affects our treatment group, that could explain potentially differences in outcomes. Similarly, if lending rates were lower (or decreasing) in the pre-period, that could potentially explain differences across the treatment and control groups. To rule out this alternative explanation, we augment our baseline specification to explicitly control for lending rates and house price growth interacted with the treatment dummy. In Section 4.3, we report the results from these exercises and show support for our identifying assumptions.

4.2 Results

We start this section by discussing the results from the LTV prediction exercise. Using the predicted LTV-ratios as our treatment indicator, we next report the extensive margin results, before moving on to the intensive margin results.

4.2.1 Predicting LTV-ratios

In order to obtain our treatment indicator, we start by predicting LTV-ratios. The full regression results from estimating equation (1) are reported in Appendix Table B1. All the demographic variables included in the regression have a significant impact on LTV-ratios. Younger homeowners have higher LTV-ratios, as do households in which the household head is male. Zip code and household type also matter, with more urban zip codes generally having higher predicted LTV-ratios. LTV-ratios are decreasing in lagged liquid assets and lagged housing wealth, and increasing in lagged non-liquid financial assets. Lagged student debt has a positive impact on LTV-ratios, while lagged non-student debt has a negative impact.

Predicting LTV-ratios attenuates our estimated coefficients by inducing measurement error in our treatment/control assignment. In order to assess the extent of the measurement error, we test how well our prediction model assigns households into high vs. low LTV-households based on years without (changes to) LTV-restrictions. Specifically, we predict LTV-ratios based on 2005 and 2006 data, and test the accuracy of assigning treatment status based on predicted LTVs in 2006 and 2007, respectively. The exercise shows that 70% of all house buyers are classified correctly, that is, they have both actual and predicted LTV-ratios above the (assumed) LTV-cap of 90%. Of the households which are falsely classified as treated (i.e. they have predicted LTV-ratios above the cap, but actual LTV-ratios below the cap), 25% have LTV-ratios at most five percentage points below the cap and 50% have LTV-ratios at most ten percentage points below the cap. In other words, most of the households that are falsely assigned as treated, have observed LTV-ratios that are in fact high and "close" to the cap.

We have also re-calculated the predictive performance when removing one or more explanatory variable from the specification, in order to better understand which characteristics are important for predicting LTV-ratios. The most important variable in terms of forecast performance is the age and wealth-variables, in line with the regression results in Appendix Table B1. However, removing age or any single wealth variable has only a moderate impact on the predictive power, suggesting that there is not one single variable which is crucial for the performance.

Motivated by the measurement error described above, we also include results based on a continuous treatment indicator $\hat{LTV}_i$, as well as a semi-continuous measure. In these cases, households which are predicted to be closer to the cap will have a higher treatment intensity than households which are predicted to be further away from the cap – regardless of whether their predicted LTV-ratio is above the regulatory cap or not.[21] Further, we have explored using standard machine learning methods such as LASSO to do the classification, without any substantial gains in forecasting performance. Overall, we judge our treatment indicator to be acceptable in terms of precision.

4.2.2 Balance sheet effects

The extensive margin In order to investigate the extensive margin effects of the regulation, i.e. whether households are less likely to purchase a (new) house, we estimate equation (2) using an indicator variable for house purchase as our dependent variable. The results are reported in Table 5, and confirm that the probability of buying a house decreases following the reform.

We start by considering the initial LTV-cap introduced in 2010. In the first column, we compare the house purchase probability in the year prior to the reform to the house purchase probability in the reform year. In this case, the coefficient estimate is negative and statistically significant, but rather small. Note however, that this implies comparing 2009 to 2010, which might be a noisy comparison as the initial LTV-cap was implemented half-way through 2010. If we instead consider the year prior to the reform and the year *after* the reform, the effect becomes a bit larger. In this case, households with high predicted LTV-ratios have a 0.1 percentage points lower probability of purchasing a house following the new regulation – a decrease of just above two percent.

Interestingly, we find larger extensive margin effects following the LTV-tightening in 2012. The results are reported in the two last columns of Table 5. Considering first the year prior to the reform and the reform-year, we see that the house purchase probability for households with high predicted LTV-ratios falls by 0.3 percentage points or more than six percent. Considering the reform year

[21] For the continuous measure this holds for all households. For the semi-continuous measure, this holds only if $\hat{LTV}_i$ is above the cap.

and the year *after* the reform yields similar results. Hence, the results from the difference-in-differences analysis suggests that especially the LTV-tightening in 2012 had non-trivial extensive margin effects.

	(1) House Purchase	(2) House Purchase	(3) House Purchase	(4) House Purchase
$\hat{LTV}^{high} \times Post^{2010}$	-0.0776***	-0.1095***		
	(0.00725)	(0.00761)		
$\hat{LTV}^{high} \times Post^{2012}$			-0.3364***	-0.3643***
			(0.00354)	(0.003644)
N	4,352,860	6,583,923	4,508,483	6,788,070
Clusters	430	431	430	431
Mean	4.66	4.66	5.20	5.22
Sample period	2009-2010	2009-2011	2011-2012	2011-2013
Year FE	Yes	Yes	Yes	Yes

Table 5: House purchase probability (%).

Notes: Results from estimating equation (2), with dependent variable house purchase probability (%). $LTV^{high} = 1$ if $\hat{LTV} > 90$ (85) for the 2010 (2012) requirement and zero otherwise. $Post^{2010} = 1$ if year$\geq$ 2010 and zero otherwise. $Post^{2012} = 1$ if year$\geq$ 2012 and zero otherwise. Standard errors are clustered at the municipality level and bootstrapped to correct for estimated regressor bias. Bootstrapping is done on based on 1000 draws. * p<0.1, ** p<0.05, ***p<0.01.

Why are the extensive margin results from the subsequent LTV-tightening in 2012 larger than those of the initial LTV-cap introduced in 2010? While we do not have a definitive answer to this, we offer some potential explanations. First, it is possible that banks and households adjusted to the new regulation over time, and that the initial guidelines were not immediately fully incorporated. In fact, part of the motivation for the tightening of the guidelines in 2012 was the result of the FSAs monitoring of the bank sectors response to the initial guidelines in 2010. Second, one could imagine that households responded to the first regulation mainly by adjusting along the intensive margin, i.e. by buying less expensive housing or by depleting more of their liquid assets. When the second and more restrictive LTV-cap was introduced shortly thereafter, this option may have seemed less attractive or attainable, causing more households to cancel or delay their home purchases. Supportive of this explanation, we note that while 49 percent of new mortgages had LTV-ratios in excess of 90 percent in the year before the initial guidelines were introduced, 59 percent of new mortgages had LTV-ratios in excess of 85 percent in the year before the revised guidelines were introduced. Hence, the 2012 guidelines were more restrictive. Finally, part of the size difference may be the result of the 2010-results being less precise due to the mid-year implementation. For this reason, we prefer the 2012-estimates.

Does the reduction in the house purchase probability indicate a transitory or permanent effect?

If households are simply postponing their house purchase one year, the effects on aggregate credit growth will be smaller compared to a state of the world in which the house purchase probability is permanently lower. Identifying the long-term effects are more challenging, and so we have focused our analysis on a short time window around the introduction of the new requirements. The results in Table 5 suggest that the negative effect on house purchases is not limited to the reform-year, but seems to persist at least into the following year as well. Moreover, in Appendix B2, we show that the age of entry into the housing market increases by between 1.5 and 3.5 years. The data is thus consistent with there being at least a somewhat more persistent effect on housing transactions.[22]

To summarize, our results indicate that affected households are 2-6 percent less likely to purchase a new house immediately following the regulation, suggesting that the LTV restrictions home purchase at the extensive margin.

The intensive margin The intensive margin results for the 2010 regulation, i.e. the balance sheet effects conditional on purchase, are reported in Table 6. As seen from the first column, affected borrowers respond to the regulation by reducing their LTV-ratios, as could be expected. On average, LTV-ratios fall by 0.9 percentage points or 1.2 percent. Affected borrowers also reduce their non-student debt holdings by almost six percent, as seen from the second column. As a result of lower debt, interest expenses also decrease. On average, interest expenses fall by roughly two percent. Also the denominator in the loan-to-value ratio is affected, as seen from the fourth column. Affected borrowers reduce the house purchase price by 5.5 percent in response to the regulation. As a result of these changes, household solvency increases.

	(1) LTV	(2) Debt	(3) Int.Expenses	(4) House price	(5) Liquid assets
$\hat{LTV}^{high} \times Post^{2010}$	-0.934***	-19,107***	-208.1	-23,150***	-3,303***
	(0.331)	(3,186)	(126.92)	(5,006)	(1,105)
N	192,529	192,529	192,529	192,529	192,529
Clusters	431	431	431	431	431
Mean	76.22	333,278	11,008	424,514	38,569
Year FE	Yes	Yes	Yes	Yes	Yes

Table 6: Balance sheet effects, 2010-regulation.

Notes: Results from estimating equation (3), with dependent variables LTV-ratios (%), non-student debt (USD), interest expenses (USD), house purchase prices (USD) and liquid assets (USD). $LTV^{high} = 1$ if $\hat{LTV} > 90$ zero otherwise. $Post^{2010} = 1$ if year$\geq$ 2010 and zero otherwise. Sample: house buyers with $LTV \in [60, 110]$. Sample period: 2007-2011. Standard errors are clustered at the municipality level and bootstrapped to correct for estimated regressor bias. * p<0.1, ** p<0.05, ***p<0.01.

[22]Further, in Table B3 in the Appendix, we show that the extensive margin results in Table 5 is driven by households with low initial liquidity.

In the final column in Table 6, we report the results for household liquidity. Affected borrowers respond to the regulation by reducing liquid assets. On average, liquid assets fall by almost nine percent following the reform. As reported in Appendix Table B4, there is also a fall in total financial wealth, but this is not statistically significant.

How persistent is this negative effect on liquid assets? Regression results using liquid assets one and two years ahead as the dependent variable indicate that the effect is not immediately reversed (see Appendix Table B4). We explore this further in an event study setup below, and show that even four years after the house purchase there is no sign of convergence.

Regression results reported in Table 7 show qualitatively similar effects from the 2012 regulation. Quantitatively however, the effects from this subsequent LTV-tightening are again larger. LTV-ratios are reduced by 2.4 percentage points, which implies a reduction of three percent. Debt falls by eleven percent, and average interest expenses by slightly more. As before, the denominator of the LTV-ratio is also affected, with average house prices falling by nine percent. Finally, liquid assets fall by ten percent as well – just slightly more than in the previous reform. As was the case before, total financial wealth is not significantly affected, but the negative impact on liquid assets persists in the years following the house purchase – see Appendix Table B5.

	(1) LTV	(2) Debt	(3) Int.Expenses	(4) House price	(5) Liquid assets
$\hat{LTV}^{high} \times Post^{2012}$	-2.365***	-41,833***	-1,894***	-43,508***	-4,656***
	(0.328)	(5,914)	(280.1)	(8,772)	(1,562)
N	222,156	222,156	222,156	222,156	222,156
Clusters	433	433	433	433	433
Mean	73.59	385,650	12,073	510,708	44,771
Year FE	Yes	Yes	Yes	Yes	Yes

Table 7: Balance sheet effects, 2012-regulation.
Notes: Results from estimating equation (3), with dependent variables LTV-ratios (%), non-student debt (USD), interest expenses (USD), house purchase prices (USD) and liquid assets (USD). $LTV^{high} = 1$ if $\hat{LTV} > 85$ and zero otherwise. $Post^{2012} = 1$ if year$\geq$ 2012 and zero otherwise. Sample: house buyers with $LTV \in [60, 110]$. Sample period: 2010-2014. Standard errors are clustered at the municipality level and bootstrapped to correct for estimated regressor bias. Bootstrapping is done on based on 1000 draws. * p<0.1, ** p<0.05, ***p<0.01.

While the reduction in LTV-ratios and debt burdens was part of the desired effect, the decrease in liquid assets may have been a less welcome side effect. The reduction in liquid financial buffers might be concerning in terms of the financial resilience of households, as a large literature has emphasized the importance of liquidity for household's consumption responses to negative shocks. See for instance the discussion of "hand-to-mouth" households in Kaplan and Violante (2014). To further explore the dynamics of liquid assets in relation to housing investments, we perform an event study with liquid assets as the dependent variable.

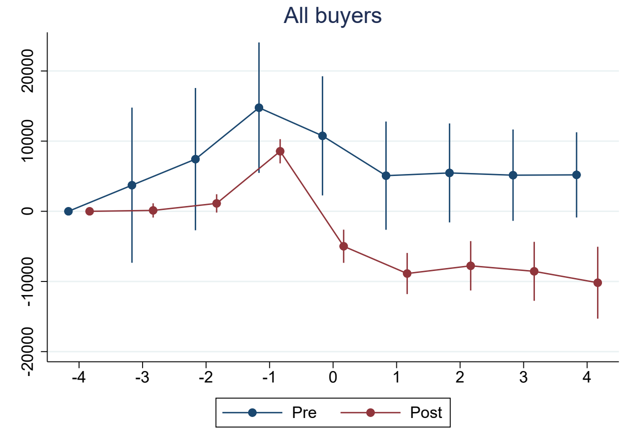

Figure 2: Liquid assets event study around house purchase, pre- and post-regulation.
Notes: Regression results from estimating equation (15), using liquid assets (USD) as the dependent variable, for households purchasing a home prior to the initial regulation (*Pre*) and following the subsequent regulation (*Post*). Households with high predicted LTV-ratios only. House purchase occurs at year $t = 0$. Year $t = -4$ is used as the base level and normalized to zero. Vertical bars correspond to 95 % confidence intervals.

Figure 2 separately depicts the evolution of liquid assets in the years around a house purchase for households who purchase a home before and after the requirements. For the event-study, we increase precision by considering the two requirements jointly. That is, we define the pre-period to be prior to the first requirement and the post period to be after the second requirement. Also, to better identify the reform-induced changes, we restrict the sample to households with high predicted LTV-ratios. The blue line captures the pre-reform buyers, and shows an increase of roughly USD 15,000 in the years prior to the purchase. This increase is partly reversed in the year of the house purchase, and in the following year liquid savings are no longer significantly different from the baseline level. The outcomes are quite different for households who purchase a home following the reform, as captured by the red line. While the increase in liquid savings prior to the reform is relatively similar, there is a larger decline in liquid assets following the purchase. Liquid assets fall by almost USD 20,000 from year $t - 1$ to year $t + 1$. Four years after the purchase, liquidity is still significantly lower than at baseline, with no sign of convergence.[23]

The results are qualitatively similar when considering first-time buyers only. This suggests that the increase in liquid savings prior to a house purchase is not (only) due to households selling an existing home before purchasing a new one. Four years after the house purchase, first-time

[23] In the Appendix Figure A3, we show that consumption increases around house purchases, consistent with for instance Benmelech et al. (2023). Consistent with the drop in liquidity, the consumption increase is slightly larger after the reform, although estimates are somewhat imprecise.

borrowers who purchased their house following the reform had roughly USD 14,000 less in liquid assets – compared to a slight increase for those who purchased their home prior to the reform. Importantly, in Appendix Table B13, we confirm that the drop in liquidity for the treated group is not something systematic about the treatment vs. control group, i.e. it is not present in non-reform years.

We discuss the economic significance of our results in Appendix C. In terms of the liquidity effect, policy makers might be interested in the distributional dimension, that is, who are the households who are severely cutting back on liquid assets. An average reduction in liquid assets of ten percent might be more worrisome with respect to financial vulnerability if it is driven mainly by large reductions in liquidity for initially vulnerable households. We find that the share of households who reduce liquid assets by at least half increases by 40% after the regulation. Moreover, the share of households who reduce liquid assets to *less than ten percent* following a house purchase increases by 50%. This group is quantitatively small, but highlights that for some households, the reduction in liquidity leaves them with virtually no financial buffers after a house purchase. See Appendix Table C1 for further details.

Another dimension of economic significance which policy makers might be interested in is the implications for aggregate credit growth. Introductions of borrower-based macroprudential policies are often preceded and motivated by rapid increases in household credit. We perform some back of the envelope calculations in Appendix C and show that aggregate household credit growth was likely reduced by around 0.3 percentage points, or four percent. This comes both from the reduction in house purchase probabilities, and from the reduction in debt uptake conditional on purchase. Estimates from the VAR-literature suggests that the reduction in aggregate credit growth is similar in size to that resulting from a 25 basis points increase in the interest rate. We view this effect as non-trivial, but relatively modest in size.

Finally, we note that the high persistence of our liquidity results stands in contrast to the short-lived response found in Van Bekkum, Gabarro, Irani, and Peydró (2019). We believe the cause to be institutional differences, in which Dutch households have incentives to increase their liquid asset holdings rather than repay their mortgage due to an *extraordinarily* high mortgage rate tax deductibility – discussed in Appendix D.

4.3 Robustness

To evaluate the robustness of our results, we explore several alternative specifications and robustness tests. First, we show that our findings are robust to alternative treatment measures. Second, we discuss whether house price growth or aggregate lending rates could be affecting our results, and interact our treatment indicator with these two macroeconomic variables. Finally, we report several placebo tests, confirming that we do not find similar results in years without regulatory changes.

Measurement error in our treatment indicator As discussed above, our treatment indicator is based on predicted LTV-ratios, and is affected by measurement error. Here we explore three robustness tests related to measurement error in our treatment indicator. First, we consider an alternative and *continuous* treatment indicator. Second, we consider a combination between the binary and the continuous treatment indicator, referred to as a *semi-continuous* treatment indicator. We focus on the 2012-regulation to conserve on space.

First, we consider an alternative and continuous treatment indicator, in which we replace $L\hat{T}V_i^{high}$ with $L\hat{T}V_i$ in the specifications in equations (2) and (3).[24] The motivation being that, even in cases when the binary assignment to treatment is incorrect, the distance between the predicted LTV-ratio and the counterfactual LTV-ratio is mostly small – see Section 4.2.1. Considering first the extensive margin effect, we find that the 2012-regulation lead to a reduction in house purchase probabilities as before – see Appendix Table B6. In terms of magnitudes, the regression results indicate that a household which has a 10 percentage points higher predicted LTV-ratio, has a house purchase probability which is almost 3 percentage points lower due to the reform.

The intensive margin results are reported in Appendix Table B7, and are again robust to the use of a continuous treatment indicator. The impacts on LTV-ratios, debt, interest expenses, house prices and liquid assets remain negative and statistically significant. In terms of magnitudes, a household which has a 10 percentage points higher predicted LTV-ratio, has a reform-induced decline in observed LTV-ratios of 3 percentage points. In addition, the household has a reform-induced decline in debt of roughly $29,000 and a reform-induced decline in liquid assets of roughly $5,000. The impact on liquid assets relative to debt is about the same proportion as in the baseline. We thus conclude that our results are robust to the use of a continuous treatment indicator.

We also consider a semi-continuous treatment indicator in which households with predicted LTV-ratios below the cap are considered non-treated, and households with predicted LTV-ratios above the cap are given a treatment intensity based on the distance between their predicted LTV-ratio and the cap. Using instead the semi-continuous treatment indicator again yields similar results. The extensive margin results are reported in Table B8, and confirm a statistically significant effect of the 2012-regulation. The intensive margin results are reported in Table B9, and show significant reductions in leverage, debt, interest expenses, house prices and liquid assets as before. Again, the relative reduction in liquid assets compared to debt is similar to our baseline results.

House price growth In addition to regulatory constraints, house price growth is an important input variable in decisions regarding house purchases – both from the household and the banks perspective. All our specifications include year fixed effects, meaning that any effect of house price growth which is common to all is accounted for in the analysis. However, there is a concern that

[24] We have also tried an alternative specification in which we include the continuous treatment indicator in squared form as well, allowing for a convex relationship between predicted LTV-ratios and reform-effects. However, the squared term is not statistically significant.

households with high predicted LTV-ratios might be differentially affected by house price changes. To evaluate whether this is the case, we augment our regressions with an additional term, in which we interact treatment status with house price growth.

A specific and relevant concern is that house price growth might be associated with lax credit standards, particularly benefiting households with high predicted LTV-ratios. If house price growth was stronger in the pre-regulation period, this could potentially lead to lower house purchase probabilities and debt uptake for our treatment group following the reform. Appendix Table B10 reports results in which treatment status in interacted with house price growth. First note that, in the specifications in which we have only one year in the pre-period and one year in the post-period (i.e. Columns 1 and 3), adding house price growth to the analysis will not have any impact on the coefficient of interest, due to the year fixed effects. In the specifications with multiple years in the pre-period and/or post-period however (i.e. Columns 2 and 4), the interaction of treatment status and house price growth will generally have an effect. As seen from the table, the house purchase impact from the initial 2010-reform increases in absolute magnitude but ceases to be significant when house price growth is accounted for. For the subsequent 2012-reform however – which is our preferred specification – the coefficient is virtually unchanged, and remains statistically significant at the one percent level.

What about the intensive margin results? As seen from Appendix Table B11, the impact on LTV-ratios, debt, interest expenses, house purchase prices and liquid assets remains negative and statistically significant when treatment status is interacted with house price growth. Interestingly however, while the negative impact on LTV-ratios and liquid assets is virtually unchanged in size, the negative impact on debt and house purchase prices is smaller once house price growth is accounted for. The results in Appendix Table B11 capture the impact of the 2012-regulation. For the case of the 2010-regulation, the coefficient estimates are even less affected, i.e. very similar to the baseline results and statistically significant.

Another macroeconomic variable which could be affecting our results is aggregate lending rates. We have repeated the above analysis, interacting treatment status with aggregate lending rates rather than house price growth, and the results are similar (available upon request). That is, the extensive margin effects from the initial 2010-regulation ceases to be statistically significant, while the extensive margin effects from the 2012-regulation are unchanged. All intensive margin results remain statistically significant, and the coefficient magnitudes are similar.

Although accounting for differential house price growth has some effect on the 2010 extensive margin results, we conclude that our results are generally robust to allowing for differential impacts of important macroeconomic variables on our treatment group.

Placebo tests We end the robustness section by reporting some placebo tests – using both pre-reform and post-reform years – to show that the results discussed above are unique to reform years. Placebo tests for the extensive margin results are reported in Appendix Table B12. During post-reform years, we do not find any significant impact on house purchase probabilities. When using pre-reform years, we do however find a negative impact on purchase probabilities. Importantly though, this effect disappears, i.e. the coefficient becomes positive and insignificant, once we allow for a differential effect of house price growth on our treatment group. This means that the extensive margin results from the LTV-tightening in 2012, which was robust to allowing for a differential house price growth effect, is indeed unique to reform years.

Placebo tests for the intensive margin results are reported in Appendix Table B13. Reassuringly, we find no significant effect on debt uptake, house purchase prices or bank deposits in non-reform years.[25] This holds both in pre-reform years and in post-reform years. There is however a negative impact on LTV-ratios, but this is driven not by a decline in debt - but by a relative *increase* in house purchase values. Hence, this is a very different mechanism than the one identified in Tables 6 and 7. We thus conclude that our balance sheet findings – lower debt uptake, house purchase values and bank deposits – are unique to the reform years.

5 Consumption responses to income and wealth shocks

We have documented a potentially important trade-off in terms of macroprudential policies such as LTV-restrictions. As intended, this kind of regulation is successful in reducing leverage, plausibly making household demand more robust to large fluctuations in asset values. However, our results also suggest a persistent decline in liquidity, plausibly making household demand *less* robust to large income fluctuations. This is especially relevant for households that are typically impacted by LTV-restrictions, as shown in Tables 3 and 4 these households are more likely to face job loss and have fewer assets available to mitigate the impact of income fluctuations. The remainder of the paper is devoted to understanding and quantifying the impact of this trade-off.

In this section, we explicitly investigate whether the reform-induced decline in liquidity and leverage have lead to differential consumption responses to income and wealth shocks. We use unemployment as a source of income shock, while we rely on changes in house prices and international stock market returns to capture wealth shocks. The key take-away from this analysis is that households with reform induced declines in liquidity have larger consumption responses to income falls resulting from unemployment. Most of the increased consumption response (75 %) is driven by an increase in the response of *durable* consumption to unemployment. In contrast, we do not find evidence that the reduction in leverage documented above has dampened the sensitivity of household demand to wealth changes. In the next section, we use our empirical estimates, combined with findings from the existing literature, to evaluate the net impact of LTV-restrictions on household consumption volatility. The main conclusion is that LTV-restrictions are *not* successful in stabilizing household consumption – at least not through changes in household balance sheets.

[25]The house purchase price coefficient is statistically significant at the ten percent level - but *positive* - in one of the two placebo tests.

5.1 Income shocks

The existing literature highlights the important role of liquidity in determining household responses to income fluctuations. In this section we confirm that this is relevant also in our setting. Specifically, we investigate whether the reform-induced decrease in liquidity affects the marginal propensity to consume (MPC) out of income losses resulting from unemployment. We find that – in line with the existing literature – consumption falls by more in response to unemployment for households which are affected by the LTV-restrictions.

In order to investigate whether households affected by LTV-requirements respond differently to unemployment spells compared to non-affected households, we use a selected sample of households with a high predicted LTV-ratio, i.e. $L\hat{T}V_i^{high} = 1$, as these are the households likely to be affected by the LTV-restrictions. We consider two alternative comparisons. In the first approach, we compare households who purchase a home right before or right after the requirement, and who become unemployed in the subsequent year. In the second approach, we also include households that did not purchase homes in the control group. For both these samples, we then estimate

$$\tilde{C}_{i,t} = \alpha_i + \delta_t + \beta\, T_i \times \text{U-Year}_{i,t} + \gamma \text{U-Year}_{i,t} + \epsilon_{i,t} \tag{4}$$

where $\tilde{C}_{i,t}$ is real total consumption, real durable consumption, real non-durable consumption, or the various components of durable or non-durable consumption. α_i captures household fixed effects and δ_t captures year fixed effects. The household fixed effects ensure that we are only capturing consumption responses relative to average consumption within the household, while the year fixed effects control for any aggregate time trends. The treatment indicator in this case, T_i is one for (unemployed) homeowners affected by the regulation. Specifically, $T_i = 1$ if a household purchased a home in 2012, and became unemployed in the subsequent year. The treatment indicator is zero for households that become unemployed and (in the first approach) that are homeowners but not affected by the regulation. The variable $\text{U-Year}_{i,t} = 1$ if household i became unemployed in year t, i.e. was employed in year $t-1$ and unemployed in year t.

Regression results from estimating equation (4) are reported in Table 8. In the first column we focus on total consumption, in the second column we focus on non-durable consumption and in the third column we focus on durable consumption. In all three cases, we see that consumption expenditures in the year of unemployment is significantly lower for homeowners affected by the regulation, i.e. households who bought a home in 2012 and became unemployed the following year. The majority of the consumption response (approximately 75%) is accounted for by *durable* consumption. The decline in durable consumption is in turn driven by lower expenses on furniture and utilities, see Table B14 in the Appendix for a more detailed breakdown of the different consumption categories.[26] For non-durables, the consumption decline is driven mainly by food and restaurants

[26] One explanation for the decline in utilities – supported by the data – is that some households go from being homeowners to renting in response to unemployment.

expenses. In terms of magnitude, the relative reduction in total consumption is economically large, and corresponds to more than 10% of average total consumption. In columns (4) - (6), we redo the analysis but also include households that are not homebuyers in the control group. The coefficients are smaller, which is natural as a large part of the control group now did not purchase a home (potentially as a consequence of the reform) and should all else equal therefore have more liquidity available to soften the drop in income. However, the general pattern persists that affected households are more responsive to unemployment in terms of their consumption.

	(1) Total consumption	(2) Non-durable consumption	(3) Durable consumption	(4) Total consumption	(5) Non-durable consumption	(6) Durable consumption
T_i × U-Year$_{i,t}$	-8129.5***	-2181.1***	-5948.4***	-2064.9***	-681.6***	-1383.3***
	(1511.5)	(552.5)	(1353.6)	(537.1)	(212.1)	(457.7)
N	27,359	27,359	27,359	467,737	467,737	467,737
Clusters	363	363	363	418	418	418
Mean	58010.5	35061.6	22949.0	59274.2	36225.2	23049.0
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
HH FE	Yes	Yes	Yes	Yes	Yes	Yes
Control group	Home buyers pre-reform	Home buyers pre-reform	Home buyers pre-reform	All	All	All

Table 8: Consumption response to unemployment.

Notes: Results from estimating equation (4), with dependent variable either total consumption, durable consumption or nondurable consumption, all in real terms and measured in USD. Durable consumption is defined as the sum of purchases of vehicles, furnishing and utilities, while non-durable consumption is defined as purchases of food and restaurants, clothing and footwear, recreation and culture and miscellaneous. T_i is the treatment indicator and equals one for households who purchased a home in 2012 and became unemployed the subsequent year. In columns (1) - (3) it equals zero for households who purchased a home in 2010 and became unemployed in the subsequent year. In columns (4) - (6) it equals zero for all other households. U-Year$_{i,t} = 1$ in the year of unemployment. Sample: households with predicted LTV-ratio above cap. Standard errors are clustered at the municipality. level. * p<0.1, ** p<0.05, ***p<0.01.

5.2 Wealth shocks

Next, we investigate whether the LTV-regulation – and the induced reduction in leverage – has affected the consumption response to *wealth* shocks. We consider changes to both house prices and stock returns, without finding any significant impact on household consumption responses.

As in the previous subsection, we focus on home buyers who buy either right before the regulation or right after, *and* who have high predicted LTV-ratios. For this sample, we estimate the following

$$\tilde{C}_{i,t} = \alpha_i + \delta_t + \beta\, T_i \times \text{wealth change}_{i,t} + \gamma \text{wealth change}_{i,t} + \epsilon_{i,t} \tag{5}$$

where T_i is defined as before and wealth change$_{i,t}$ is a measure of wealth changes in the year after the home purchase. We focus on two measures of wealth changes, i.e. changes in housing wealth and changes in stock market wealth. For the first measure, we simply use the change in the value of the primary residence of the household from year $t-1$ to t, i.e.

$$\text{wealth change}_{i,t} = \Delta\text{Value of primary residence}_{i,t}$$

For the second measure, we combine the annual return on the MSCI World Index r_t with household-level holdings of stocks and mutual funds at the end of $t-1$ to compute

$$\text{wealth change}_{i,t} = \text{stock wealth}_{i,t-1} \times r_t$$

The regression results are reported in Table 9. None of the estimated coefficients are significant, meaning that we are unable to identify any changes in *total* consumption responses, *non-durable* consumption responses or *durable* consumption responses in response to house price changes or stock wealth changes. This is perhaps not surprising, given that our estimate of the reform-induced change in leverage (for home buyers) is fairly small, see Table 6 and 7. It is also roughly consistent with the existing literature, which has generally found that the *quantitative* impact of leverage on MPCs out of wealth shocks is moderate in size. Note however, that because this analysis only included home buyers, it does not capture the impact of lower leverage resulting from *fewer* house purchases. This extensive margin effect of the reform will be part of the back-of-the-envelope calculations used to evaluate the net impact on consumption volatility in the next section.

	(1) Total consumption	(2) Non-durables	(3) Durables	(4) Total consumption	(5) Non-durables	(6) Durables
$T_i \times$ stock wealth$_{i,t-1} \times r_t$	0.0602	0.757	-0.155			
	(1.144)	(0.477)	(0.822)			
$T_i \times \Delta$Value of primary residence$_{i,t}$				-0.00284	-0.00976	0.00693
				(0.0213)	(0.00968)	(0.0168)
N	23758	23758	23758	23758	23758	23758
Clusters	361	361	361	361	361	361
Mean	59012.1	35160.1	23852.0	59012.1	35160.1	23852.0
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
HH FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 9: Consumption response to wealth shocks.

Notes: Results from estimating equation (5), with dependent variable either total consumption, durable consumption or nondurable consumption, all in real terms and measured in USD. Durable consumption is defined as the sum of purchases of vehicles, furnishing and utilities, while non-durable consumption is defined as purchases of food and restaurants, clothing and footwear, recreation and culture and miscellaneous. T_i is the treatment indicator and equals one for households who purchased a home in 2012, and equals zero for households who purchased a home in 2010. Sample: households with predicted LTV-ratio above cap. Standard errors are clustered at the municipality. level. * p<0.1, ** p<0.05, ***p<0.01.

6 The net impact of LTV-caps on consumption volatility

In this section, we use the estimates from the preceding sections combined with estimates from the literature to conduct a back-of-the-envelope calculation of the net impact of LTV changes on household vulnerability. Given the focus in the existing theoretical literature on aggregate demand externalities – as well as the reasoning used by policy makers – we focus on *consumption volatility* as our measure of household vulnerability. To the extent that LTV-caps dampen (increase) consumption volatility, it is also likely that LTV-caps dampen (increase) negative externalities arising from adverse income and/or wealth shocks.

6.1 Framework

Let changes in consumption be governed by income shocks Δy and wealth shocks Δw, as well as the respective consumption responses to these two types of shocks, i.e.

$$\Delta c = \Delta y MPC^{y} + \Delta w MPC^{w} \tag{6}$$

Because we are interested in the volatility of consumption growth, we rewrite equation (6) in terms of variances

$$\sigma^2_{\Delta c} = (MPC^{y})^2\sigma^2_{\Delta y} + (MPC^{w})^2\sigma^2_{\Delta w} + 2MPC^{y}MPC^{w}\sigma_{\Delta y}\sigma_{\Delta_w}\rho_{\Delta w,\Delta y} \tag{7}$$

In principle, LTV-restrictions might affect the volatility of consumption by altering both the MPCs out of a given shock to income or wealth, and the shock processes themselves, i.e. $\sigma^2_{\Delta y}$ and $\sigma^2_{\Delta w}$. We assume however, that LTV-caps do not affect the income shock process, but allow them to affect the wealth shock process. The intuition being that LTV-caps might plausibly impact house price volatility. Using equation (7), the effect of LTV-restrictions on consumption volatility is therefore given by

$$\begin{aligned}\frac{\partial\sigma^2_{\Delta c}}{\partial \text{LTV-caps}} &= 2\frac{\partial MPC^{y}}{\partial \text{LTV-caps}}MPC^{y}\sigma^2_{\Delta y} + 2\frac{\partial MPC^{w}}{\partial \text{LTV-caps}}MPC^{w}\sigma^2_{\Delta w} \\ &+ 2\sigma_{\Delta y}\sigma_{\Delta w}\rho_{\Delta w,\Delta y}\left(\frac{\partial MPC^{y}}{\partial \text{LTV-caps}}MPC^{w} + \frac{\partial MPC^{w}}{\partial \text{LTV-caps}}MPC^{y}\right) \\ &+ 2MPC^{y}\frac{\partial \sigma_{\Delta w}}{\partial \text{LTV-caps}}\left(MPC^{y}\sigma_{\Delta w} + MPC^{w}\sigma_{\Delta y}\,\rho_{\Delta w,\Delta y}\right)\end{aligned} \tag{8}$$

Equation (8) is our starting point for evaluating how LTV-restrictions affect consumption volatility. In the next paragraphs, we outline how we measure the various components.

6.2 Measuring the impact of LTV-caps on MPCs

MPC out of income shocks To quantify the impact of LTV-caps on the marginal propensity to consume out of income shocks, consider equation (9), in which $P = 1$ indicates a house purchase. As seen from the first line, this impact is pinned down by i) the impact of liquidity on MPC^y and ii) the impact of LTV-caps on liquidity. The latter can be further decomposed to capture both the intensive and the extensive margin. This is done in the bottom line in equation (9), in which we assume that the change in liquidity for those who do not purchase a house is unaffected by LTV-caps.

$$\begin{aligned} \frac{\partial MPC^y}{\partial \text{LTV-caps}} &= \frac{\partial MPC^y}{\partial \text{Liquidity}} \times \frac{\partial \text{Liquidity}}{\partial \text{LTV-caps}} \\ &= \frac{\partial MPC^y}{\partial \text{Liquidity}} \left(P \times \frac{\partial \text{Liquidity}|_P}{\partial \text{LTV-caps}} + \frac{\partial P}{\partial \text{LTV-caps}} (\text{Liquidity}|_P - \text{Liquidity}|_{1-P}) \right) \end{aligned} \tag{9}$$

We now discuss how we measure each component in equation (9). First, we rely on two different measures of $\partial MPC^y/\partial\text{Liquidity}$. One we take from the literature, and one we take from the results in Section 5.1. Starting with the former, Fagereng, Holm, and Natvik (2021) use Norwegian tax data to estimate MPCs out of income shocks, and how these MPCs vary with household observables such as leverage and liquidity. We use the results from Column 2 of Table 4 in Fagereng, Holm, and Natvik (2021), which says that a reduction in liquidity of \$1,000 increases the MPC out of income shocks by 0.3 cents on the dollar. In addition, we also use our estimated consumption responses from Table 8. Our estimates showed an additional decrease in consumption of \$8,130. Combining this figure with information on the average income fall upon unemployment results in an increase in the MPC of 0.31.[27] Liquid assets fell by \$4,656 according to Table 7. Our results thus imply that a reduction in liquidity of \$1,000 reduces the MPC out of income shocks by 0.07 cents on the dollar – which is lower than the results from Fagereng, Holm, and Natvik (2021).

We measure $\partial\text{Liquidity}/\partial\text{LTV-caps}$ separately for home buyers and for the average household. For home buyers, we can use the first line in equation (9), letting $\partial\text{Liquidity}/\partial\text{LTV-caps} = -4.66$, in accordance with the estimated reduction in liquidity in \$1,000 from Table 7. For the total population, we use the final line in (9), letting $P = 0.052$ in accordance with the average purchase probability from Table 5, and letting $\partial\text{Liquidity}|_P/\partial\text{LTV-caps} = -4.66$ in accordance with the estimated reduction in liquidity in \$1,000 from Table 7. Moreover, we set $\partial P/\partial\text{LTV-caps}(\text{Liquidity}|_P - \text{Liquidity}|_{1-P}) = 0$, based on the observation that liquidity in the pre-period is unaffected by house purchases according to Figure 2.[28]

[27] The average wage income in 2012 was NOK 478,942. The unemployment insurance replacement ratio was 62%, and the average unemployment duration was 10.6 months. This results in an average income fall of NOK 158,065 or \$27,253.

[28] There is a significant increase in liquidity prior to the house purchase, which is subsequently reversed once the purchase is made. Liquidity in period $t = 1$ (or later) is therefore not significantly different from liquidity in period

Summing up, this means that for home buyers, the change in the MPC^y due to LTV-caps is $-0.003*(-4.66) = 0.0140$ when we use the MPC-estimates from the literature. In other words, the MPC out of income shocks increases by 1.4 cents on the dollar. Using instead our own estimates from Section 5.1 implies a reduction of $-0.00007*(-4.66) = 0.0003$. In other words, the MPC out of income shocks increases by 0.03 cents on the dollar. For the average household, the impact on MPC^y is considerably smaller, as most households are not (potential) home buyers in a given year. For the average household, the impact is given by $-0.003*0.052*(-4.66) = 0.0007$ when we use the MPC-estimates from the literature and $-0.00007*0.052*(-4.66) = 0.00002$ when we use the MPC-estimates from Section 5.1. These figures imply that the MPC out of income shocks increases by 0.07 cents or 0.002 cents on the dollar respectively, due to the LTV-caps.

MPC out of wealth shocks To quantify the impact on MPCs out of wealth shocks, we use the same approach as above – measuring the different components of equation (10). Note that this expression again relies on the assumption that leverage for non-home buyers is unaffected by LTV-caps.

$$\begin{aligned} \frac{\partial MPC^w}{\partial \text{LTV-caps}} &= \frac{\partial MPC^w}{\partial \text{Leverage}} \times \frac{\partial Leverage}{\partial \text{LTV-caps}} \\ &= \frac{\partial MPC^w}{\partial \text{Leverage}} \left(P \times \frac{\partial Leverage|_P}{\partial \text{LTV-caps}} + \frac{\partial P}{\partial \text{LTV-caps}} (Leverage|_P - Leverage|_{1-P}) \right) \end{aligned} \tag{10}$$

First, we need a measure of how leverage affects MPC^w. Again, we rely on the same approach of using both estimates from the literature and our own estimates from Section 5.2. Fagereng, Holm, and Natvik (2021) find that leverage does not affect MPC^y, in fact the coefficient is exactly zero and precisely measured. However, we want to consider the possibility that lower leverage is in fact beneficial for consumption volatility, and so we instead rely on findings from Mian, Rao, and Sufi (2013), who find that MPCs out of housing wealth shocks are increasing in leverage. Specifically, they show in Column 3 of Table 4 in their paper, that an increase in LTV-ratios of 0.1 leads to an increase in MPC^w of 0.021 cents on the dollar, implying implying $\partial MPC^w/\partial\text{Leverage} = 0.021$.[29] However, we also use our own estimates from Table 11, in which there is no significant impact of leverage on consumption responses, implying $\partial MPC^w/\partial\text{Leverage} = 0$

As before, we measure $\partial Leverage/\partial\text{LTV-caps}$ separately for home buyers and the average household. For home buyers, the change in leverage is given by $\partial Leverage/\partial\text{LTV-caps} = -0.0237$,

$t = -2$ (or earlier).

[29]The literature in general find larger responses to housing wealth shocks compared to financial wealth shocks. Garbinti, Lamarche, Savignac, et al. (2022) use survey data from five European countries to estimate consumption responses to wealth shocks. They find that the MPC out of housing wealth shocks is statistically significant in all five countries, while the MPC out of financial wealth is only statistically significant in one out of the five countries (Italy). Kundan Kishor (2007) finds that the MPC out of housing wealth shocks is more than twice that of financial wealth shocks, while Bostic, Gabriel, and Painter (2009) find that the MPC out of housing wealth shocks is three times larger than that of financial wealth shocks. Moreover, in our sample, average housing wealth is almost twenty times as large as average financial wealth. Hence, we focus on the MPC out of housing wealth shocks.

in accordance with the estimates from Table 7. For the average household, we use the final row of equation (10), and set $P = 0.052$ in accordance with the average purchase probability from Table 5. Again, $\partial Leverage|_P / \partial$LTV-caps $= -0.0237$, in accordance with the estimates from Table 7, while $\partial P / \partial$LTV-caps $= -0.0034$ in accordance with the estimated change in purchase probability from Column 3 in Table 5. Finally, we calculate $Leverage|_P - Leverage|_{1-P} = 0.37$ in the data, by comparing average leverage for home-buyers and non-home buyers.[30]

Summing up, this means that if we use our own estimates for the impact on MPC^w, there is no effect of LTV-caps on the marginal propensity to consume out of wealth shocks. Using instead the estimates from the literature, we have that, for home buyers, the change in the MPC^w due to LTV-caps is $0.021 * (-0.0237) = -0.0005$. In other words, the MPC out of wealth shocks falls modestly by 0.05 cents on the dollar. For the average household, the impact is given by $0.021 * (0.052 * (-0.0237) + (-0.0027) * 0.37) = 0.00005$. This impact is quite small, and implies that the MPC out of wealth shocks falls by 0.005 cents on the dollar.

Note that, taking the estimates from the literature, the increase in the MPC^y for homebuyers is almost 30 times as large as the decrease in the MPC^w, in absolute temrs. For the average household, the increase in the MPC^y is nearly 15 times as large as the decrease in the MPC^w. This already suggests that the total impact on household consumption volatility is unlikely to be beneficial – at least in so far that the impact is working through household balance sheets.

6.3 Measuring asset and income volatility

In order to calculate the impact on total consumption volatility, we also need to take a stand on the volatility of income and wealth shocks. We adopt two approaches. First, we assume that LTV-caps do not affect income and wealth volatility. Second, we allow for LTV-caps to impact house price volatility, and estimate this relationship in the data based on municipality level house prices and LTV-ratios. We use volatility measures from Norwegian micro and macro data to proxy the shock series.

Volatility measures are reported in Table 10. The standard deviation of aggregate income growth, measured by Norwegian GDP, is 1%. The standard deviation of aggregate house price growth is 1.7%. For comparison, we have also included the equivalent figures for US macro data, which results in very similar measures of volatility.[31] Aggregate volatility is, however, likely to substantially understate the volatility that households face. To get a better sense of the household level volatility, we also compute the standard deviation of total pre-tax income at the household level using Norwegian tax data and the standard deviation of municipality level house prices. We

[30] Note that we in this case calculate LTV-ratios based on the tax value of housing rather than the purchase price, as we do not have a purchase price for non-home buyers.

[31] Not surprisingly, stock price growth is more volatile, with the standard deviation being roughly 6 times as large as that of aggregate income growth in the US, and roughly 8 times as large as that of aggregate income growth in Norway.

find that the volatility of individual income is 16 times larger than aggregate income, but smaller than that reported in Dynan, Elmendorf, and Sichel (2012) based on US individual income from the PSID data. House price growth at the municipality level is found to be almost five times as volatile as house price growth at the national level.

Standard deviation	Norway	US
Aggregate income (y)	1.0	0.9
Aggregate house prices (w)	1.7	1.7
Individual income (y_i)	16	-
Municipality level house prices (w_m)	8.1	-

Table 10: Standard deviation of wealth and income growth 1992-2021.
Notes: Income (y) is measured by Norwegian GDP (mainland) and US GDP, individual income (y_i) is measured by household after-tax income (excluding student households, households where head is above the age of 61, and households with less than 1G in income[32]), house prices ($w - hp$) are captured by the Statistics Norway house price index for used housing (the municipality level data is not reported for all municipalities), and the St. Louis FED All-Transactions House Price Index for the United States. Growth in variable x is defined as $(ln(x_t) - ln(x_{t-1})) * 100$.

In order to allow for LTV-caps potentially affecting the shock processes that households face, we estimate the impact of LTV-restrictions on house price volatility directly. In order to do so, we compare municipalities with high and low average predicted LTV-ratios. The reasoning being that municipalities with higher predicted LTV-ratios are likely to be more affected by the regulation. House price growth at the municipality level is available from Statistics Norway, and covers all but the smallest municipalities – or more than ninety percent of the population. We calculate the standard deviation in house price growth $\sigma(\Delta w_{m,t})$ five years prior to and five year following the LTV-restrictions at the municipality level, and estimate the following based on 245 municipalities and 490 observations

$$\sigma(\Delta w_{m,t}) = \alpha_m + \beta L\hat{T}V_m^{high} \times I_t^{post} + \gamma L\hat{T}V_m^{high} + \epsilon_{m,t} \tag{11}$$

The estimation results in $\hat{\beta} = -0.80$, with a standard error of 0.64. This implies that there is a relative reduction in house price volatility in municipalities with higher predicted LTV-ratios, but not significantly so. Still, we extend our baseline results to also allow for the possibility that LTV-caps reduce house price volatility. The difference in average predicted LTV-ratios between high- and low-LTV municipalities is four percentage points. This compares to an estimated reduction in average LTV-ratios of 2.4 percentage points. We thus scale the impact on house price volatility, so that $\partial\sigma_{\Delta w}/\partial\text{LTV-caps} = -0.008\frac{2.4}{4} = -0.005$.

6.4 Results

In order to numerically evaluate equation (8), we use the values computed based on equations (9) and (10), and the standard deviations reported in Table 10 as well as the estimate from equation (11). In addition, we need values for MPC^y and MPC^w, which we assign using numbers provided in Fagereng, Holm, and Natvik (2021) and Mian, Rao, and Sufi (2013) adjusted for average leverage and liquidity in our sample.[33] The results are reported in Table 11 and convey two main takeaways.

MPC-estimates	Volatility	Volatility-response	$\frac{\partial \sigma_c^2}{\partial \text{LTV-cap}}$ Home buyers	Full sample
From paper	Micro-data	None	0.00 (0.1%)	0.00 (0.00%)
From paper	Macro-data	None	0.00 (0.0%)	0.00 (0.00%)
From literature	Micro-data	None	0.03 (3.1%)	0.00 (0.0%)
From literature	Macro-data	None	0.00 (0.0%)	0.00 (0.0%)
From paper	Micro-data	Equation (11)	-0.01 (-1.2%)	-0.01 (-0.1%)
From paper	Macro-data	Equation (11)	-0.00 (-0.3%)	0.00 (0.0%)
From literature	Micro-data	Equation (11)	0.02 (1.8%)	-0.01 (-0.1%)
From literature	Macro-data	Equation (11)	0.00 (-0.3%)	-0.00 (-0.0%)

Table 11: The computed change in consumption volatility in response to LTV-caps in cents (%). *Notes:* Calculations are based on equation (8). The change in MPC^y with respect to LTV-caps is based on the results in Section 5.1 or Fagereng et al. (2021). The change in MPC^w is based on the results in Section 5.2 or Mian et al. (2013). The standard deviation in income and wealth are based on Norwegian micro or macro data from Table 10. The response in the standard deviation of income wrt LTV-caps is assumed to be zero. The response in the standard deviation of wealth wrt LTV-caps is assumed to be zero, or is based on equation (11).

First, if LTV-caps do not affect the volatility of the shock processes that households face, then consumption volatility never improves as a result of LTV-caps. This is seen by the first four rows in Table 11, in which all effects are positive, i.e. consumption volatility increases. The intuition being that the destabilizing impact of lower liquidity outweighs the stabilizing impact of lower leverage. For homebuyers, the increase in consumption volatility is non-trivial in size only when we use micro data to estimate the volatility of income and wealth, and when we rely on estimates from the literature for the impact of leverage and liquidity on MPCs. In this case, consumption volatility increases by 3%. For the average household however, the impact on consumption volatility is always trivial in size. This is due to most households not being (potential) homebuyers in a given period, leaving them unaffected by LTV-caps.

Second, in order for consumption volatility to increase in response to LTV-caps, there has to be a stabilizing impact working through lower shock volatility. If house price volatility is reduced

[33] Specifically, we set MPC^y=0.39 and MPC^w=0.01. We also use that $\rho_{\Delta w,\Delta y} = 0.01$ based on micro data, and $\rho_{\Delta w,\Delta y} = 0.6$ based on macro data.

according to the (statistically insignificant) effects estimated based on equation (11), the impact on consumption volatility can be both positive and negative, as seen from the bottom four rows in Table 11. Still, the magnitudes are very modest, especially for the average household. Consumption volatility improves or deteriorates by a maximum of 0.1%.

To summarize, we have quantified the impact on the marginal propensities to consume out of income shocks and wealth shocks based on findings from the existing literature and our empirical estimates. We find much larger responses in MPCy due to lower liquidity than increases in MPC^w due to lower leverage. In order to evaluate the implications for total consumption volatility, we consider income and wealth volatility based on macro and micro level data. We find that, if LTV-caps do not affect the shock processes households face, consumption volatility always increases in response to the regulation. However, for the average household, the effect is very limited in size.

7 Conclusion

To summarize, we have shown that the LTV-regulation introduced in the aftermath of the financial crisis lead to a reduction in house purchase probabilities of six percent. This reduction was driven entirely by low liquidity households. Intuitively, households with high liquid wealth holdings – who should be more able to meet the higher downpayment requirement – did not experience any reduction in purchase probabilities.

In terms of intensive margin effects, we have shown that – conditional on a house purchase – the regulation reduced average LTV-ratios by three percent and reduced average debt holdings by eleven percent. These effects improve household solvency, and are likely to make households more resilient against large fluctuations in asset values. At the same time however, we also documented a reduction in liquid assts of ten percent. Intuitively, for a given house purchase, the LTV-restrictions imply that a higher downpayment is required, inducing households to deplete more of their liquid assets at the time of purchase. We showed in an event study setup that this negative effect was highly persistent, showing no sign of convergence four years after the purchase.

The documented leverage-liquidity tradeoff makes it ex-ante unclear how LTV-caps will affect consumption volatility. Typically, lower leverage is associated with smaller consumption responses to wealth shocks, while lower liquidity is associated with higher consumption responses to income shocks. Using comprehensive individual consumption data, we found that affected home buyers had larger consumption falls in response to income loss resulting from unemployment, and that 75% of the consumption response was due to durables. We did not pick up any significant difference in the consumption response to wealth shocks, perhaps caused by the very modest effect on leverage conditional on a house purchase.

Finally, back-of-the-envelop calculations suggested that LTV-caps will not stabilize household consumption through balance sheet responses. The reason being that the detrimental liquidity effect outweighs the beneficial leverage effect, also when the external margin effects are taken into account. The only way for LTV-caps to have a stabilizing impact on consumption is if the regulation affects the shock processes that households face – most plausibly by reducing house price volatility. Using municipality level data, we estimated a statistically insignificant reduction in house price volatility. Incorporating this (insignificant) effect into the back-of-the-envelope calculations, we find mixed results. Consumption volatility can increase or decrease based on the exact assumptions made, but, in all cases, the impact on consumption volatility for the average household is trivial in size, equalling at the most 0.1%.

It is of course possible that consumption volatility is not the only measure that policy makers care about when implementing restrictions on LTV-ratios. Another goal might be to reduce aggregate credit growth. Our results show that LTV-restrictions are successful in dampening mortgage debt, as affected home buyers reduce their mortgage debt by $42,000 or eleven percent. In addition, the reduction in house purchase probabilities working through the extensive margin also contributes to lower debt growth. With some back of the envelope calculations – taking into account both the intensive and the extensive margin effects – we find that the LTV-restrictions reduce aggregate credit growth by about four percent, see Appendix C. This is the same quantitative effect as a 25 basis point hike in the interest rate, according to the VAR literature on monetary policy shocks.[34]

However, reducing credit growth is rarely considered a goal in and of itself. Instead, it is typically assumed that lower credit growth reduces household vulnerability. The assumption being that lower debt will reduce the households need to cut back on consumption in response to adverse shocks.[35] As a result, the likelihood that demand externalities will greatly amplify macroeconomic shocks is reduced. A key takeaway from this paper is that this reasoning is unlikely to hold in the data, as it misses the relatively large and persistent impact of LTV-caps on liquidity.

All in all, we conclude that although LTV-restrictions do improve household leverage and reduce aggregate credit growth, their impact on household demand resilience may not be what policy makers are hoping for for. Due to a persistent decrease in liquidity, household consumption is unlikely to become more stable in response to the regulation. This consideration comes in addition to other important issues not explored in this paper, such as distributional impacts (e.g. Blickle and Brown (2019); Brandsaas (2021); Wold et al. (2023)) and the benefits of high leverage for consumption smoothing over the life cycle.

[34] Another potential objective is to dampen house price growth. This was for instance the main motivation for The Reserve Bank of New Zealand's introduction of LTV-restrictions in 2014. Our results do not explicitly capture the impact on aggregate house price growth, but our results are indicative of lower house price growth as i) the extensive margin effect implies reduced demand for housing and ii) the intensive margin effect shows that affected home buyers purchase cheaper housing.

[35] If households react to negative shocks by substantially reducing consumption, this reduction in household demand could negatively affect firm profitability and therefore wages, causing further declines in household demand, and so on. In principle, interest rate reductions could counteract these negative effects. However, in the presence of a (binding) lower bound on interest rates, or in response to a regional shock, monetary policy may not be sufficiently responsive to neutralize the negative demand externalities.

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A Additional Figures

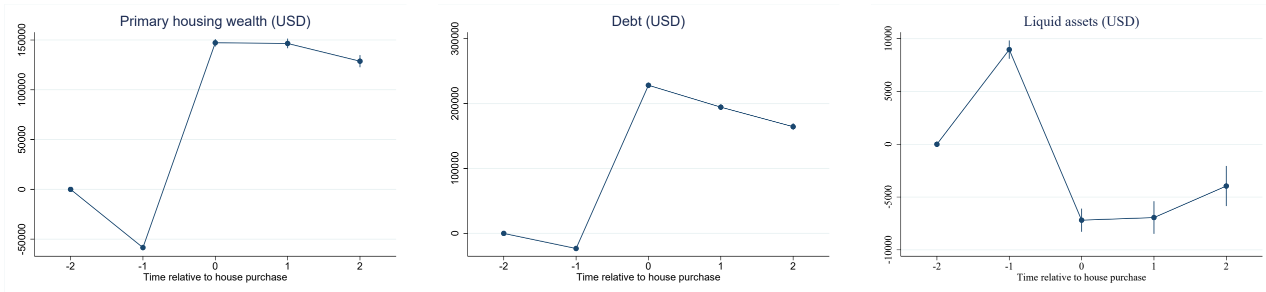

Figure A1: Event study around house purchase for first-time buyers.

Notes: Regression results from estimating equation (15), using primary housing wealth, debt and liquid assets (USD) as the dependent variable. First-time buyers only. House purchase occurs at year $t = 0$. Year $t = -2$ is used as the base level and normalized to zero. Vertical bars correspond to 95 % confidence intervals.

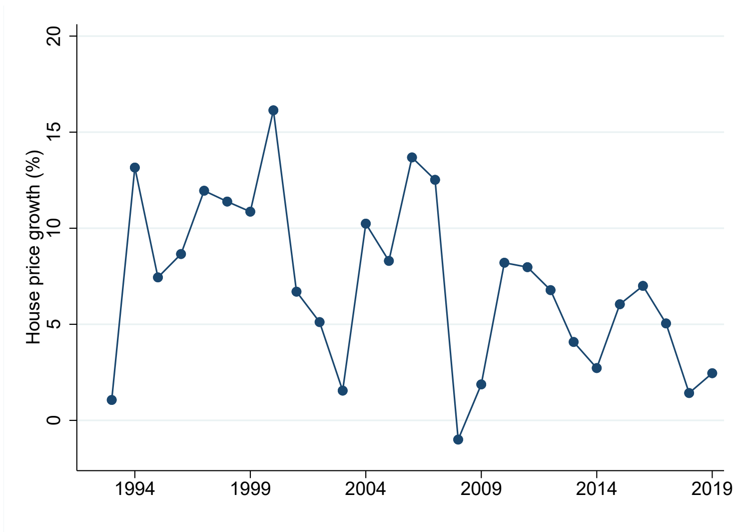

Figure A2: Annual house price growth in Norway (%).

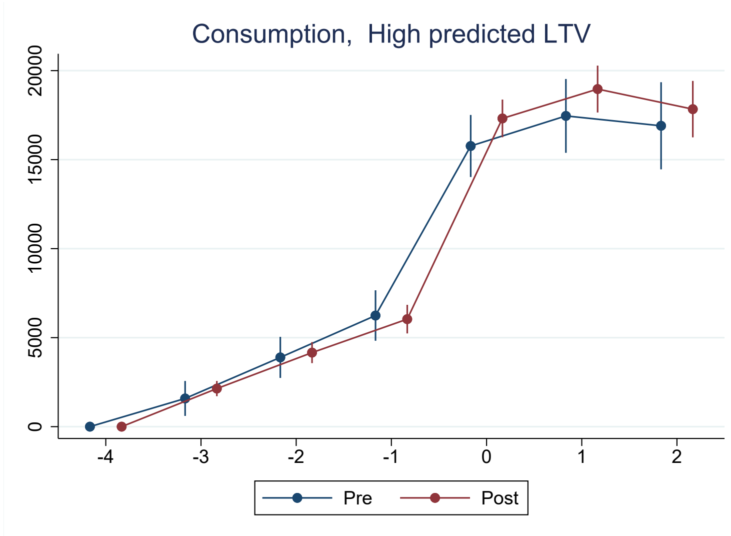

Figure A3: Evolution of consumption.

Notes: Regression results from estimating equation (15), using primary consumption (USD) as the dependent variable. House purchase occurs at year $t = 0$. Year $t = -2$ is used as the base level and normalized to zero. Vertical bars correspond to 95 % confidence intervals.

B Additional Tables

	(1) LTV 2010	(2) LTV 2012
Age	-0.756***	-0.736***
	(0.0453)	(0.0520)
Age^2	0.00690***	0.00692***
	(0.000517)	(0.000597)
Pre-tax income	0.00000763	-0.00000926**
	(0.00000715)	(0.00000409)
L.Pre-tax income	0.00000933	0.0000107**
	(0.00000700)	(0.00000544)
L.Bank deposits	-0.0000250***	-0.0000235***
	(0.00000110)	(0.00000132)
L.Financial wealth	0.000000655***	0.000000645***
	(0.000000176)	(0.000000128)
Post-tax income	-0.00000634	0.0000112***
	(0.0000101)	(0.00000434)
L.Post-tax income	-0.0000125	-0.00000803
	(0.00000980)	(0.00000766)
L.Interest income	0.0000547**	0.0000681***
	(0.0000273)	(0.0000159)
L.Student debt	0.0000117***	0.0000225***
	(0.00000252)	(0.00000310)
L.Mortgage debt	-0.00000226***	-0.000000714*
	(0.000000417)	(0.000000393)
L.Housing	-0.0000146***	-0.0000331***
	(0.00000115)	(0.00000223)
Male	-0.642***	-0.828***
	(0.149)	(0.169)
Zip code indicator variable	YES	YES
Household type	YES	YES
N	43,899	36,674
R^2	0.147	0.152

Table B1: Predicting LTV-ratios (%).

Notes: Results from estimating equation (1), with dependent variable LTV-ratio. All control variables except the demographic variables are in USD. * p<0.1, ** p<0.05, ***p<0.01.

	(1) Age of entry	(2) Age of entry
$\hat{LTV}^{high} \times Post^{2010}$	1.540***	
	(0.132)	
$\hat{LTV}^{high} \times Post^{2012}$		3.493***
		(0.167)
N	2,880,232	2,880,232
Clusters	434	434
Year FE	Yes	Yes

Table B2: Effects of mortgage regulation on the age of entry

Notes: Results from estimating Age of entry$_{i,t} = \alpha + \delta_t + \beta\, \hat{LTV}_i^{high} \times I_t^{post} + \gamma \hat{LTV}_i^{high} + \epsilon_{i,t}$ where the dependent variable is the age of first purchase. * p<0.1, ** p<0.05, ***p<0.01.

	(1) House Purchase	(2) House Purchase
$\hat{LTV}^{high} \times Post^{2010}$	-0.692***	
	(0.00724)	
$\hat{LTV}^{high} \times Post^{2010} \times$ Liquid assets$_{t-1}^{high}$	1.76***	
	(0.00592)	
$\hat{LTV}^{high} \times Post^{2012}$		-1.35***
		(0.00406)
$\hat{LTV}^{high} \times Post^{2012} \times$ Liquid assets$_{t-1}^{high}$		2.57***
		(0.00517)
N	4,352,860	4,508,483
Clusters	430	430
Mean	4.66	5.20
Sample period	2009-2010	2011-2012
Year FE	Yes	Yes

Table B3: House purchase probability (%) by liquidity.

Notes: Results from estimating equation (2), with dependent variable house purchase probability (%) and an additional interaction term with liquid assets, measured by bank deposits. $LTV^{high} = 1$ if $\hat{LTV} > 90$ (85) for the 2010 (2012) requirement and zero otherwise. $Post^{2010} = 1$ if year$\geq$ 2010 and zero otherwise. $Post^{2012} = 1$ if year$\geq$ 2012 and zero otherwise. Liquid assets$_{t-1}^{high} = 1$ if deposits are above median and zero otherwise. Standard errors are clustered at the municipality level and bootstrapped to correct for estimated regressor bias. * p<0.1, ** p<0.05, ***p<0.01.

	(1) GFW	(2) Liquid assets	(3) Liquid assets t+1	(4) Liquid assets t+2
$\hat{LTV}^{high} \times Post^{2010}$	-20,276	-3,390***	-2,475***	-2,186***
	(13,658)	(1,163)	(511)	(562)
N	192,529	192,529	186,622	179,899
Clusters	431	431	431	431
Mean	101,569	38,569	40,984	47,385
Year FE	Yes	Yes	Yes	Yes

Table B4: Balance sheet effects financial wealth, 2010-regulation.

Notes: Results from estimating equation (3), with dependent variables gross financial wealth (GFW) (USD), liquid assets (USD), liquid assets one year ahead and liquid assets two years ahead. $LTV^{high} = 1$ if $\hat{LTV} > 90$ zero otherwise. $Post^{2010} = 1$ if year$\geq$ 2010 and zero otherwise. Sample: house buyers with $LTV \in [60, 110]$. Sample period: 2007-2011. Standard errors are clustered at the municipality level. * p<0.1, ** p<0.05, ***p<0.01.

	(1) GFW	(2) Liquid assets	(3) Liquid assets t+1	(4) Liquid assets t+2
$\hat{LTV}^{high} \times Post^{2012}$	14,898	-4,340***	-3,294**	-5,160***
	(19,267)	(1,616)	(1,633)	(858)
N	222,156	222,156	213,128	201,735
Clusters	433	433	433	433
Mean	94,795	44,771	47,227	52,779
Year FE	Yes	Yes	Yes	Yes

Table B5: Balance sheet effects financial wealth, 2012-regulation.

Notes: Results from estimating equation (3), with dependent variables gross financial wealth (GFW) (USD), liquid assets (USD), liquid assets one year ahead and liquid assets two years ahead. $LTV^{high} = 1$ if $\hat{LTV} > 85$ zero otherwise. $Post^{2012} = 1$ if year$\geq$ 2012 and zero otherwise. Sample: house buyers with $LTV \in [60, 110]$. Sample period: 2009-2014. Standard errors are clustered at the municipality level. * p<0.1, ** p<0.05, ***p<0.01.

	(1) House Purchase	(2) House Purchase	(3) House Purchase	(4) House Purchase
$\hat{LTV} \times Post^{2010}$	0.00460	-0.00771**		
	(0.00375)	(0.00366)		
$\hat{LTV} \times Post^{2012}$			-0.0284***	-0.0287***
			(0.00599)	(0.00614)
N	4,275,940	6,462,303	4,420,265	6,656,235
Clusters	430	431	430	431
Mean	4.66	4.66	5.20	5.20
Year FE	Yes	Yes	Yes	Yes

Table B6: House purchase probability (%), continuous treatment.

Notes: Results from estimating equation (2), with dependent variable house purchase probability (%). $\hat{LTV}$ is the predicted LTV-ratio. $Post^{2010} = 1$ if year$\geq$ 2010 and zero otherwise. $Post^{2012} = 1$ if year$\geq$ 2012 and zero otherwise. Standard errors are clustered at the municipality level. * p<0.1, ** p<0.05, ***p<0.01.

	(1) LTV	(2) Debt	(3) Int.Expenses	(4) House price	(5) Liquid assets
$\hat{LTV} \times Post^{2012}$	-0.315***	-2,911***	-144.8***	-2,401***	-536.6**
	(0.0161)	(581.2)	(19.54)	(686.5)	(223.0)
N	220,266	220,266	220,266	220,266	220,266
Clusters	433	433	433	433	433
Mean	73.59	385,650	12,073	510,708	44,771
Year FE	Yes	Yes	Yes	Yes	Yes

Table B7: Balance sheet effects, 2012-regulation continuous treatment.

Notes: Results from estimating equation (3), with dependent variables LTV-ratios (%), non-student debt (USD), interest expenses (USD), house purchase prices (USD) and liquid assets (USD). $\hat{LTV}$ is the predicted LTV-ratio. $Post^{2012} = 1$ if year$\geq$ 2012 and zero otherwise. Sample: house buyers with $LTV \in [60, 110]$. Sample period: 2010-2014. Standard errors are clustered at the municipality level. * p<0.1, ** p<0.05, ***p<0.01.

	(1) House Purchase	(2) House Purchase	(3) House Purchase	(4) House Purchase
$\text{Post}^{2009} \times \hat{LTV}^{distance,2009}$	0.000176	0.000425**		
	(0.000233)	(0.000213)		
$\text{Post}^{2012} \times \hat{LTV}^{distance,2012}$			-0.000270***	-0.000174**
			(0.0000735)	(0.0000873)
N	4,276,333	6,462,998	4,420,814	6,657,063
Clusters	430	431	430	431
Mean	0.0466	0.0466	0.0520	0.0520
Year FE	Yes	Yes	Yes	Yes

Table B8: Extensive margin results, semi-continuous treatment measure

Notes: Results from estimating equation (2), with a house purchase dummy as dependent variable. $\hat{LTV}^{distance,2009}$ is defined as $\hat{LTV} - 90$ if $\hat{LTV} > 90$ and zero otherwise. $\hat{LTV}^{distance,2012}$ is defined as $\hat{LTV} - 86$ if $\hat{LTV} > 85$ and zero otherwise. $Post^{2010} = 1$ if year$\geq$ 2010 and zero otherwise. Standard errors are clustered at the municipality level. * p<0.1, ** p<0.05, ***p<0.01.

	(1) LTV	(2) Debt	(3) Int.Expenses	(4) House price	(5) Liquid assets
$\hat{LTV}^{distance} \times \text{Post}^{2012}$	-0.347***	-3438.3***	-172.8***	-3243.0***	-470.2***
	(0.0254)	(432.0)	(23.87)	(564.4)	(133.9)
N	220,266	220,266	220,266	220,266	220,266
Clusters	433	433	433	433	433
Mean	73.59	385649.9	12073.0	510708.4	44771.0
Year FE	Yes	Yes	Yes	Yes	Yes

Table B9: Balance sheet results, 2012-regulation, semi-continuous treatment measure

Notes: Results from estimating equation (3), with dependent variables LTV-ratios (%), non-student debt (USD), interest expenses (USD), house purchase prices (USD) and liquid assets (USD). $\hat{LTV}^{distance}$ is defined as $\hat{LTV} - 85$ if $\hat{LTV} > 85$ and zero otherwise. $Post^{2012} = 1$ if year$\geq$ 2012 and zero otherwise. Standard errors are clustered at the municipality level. * p<0.1, ** p<0.05, ***p<0.01.

	(1) House Purchase	(2) House Purchase	(3) House Purchase	(4) House Purchase
$\hat{LTV}^{high} \times Post^{2012}$	-0.0855	-0.493		
	(0.0562)	(1.81)		
$\hat{LTV}^{high} \times Post^{2010}$			-0.274***	-0.261***
			(0.0504)	(0.0662)
N	4,352,860	6,583,923	4,508,483	6,788,070
Clusters	430	431	430	431
Mean	4.66	4.66	5.20	5.20
Year FE	Yes	Yes	Yes	Yes
House price interaction term	Yes	Yes	Yes	Yes

Table B10: House purchase probability (%) with house price growth interaction term
Notes: Results from estimating equation (2), with dependent variable house purchase probability (%) and an additional house price interaction term $\hat{LTV}^{high} \times g_t^{HP}$. $LTV^{high} = 1$ if $\hat{LTV} > 90$ (85) for the 2010 (2012) requirement and zero otherwise. $Post^{2010} = 1$ if year$\geq$ 2010 and zero otherwise. $Post^{2012} = 1$ if year$\geq$ 2012 and zero otherwise. Standard errors are clustered at the municipality level. * p<0.1, ** p<0.05, ***p<0.01.

	(1) LTV	(2) Debt	(3) Int. Expenses	(4) House price	(5) Liquid assets
$\hat{LTV} \times Post^{2012}$	-2.309***	-20307.0***	-1056.6***	-15239.8*	-4190.5***
	(0.304)	(6461.3)	(263.2)	(9197.3)	(1265.1)
N	222,156	222,156	222,156	222,156	222,156
Clusters	433	433	433	433	433
Mean	73.59	385649.9	12073.0	510708.4	44771.0
Year FE	Yes	Yes	Yes	Yes	Yes
House price interaction term	Yes	Yes	Yes	Yes	Yes

Table B11: Balance sheet results, 2012-regulation with house price growth interaction term
Notes: Results from estimating equation (3), with dependent variables LTV-ratios (%), non-student debt (USD), interest expenses (USD), house purchase prices (USD) and liquid assets (USD), and an additional house price interaction term $\hat{LTV}^{high} \times g_t^{HP}$. $LTV^{high} = 1$ if $\hat{LTV} > 85$ and zero otherwise. $Post^{2012} = 1$ if year$\geq$ 2012 and zero otherwise. Standard errors are clustered at the municipality level. * p<0.1, ** p<0.05, ***p<0.01.

	(1) House Purchase	(2) House purchase
$\hat{LTV}^{high} \times Post^{2007}$	-0.333***	
	(0.0856)	
$\hat{LTV}^{high} \times Post^{2013}$		0.0920
		(0.0569)
N	6,372,876	4,595,743
Clusters	430	429
Mean	5.33	5.30
Year FE	Yes	Yes

Table B12: Placebo test. House purchase probability (%).

Notes: Results from estimating equation (2), with dependent variable house purchase probability (%). $LTV^{high} = 1$ if $\hat{LTV} > 90$ (85) for the "2007-regulation" ("2013"-regulation) and zero otherwise. $Post^{2007} = 1$ if year$\geq$ 2007 and zero otherwise. $Post^{2013} = 1$ if year$\geq$ 2013 and zero otherwise. Standard errors are clustered at the municipality level. * p<0.1, ** p<0.05, ***p<0.01.

	(1) Debt	(2) House price	(3) Liq.assets	(4) Debt	(5) House price	(6) Liq.assets
$\hat{LTV}^{high} \times Post^{2007}$	-6,028	-2,064	530			
	(4,150)	(5,798)	(1,527)			
$\hat{LTV}^{high} \times Post^{2014}$				-8,122	-9,788	-1,186
				(5,548)	(6,852)	(1,692)
N	127,545	127,545	127,545	142,474	142,474	142,474
Clusters	432	432	432	429	429	429
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	2005-2008	2005-2008	2005-2008	2013-2015	2013-2015	2013-2015

Table B13: Placebo test. Balance sheet results.

Notes: Results from estimating equation (3), with dependent variables non-student debt (USD), house purchase price (USD) and liquid assets (USD). $LTV^{high} = 1$ if $\hat{LTV} > 90$ (85) for the "2007-regulation" ("2013"-regulation) and zero otherwise. $Post^{2007} = 1$ if year$\geq$ 2007 and zero otherwise. $Post^{2013} = 1$ if year$\geq$ 2013 and zero otherwise. Sample: house buyers with $LTV \in [60, 110]$. Standard errors are clustered at the municipality level. * p<0.1, ** p<0.05, ***p<0.01.

	Total	Non-durable consumption				Durable consumption		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total consumption	Food and restaurants	Clothing and footwear	Recreation and culture	Consumption, misc.	Utilities	Furnishing	Vehicles
T_i × U-Year$_{i,t}$	-8129.5***	-1133.0***	-310.9**	-408.8	-328.4*	-4418.3***	-1470.5***	-59.56
	(1511.5)	(316.9)	(148.7)	(257.2)	(171.2)	(1204.2)	(167.0)	(413.9)
N	27,359	27,359	27,359	27,359	27,359	27,359	27,359	27,359
Clusters	363	363	363	363	363	363	363	363
Mean	58010.5	17578.6	3711.4	6641.9	7129.6	14284.7	6105.8	2558.4
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HH FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table B14: Consumption response to unemployment.

Notes: Results from estimating equation (4), with dependent variable different consumption categories measured in USD. T_i is the treatment indicator and equals one for households who purchased a home in 2012 and became unemployed the subsequent year, and equals zero for households who purchased a home in 2010 and became unemployed in the subsequent year. U-Year$_{i,t} = 1$ in the year of unemployment. Sample: households with predicted LTV-ratio above cap. Standard errors are clustered at the municipality. level. * p<0.1, ** p<0.05, ***p<0.01.

C Economic significance

In this appendix we briefly discuss the economic magnitude of the leverage and liquidity results documented in Section 4. For the liquidity effect, we focus on the distributional impacts. For the leverage effect, we focus on the implications for aggregate credit growth, as this is a macroeconomic indicator which has received substantial attention from macroprudential policy makers.

Households in our sample generally have relatively large holdings of liquid assets. However, the distribution is quite skewed. To get a sense of how different households are affected, we report some simple summary statistics in Table C1. Prior to the reform, 23 percent of house buyers reduce liquid assets to less than 75 percent of the baseline value. Following the reform, this share increases to 34 percent. The median household in this group has \$12,200 in liquid asset holdings following the house purchase, while the 25th percentile has \$3,400. A smaller share – 4 percent in the pre-period and 6 percent in the post-period – reduce deposits to *less than ten percent* of their baseline value. For this group, the median household has \$1,700 worth of liquid assets following the house purchase, and the 25th percentile has \$300. Hence, this group – albeit quantitatively small – is left with virtually no liquid savings following their house purchase.

			Liquid assets at time $t+1$ (USD)	
Share who reduce liquid assets to less than:	Pre-reform	Post-reform	50th prct.	25th prct.
75 % of $t-1$ value	23 %	34 %	12,200	3,400
50 % of $t-1$ value	17 %	24 %	8,600	2,300
25 % of $t-1$ value	9 %	13 %	4,500	1,000
10 % of $t-1$ value	4 %	6 %	1,700	300

Table C1: Reduction in liquid assets.
Notes: Share of house buyers (%) who reduce liquid assets to less than X % of the $t = -1$ value in year $t = 1$, in which house purchase takes place at time $t = 0$.

We now move on to computing some rough estimates of the impact on aggregate credit growth, resulting from i) lower house purchase probabilities and ii) lower debt uptake conditional on purchase. Although our cross-sectional estimates do not necessarily map directly into aggregate effects, we still find it useful to provide some back of the envelope calculations to get a sense of the magnitudes. We focus here on the 2012-reform. Considering instead the 2010-reform would yield smaller effects, as especially the extensive margin results were smaller in response to the initial regulation. We assume that total credit growth is given by

$$\Delta D = P \times \Delta D|_P + (1 - P) \times \Delta D|_{1-P} \tag{12}$$

in which D stands for debt and P indicates a house purchase. We assume that the reform does not affect debt growth if a purchase does not take place, so that $\frac{\partial \Delta D|_{1-P}}{\partial \text{LTV-caps}} = 0$. This means that the impact of the regulation is governed by equation (13), in which the value of $\frac{\partial P}{\partial \text{LTV-caps}}$ and P are from Table 5, $\frac{\partial \Delta D|_P}{\partial \text{LTV-caps}}$ is from Table 7 and $(\Delta D|_P - \Delta D|_{1-P})$ is calculated directly in the data.

$$\begin{aligned}\frac{\partial \Delta D}{\partial \text{LTV-caps}} &= \frac{\partial P}{\partial \text{LTV-caps}}(\Delta D|_P - \Delta D|_{1-P}) + P\frac{\partial \Delta D|_P}{\partial \text{LTV-caps}} \\ &= -0.0027 \times 182,231 + 0.052 \times 41,833 = -2,667\end{aligned} \tag{13}$$

This means that debt growth is reduced by an average of $2,667 per person. Multiplying this figure by the number of households in 2012, and adding this to total household debt, we find that in the absence of the reform, credit growth would have been 6.12%. This compares to an observed household credit growth of 5.85%. Hence, our simple calculations suggest that the LTV-regulation reduced aggregate credit growth by close to 0.3 percentage points, or just above four percent. This is the same magnitude as would be expected from a 25 basis point increase in the policy rate, according to the VAR-literature.[36] We interpret this as saying that the dampening effect on aggregate credit growth from the regulation is non-trivial, but at the same time not very large. It is worth noting however, that this only captures the immediate impact. Looking at LTV-distributions over time in Figure 1 indicates that households and banks continue to adjust to the regulation in the years following the reform, meaning that the full effect on credit growth might plausibly be larger.

D Tax benefits of mortgage debt

The mortgage rate tax deductibility may be important for how households respond to LTV-restrictions. As the Dutch mortgage rate tax deductibility is especially high[37], this might explain why Van Bekkum, Gabarro, Irani, and Peydró (2019) find a short lived liquidity response, whereas we – under more typical institutional features – document a persistent effect. To see how tax deductibility matters, let the marginal benefit of reducing ones mortgage by one dollar, relative to placing that dollar in a saving account be given by

$$i^m(1 - \frac{MRD}{100}) - i^s, \tag{14}$$

in which i^m is the mortgage rate, i^s is the saving rate, and MRD is the mortgage rate deductibility. If the value of this expression is positive, repaying ones mortgage is financially more attractive

[36] Robstad (2018) estimates the impact of a monetary policy shock on credit growth, and provides a table with an overview of other estimates from the literature. Averaging over all the estimates reported in the table, we find that a one percentage point increase in the policy rate reduces household credit by 1.1 percent. In our case, this number translates into a reduction in credit growth of 1.2 percentage points. Hence, this dampening effect on credit is three times larger than what we estimate as the result of the reform.

[37] In fact, according to the European Commission report "Tax Reforms in EU Member States 2014", at the time of the regulation, the Netherlands had the lowest marginal cost of investing in housing among all EU-countries, due to the especially high interest rate deductibility.

than placing the additional dollar in the bank. If it is negative however, homeowners will have an incentive to maximize their mortgage at all points in time, giving them more room to increase their liquid savings.

At the time of the LTV-regulation, the mortgage rate deductibility in the Netherlands was 52%, compared to 28% in Norway. The mortgage rate was roughly 4% in both countries, and the deposit rate was close to 2.5% in both countries. This means that Dutch homeowners – in contrast to Norwegian homeowners – had incentives to keep their mortgage as high as possible, while holding any surplus funds in a liquid bank account (i.e. the value of the expression in equation (14) was -0.6 pp in the Netherlands and +0.4 pp in Norway). We note that while this arbitrage opportunity was well understood in the Netherlands[38], it is not a common feature elsewhere, and most countries – the Netherlands included – are now moving in the direction of *reducing* the tax benefits of mortgage debt. We therefore believe the Norwegian setting, in which such an arbitrage opportunity does not exist, is more generally applicable.[39]

E Balance sheet responses to house purchase

We provide here a description of how household balance sheets typically evolve around house purchases. To investigate the balance sheet adjustments connected to a house purchase we use an event study setup, in which we estimate equation (15).

$$y_{i,t} = \alpha_i + \delta_t + \sum_{k=-1}^{2} \beta_k \, I_{i,t}^k + \epsilon_{i,t} \tag{15}$$

The outcome of interest, $y_{i,t}$ is housing wealth, debt or liquid assets for household i at time t. We define a vector of time dummies for the years prior to and following a house purchase $I_{i,t}^k$, with k denoting the number of years since the house purchase took place. α_i captures individual fixed effects, and δ_t captures time fixed effects.

As shown in Figure A4, primary housing wealth increases by roughly USD 170,000 in the year of a house purchase. This number captures the average effect over a heterogeneous group of home buyers, including households who are just entering the housing market as well as households who have been in the housing market for some time – some of whom might be downsizing. The increase in debt is approximately as large as the increase in primary housing wealth, telling us that the average house purchase is mostly debt-financed.

[38]In fact, the Governor of the Dutch central bank discussed the extraordinary large tax benefits in a speech, in which he stated that *It is in this period that households and mortgage suppliers discovered "innovative" ways to fully exploit the tax advantages of mortgage interest deduction. Borrowing at 110% LTV was not an exception anymore by the early 2000's. Additionally, households started to withdraw home equity by taking out an additional mortgage loan.* (Mortgage Interest Tax Deduction in the Netherlands: A Welcome Relief, by Klaas Knot 2019)

[39]In addition, Dutch LTV-restrictions were set to increase year-by-year going forward, making home equity less liquid, and again increasing household's incentives to quickly rebuild their liquid assets.

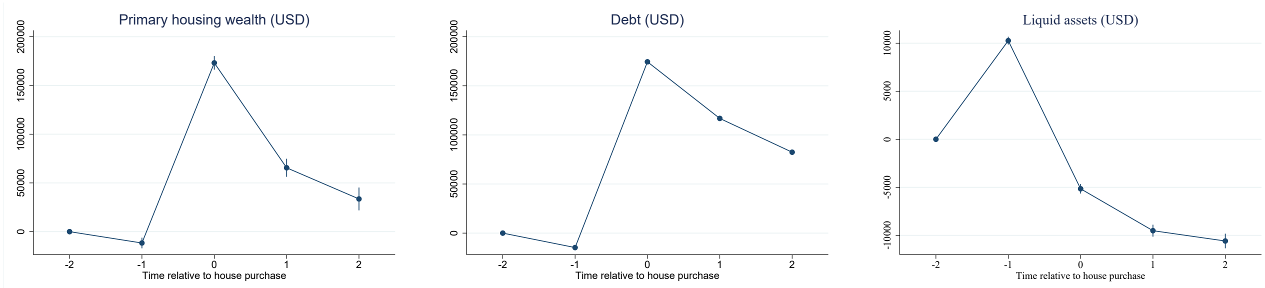

Figure A4: Event study around house purchase.
Notes: Regression results from estimating equation (15), using primary housing wealth, debt and liquid assets (USD) as the dependent variable. House purchase occurs at year $t = 0$. Year $t = -2$ is used as the base level and normalized to zero. Vertical bars correspond to 95 % confidence intervals.

Liquid assets increase by just above USD 10,000 in the year before the house purchase, and then fall by more than this once the house purchase is realized. Where does the increase in liquid savings come from? First, we do see a modest increase in income at the time of a house purchase, which can explain part of the increase. This is not surprising as many households would choose not to purchase a (new) house if experiencing a negative income shock. Second, households might reduce consumption in order to save up for the downpayment if they are planning on buying a house in the near future.[40] Third, households may sell off other financial assets, such as stocks, in preparation for the house purchase. While we cannot rule out that some households do this, we are not able to identify a statistically significant reduction in *other financial assets*, defined as total financial assets less liquid assets. Finally, households might receive gifts/inheritance/loans/transfers from other family members.

[40] Another possibility is that the household sells a house a year before buying a new one. However, we see a similar pattern for first-time buyers - see Figure A1, suggesting that this is not the main cause.