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The leverage-liquidity trade-off of mortgage regulation*

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Abstract

We evaluate the impact of loan-to-value restrictions on household financial vulnerability. Using Norwegian tax data, we first document a beneficial leverage effect, in which households respond to the regulation by reducing house purchase probabilities, debt and interest expenses. Second, we document a detrimental and persistent liquidity effect working through higher down-payment requirements. We further show that households which, due to the regulation, hold less liquid assets also have larger consumption falls upon unemployment. Finally, we provide back of the envelope calculations on the net impact of lower leverage and lower liquidity on household consumption volatility. We find that the beneficial impact of lower leverage is outweighed by the detrimental impact of lower liquidity, suggesting that LTV restrictions are not successful in reducing consumption volatility at the household level.

JEL-codes: E21, E58, G21, G28, G51

Keywords: Household leverage, Financial regulation, Macroprudential policy, Mortgage markets

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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, aimed at reducing household indebtedness and increasing household resilience to adverse shocks. 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. While several papers study the impact of LTV-restrictions using aggregate data or, more recently, loan level data, the empirical evidence at the household level is still limited.

Understanding the impact of LTV-restrictions at the household level is important for determining the net effect on financial and macroeconomic stability. One of the key objectives of this regulation is to make household demand more resilient to adverse shocks, such as house price declines or reductions in income (Korinek and Simsek, 2016). However, the effectiveness of LTV-restrictions in terms of achieving this goal will depend on how households react. Households might respond by reducing leverage through lower debt uptake, or by depleting liquid assets to meet the stricter downpayment requirement. The net impact of LTV-caps on the resilience of household demand depends crucially on how households adjust their balance sheets.

In this paper we use Norwegian administrative data to investigate how LTV-restrictions affect household balance sheets and ultimately their resilience to adverse shocks. Our first main contribution to the literature is to document what we refer to as the leverage-liquidity trade-off of LTV-restrictions. We show that households respond to the regulation with a reduction in the likelihood of a house purchase, and by decreasing debt uptake conditional on purchase. Household leverage thus declines. At the same time, however, we also observe a persistent reduction in liquid assets, as households deplete more of their savings to satisfy the LTV-restriction and meet the new downpayment requirement. In sum, this leaves affected households with lower leverage *and* lower liquidity.

Our second main contribution is to quantitatively evaluate the net impact on household demand resilience from lower leverage and lower liquidity. We capture household demand resilience by implied consumption volatility, i.e. consumption responses to income and wealth shocks. We consider a household to be more resilient if it is better equipped to smooth consumption in response to shocks. By combining our empirical estimates with findings from the existing literature on marginal propensities to consume (MPCs) out of income and wealth shocks, we can quantify the impact of lower leverage and liquidity on household consumption responses. These estimates, together with our measures of income and wealth volatility from the data, suggest that LTV-restrictions are *not* successful in increasing household demand resilience. The reason being that

[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).

LTV-restrictions reduce liquid buffers and as a result increases the MPC out of income shocks, in accordance with Fagereng et al. (2021). This detrimental effect dominates the stabilizing effect from having a lower MPC out of housing shocks as a result of lower leverage (Mian and Sufi, 2011). Overall, our results suggest that LTV-caps are unlikely to improve financial stability if the primary objective of regulators is to increase household demand resilience.

For our empirical investigation, we rely on Norwegian tax data on the population of tax-filing households, merged with housing transaction data from the Land Registry. The Norwegian tax data contains detailed information on income and wealth, and lets us study the balance sheet effects of the regulation. In addition, since we observe the full income statement and balance sheets of all households, we can impute consumption at the household level (Fagereng and Halvorsen, 2017). To identify home buyers and measure housing values, we rely on data from the Land Registry. Loan-to-value ratios are defined as non-student debt relative to the house purchase price in the year the house is purchased. Because only collateralized debt is supposed to enter the LTV calculations, we adjust for average holdings of unsecured debt. 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, in which the former constituted a new requirement whereas the latter was a tightening of an existing requirement.

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 difference setup, we need a treatment indicator. The first step in our analysis is therefore to obtain a measure of how affected a given household is by the regulation, by predicting LTV-ratios based on pre-reform data 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. We show that high predicted LTV-ratios to a large extent are driven by household demographics such as age. 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 instead using a continuous treatment indicator in the form of predicted LTV-ratios. In this case, households with higher predicted LTV-ratios are defined to have higher treatment intensities, regardless of whether their predicted LTV-ratio is just above or just below 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. 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. Perhaps not surprisingly, this effect is driven by households with low liquidity. Households with high initial liquid asset holdings do not experience a reduction in purchase probabilities as a result of the regulation. 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 reform 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 the 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.[2] The same also holds for lending rates – which is another important macroeconomic variable likely to affect households home purchase decisions. We also report placebo tests for all our results, confirming that our findings are unique to reform years.

Third and finally, we attempt to quantify the net gain of LTV-restrictions on household demand resilience. When implementing the LTV-regulation studied in this paper, the Norwegian Financial Supervisory Agency wrote that *The high debt level has increased the household sectors vulnerability to interest rate increases, unemployment and income reductions.* An interpretation of this - consistent with the theoretical literature on macroprudential regulation, e.g. Korinek and Simsek (2016) and Farhi and Werning (2016) - is that a key purpose of LTV-restrictions is to dampen the sensitivity of household demand to fluctuations in income and asset values. We therefore proceed by discussing how LTV-restrictions affect household's consumption responses to income and wealth 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

[2]The only result which is affected by including an interaction term with house price growth is the initial house purchase effect from the 2010-regulation, which becomes borderline insignificant. This result is likely to be more noisy due to the timing of the regulation, and we focus mainly on the highly robust results from the 2012-regulation.

therefore have a reform-induced reduction in liquidity, experience larger consumption declines upon unemployment – consistent with findings from the literature.

While liquidity is important for the reaction to income shocks, lower leverage is important for the reaction to wealth shocks (Mian, Rao, and Sufi (2013)). In order to quantitatively compare the two channels, we combine our empirical estimates with findings from the literature to evaluate the net impact on consumption volatility. Focusing first on home buyers, we find that that the MPC out of income shocks *increases* by 1.4 cents on the dollar due to lower liquidity, while the MPC out of wealth shocks *decreases* by 0.05 cents on the dollar due to lower leverage. Taken together with the observed volatility of individual income and municipality level house prices, this leads to an increase in consumption volatility of just above three percent due to the LTV restrictions. Home buyers only make up about five percent of the population in a given year however, meaning that the impact on the representative household will be considerably smaller. For the representative household, we find that the impact on consumption volatility is still positive in magnitude, but trivial in size. As a result, we conclude that the detrimental impact working through lower liquidity (more than) outweighs the beneficial impact working through lower leverage, and that LTV-restrictions are *not* successful in improving household demand resilience – at least not through household balance sheet adjustments.

Contribution to 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. Until recently, the literature mainly used aggregate data to evaluate the impact on house prices, household debt and bank lending.[3] Recently however, a handful of papers have used loan level data to study the effect of borrower-based macro-prudential policy, and one other study uses household level data. We discuss these papers below. As far as we know, we are the first to document a *persistent* decline in household liquidity in response to LTV-restrictions, and to show that this reduction in liquidity makes LTV-restrictions *unsuccessful* in stabilizing household consumption.

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 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 (2020) use loan level from the UK and also find dampening impacts on credit and house price growth.[4] While our findings are certainly consistent with a dampening effect on credit and house price growth, we focus on

[3]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).

[4]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.

the impact on household behavior, documenting significant responses in household debt, interest expenses, liquid buffers and the reaction to adverse income 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. 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.[5] We show that, in a setting without this non-typical institutional feature, the negative liquidity effect is in fact highly persistent. This is crucial, as we find that the de-stabilizing impact of lower liquidity outweighs the stabilizing impact of lower leverage. Moreover, our data also allows us to impute household consumption, and thereby show that the reduction in liquidity increases the marginal propensity to consume out of income shocks. Relative to the results in Van Bekkum, Gabarro, Irani, and Peydró (2019) we thus contribute by i) showing that the liquidity effect is highly persistent in a more standard institutional setting, ii) directly showing that affected households have stronger consumption responses to negative income shocks, and iii) quantitatively comparing the de-stabilizing impact of lower liquidity to the stabilizing impact of lower leverage, showing that the former outweighs the latter.

2 Data

We use data from 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 housing transaction data from the Land Registry, allowing us to precisely identify home buyers in a given year and accurately measure housing wealth.

We start by aggregating our data to the household level, and exclude the household if the

[5] 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.

household head is self-employed.[6] 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.[7] Specifically, we define $\text{Mortgage debt}_{it} = \text{Total debt}_{it} - \text{Student debt}_{it} - \overline{\text{Unsecured debt}}_t$. 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 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, 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 were almost ten times as large as median holdings of all other financial assets in the years surrounding the regulation.[8]

When investigating how affected households respond to adverse income shocks, we are interested in identifying potential consumption effects. In order to do so, we need to impute consumption. We follow Browning and Leth-Petersen (2003), Fagereng and Halvorsen (2017), Eika, Mogstad, and Vestad (2020) and Fagereng, Holm, and Natvik (2021) and impute consumption based on income and balance sheet data. By observing balance sheet components and incomes we can exploit the accounting identity that consumption is equal to income less savings (see Fagereng and Halvorsen (2017) for more details on the exact imputation procedure). As highlighted by Fagereng, Holm, and Natvik (2021), imputation of consumption from the Norwegian tax records gives a fairly accurate aggregate consumption level. We do, however, acknowledge that imputed consumption will be associated with measurement error at the household level, primarily due to lack of data granularity on individual equity holdings and related to special incidences such as house purchases, divorce etc.

We define imputed consumption as disposable income less savings. Disposable income is measured as the sum of labor income and (net) capital income, net of taxes. Following among others Fagereng, Holm, and Natvik (2021), we also include implicit housing income. Saving is measured

[6] In Norway, labor and capital income is taxed at the individual level, while the wealth tax is levied at the household level.

[7] Fagereng, Guiso, Malacrino, and Pistaferri (2020) show that the fraction of unsecured debt is fairly constant among high-leveraged households.

[8] 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.

as the change in net wealth, adjusted for unrealized capital gains. In order to obtain a symmetric treatment of implausibly low and implausibly high imputed consumption levels, we drop the top and bottom 15 % of the imputed consumption distribution. After this trimming is done, average imputed consumption for our different sample selections is between $81,000 and $85,000. This compares to an average household consumption level of $86,000 in 2012 according to Statistics Norway (Helliesen, Melsom, and Scheele (2017)), indicating that at least the average imputed consumption level is very well in line with official figures. We depict the distribution of imputed consumption at the household level in Appendix Figure A2.

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. Measured LTV-ratios are relatively insensitive to whether we use non-student debt or the change in non-student debt for this group.

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 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 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.[9]

[9] 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.

	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.

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.

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 A3.[10] 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.

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 restrictions. 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

[10] 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.

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. This is comparable to other advanced economies where the LTV-limits mostly range from 80 to 95 percent.[11] 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. 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. As in the initial guidelines, failure to comply could lead to higher capital requirements for the given bank.

The existing guidelines were formalized into regulation in 2015. At this point, banks' possibility to deviate from the requirements were specified in a flexibility quota. Specifically, eight percent of new loans in Oslo could deviate from the requirements, and ten percent of new loans outside of Oslo could deviate. In December 2016, a further requirement was added to the regulation. Specifically, a second maximum LTV-level of 60 percent was introduced for buyers of secondary housing in Oslo. As we restrict our analysis to house purchases prior to 2016, this piece of regulation should not be directly relevant for the interpretation of our results.

Alongside the requirements levied on loan-to-values, the guidelines and the following regulation also outlined some other requirements relevant for the mortgage market. The guidelines issued in 2010 stated that banks had to ensure that their customers had a sufficient payment capacity, and that loans with a "high" LTV-ratio, should normally not be *interest only*. In the updated guidelines from 2011, the former requirement was specified to mean that interest only loans should normally have an LTV-ratio of 70 percent or below. A further specification was introduced into the regulation in 2016, when banks were required to evaluate their customers payment capacity in the event of a five percentage point increase in the lending rate. Finally, the December 2016 amendments also introduced a debt-to-income (DTI) requirement of 5, stating that loans should not be granted if the customers total debt exceeded five times gross annual income.[12] Key elements of the regulation are summarized in Appendix Table B1.

[11] One exception, however, is the Netherlands where the LTV-limit was set to 106 percent.

[12] The initial 2010 guidelines also had a soft DTI-requirement, which stated that *if* banks considered DTI when deciding whether to grant a loan, then loans should normally not be granted if the DTI-ratio exceeded three. This section was removed from the guidelines in the 2011 update.

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. 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 et al. (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 is 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-distributions. 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[13],

[13] 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-restrictions 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

there is clear 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. This could indicate that households and banks continue to respond to the regulation over time. If so, the total impacts of the reform might be larger than the ones we identify here, which mostly focus on short-run responses.

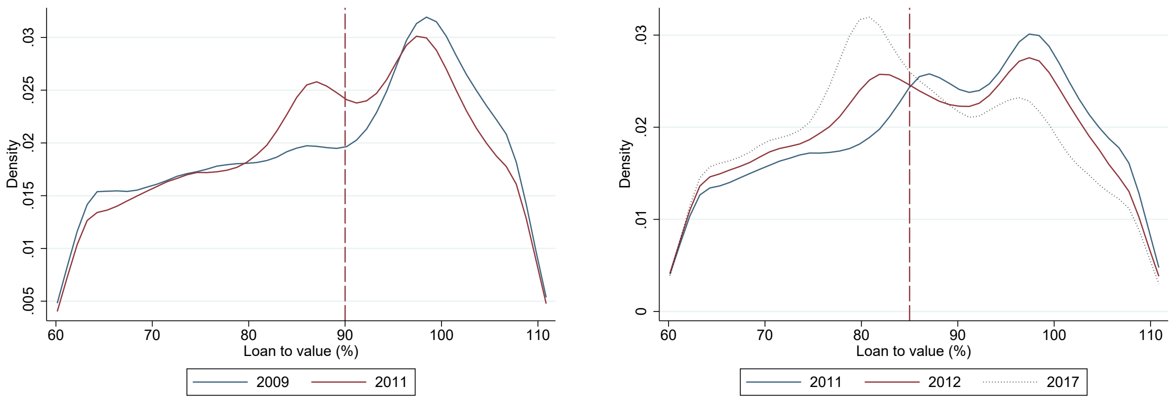

Figure 1: LTV-distributions.
Notes: LTV-distributions (%) for house buyers with $LTV \in [60, 110]$ by year. LTV is defined as non-student debt less average unsecured debt holdings, relative to house purchase price.

3.2 Balance sheet responses to house purchase

We end this section by including 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 (1).

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

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 2, primary housing wealth increases by roughly USD 170,000 in the year of a house purchase. This number captures the average effect over a heterogenous group of home

additional collateral or financial support which we do not observe, such as parental support.

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.

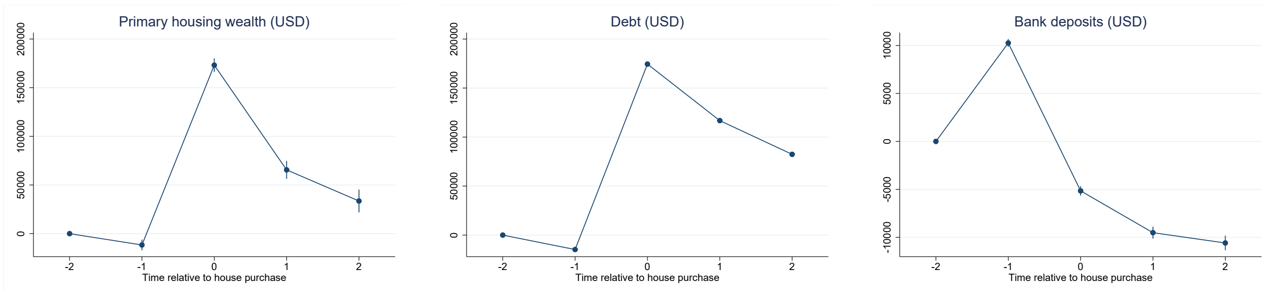

Figure 2: Event study around house purchase.
Notes: Regression results from estimating equation (1), using primary housing wealth, debt and liquid assets (USD) as the dependent variable. House purchase occurs at year $t = 0$. Year $t = -2$ is used as the base level and normalized to zero. Vertical bars 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.[14] 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.

4 Effects of LTV-regulation

We now move on to estimating the impact of LTV-regulation on household balance sheets. 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.

[14]Another possibility is that the household sells a house prior to 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.

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 house price 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 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 (2). 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.

$$LTV_{i,t} = \alpha + \beta_1 Demographics_{i,t} + \beta_2 Income_{i,t} + \beta_2 Income_{i,t-1} + \beta_2 Wealth_{i,t-1} + \epsilon_{i,t} \qquad (2)$$

Given the predicted LTV-ratios $\hat{LTV}_i$, we define a dummy variable $\hat{LTV}_i^{high}$ which is equal to one for households with predicted LTV-ratios above the limit and zero otherwise. In our baseline, treatment status will be defined based on the binary indicator variable $\hat{LTV}_i^{high}$. Conceptually, this makes sense, as only households who would optimally choose an LTV-cap above the limit are directly affected by the regulation. However, because we cannot precisely measure household's optimal LTV-ratios in absence of the reform, this binary treatment indicator will be noisy.[15] We therefore also report results where we use a continuous treatment indicator $\hat{LTV}_i$. This captures

[15]Note, however, that both in the case of falsely assigning too many households as treated, or in the opposite case of assigning too few households as treated, our estimates will be downward biased. Our baseline estimates could therefore be viewed as a lower bound of the effect of the regulation.

the notion that households with higher estimated LTV-ratios are more likely to be affected by the reform. As a third alternative, we have also tried including a squared term, i.e. $L\hat{T}V_i^2$, to allow for treatment intensity being continuous only for sufficiently high predicted LTV-ratios. However, the squared term is not statistically significant, and we do not report results from this exercise in the paper. Reassuringly however, our results are robust to all of these different treatment measures.

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 (3). That is, we estimate the impact of LTV-restrictions on house purchase probabilities using a sample of all households.

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

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.

To investigate whether LTV restrictions affect the house purchase probability of households differently according to initial characteristics, we also estimate a version where treatment is interacted with liquidity. Specifically, we include a triple interaction term with a dummy variable for having above median liquid asset holdings in the previous year, Liquid $\text{assets}_{t-1}^{high}$, in the estimation. The regression is specified in equation (4). In this case, the impact of the LTV-restrictions on households with low liquidity is captured by $\hat{\beta}_1$, while the impact on households with high liquidity is captured by $\hat{\beta}_1 + \hat{\beta}_2$.

$$\begin{aligned} Purchase_{i,t} = \alpha + \delta_t + \beta_1\, L\hat{T}V_i^{high} \times I_t^{post} + \beta_2\, L\hat{T}V_i^{high} \times I_t^{post} \times \text{Liquid assets}_{t-1}^{high} \\ + \gamma L\hat{T}V_i^{high} + \epsilon_{i,t} \end{aligned} \tag{4}$$

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.

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 (5), 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.

$$y_{i,t} = \alpha + \delta_t + \beta\, L\hat{T}V_i^{high} \times I_t^{post} + \gamma L\hat{T}V_i^{high} + \epsilon_{i,t} \tag{5}$$

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 (2) are reported in Appendix Table B2. 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 B2. However, removing only age or any single wealth variable has a very moderate impact on the predictive power, suggesting that there is not one single variable which is crucial for the performance.

Motivated by the non-trivial measurement error described above, we also include results based on a continuous treatment indicator $L\hat{T}V_i$. In this case, 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. 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 (3) using an indicator variable for house purchase as our dependent variable. The results are reported in Table 2, 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 but not statistically significant. 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 negative coefficient estimate becomes statistically significant. 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 2. 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 almost six percent. Considering the reform year 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
$\hat{LTV}^{high} \times Post^{2010}$	-0.0855	-0.0926*		
	(0.0562)	(0.0507)		
$\hat{LTV}^{high} \times Post^{2012}$			-0.274***	-0.289***
			(0.0504)	(0.0523)
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 2: House purchase probability (%).

Notes: Results from estimating equation (3), 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. * 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 2 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. Interestingly, we find different effects if restricting the sample to only considering (potential) first-time buyers – see Appendix Table B3. For households who have not yet entered the housing market, the negative effect on purchase probabilities is limited to the year of the reform. That is, in the following year, there is no significant impact on the purchase probabilities of potential first-time buyers. The data is thus consistent with there being at least a somewhat more persistent effect on housing transactions in general, compared to the impact on those not yet in the housing market.

In order to investigate the heterogeneous effects along the extensive margin, we include a triple interaction term in the regression used to estimate the impact on house purchase probability in Table 2. A natural hypothesis is that households with relatively large liquid assets holdings would be less likely to postpone or cancel a house purchase due to the regulation. As seen from Table 3, this is indeed the case. While the effect on *overall* house purchase probabilities was not statistically significant for the 2010 requirement (at least when considering the reform year only), the probability of purchasing a home falls after the regulation for households with below median deposits. As seen from the first column of Table 3, households with large holdings of bank deposits experience no such reduction. A similar picture emerges for the 2012-regulation. As before, the reduction in the probability of purchasing a home following the reform is entirely driven by households with below median deposits. This is also reassuring from an identification point of view, as our results are driven by the subgroup most likely to respond to the regulation.

To summarize, our results indicate that affected households are 2-6 percent less likely to purchase a new house immediately following the regulation. This negative effect is entirely driven by households with relatively low liquid wealth, meaning that households with sufficiently high liquid asset holdings are not affected by the regulation – at least along the extensive margin.

	(1) House Purchase	(2) House Purchase
$\hat{LTV}^{high} \times Post^{2010}$	-0.498***	
	(0.134)	
$\hat{LTV}^{high} \times Post^{2010} \times$ Liquid assets$^{high}_{t-1}$	1.12***	
	(0.392)	
$\hat{LTV}^{high} \times Post^{2012}$		-1.15***
		(0.225)
$\hat{LTV}^{high} \times Post^{2012} \times$ Liquid assets$^{high}_{t-1}$		2.11***
		(0.512)
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 3: House purchase probability (%) by liquidity.

Notes: Results from estimating equation (4), 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. $Deposits^{high}_{t-1} = 1$ if deposits are above median and zero otherwise. Standard errors are clustered at the municipality level. * p<0.1, ** p<0.05, ***p<0.01.

The intensive margin The intensive margin results for the 2010 regulation, i.e. the balance sheet effects conditional on purchase, are reported in Table 4. 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.

In the final column in Table 4, 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 bank deposits 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.

	(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***
	(0.239)	(2,422)	(80.32)	(3,745)	(938.2)
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 4: Balance sheet effects, 2010-regulation.

Notes: Results from estimating equation (5), 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. * p<0.1, ** p<0.05, ***p<0.01.

Regression results reported in Table 5 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***
	(0.181)	(4,321)	(201.5)	(5,850)	(1,737)
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 5: Balance sheet effects, 2012-regulation.

Notes: Results from estimating equation (5), 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. * 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 (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 3 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.

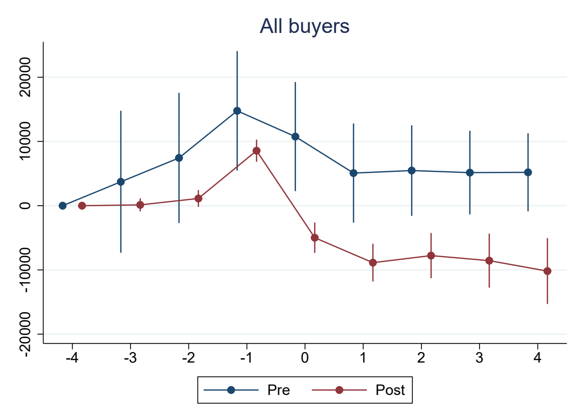

Figure 3: Liquid assets event study around house purchase, pre- and post-regulation.
Notes: Regression results from estimating equation (1), using liquid assets (USD) as the dependent variable, for households purchasing a home prior to the initial regulation (*Pre*) and following the subsequent regulation (*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.

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 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.

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 C.

4.2.3 Economic significance

We have documented a statistically significant reduction in leverage and liquidity at the household level. Before discussing the robustness of our results in the next sub-section, we pause here to briefly discuss the economic magnitude of our results. 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 6. 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 6: 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

$$\Delta D = P \times \Delta D|_P + (1 - P) \times \Delta D|_{1-P} \tag{6}$$

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 (7), in which the value of $\frac{\partial P}{\partial \text{LTV-caps}}$ and P are from Table 2, $\frac{\partial \Delta D|_P}{\partial \text{LTV-caps}}$ is from Table 5 and $(\Delta D|_P - \Delta D|_{1-P})$ is calculated directly in the data.

$$\begin{aligned}\frac{\partial \Delta D}{\partial \text{LTV-caps}} &= \frac{\partial P}{\partial \text{LTV-caps}} (\Delta D|_P - \Delta D|_{1-P}) + P \frac{\partial \Delta D|_P}{\partial \text{LTV-caps}} \\ &= -0.0027 \times 182,231 + 0.052 \times 41,833 = -2,667\end{aligned} \tag{7}$$

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.[16] 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.

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 using a continuous treatment indicator instead

[16] 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.

of a binary treatment indicator. 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.

Continuous treatment As discussed above, our treatment indicator is based on predicted LTV-ratios, and is affected by non-trivial measurement error. By evaluating its precision in non-reform years, we find that most of the households which are wrongly assigned as having optimal LTV-ratios above the cap do in fact have optimal LTV-ratios which are very *close to* the cap. That is, while the predictive model is not able to get all the binary outcomes correct (i.e. above or below the cap), the magnitude of the estimation error is limited in most cases. To take advantage of this, we also report results based on a continuous treatment indicator. That is, we replace $L\hat{T}V_i^{high}$ with $L\hat{T}V_i$ in the specifications in equations (3) and (5). This has the benefit of capturing the fact that households with high predicted LTV-ratios are more likely to be affected than households with low predicted LTV-ratios, regardless of whether the predicted LTV-ratios are above or below the cap. However, it has the downside of imposing this relationship on all households, even households with very low predicted LTV-ratios. To accommodate this concern, 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.

The results are robust to using a continuous treatment indicator. Considering first the extensive margin effect, we find that the house purchase impact from the initial 2010-regulation is again statistically significant only when including the year after the reform in the sample. The house purchase impact from the subsequent 2012-regulation is as before negative and statistically significant in both specifications, as reported in Appendix Table B6. In terms of magnitudes, the 2012-reform 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 qualitatively robust to the use of a continuous treatment indicator.

House price growth In addition to regulatory constraints, house price growth is an important 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 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 B8 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. This means that we cannot rule out that the initial LTV-regulation in fact did not have any impact on household's home purchase probabilities. 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 B9, 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 B9 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. 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.

In sum, all our *intensive* margin results are robust to accounting for important macroeconomic variables which might have differential effects on our treatment group. In terms of our *extensive* margin results however, while the negative effect on house purchase probabilities resulting from the LTV-tightening in 2012 is highly robust, we cannot rule out that the initial LTV-regulation in 2010 had no effect on house purchase probabilities.

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 B10. 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 B11. Reassuringly, we find no significant effect on debt uptake, house purchase prices or bank deposits prior to the reform.[17] 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 4 and 5. We thus conclude that our balance sheet findings – lower debt uptake, house purchase values and bank deposits – are unique to the reform years.

5 The leverage-liquidity trade-off

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. In this section we explore this trade-off further.

First, we explicitly show that households with reform-induced declines in liquidity have larger declines in imputed consumption in response to income falls resulting from unemployment. This confirms the detrimental impact working through the liquidity channel. Second, we use our empirical estimates, combined with findings from the existing literature, to evaluate the net impact of LTV-restrictions on household resilience towards shocks. Our calculations indicate that LTV-restrictions are *not* successful in making household consumption more stable. In fact, we find that the regulation, if anything, increases consumption volatility – especially for home buyers.

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

5.1 The reaction to negative 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 – imputed consumption falls by more in response to unemployment for households which are affected by the LTV-restrictions.

Because consumption is unobserved in the tax data, we have to rely on imputed consumption. Conceptually, imputing consumption based on the household budget constraint is relatively straightforward, and we follow the recipe outlined in Section 2. In practice however, the procedure is likely to induce non-trivial measurement error. The measurement error is likely to be larger when dealing with special events such as house purchases – which is important in our analysis – implying that our results might be imprecise. However, we still find the exercise useful, at least qualitatively.

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. First, we only consider 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. Second, we only consider households who purchase a home right before or right after the requirement, and who become unemployed in the subsequent year. For this limited sub-sample, we then estimate

$$\tilde{C}_{i,t} = \alpha_i + \delta_t + \beta\, T_i \times \text{U-Year}_{i,t} + \gamma \text{U-Year}_{i,t} + \epsilon_{i,t} \tag{8}$$

where $\tilde{C}_{i,t}$ is real imputed 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 (unemployed) homeowners 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. In order to restrict our sample to households with more stable and precisely measured imputed consumption, we also estimate equation (8) using a sample of households who never earn business income and in which the number of adult household members is unchanged.

Regression results from estimating equation (8) are reported in Table 7. In the first column we include the full sample, and in the second column we exclude households with business income, and households in which the number of adults change. This more restricted sample is probably less prone to measurement error and is therefore our preferred estimate. In both cases, we see that imputed consumption 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.

	(1) Imputed C	(2) Imputed C
$T_i \times$U-Year$_{i,t}$	-12,052***	-11,713**
	(3,387)	(5,288)
N	9,864	5,803
Clusters	347	305
Mean	84,955	81,805
Year FE	Yes	Yes
HH FE	Yes	Yes
Restricted sample	No	Yes

Table 7: Imputed consumption response to unemployment.
Notes: Results from estimating equation (8), with dependent variable imputed consumption (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.

In terms of magnitude, a relative reduction in consumption of $11,700 might seem large (Column 2). Note however, that because the estimates are somewhat noisy, we cannot rule out consumption responses as low as -$1,300 within the 95 percent confidence interval. This is less than 30% of the estimated reduction in liquid assets resulting from the reform. Hence, we find the estimates to be within the range of plausible responses, and supportive of the hypothesis that lower liquidity increases the consumption response to negative income shocks.

5.2 The impact on household vulnerability

While lower liquidity makes household demand less resilient against negative income shocks, the reform-induced reduction in leverage should make household demand more resilient to large falls in asset values. Knowing which of these effects dominates is therefore crucial in terms of assessing the effectiveness of LTV-restrictions in reducing consumption volatility at the household level. A proper welfare analysis in a large-scale general equilibrium model is beyond the scope of this paper. Instead, we combine our empirical estimates with findings from the existing literature, in order to quantitatively compare the two channels. We find that the detrimental impact of lower liquidity seems to outweigh the beneficial impact of lower leverage.

In order to investigate the impact on household vulnerability, we focus on consumption responses and marginal propensities to consume. We then evaluate whether the combined reductions in liquidity and leverage increase or decrease consumption volatility. As before, we base our calculations on the 2012-regultion results. Note however, that because the leverage-decline relative to the liquidity-decline is larger in 2012 than in 2010, this implies that LTV-caps will in fact have a more beneficial effect on household demand resilience than if we used the 2010-regulation results.

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.

$$\Delta c = \Delta y MPC^y + \Delta w MPC^w \tag{9}$$

Because we are interested in the volatility of consumption growth, we rewrite equation (9) in terms of variances

$$\sigma^2_{\Delta c} = (MPC^y)^2 \sigma^2_{\Delta y} + (MPC^w)^2 \sigma^2_{\Delta w} + 2MPC^y MPC^w \sigma_{\Delta y} \sigma_{\Delta_w} \rho_{\Delta w, \Delta y} \tag{10}$$

In principle, LTV-restrictions might change not only the MPCs, but also the shock processes to income and, perhaps most plausibly, housing wealth. By comparing municipalities with high and low predicted LTV-ratios, we find some evidence of reduced house price volatility – although not significantly so. Here we assume that LTV-restrictions do not impact the shock processes. However, we allow for the possibility that LTV-restrictions directly influence house price volatility in Appendix D.

Given that the requirements only affect MPCs, the impact of LTV-restrictions on consumption volatility is given by

$$\begin{aligned} \frac{\partial \sigma^2_{\Delta c}}{\partial \text{LTV-caps}} &= 2\frac{\partial MPC^y}{\partial \text{LTV-caps}} MPC^y \sigma^2_{\Delta y} + 2\frac{\partial MPC^w}{\partial \text{LTV-caps}} MPC^w \sigma^2_{\Delta w} \\ &+ 2\sigma_{\Delta y}\sigma_{\Delta w}\rho_{\Delta w, \Delta y}\left(\frac{\partial MPC^y}{\partial \text{LTV-caps}} MPC^w + \frac{\partial MPC^w}{\partial \text{LTV-caps}} MPC^y\right) \end{aligned} \tag{11}$$

In order to numerically evaluate the expression in equation (11), we first have to find values for the marginal impact of the regulation on the MPCs out of income and wealth shocks. To do so, we combine our empirical estimates with findings from the existing literature. 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. They find that leverage does not affect MPC^y at all, in fact the coefficient is exactly zero and precisely measured. However, they show that lower liquidity significantly increases their MPC^y estimate. These findings are consistent with the results developed in Kaplan and Violante (2014), and subsequently explored in several studies, regarding the high consumption responses of (wealthy) hand-to-mouth households. 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. Hence, the marginal impact of the regulation on the MPC out of income shocks is given by

$$\frac{\partial MPC^y}{\partial \text{LTV-caps}} = \frac{\partial MPC^y}{\partial \text{Liquidity}} \times \frac{\partial Liquidity}{\partial \text{LTV-caps}} = -0.003 \frac{\partial Liquidity}{\partial \text{LTV-caps}} \tag{12}$$

What about the marginal propensity to consume out of wealth shocks? First note that the literature has generally found larger responses to housing wealth shocks than to financial wealth shocks.[18] 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. Mian, Rao, and Sufi (2013) 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. They do not consider the impact of liquidity on MPC^w however, and we are not aware of studies which consider the simultaneous impact of liquidity and leverage on MPCs out of housing wealth shocks. For this reason, we assume that liquidity does not affect MPC^w. Note that this assumption – if incorrect – will lead to downward bias in our estimate of the impact of LTV-regulation on MPC^w, i.e. we will be erring on the side of concluding that LTV-restrictions are more effective in reducing household vulnerability than is actually the case. The impact on the marginal propensity to consume out of wealth shocks from the reform is then given by

$$\frac{\partial MPC^w}{\partial \text{LTV-caps}} = \frac{\partial MPC^w}{\partial \text{Leverage}} \times \frac{\partial Leverage}{\partial \text{LTV-caps}} = 0.021 \frac{\partial Leverage}{\partial \text{LTV-caps}} \tag{13}$$

Our empirical estimates allow us to obtain measures of the impact of LTV-restrictions on liquidity and leverage. We calculate these numbers separately for home buyers and for the general population, as policy makers may be particularly interested in the MPC for home buyers and these are the households most likely to be affected by the regulation.

For home buyers, the total impact on MPC^y is given by

$$\frac{\partial MPC^y|_P}{\partial \text{LTV-caps}} = -0.003 \frac{\partial Liquidity|_P}{\partial \text{LTV-caps}} = -0.003 \times -4.66 = 0.014 \tag{14}$$

in which -4.66 is the estimated reduction in liquidity in \$1000 from Table 5. Similarly, for home buyers, the total impact on MPC^w is given by

[18] 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.

$$\frac{\partial MPC^w|_P}{\partial \text{LTV-caps}} = 0.021 \frac{\partial Leverage|_P}{\partial \text{LTV-caps}} = 0.021 \times (-0.0237) = -0.0005 \tag{15}$$

in which -0.0237 is the estimated change in leverage from Table 5. This means that the MPC out of income shocks for home buyers has increased by \$0.014 or 1.4 cents, while the MPC out of wealth shocks for home buyers has decreased by \$0.0005 or 0.05 cents. Hence, the *increase* in consumption responses to income shocks is almost thirty times as large as the *reduction* in consumption responses to wealth shocks.

For the general population, the estimated MPC effects also need to take into account that the intensive margin effects used above only applies to the small subset of households purchasing a (new) home in a given period, as well as the extensive margin effects which reduce the overall house purchase probability. This implies that the estimated MPC effects will be substantially smaller for the general population than for home buyers. Specifically, for the representative household we have that

$$\begin{aligned}\frac{\partial Liquidity}{\partial \text{LTV-caps}} &= P \times \frac{\partial Liquidity|_P}{\partial \text{LTV-caps}} + (1-P) \times \frac{\partial Liquidity|_{1-P}}{\partial \text{LTV-caps}} + \frac{\partial P}{\partial \text{LTV-caps}}(Liquidity|_P - Liquidity|_{1-P}) \\ &= 0.052(-4.66) = -0.242\end{aligned} \tag{16}$$

in which 0.052 is the average purchase probability from Table 2 and -4.66 is the estimated reduction in liquidity in \$1000 from Table 5. We assume that $\frac{\partial Liquidity|_{1-P}}{\partial \text{LTV-caps}} = 0$, i.e. liquidity for non-home buyers is unaffected by the reform. Moreover, we set $\frac{\partial P}{\partial \text{LTV-caps}}(Liquidity|_P - Liquidity|_{1-P}) = 0$, based on the observation that liquidity in the pre-period is unaffected by house purchases according to Figure 3.[19] This implies that the MPC out of income shocks for the general population has increased by \$0.0007 or 0.07 cents:

$$\frac{\partial MPC^y}{\partial \text{LTV-caps}} = (-0.003)(-0.242) = 0.0007 \tag{17}$$

Similarly, for the MPC out of wealth shocks we have that

$$\begin{aligned}\frac{\partial Leverage}{\partial \text{LTV-caps}} &= P \times \frac{\partial Leverage|_P}{\partial \text{LTV-caps}} + (1-P) \times \frac{\partial Leverage|_{1-P}}{\partial \text{LTV-caps}} + \frac{\partial P}{\partial \text{LTV-caps}}(Leverage|_P - Leverage|_{1-P}) \\ &= 0.052(-0.0237) + (-0.0027)0.37 = -0.0022\end{aligned} \tag{18}$$

in which 0.052 is the average purchase probability from Table 2 and -0.0237 is the estimated change in leverage from Table 5. Again, we assume that $(1-P) \times \frac{\partial Leverage|_{1-P}}{\partial \text{LTV-caps}} = 0$, i.e. the

[19] 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 $t = -2$ (or earlier).

regulation does not have any impact on leverage for non-home buyers. In contrast to the above liquidity results however, average leverage does change in connection to a house purchase. We calculate $Leverage|_P - Leverage|_{1-P} = 0.37$ in the data, by comparing average leverage for home-buyers and non-home buyers.[20] Finally, $\frac{\partial P}{\partial \text{LTV-caps}} = -0.0027$ is the estimated change in purchase probability from Table 2. This implies that the MPC out of wealth shocks decreases by \$0.00005 or 0.005 cents:

$$\frac{\partial MPC^w}{\partial \text{LTV-caps}} = 0.021(-0.0022) = -0.00005 \tag{19}$$

Hence, the increase in MPC^y is 14 times as large as the decrease in MPC^w for the average household. In order to calculate the impact on total consumption volatility, we also need the shock processes for income and wealth. A comprehensive estimation of income shocks and wealth shocks is beyond the scope of this paper. Instead, we have included an overview over income and wealth volatility in Table 8. 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.[21] 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 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.

[20] 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.

[21] 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.

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 8: 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[22]), 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 numerically evaluate equation (11) for home buyers, we use the values computed in equations (14) and (15), and the standard deviations reported in Table 8. In addition, we need values for MPC^y and MPC^w, which we again calculate using the numbers provided in Fagereng, Holm, and Natvik (2021) and Mian, Rao, and Sufi (2013) adjusted for average leverage and liquidity in our sample. In order to numerically evaluate equation (11) for the representative household, we follow the same procedure using instead the values computed in equations (17) and (19). The results are reported in Table 9 and convey two main take-aways.

First, the impact on consumption volatility is always non-negative. That is, regardless of whether we use aggregate or individual data, Norwegian or US macro series, or whether we consider the full sample or only home buyers, it is never the case that LTV-restrictions are found to stabilize consumption. This means that the beneficial effect working through lower leverage is being outweighed by the detrimental effect working through lower liquidity.

Second, the destabilizing impact on consumption is only non-trivial in size when we use individual level data and restrict our attention to home buyers. In this case, consumption volatility increases by 0.03 percentage points, which implies an increase of three percent. Why is it the case that the impact is only quantitatively non-trivial when considering home buyers and using micro level data to compute the shock series? Intuitively, the impact on consumption volatility is small when using aggregate data to measure income and wealth shocks, as there is little volatility in these series. Considering individual income increases the standard deviation by a factor of sixteen, which means that the change in the marginal propensity to consume out of income shocks becomes substantially more important. However, the impact for the full sample is still limited – mainly because only a small fraction of households buy a (new) home in a given period.

Income measure	Wealth measure	$\frac{\partial \sigma_c^2}{\partial \text{LTV-restrictions}}$ Home buyers	Full sample
y^{NO}	w^{NO}	0.00 (0.01%)	0.00 (0.00%)
y^{US}	w^{US}	0.00 (0.01%)	0.00 (0.00%)
y_i^{NO}	w_m^{NO}	0.03 (3.07%)	0.00 (0.15%)

Table 9: The computed change in consumption volatility in response to LTV-restrictions. *Notes: Calculations are based on equation* (11). *The income and wealth series are the same as those described in Table 8*

Even though we do not identify any statistically significant impact on house price growth volatility, we allow LTV-caps to affect wealth shocks directly in Appendix D. Doing so results in a slight decrease in consumption volatility for the representative household and a slight increase in consumption volatility for home buyers. We interpret this to mean that the only way in which LTV-caps can have a stabilizing impact on household consumption is through other channels than household balance sheet responses. Lower house price growth volatility might be one such channel, although we are not able to identify this type of effect in our setting.

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. Due to lower liquidity, we find that the MPC^y increases by 1.4 cents on the dollar for home buyers, and by 0.07 cents for the full sample. At the same time, we find that the MPC^w decreases by 0.05 cents on the dollar for home buyers and by 0.005 cents for the full sample. In order to evaluate the implications for total consumption volatility, we consider income and wealth volatility based on aggregate and household level data. We find that the detrimental impact working through lower liquidity always dominates the beneficial impact working through lower leverage, so that consumption volatility increases. However, the effect is quantitatively trivial except when using household level data to measure income volatility and when restricting our attention to home buyers. In this case, consumption volatility increases by three percent in response to the regulation. Hence, we believe LTV-restrictions are unlikely to stabilize household consumption – at least through adjustments in household balance sheets. At the same time however, any destabilizing impact is likely to be small, with the potential exception of groups which are especially affected, such as home buyers. On average, this group constitutes just above 5% of the population each year, meaning that it is probably not sufficiently large to pose a threat to macroeconomic stability. However, given the persistence of the detrimental liquidity effect, this effect could accumulate and become more sizable over time.

6 Conclusion: do LTV-restrictions work as intended?

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.

Our results underline the complexity of borrower-based mortgage regulation, and its effect on financial vulnerability. The documented leverage-liquidity trade-off begs the question of whether LTV-restrictions work as intended. In order to answer this, one first needs to define what exactly is the intention behind this kind of regulation. This might differ over time and space, and policy makers may of course have multiple goals in mind. Here we discuss some of the arguments which are often used in favor of LTV-restrictions.

First, and not surprisingly, LTV-restrictions are successful in lowering average LTV-ratios. In our case, we find that the 2012-regulation lowered average LTV-ratios by 2.4 percentage points or just above three percent. If this is a goal in and of itself, the policy should be considered effective. However, we suspect that policy makers do not care about LTV-ratios per say, but rather their impact on other economic outcomes such as credit growth and financial stability.[23] Our results show that LTV-restrictions are also 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. 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.

While financial supervisory authorities undoubtedly care about debt levels and credit growth,

[23] 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.

this may again be a means to an end, rather than the ultimate goal of regulatory measures. For instance, when implementing the LTV-regulation studied in this paper, the Norwegian Financial Supervisory Agency wrote that *The high debt level has increased the household sectors vulnerability to interest rate increases, unemployment and income reductions.*[24] While household vulnerability may have several interpretations, we have focused on household consumption and marginal propensities to consume (MPCs). 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.[25] This negative spiral would then work as an amplification of negative shocks, reducing macroeconomic stability.

We have shown that households with a reform-induced reduction in liquidity have larger consumption falls in response to income losses resulting from unemployment, consistent with existing literature showing that lower leverage increases the MPC out of income shocks. However, the literature has also highlighted the importance of lower leverage for the MPC out of wealth shocks. Combining our empirical estimates with findings from the literature, we find that the detrimental effect working through lower liquidity outweighs the beneficial effect working through lower leverage. That is, LTV-restrictions are unsuccessful in stabilizing household consumption. While the destabilizing effect is generally small, the impact on homebuyers is non-trivial in size.

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 hoped for. Due to a persistent decrease in liquidity, household consumption does not 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 and the benefits of high leverage for consumption smoothing over the life cycle.

[24]Our translation. In Norwegian: Det høye gjeldsnivået har økt husholdningssektorens sårbarhet ved renteoppgang, arbeidsledighet og redusert inntekt. (FSA 28.09.2011)

[25]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

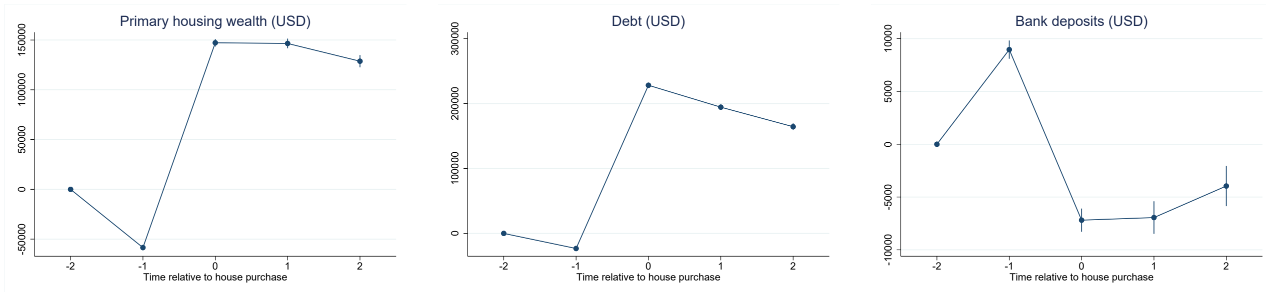

Figure A1: Event study around house purchase for first-time buyers.
Notes: Regression results from estimating equation (1), using primary housing wealth, debt and liquid assets (USD) as the dependent variable. First-time buyers only. House purchase occurs at year $t = 0$. Year $t = -2$ is used as the base level and normalized to zero. Vertical bars correspond to 95 % confidence intervals.

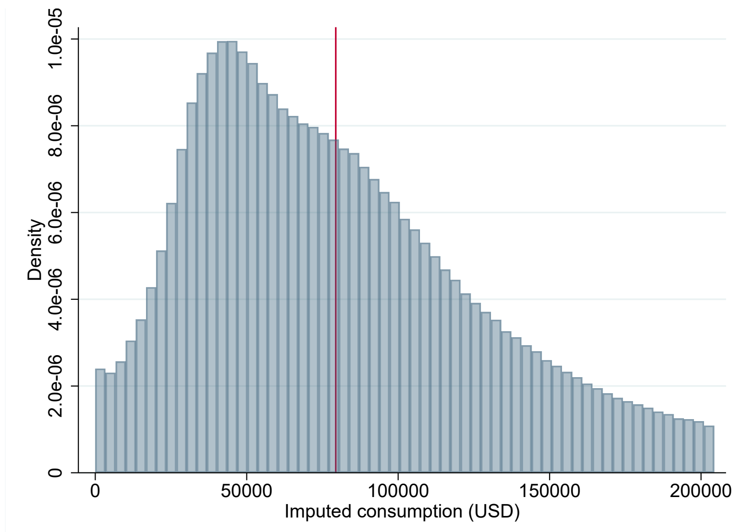

Figure A2: The distribution of imputed consumption (USD).
Notes: Consumption is imputed from tax data based on the description in Section 2. The lowest and highest 15% of imputed consumption values are dropped. The red line captures average imputed consumption at $85,000.

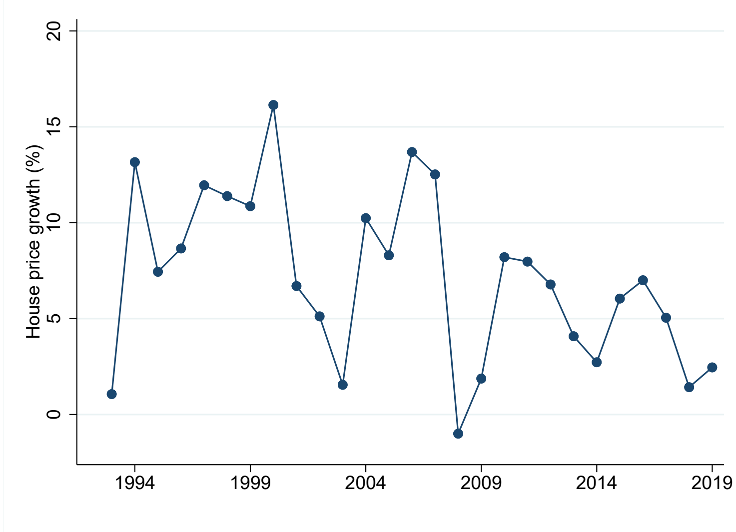

Figure A3: Annual house price growth (%).

B Additional Tables

Date	Regulation
2010 - March	LTV-cap of 90 % introduced
	Soft DTI-cap of 3 introduced
2011 - December	LTV-cap reduced to 85 %
	Soft DTI-cap removed
	Amortization requirement for loans with LTV > 70 % introduced
	Debt service capacity should be robust to a 5 pp interest rate increase
2015 - July	Current guidelines formalized into regulation
	Flexibility quota of 10 % introduced
2017 - January	DTI-cap of 5 introduced
	LTV-cap of 60 % for secondary housing in Oslo introduced
	Oslo specific flexibility quota of 8 % introduced
	Amortization requirement for loans with LTV > 60 % introduced

Table B1: Mortgage regulation

Notes: Key elements of the borrower-based mortgage regulation introduced between 2010 and 2017 for installment loans.

	(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
Observations	43,899	36,674
R^2	0.147	0.152

Table B2: Predicting LTV-ratios (%).

Notes: Results from estimating equation (2), 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) House Purchase	(2) House Purchase	(3) House Purchase	(4) House Purchase
$\hat{LTV}^{high} \times Post^{2010}$	-0.0494	-0.0319		
	(0.0793)	(0.109)		
$\hat{LTV}^{high} \times Post^{2012}$			-0.324***	-0.0649
			(0.115)	(0.152)
N	1,591,646	1,557,994	1,495,477	1,455,530
Clusters	430	431	430	431
Mean	5.38	5.38	5.47	5.47
Sample period	2009-2010	2009-2011	2011-2012	2011-2013
Year FE	Yes	Yes	Yes	Yes

Table B3: House purchase probability, (potential) first-time buyers

Notes: Results from estimating equation (3), with dependent variable house purchase probability (%). First time buyers only. $LTV^{high} = 1$ 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) 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 (5), 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 (5), 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 (3), 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**
	(0.0159)	(534.7)	(19.53)	(675.3)	(255.8)
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 (5), 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
$\hat{LTV}^{high} \times Post^{2012}$	-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 B8: House purchase probability (%) with house price growth interaction term
Notes: Results from estimating equation (3), 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	222156	222156	222156	222156	222156
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 B9: Balance sheet results, 2012-regulation with house price growth interaction term
Notes: Results from estimating equation (5), 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 B10: Placebo test. House purchase probability (%).
Notes: Results from estimating equation (3), 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) Deposits	(4) Debt	(5) House price	(6) Deposits
$\hat{LTV}^{high} \times Post^{2007}$	-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 B11: Placebo test. Balance sheet results.

Notes: Results from estimating equation (5), 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.

C 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[26], 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

$$i^m(1 - \frac{MRD}{100}) - i^s, \tag{20}$$

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 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

[26] 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.

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 (20) 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[27], 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.[28]

D Impact of LTV-restrictions on house price volatility

Allowing LTV-restrictions to also affect house price volatility (but not the correlation between house price growth and income growth), the partial derivative of equation (10) is now given by

$$\begin{aligned}\frac{\partial \sigma^2_{\Delta c}}{\partial \text{LTV-caps}} &= 2\frac{\partial MPC^y}{\partial \text{LTV-caps}} MPC^y \sigma^2_{\Delta y} + 2\frac{\partial MPC^w}{\partial \text{LTV-caps}} MPC^w \sigma^2_{\Delta w} \\ &+ 2\sigma_{\Delta y}\sigma_{\Delta w}\rho_{\Delta w,\Delta y}\left(\frac{\partial MPC^y}{\partial \text{LTV-caps}} MPC^w + \frac{\partial MPC^w}{\partial \text{LTV-caps}} MPC^y\right) \\ &+ 2MPC^y \frac{\partial \sigma_{\Delta w}}{\partial \text{LTV-caps}} \left(MPC^y \sigma_{\Delta w} + MPC^w \sigma_{\Delta y}\, \rho_{\Delta w,\Delta y}\right)\end{aligned} \tag{21}$$

, in which the last term is new relative to equation (11). In order to evaluate whether LTV restrictions also impact house price volatility 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

$$\sigma(\Delta w_{m,t}) = \alpha_m + \beta L\hat{T}V^{high}_m \times I^{post}_t + \gamma L\hat{T}V^{high}_m + \epsilon_{m,t} \tag{22}$$

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,

[27] 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)

[28] 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.

but not significantly so. Still, we compute the impact on consumption volatility under the assumption that the coefficient estimate gives us the actual impact of LTV-restrictions on house price volatility. We know that 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 $\frac{\partial \sigma_{\Delta w}}{\partial \text{LTV-caps}} = -0.008 \frac{4}{2.4} = 0.005$, meaning that the final term is negative and thus contributes to reduced consumption volatility. Plugging in the numbers discussed in the main text, we find that the consumption volatility for the full sample now falls by 1.6%, while consumption volatility for home buyers increases by 1.3%. Hence, the dampening impact on house price volatility is sufficient to make up for the increase in average MPCs, but not sufficient to make up for the increase in MPCs for house buyers.