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Mortgage Regulation and Financial Vulnerability at the Household Level*

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Abstract

We evaluate the impact of mortgage regulation on credit volumes, household balance sheets and the reaction to adverse economic shocks. Using a comprehensive dataset of all housing transactions in Norway matched with buyers' balance sheet information from official tax records, we identify causal effects of mortgage loan-to-value (LTV) limits. Our results show that LTV-requirements have substantial effects on credit volumes, especially on the extensive margin. As a result, such requirements contribute to dampening aggregate credit growth. We find that affected households lower their debt uptake and face lower interest expenses, thereby reducing their vulnerability to adverse shocks. However, affected households also deplete liquid assets when purchasing a home, in order to meet the new requirement. This negative effect on liquid savings persists in the years following the house purchase, suggesting that the impact on financial vulnerability at the household level is in fact ambiguous. We illustrate this further by documenting that households affected by the regulation are more likely to sell their home when becoming unemployed compared to non-affected households.

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

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

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1 Introduction

The financial crisis and its aftermath made the importance of household leverage for financial stability strikingly clear. Subsequently, a large literature has documented the potential risks of rapid growth in house prices and debt for economic developments, some prominent examples including Mian and Sufi (2011), Eggertsson and Krugman (2012), Korinek and Simsek (2016), Farhi and Werning (2016) and Mian et al. (2017). In response to these concerns, a broad range of countries have implemented macro-prudential policies in the form of mortgage market regulation. These policies are intended to reduce the financial vulnerability of households, making them more robust to adverse economic shocks, see e.g., Corbae and Quintin (2015) and Greenwald (2018). However, despite the global adoption of such policies, the empirical evidence for their efficacy and impact at the household level is still limited.

In this paper, we provide evidence on how mortgage regulation impacts leverage, liquidity and decision-making at the household level. We study the effects of loan-to-value (LTV) requirements using administrative Norwegian tax data merged with housing transaction data from the Land Registry. We show that the introduction of LTV-limits leads to both a reduction in leverage for observed mortgages (the intensive margin), and to a reduction in the number of mortgages (the extensive margin). Especially the latter effect implies that LTV-limits have substantial effects on aggregate credit growth.[1] Considering household balance sheets, we show that conditional on purchasing a house, affected borrowers respond to the regulation by reducing both total debt and the purchase price of the house.[2] As a result of having less debt, they also face lower interest expenses. However, affected borrowers further respond to the regulation by reducing their liquid wealth. This implies that households are left with smaller financial buffers after purchasing a house, making the total effect on their financial resilience ambiguous. We show that when subject to an adverse economic shock, affected households are *more* likely to sell their homes. These results suggest that reduced liquid buffers due to LTV-requirements can make households more prone to amplifying economic downturns, in response to systemic increases in unemployment, by contributing to depressed house prices.

Our data contains detailed information on income and wealth, and lets us study the balance sheet effects of the regulation. In order to identify house buyers and ly 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 when the house is purchased. Because only collateralized debt is supposed to enter into 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

[1]This supplements earlier survey evidence (Fuster and Zafar, 2016, 2020) and the theoretical work by Gete and Reher (2016).

[2]As Dougherty and Van Order (1982) show, the introduction of credit constraints raises the real user cost of housing, and thus lowers housing demand for constrained households.

of 90 percent in 2010, and then lowered this to 85 percent in 2012. We study the effect of both of these policy changes, in which the former constituted a new requirement whereas the latter was a tightening of the existing requirement.

Using methods from the bunching literature (see for instance Kleven (2016) for a review), we estimate the intensive and extensive margin effects on loan-to-value distributions. We rely on the time series dimension of the data to construct counterfactual distributions, allowing us to quantify the two effects. The introduction of the first LTV-limit led to a reduction in LTV-ratios for four percent of affected mortgages, while causing five percent of affected mortgages to be eliminated from the market. The following tightening of the LTV-limit had a similar effect on the *intensive* margin, but a substantially larger effect on the *extensive* margin. In this case, twenty-five percent of affected mortgages were eliminated from the market, causing aggregate credit growth to fall by 1.3 percentage points or twenty percent. We show that both the extensive and intensive margins are driven by low-liquid households. In line with the effects on credit volumes, we also find statistically significant effects on the price of credit. Compared to the pre-regulation period, the interest rate on high LTV-mortgages increase by 3-5 percent. This result is robust to controlling for variables such as demographics, income and wealth, suggesting that the price effect is not driven by differential selection into high LTV-mortgages following the introduction of the new requirements.

Given the administrative tax data, we can further explore the effects on household balance sheets. In order to do so, we predict LTV-ratios based on pre-regulation data. This allows us to use a difference in difference framework, comparing households likely to be affected by the regulation to other households, before and after the regulation is introduced. The difference in difference analysis confirms that – conditional on buying a house – affected households reduce LTV-ratios in response to the limits. Moreover, affected households reduce both total non-student debt and the purchase price of the house. As a result of lower debt, these households also have lower interest expenses. We thus conclude that the introduction of LTV-limits reduces household debt burdens, potentially reducing financial vulnerability.[3]

While the reduction in debt burdens was part of the intended consequences of these policies, we also find evidence of a potentially unintended consequence. Affected households respond to the LTV-limits by reducing liquid savings in the form of bank deposits. Intuitively, for a given house purchase price, a stricter LTV-limit requires the borrower to use more equity when buying the house. Compared to the pre-regulation period, affected borrowers reduce bank deposits by almost ten percent. This finding can have important macro-economic consequences, as recent papers have emphasized the importance of liquid wealth in affecting household consumption behavior (see for instance Kaplan and Violante (2014) for theoretical predictions and Fuster et al. (2018), Christelis et al. (2019) and Fagereng et al. (2019) for empirical support). In principle, the reduction in liquid savings could be short lived, if households use the lower interest expenses to rebuild their financial

[3] Baker (2018) shows that highly indebted households tend to cut consumption more in response to negative income shocks than relatively less indebted households.

buffers following a house purchase. However, we find little support for this. Instead, we show in an event study setup that the reduction in bank deposits shows no sign of convergence even four years after the house purchase. As a result, we conclude that the introduction of LTV-limits led to a reduction in financial buffers for affected home buyers which in turn has increased the fraction of "wealthy hand-to-mouth" households (Kaplan et al., 2014).[4]

To further assess the implications of lower liquid buffers for household behavior, we investigate how affected households respond to negative economic shocks. Specifically, we focus on households experiencing unemployment, and compare the behavior of high predicted LTV households who purchased a home *after* the requirement to those who purchased a home *before* the requirement. We show that affected households are more likely to liquidate their illiquid wealth in response to unemployment. That is, the likelihood of a house sale in response to unemployment is significantly higher for borrowers who purchased their house following the new regulation. This indicates that the reduction in liquid wealth caused by the reforms increases the need for households to access their illiquid wealth when subject to an adverse shock. Given that many households may experience unemployment simultaneously due to macroeconomic conditions, this result suggests that LTV-regulation can amplify the effect of economic downturns on house prices (Shleifer and Vishny, 2011).

Our paper contributes to the empirical literature on the consequences of macroprudential policies by focusing on household behavior. While there is a large literature examining the effects of many of the ex post policies that were primarily aimed at the immediate problems generated by the financial crisis[5], there has been less empirical work evaluating ex ante policies that look to regulate household leverage going forward. Despite the prominent role of LTV-requirements in the past decade, the quantitative evidence on their efficacy is still limited. Moreover, the majority of earlier studies have paid most attention to the aggregate benefits of LTV tightenings on house prices and household debt (Vandenbussche et al., 2015; Kuttner and Shim, 2016; Cerutti et al., 2017; Akinci and Olmstead-Rumsey, 2018), or bank lending (Claessens et al., 2013; Morgan et al., 2019). Yet, the effects of LTV-limits at the household level are not well understood.

Recently, a handful of papers have used micro data to study the impact of borrower-based macro-prudential policy. Epure et al. (2018) use credit registry data from Romania to study the impact of several macro-prudential policies – including LTV-limits – on bank credit to households. They find that tighter macro-prudential conditions are associated with a decline in household credit, especially for riskier loans in foreign currency. Acharya et al. (2019) use loan level data from Ireland to study the impact of loan-to-value and loan-to-income requirements. Their results indicate that mortgage credit is reallocated from low to high income borrowers, and from urban

[4]"Wealthy hand-to-mouth" households have high net wealth, but hold little of their wealth in liquid assets. Such households are characterized as having high marginal propensities to consume.

[5]This includes, among others, policies aimed at restructuring household debt (Mayer et al., 2014; Agarwal et al., 2017; Ganong and Noel, 2018) and policies providing direct fiscal stimulus (Mian and Sufi, 2012; Berger et al., 2020).

areas to rural areas – where borrowers typically are further away from the lending limits. Moreover, the authors show that this reallocation of credit slows down the feedback effect between mortgage credit and house prices. Borchgrevink and Torstensen (2018) analyze the effect of a Norwegian debt-to-income (DTI) requirement, and find that higher exposure to the requirement at the district level is associated with lower house price growth.

The two papers most similar to ours are perhaps DeFusco et al. (2020) and Van Bekkum et al. (2019). The former uses loan level data to study the impact of a US DTI requirement on credit volumes and credit prices. They find that 15 percent of the affected market was eliminated as a result of the regulation, an effect which is similar to the average effect documented across the two policy changes in this paper. However, they find larger effects along the intensive margin. While our findings are largely consistent with those of DeFusco et al. (2020), we contribute to the literature by using *household level* data to further explore the impact on household balance sheets and show that affected borrowers respond to the regulation by reducing total debt and buying cheaper housing. In line with the extensive margin effect, we also show that the probability of purchasing a house is reduced, and that this effect is not limited to the year in which the regulation is introduced.

To the best of our knowledge, the only other paper using administrative household data to study the impact of borrower-based macro-prudential regulation is Van Bekkum et al. (2019). They study the impact of a Dutch LTV-limit, and also find that affected borrowers respond to the regulation by reducing LTV-ratios and total debt. While Van Bekkum et al. (2019) also find a negative impact on liquid savings, this effect is – in their case – short lived, as liquidity positions are fully recovered within two years. On the contrary, our results show liquid savings being consistently lower also four years after the house purchase. As a result, our findings suggest that the negative impacts of this kind of regulation might be long-lasting and severe, and should be taken into account when considering the net effect. This is further illustrated by our novel finding that affected households have a higher probability of liquidating their housing wealth when facing an unemployment spell, which might pose a threat to macroeconomic stability.

2 Institutional background

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 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 decreased from 90 to 85 percent.[6] 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.

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.

Alongside the requirements levied on loan-to-values, the guidelines and the preceding 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.[7] Key elements of the regualation are summarized in Appendix Table B1.

In this paper, we focus on the two LTV-caps 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, while there might be some effect of the first requirement in 2010, we consider 2011 as the first year in the post-period. 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 are simpler. 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.

[6] This is comparable to other advanced economies where the LTV-limits mostly range from 80 to 95 percent. One exception, however, is the Netherlands where the LTV-limits were set to 106.

[7] 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.

3 Data

We use administrative Norwegian tax data, 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 provide 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 house buyers in a given year. We note that Norway has a relatively high homeownership rate, in which roughly 80 percent of households live in owner-occupied housing. As a comparison, homeownership rates in the US are around 65 percent. In order to calculate LTV-ratios, we use non-student debt from the tax data and house purchase prices from the Land Registry.

We start by aggregating our data to the household level, and exclude the household if the household head is self-employed.[8] 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.[9] Specifically, we define $Mortgage\, debt_{it} = Total\, debt_{it} - Student\, debt_{it} - \overline{Unsecured\, debt}_t$. This shifts the estimated debt distribution slightly to the left, but does not alter the shape of the distribution.[10]

In addition to studying the impact on LTV-ratios, we also evaluate the impact on liquid savings and interest expenses. The former is proxied by bank deposits, although we also consider total financial assets. Bank deposits is 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.[11]

Interest rates are not directly reported in the tax data. Because we observe interest expenses and debt however, we can back out an implied interest rate. Note that interest expenses capture only the interest payments on the loan, and not the principal/installment. As this estimate is

[8] By combining the individual tax data with household identifiers from the population register, we aggregate all income and wealth information to the household level. In Norway, labor and capital income is taxed at the individual level, while the wealth tax is levied at the household level.

[9] Fagereng et al. (2020) show that the fraction of unsecured debt is fairly constant among high-leveraged households.

[10] If we do not adjust our mortgage debt measure for average holdings of unsecured debt, we obtain LTV-distributions in which parts of the observed bunching occurs slightly to the right of the LTV-cap, see the unadjusted distributions in Appendix Figure A2. For appropriate parameter values (i.e. $\underline{b}$ and B - see Section 4) we can replicate our baseline findings resulting from the *adjusted* mortgage debt measure using instead the *unadjusted* measure.

[11] 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.

somewhat noisy at the individual level, we drop estimates in the top and bottom fifth percentile, and show that the aggregated interest rate series match official interest rate data. It is also worth noting that Norway has a high share of floating rate mortgages, with less than ten percent of all mortgages having a fixed rate.

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 measure mortgage debt more directly using 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.

For much of the analysis, we restrict our sample to house buyers in the given year. Our sample is then a repeated set of cross-sections. When considering event studies around house purchases, the probability of a house purchase and the reaction to adverse shocks, we use the full panel dimension of the data.

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 table includes information on home-buyers balance sheets, house purchase price, age, LTV-ratios, 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.[12]

	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	29	2,000	7,000	54,000	69	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 for house buyers with $LTV \in [60, 110]$ in USD if not otherwise stated. All amounts in USD are rounded to the closest 1000.

After the requirements are introduced, we see a decline in the median LTV-ratio from 90 to

[12] 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 has been substantial fluctuations in the exchange rate over the sample period.

85 percent. Likewise, LTVs for the 25th and 75th percentile also fall, suggesting that the new restrictions had an impact on the aggregate distribution. Interestingly, we also observe that the fraction of first-time buyers fall from 54 to 43 percent. Furthermore, we observe an increase in the 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 A1.[13]. 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 lower than the increase in debt, resulting in higher DTI levels.[14]

3.1 LTV-distributions

Figure 1 depicts the distribution of LTV-ratios in our sample. Before discussing the effect of the requirements, we make two comments. First, there is a substantial share of loans which have LTV-ratios in excess of the maximum levels of 90 and 85 percent. This is evident in all years, and we can think of at least three plausible explanations. First, banks are allowed to exceed the requirements for 8-10 percent of new loans each year since 2015, or if an extraordinary risk assessment is undertaken in earlier years, suggesting that some of these loans are indeed in breach with the maximum LTV-level.

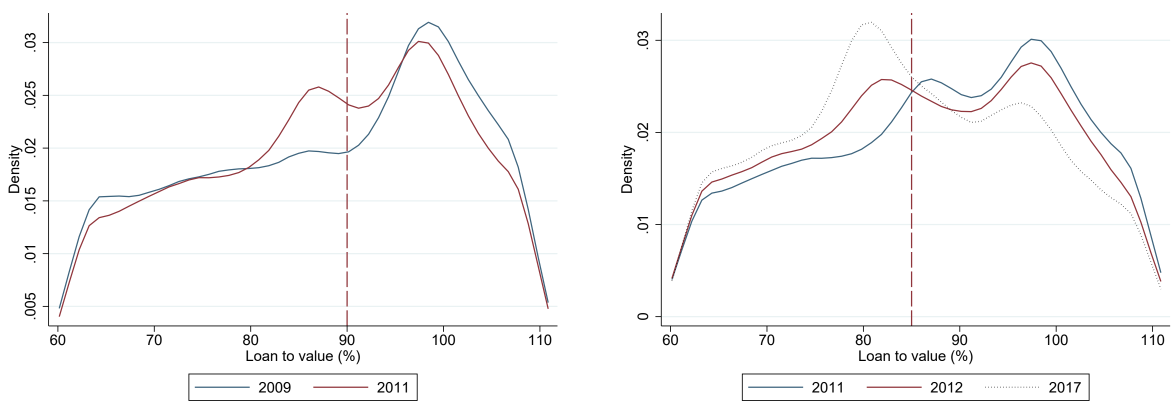

Figure 1: Kernel density plot of LTV-ratios for house buyers with LTV-ratios $\in [60, 110]$ by year.

Second, we do not directly observe mortgage debt, only total non-student debt. Holdings of car loans and consumer debt could therefore inflate our estimated LTV-ratios, as banks are only asked to consider debt backed by housing collateral in their LTV-calculations. Although we subtract the average amount of unsecured debt when calculating LTV-ratios, we do not know the distribution of unsecured non-student debt. As a result, the adjustment will not be precise, and will entail inflated

[13]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.

[14]As mentioned in Section 2, a DTI requirement of 5 was introduced in December 2016.

LTV-ratios for those with above average holdings of unsecured debt. Third, the denominator in our estimated LTV-ratio is based on the house purchase price only. It is, however, fully possible for the borrower to have additional collateral which will be included in the banks LTV-calculations. This would for example be the case if the borrower has a co-signer on the mortgage which is not a household member, such as a parent.

Some of the data issues discussed above can be alleviated by considering first-time buyers only, defined as households which have not previously had any reported housing wealth and have not previously purchased a home. For this group we calculate LTV-ratios using the *change* in non-student debt, rather than the stock of non-student debt. By definition, this group has no mortgage debt prior to the house purchase, and by subtracting previous non-student debt from our debt measure we can get rid of any pre-existing consumer debt or car loans. If the household takes on new non-mortgage debt in the year of the house purchase however, this would still be included in our measure. As seen from Figure 2, there is still substantial mass both in excess of the maximum LTV-levels and in excess of 100 percent. This suggests that measurement issues are unlikely to be the *only* explanation for the high observed LTV-mortgages.

In both Figures 1 and 2, the requirements seem to affect the distribution of LTV-ratios in our sample. In the left panels, we see a substantial increase in the share of mortgages which have LTV-ratios roughly equal to or just below 90 percent from 2009 to 2011. Similarly, as illustrated in the right panels, there is a substantial increase in the share of mortgages with LTV-ratios roughly equal to or just below 85 percent from 2011 to 2012. Note that the amount of bunching increases further year by year, resulting in a substantial increase from 2012 to 2017. This could be due to the formalization of the guidelines into regulation in 2015, or could be the result of both banks and borrowers adapting to the new regulation over time. We now move on to more formally study the impact of LTV-requirements.

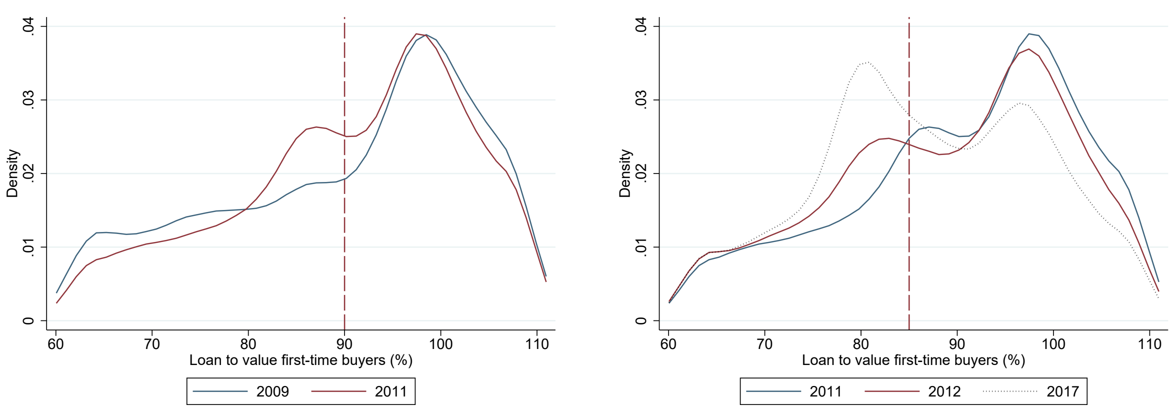

Figure 2: Kernel density plot of LTV-ratios for first-time house buyers with LTV-ratios $\in [60, 110]$ by year. First-buyers are defined as individuals who do not previously have housing wealth and have not previously bought a house.

4 The effect of LTV-restrictions on loan volumes and prices

In this section we first outline the bunching methodology used to estimate the intensive and extensive margin effects on credit volumes. We proceed by presenting results from the bunching analysis, documenting especially large effects along the extensive margin from the LTV-limit tightening in 2012. We further show that in line with the reduction in credit, there is also an increase in the price of credit, albeit quantitatively modest. The implications for aggregate credit growth are discussed in Section 6.

4.1 Research design

4.1.1 Assessing the impact on loan volumes

In order to evaluate the impact on loan volumes, we use methods from the bunching literature (see Kleven (2016) for a review). Conceptually, we use the distribution of LTV-ratios prior to and following the requirements to construct counterfactual LTV-distributions. Armed with these counterfactual distributions, we can measure the amount of *missing loans* with LTV-ratios in excess of the limit and the amount of *surplus loans* with LTV-ratios just below the limit. The latter is used to identify the intensive margin effect, whereas the difference between the former and the latter is used to identify the extensive margin effect.

We follow the setup in DeFusco et al. (2020), but adjust the structural assumptions to fit our data. While DeFusco et al. (2020) use cross-sectional variation to adjust for changes in the distribution not due to regulation, we instead rely on time series variation.[15] Every loan is assigned to an LTV bin denoted by b. We restrict our sample to only include loans with LTV-ratios between $b_{min} = 60$ and $b_{max} = 110$. The maximum LTV-ratio for which we expect bunching is denoted by B. Conceptually, B should simply be equal to the requirement. For instance, if the maximum LTV-level is set at 85 %, we would expect all bunching to occur exactly at this point, or slightly to the left of this point, in the distribution. For simplicity, we set B exactly equal to the maximum limit, but acknowledge that measurement issues could cause us to underestimate the effect if there is also bunching slightly above this level.

Let $n_{b,t}$ denote the number of loans at bin b in year t, and let $\hat{n}_{b,t}$ denote the counterfactual number of loans in the absence of the requirement. We make three assumptions which will allow us to construct counterfactual LTV-distributions.

1. Assumption I: No effect of requirement in the two years prior to implementation in year t.

   (a) Formally, $\hat{n}_{b,t-k} = n_{b,t-k}$ for $k = \{1, 2\}$

[15] Because the regulation in general applied to all mortgages, there is no cross-sectional variation available. The time series variation has the benefit of comparing identical products over time, instead of relying on developments in other markets. However, the downside is that we have to assume that changes from previous years are relevant also in reform-years, i.e. that any new time trends are caused by the reform.

2. Assumption II: Bottom end of the distribution is unaffected.

   (a) Formally, $\sum_{b_{min}}^{\underline{b}} \hat{n}_{b,t} = \sum_{b_{min}}^{\underline{b}} n_{b,t} \equiv N_{\underline{b},t}$ for some $\underline{b} < B$

3. Assumption III: Constant time trend.

   (a) Formally, $\frac{\hat{n}_{b,t}}{N_{\underline{b},t}} = \frac{n_{b,t-1}}{N_{\underline{b},t-1}} + \left(\frac{n_{b,t-1}}{N_{\underline{b},t-1}} - \frac{n_{b,t-2}}{N_{\underline{b},t-2}}\right) \equiv \hat{\pi}_{b,t}$

Assumption I simply states that the requirements have no effect in the years prior to implementation. This is crucial for constructing the counterfactual LTV-distributions, and implies limited anticipation effects.

Assumption II states that there exists some LTV-ratio $\underline{b}$, for which loans with LTV-ratios below this level are unaffected. The intuition being that if you find it optimal to take up a mortgage with an LTV-ratio of for instance 60 percent, a maximum LTV-level of 85 or 90 percent is unlikely to directly affect your behavior. This second assumption is useful as it gives us a way to scale the number of loans at each bin. Because the total number of mortgages change from year to year also for reasons unrelated to the regulation, a normalization is needed for meaningful comparisons across time.

Finally, given Assumption III we can use the change in LTV-distributions from one year to the next *prior* to the regulation, to construct counterfactual LTV-distributions *following* the regulation. That is, we allow for some change in distributions over time, also in absence of the requirement. This final assumption is perhaps the strongest of the three, and is somewhat similar to the standard assumption of parallel trends in a difference in difference analysis. Intuitively, we are assuming that except for the predicted development, any other change in the distribution of LTV-ratios is entirely due to the regulation.

Given Assumption 3, the counterfactual distribution in year t is given by $\hat{n}_{b,t} = \hat{\pi}_{b,t} \times N_{\underline{b},t}$. Bunching $\hat{B}$ is then defined as the amount of excess loans with LTV-ratios between the minimum bunching level $\underline{b}$ and the maximum bunching level B. Formally,

$$\hat{B} = \sum_{\underline{b}}^{B} (n_{b,t} - \hat{n}_{b,t}) \tag{1}$$

While we expect to see excess mass below the requirement, we also expect to see missing mass above the requirement. Specifically, missing mass is given by the number of missing loans $\hat{M}$ with LTV-ratios above the maximum bunching level. Formally,

$$\hat{M} = \sum_{B+1}^{b_{max}} (\hat{n}_{b,t} - n_{b,t}) \tag{2}$$

The intensive margin effect is defined as bunching relative to potentially affected loans, which is given by the number of loans with LTV-ratios in excess of the maximum bunching bin in the counterfactual distribution. That is, we define

$$\text{Intensive margin} = \frac{\hat{B}}{\hat{N}_{B+1,t}} \tag{3}$$

The extensive margin effect is defined as the difference between the amount of missing loans to the right of the limit and the amount of excess loans to the left of the limit, relative to the number of potentially affected loans. Intuitively, if the number of additional loans with relatively low LTV-levels perfectly equals the number of missing loans with relatively high LTV-levels, the extensive margin effect is zero. On the other hand, if the amount of bunching is small relative to the amount of missing loans, the extensive margin effect will be large. We define,

$$\text{Extensive margin} = \frac{\hat{M} - \hat{B}}{\hat{N}_{B+1,t}} \tag{4}$$

In order to provide confidence intervals for the estimated effects, we draw 100 bootstrapped samples from our data and redo the analysis for each sample. The results are then used to calculate standard errors for the intensive and extensive margin effects.

In order to validate Assumption I - III, we conduct a placebo test in which we estimate the counterfactual distribution in a year with no new regulation (the results are reported in Appendix A). Reassuringly, the counterfactual distribution is shown to closely match the actual distribution in this case.

4.1.2 Assessing the impact on prices

The bunching analysis lets us determine the effect on loan volumes for our sample. In order to look at price responses we use a difference in difference approach, with interest rates as our left-hand side variable. While we do not observe interest rates directly in our data, we back them out using interest expenses and debt.

We estimate equation (5), in which LTV_i^{high} is a dummy variable for having an LTV-ratio above the maximum limit (i.e. $LTV > B$). I_t^{post} is an indicator variable equal to one following the requirement, and equal to zero prior to the requirement. We include year fixed effects δ_t to capture macro level changes in interest rate levels. If banks charge customers extra to have LTV-ratios in excess of the maximum limit after the requirement is introduced, we expect to find $\hat{\beta} > 0$.

$$i_{i,t} = \alpha + \delta_t + \beta\, LTV_i^{high} \times I_t^{post} + \gamma LTV_i^{high} + \gamma' X_i + \epsilon_{i,t} \tag{5}$$

It is however possible that the requirement itself changes the quality of mortgage clients with high and low LTV-ratios. Specifically, having an LTV-ratio above the maximum level could be a worse signal after the requirement is introduced. On the other hand, those with high LTV-ratios following the requirement could be more likely to have access to additional, unobserved collateral, making them relatively less risky. In any event, we address this concern by controlling for variables such as financial wealth, income and demographics, captured by X_i.

4.2 Results

4.2.1 The effect of the 2010 LTV-cap

The first LTV-cap of 90 percent was introduced in mid-2010. We therefore use the 2009 distribution as the pre-reform baseline, and the 2011 distribution as the post-reform outcome. The time trend used in Assumption 3 is based on the change from 2008 to 2009, i.e. the last year-on-year change plausibly assumed to be unaffected by the reform. We note that the change in LTV-distributions from 2008 to 2009 might be influenced by the financial crisis, and a lower willingness to supply high LTV-loans. If this is the case, our counterfactual distributions will have "too few" high LTV-loans, which would cause us to *understate* the impact of the regulation.

Two key parameters need to be chosen, i) B: the maximum bin for which we expect to see bunching and ii) $\underline{b}$: the bin for which loans with lower LTV-ratios are assumed to be unaffected. We set $B = 90$, i.e. equal to the LTV-limit itself. This makes sense conceptually, as there should be no incentives to choose a LTV-ratio just above the regulated limit. However, by comparing the distributions in 2009 and 2010, see Appendix Figure A3, we find some evidence of excess mass also just above the maximum level. This could be due to measurement issues, and means that our choice of B is likely to be somewhat conservative.

There are less theoretical foundations for choosing $\underline{b}$. Based on the empirical distributions in 2009 and 2010, we set the minimum affected LTV-ratio parameter fifteen percentage points below the bunching parameter, i.e. $\underline{b} = 75$. This implies that mortgages which in absence of the regulation would have had an LTV-ratio below 75 percent are assumed to not be affected by the maximum LTV-limit of 90 percent. In the robustness section we explore how our results depend on the parameter values of B and $\underline{b}$.

From Assumption 3, the counterfactual LTV-distribution in 2011 is then given by $\hat{n}_{b,2011} = \hat{\pi}_{b,2011} \times N_{75,2011}$. We plot the counterfactual distribution alongside the observed distribution in Figure 3. As seen from the figure, the observed distribution has more loans with LTV-ratios just below 90 percent, and fewer loans with LTV-ratios above 90 percent.

Using the definitions in equations (1) and (3), we find that the intensive margin effect is 3.8 percent with a 95 percent confidence interval of $[0.3, 6.1]$. This implies that almost four percent of loans with LTV-ratios in excess of B (i.e. the "affected loans") had a lower LTV-ratio due to the reform. Similarly, we use the definitions in equations (2) and (4), and find that the extensive margin effect is 4.6 percent with a 95 percent confidence interval of $[-3.9, 13.3]$. This implies that the amount of missing mass to the right of B is larger than the amount of excess mass to the left of B, and that close to five percent of the affected loans were eliminated due to the reform. However, the extensive margin effect is not statistically significant for this initial LTV-cap.

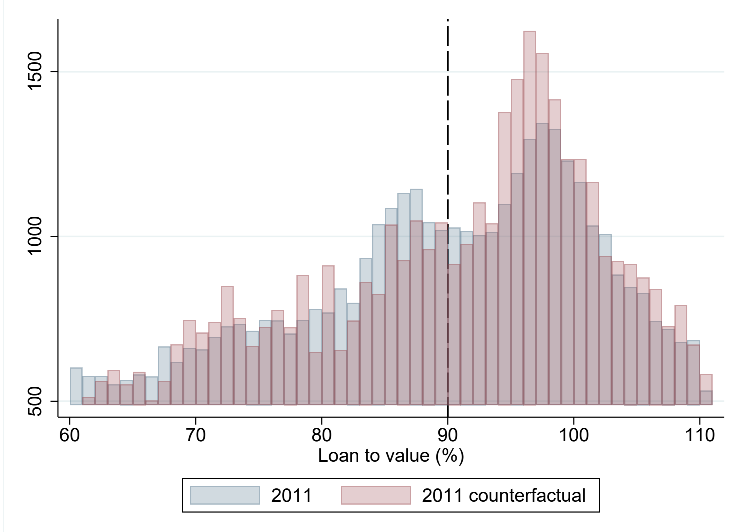

Figure 3: Histogram frequency plot of LTV-ratios in 2011 for house buyers with LTV-ratios $\in [60, 110]$ – observed and counterfactual.

4.2.2 The effect of the 2012 LTV-cap

The 2012 maximum LTV-level of 85 percent was announced in December 2011, and was enforced from early 2012. We therefore use 2011 as the pre-reform baseline distribution, and 2012 as the post-reform distribution. Because the change from 2010 to 2011 was affected by the 2011-requirement, we cannot use these years to adjust for the time trend in Assumption 3. Instead, we again rely on the change from 2008 to 2009, as this is the last year-on-year change assumed to be unaffected by regulation.

The 2011 and 2012 distributions are depicted in Appendix Figure A4. We pick the parameters B and $\underline{b}$ in the same way as for the previous limit. That is, we set $B = 85$, exactly equal to the new maximum level. The minimum affected LTV-level is again assumed to be fifteen percentage points below the bunching limit, implying $\underline{b} = 70$. In the robustness section, we illustrate how both the intensive and the extensive margin effects depend on these two parameter values.

From Assumption 3, the counterfactual LTV-distribution in 2012 is given by $\hat{n}_{b,2012} = \hat{\pi}_{b,2012} \times N_{70,2012}$. We plot the counterfactual distribution alongside the observed distribution in Figure 4. As seen from the figure, there is some excess mass just below the new maximum level, and considerably missing mass at higher bins. Using the definitions in equations (1) and (3), we find that the intensive margin effect is 3.0 percent with a 95 percent confidence interval of $[0.5, 4.9]$. This implies that three percent of loans with LTV-ratios in excess of B (i.e. the "affected loans") had a lower LTV-ratio due to the reform. Similarly, we use the definitions in equations (2) and (4), and find that the extensive margin effect is 25 percent with a a 95 percent confidence interval of $[14.2, 29.8]$. This implies that twenty-five percent of the affected loans were eliminated due to the reform. As discussed in Section 6, this will have a non-trivial impact on aggregate credit growth.

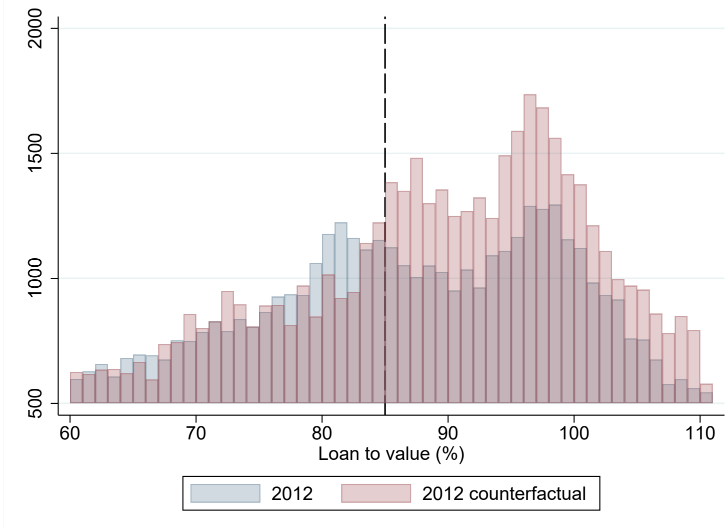

Figure 4: Histogram frequency plot of LTV-ratios in 2012 for house buyers with LTV-ratios $\in [60, 110]$ – observed and counterfactual.

Why are the extensive margin effects so much larger for the 2012 LTV-cap than for the 2010 LTV-cap? 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.[16]

Second, one could imagine that households responded to the first regulation mainly by adjusting alongside 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 purchase. 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. This implies that the strictness of the mortgage regulation may affect the response, including the relative importance of the intensive and extensive margin effects.

[16]It is also possible that there is more measurement error in the analysis of the first requirement. Because the regulation is introduced in mid-2010, we use 2009 as the baseline year in our construction of the counterfactual LTV-distribution in 2011. Ideally, using the year just prior to the new regulation – if this year was indeed unaffected by the requirements – would probably yield more precise results.

4.2.3 Price effects

Prior to 2015, banks were allowed to deviate from the LTV-requirement if an extraordinary risk assessment was undertaken. Since 2015, banks have been allowed to deviate from the requirement for a fixed share of all new mortgages. Given that deviating is costly, banks may charge a higher interest rate for mortgages in breach with the requirements.

While we do not observe interest rates directly, we can back them out by dividing interest expenses by debt. As this is a somewhat noisy measure, due to for instance individuals making large changes to their debt stock right before the end of the year, we drop interest rates in the top and bottom fifth percentile. As illustrated in Figure A5 in the appendix, our estimated aggregate interest rate series seems to follow official interest rate data closely. Our interest rate measure is however somewhat lower than the average mortgage rate for all new loans, perhaps reflecting the non-trivial share of student debt which enters into our measure – which generally has a lower interest rate than mortgage debt.

Regression results from estimating equation (5) are presented in Table 2. To be consistent with the above analyses, we let $LTV^{high} = 1$ if the LTV-ratio is above the maximum bunching bin B. This implies that when evaluating the 2011 requirement we define $LTV^{high} = 1$ if the LTV-ratio exceeds 90 percent, and when evaluating the 2012 requirement we define $LTV^{high} = 1$ if the LTV-ratio exceeds 85 percent.

Following the 2011 requirement, borrowers with high LTV-ratios faced a 0.18 percentage points higher mortgage rate relative to the pre-period, as seen from the first column in Table 2. This constitutes a five percent increase in interest rates for mortgages with LTV-ratios above the maximum level.[17] As seen from the second column, this coefficient estimate is largely unchanged when control variables are added.

The two final columns capture the price effect of the second requirement, effective from 2012. In this case, borrowers with high LTV-ratios faced a 0.08 percentage points higher interest rate relative to the pre-period. This implies an interest rate increase of about three percent. As before, the coefficient estimate is quantitatively robust to adding control variables, suggesting that the selection of borrowers into high or low LTV-ratios did not change substantially as the regulation was introduced.

While the results are consistent with banks charging a higher interest rate for mortgages with LTV-ratios that exceed the limits, there is also an alternative explanation. Because our backed-out interest rate applies to *all* debt, a higher interest rate could also be driven by an increase in unsecured debt. This would be the case if the regulation is inducing more borrowers with high

[17] Adelino et al. (2012) study the impact of changes in the conforming loan limit in the US on house prices, and find an elasticity of house prices to interest rates below 10, i.e. lower than many previous studies. Di Maggio and Kermani (2017) study the effect of increased credit to riskier borrowers on economic outcomes, and find that at ten percent increase in loan origination leads to 3.3 percentage points increase in the growth rate of house prices. Favara and Imbs (2015) study the impact of expansions in mortgage credit across US states on house prices, and find that a ten percent increase in credit increase the growth rate of house prices by 2 percent.

LTV-ratios to finance their house purchase with non-mortgage debt.

	(1) Interest Rate	(2) Interest Rate	(3) Interest Rate	(4) Interest Rate
$LTV^{high} \times Post^{2010}$	0.177***	0.174***		
	(0.0204)	(0.0217)		
$LTV^{high} \times Post^{2012}$			0.0788***	0.0726***
			(0.0141)	(0.0143)
N	182,111	182,111	208,772	208,772
Clusters	431	431	433	433
Mean interest rate	3.22	3.22	3.13	3.13
Controls	no	yes	no	yes
Sample period	2007-2011	2007-2011	2010-2014	2010-2014
Year FE	Yes	Yes	Yes	Yes

Table 2: Price effects.

Notes: Results from estimating equation (5), with dependent variable computed interest rate (%). $LTV^{high} = 1$ 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. 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.

4.2.4 Distributional impacts

In order to evaluate which type of households are more likely to be affected by (the intensive margin effect of) the regulation, we define *compliers* as households which have a predicted LTV-ratio in excess of the maximum bunching level B and an observed LTV-ratio in the interval $[\underline{b}, B]$. Looking at these households in 2011 and 2012, we find that compliers are younger, poorer, and more likely to be first time buyers than other households. These results are summarized in Table 3, and also hold when we condition on the observed LTV-ratio being in the interval $[\underline{b}, B]$ for the comparison group as well.

In section 5 we evaluate the impact on household balance sheets, and find that affected house buyers reduce debt levels, house purchase prices and liquid savings in response to the regulation. Keeping in mind which type of households are likely to be affected is important when interpreting the consequences of these results.

	2011			2012		
	LTV $\in [\underline{b}, B]$			LTV $\in [\underline{b}, B]$		
	Compliers	Others	Others	Compliers	Others	Others
LTV (%)	84	83	87	79	77	88
Age	29	39	37	33	43	37
Pre-tax income (USD)	99,000	135,000	128,000	121,000	155,000	134,000
Bank deposits (USD)	29,000	43,000	38,000	35,000	63,000	42,000
Gross financial wealth (USD)	73,000	90,000	81,000	63,000	157,000	140,000
First time buyers (%)	70	34	44	50	22	44

Table 3: Outcomes for compliers and others. For 2011, $\underline{b} = 75$ and $B = 90$. For 2012, $\underline{b} = 70$ and $B = 85$. Sample: house buyers with $LTV \in [60, 110]$.

4.2.5 Robustness

Before proceeding to the balance sheet results, we briefly discuss the robustness of our results, starting with a discussion of the key parameters in the bunching analysis.

In order to investigate the sensitivity of our results with respect to the chosen parameters, we investigate how the results change when we change the key parameters. Consider first the maximum bunching bin B. This was set to exactly match the regulated maximum levels. In practice, there appeared to be some bunching also in excess of the requirement, suggesting that our chosen values for B were somewhat conservative – especially for the first requirement. In the left panel of Appendix Figure A6, we illustrate how the intensive and extensive margin results vary for different values of B. If we increase B to values slightly above the maximum level, especially the intensive margin effect becomes somewhat larger. However, even if we set B equal to the maximum level plus five percentage points, the intensive margin effects are still only around five percent.

Varying $\underline{b}$ has a less monotonic effect on the results, as illustrated in the right panel of Appendix Figure A6. Lower values of $\underline{b}$ tend to increase the extensive margin effect and decrease the intensive margin effect, although not consistently so. Higher values of $\underline{b}$, if anything, has the opposite effect. Overall, we conclude that the results are relatively robust to different parameter values of the minimum affected LTV bin. This is reassuring, as it addresses a potential concern related to focal points for LTV-ratios. It is possible that by setting a standard for loan-to-value ratios, these types of requirements can induce borrowers also to *increase* their leverage. In that case, high values of $\underline{b}$ might result in Assumption 2 not holding, as also borrowers with relatively low LTV-ratios could be affected by the requirement. By setting a sufficiently low $\underline{b}$ however, this concern seems less relevant.

While the magnitudes of the estimated effects vary somewhat for different parameter values, the variation is within the 95 percent confidence intervals of our baseline results and the broad conclusions remain intact. That is, for the 2010 regulation, the intensive and extensive margin effects are of similar magnitudes, with the intensive margin effect generally being slightly below the extensive margin effect. In the case of the 2012 regulation, the extensive margin effect is consistently substantially higher than the intensive margin effect, and substantially higher than both effects in the previous regulation.

Another way to test the robustness of our assumptions is to use the methodology outlined in Section 4.1.1 to estimate the counterfactual LTV-distribution in years with no new regulation. Ideally, the counterfactual and the actual distribution should line up closely in this case. Because the degree of bunching seems to increase somewhat also in the years following the reform, we pick the years prior to the reform for our placebo tests. Specifically, we use 2006 as our baseline year, and the change from 2005 to 2006 for the adjustment term, to construct a counterfactual LTV distribution for 2007. Similarly, we use 2007 as our baseline year, and the change from 2006 to 2007 to construct a counterfactual LTV distribution for 2008. The resulting distributions are illustrated in Appendix Figure A7, and show that the counterfactual and actual distributions match relatively well in these non-reform years. The estimated intensive margin effects are less than two percent in both cases, and not statistically significant at the 95 and 99 percent level, respectively. The estimated extensive margin effect is negative in 2007, and quantitatively modest and statistically insignificant in 2008.

An important variable when evaluating household leverage is house prices. House price growth in Norway was strong in the years leading up to the financial crisis, and then negative to moderate in 2008 and 2009. Between 2010 and 2012 house price growth was stable at roughly eight percent, before falling somewhat in the following years. Does the growth in house prices affect our results?

Note that, we use the change in LTV distributions from 2008 to 2009 to construct counterfactual LTV distributions in the years in which new regulation was introduced. This means that the adjustment is done based on years with relatively low house price growth. If credit standards for borrowers with high LTV-ratios are stricter in times of low or negative house price growth, we might underestimate the magnitude of mortgages with high LTV-ratios. If such, our counterfactual distributions will have too few high LTV-mortgages, causing us to *underestimate* the impact of the regulation.

We end the robustness discussion by performing a placebo test for the interest rate findings, with results reported in Appendix Table B2. In our main price effect results in Table 2, households with LTV-ratios above the limit are charged a higher interest rate after the regulation is introduced. Reassuringly, when we replicate the analysis for previous years, i.e. years before the regulation is in place, we do not find evidence of such a price effect. In fact, the coefficient estimates are negative, suggesting that households with high LTV-ratios are charged a *lower* interest rate in the "post"-period.

5 The effect of LTV-restrictions on household balance sheets and financial vulnerability

In this section we start by discussing how we use predicted LTV-ratios to further investigate the impact on household balance sheets and the reaction to adverse economic shocks. In terms of household balance sheets, we document that affected households respond to the regulation by reducing LTV-ratios, non-student debt, interest expenses and house purchase prices. However, we also document a decline in liquid savings, implying that the net effect on financial vulnerability is ambiguous. To further explore this trade-off, we end the section by documenting that affected households are more likely to liquidate their housing wealth in response to an adverse economic shock. In the event of a recession, such an increase in the propensity for unemployed individuals to sell their house might amplify the economic downturn through house price depreciations.

5.1 Research design

5.1.1 Assessing the impact on household balance sheets

To evaluate the impact of LTV-restrictions on household balance sheets, we follow Van Bekkum et al. (2019), and compare individuals predicted to have a high LTV-ratio prior to and following the requirements in a difference in difference analysis. We analyze the impact on actual LTV-ratios, debt, interest expenses, housing values and liquid wealth.

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. Specifically, in the year prior to the requirement, we regress LTV-ratios on age, zip code, household type, sex, current and lagged income before and after tax, bank deposits, gross financial wealth, interest income, student debt, lagged non-student debt and lagged housing wealth. Given the predicted LTV-ratios $L\hat{T}V_i$, we define a dummy variable $L\hat{T}V_i^{high}$ which is equal to one for households with predicted LTV-ratios above the limit and zero otherwise. For robustness purposes, we also document that the results are not sensitive to using other, earlier, years to predict LTV-ratios.[18]

We then estimate equation (6) with dependent variables including observed LTV-ratios, debt volumes, interest expenses, house purchase prices and liquid assets such as bank deposits. Year fixed effects δ_t are included in order to capture common time varying factors. The coefficient of interest $\hat{\beta}$ captures the effect of being an affected household after the regulation is implemented, i.e. $I_t^{post} = 1$. Standard errors are clustered at the municipality level.

[18]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-caps. 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. Roughly 60% of all house buyers are classified correctly.

$$y_{i,t} = \alpha + \delta_t + \beta\, L\hat{T}V_i^{high} \times I_t^{post} + \gamma L\hat{T}V_i^{high} + \gamma' X_i + \epsilon_{i,t} \tag{6}$$

While the above analysis is done using a sample of home-buyers, we also consider the full panel and evaluate the impact on the probability of buying a house. This is done by estimating equation (6) with house purchase probability as the outcome variable.

Event study In addition to the difference in difference analysis outline above, we also do an event study around the time of the house purchase. This is useful as it allows us to map out the dynamics, and evaluate the persistence of the effects. Note that for this analysis we extend our sample to be a panel data set consisting of households with one house purchase, rather than the repeated cross-section we used previously. This allows us to study household balance sheets in the years prior to and following a house purchase.

We define a vector of time dummies for the four years pre- and post house purchase $I_{i,t}^k$, with k denoting the number of years since the house purchase, and estimate

$$y_{i,t} = \alpha_i + \delta_t + \sum_{k=-4}^{4} \beta_k\, I_{i,t}^k + \epsilon_{i,t} \tag{7}$$

where α_i captures individual fixed effects. The estimation of equation (7) is done separately for those who purchase a house prior to and following the requirements. In order to increase precision, we restrict the sample to only include households with high predicted LTV-ratios, as these are the ones likely to be affected by the regulation.

5.1.2 Assessing the reaction to adverse economic shocks

Finally, we investigate whether households affected by LTV-requirements respond differently to unemployment spells compared to non-affected households. In order to do so, we condition on three observables. First, we only consider households with a high predicted LTV-ratio, i.e. $L\hat{T}V_{i,t}^{high} = 1$, as these are the affected households. Second, we only consider households who purchased exactly one house in a one-year interval around the reform. Finally, we only include households who experience an unemployment spell after purchasing the house. For this sub-sample, we then estimate

$$y_{i,t} = \alpha_i + \delta_t + \beta\, HP_i^{post} \times U_{i,t} + \gamma U_{i,t} + \epsilon_{i,t} \tag{8}$$

where $y_{i,t}$ is either an indicator variable equal to 1 if the households sells their home, bank deposits or imputed consumption.[19] $U_{i,t}$ is an indicator variable equal to 1 if someone in the

[19] A challenge with studying the consumption and savings behavior of households is the lack of reliable panel data on household expenditures. Traditionally, studies have employed data on household consumption from surveys. However, surveys that follow the same households over time are rare, often have small sample sizes and face significant measurement issues. Instead, we follow Browning and Leth-Petersen (2003), Fagereng and Halvorsen (2017), Fagereng

household receives unemployment benefits in year t and HP_i^{post} is an indicator variable equal to 1 if the household purchased their home *after* the reform and are thereby *affected*. The individual and year fixed effects α_i and δ_t are included to make sure that we only consider within-household and within-year variation.

5.2 Results

5.2.1 Balance sheet effects

In this section we estimate equation (6) with different left-hand side variables from the households' balance sheets. In order to do so we first need to predict LTV-ratios based on pre-reform data, as outlined above. We regress LTV-ratios on demographics, income and wealth based on pre-reform data and use these estimates to predict LTV-ratios in the post-reform period. That is, for the 2010/2011 reform, we predict LTV-ratios based on 2009-data. Similarly, for the 2012 reform we predict LTV-ratios based on 2011 data. In both cases, the results are robust to using data from previous years as well.

We refer to households with high predicted LTV-ratios, i.e. $L\hat{T}V > B$, as affected households. It is worth noting that, perhaps not surprisingly, borrowers who are predicted to have high LTV-ratios are younger and more likely to be first time buyers compared to the full sample. As a result, they also have lower average income and wealth.

The results for the 2010 regulation 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 just above one percent. Affected borrowers also reduce their non-student debt holdings by more than six percent, as seen from the second column. As a result of lower debt, interest expenses also decrease. On average, interest expenses fall by three 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 roughly six percent in response to the regulation.

While the above responses can perhaps be described as intended consequences of the reform, there are also some potentially negative effects. As seen from the final column in Table 4, affected borrowers respond to the regulation also by reducing bank deposits. On average, bank deposits fall by close to nine percent following the reform. As reported in Appendix Table B3, there is also a fall in total financial wealth, but this is not statistically significant.

How persistent is this negative effect on deposits? 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 B3). We explore this further in an event study setup below, and show that even four years after the house purchase there is little sign of convergence.

et al. (2019) and Eika et al. (2020) and impute consumption based on household's balance sheets, disposable income and capital gains.

	(1) LTV	(2) Debt	(3) Int.Expenses	(4) House price	(5) Deposits
$\hat{LTV}^{high} \times Post^{2010}$	-0.847***	-21,536***	-329***	-26,045***	-3,390***
	(0.207)	(3,386)	(104)	(4,850)	(1,163)
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 requirement.

Notes: Results from estimating equation (6), with dependent variables LTV-ratios (%), non-student debt (USD), interest expenses (USD), house purchase prices (USD) and bank deposits (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 of the 2012 regulation. LTV-ratios are reduced by three percent, while debt is reduced by eleven percent. The negative impact on interest expenses is also larger than previously found, with average interest expenses declining by around fifteen percent. As before, the denominator is also affected, with average house prices falling by nine percent. Finally, bank deposits fall by roughly nine percent as well, the same magnitude as in the previous reform. As was the case before, total financial wealth is not significantly affected, but the negative impact on bank deposits persists in the two years following the house purchase – see Appendix Table B4.

	(1) LTV	(2) Debt	(3) Int.Expenses	(4) House price	(5) Deposits
$\hat{LTV}^{high} \times Post^{2012}$	-2.232***	-44,320***	-1,975***	-46,883***	-4,340***
	(0.173)	(4,047)	(192)	(5,359)	(1,616)
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 requirement.

Notes: Results from estimating equation (6), with dependent variables LTV-ratios (%), non-student debt (USD), interest expenses (USD), house purchase prices (USD) and bank deposits (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.

In Appendix Table B5 we report results from a placebo test for the balance sheet results. As discussed in the previous section, we use years prior to the reform for the placebo tests, as there appears to be continued bunching in the years following the regulation. Prior to the reform, we find no significant effect on debt uptake, house purchase prices or bank deposits.[20] 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.

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. In order to further explore the dynamics of liquid assets in relation to housing investments, we perform an event study with bank deposits as the dependent variable.

The left panel of Figure 5 separately depicts the evolution of bank deposits 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. 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 bank deposits 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 bank deposits prior to the reform is relatively similar, there is a larger decline in bank deposits following the purchase. Bank deposits fall by almost USD 20,000 from year $t-1$ to year $t+1$. Four years after the purchase, bank deposits are still significantly lower than at baseline, with no sign of convergence.

As seen from the right panel of Figure 5, the negative effect on bank deposits is persistent also for first time buyers. 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 bank deposits – compared to a slight increase for those who purchased their home prior to the reform.

Our results differ from those in Van Bekkum et al. (2019), who find that liquid savings quickly converges after the house purchase. While we do not know what is causing this difference, we offer two possible explanations. The first relates to actual and expected house price growth. House price growth has been stronger in Norway than in the Netherlands over the relevant period, causing Norwegian households to have relatively high expectations for house price gains. *As long as house prices are increasing*, a home buyer will be able to extract liquidity from his or her house in the near future, reducing the need for precautionary saving in the form of bank deposits. The

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

second explanation relates to differences in the value of the LTV-limits. The maximum LTV-level in Norway (85 percent) is considerably stricter than in the Netherlands (106 percent), making Norwegian households more likely to tear down their liquid assets when purchasing a house.

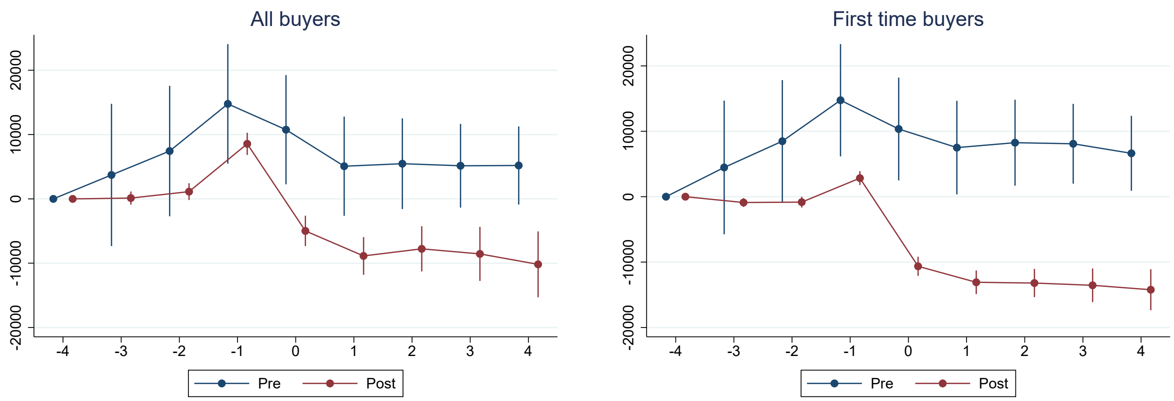

Figure 5: Bank deposits event study (USD). Year $t = -4$ is used as the base level and normalized to zero. Households with high predicted LTV-ratios who undertake one house purchase in the period 2008-2015.

Do the estimated reductions in bank deposits pose a threat to the financial vulnerability at the household level? While households on average have relatively large holdings of liquid assets, the distribution is quite skewed. In order to get a sense of the vulnerability, we report some simple summary statistics in Table 6. Prior to the reform, 22 percent of house buyers reduce bank deposits to less than 75 percent of the baseline value. Following the reform, this share increases to 30 percent. The median household in this group has USD 12,200 in bank deposits following the house purchase, while the 25th percentile has USD 3,400. A smaller share – 4.4 percent in the pre-period and 5.3 percent in the post-period – reduce deposits to *less than ten percent* of their baseline value. For this group, the median household has USD 1,700 worth of bank deposits following the house purchase, and the 25th percentile has USD 300. Hence, this group – albeit quantitatively small – is left with virtually no liquid savings following their house purchase.

			Deposits at time $t+1$ (USD)	
Share who reduce bank deposits to less than:	Pre-reform	Post-reform	50th prct.	25th prct.
75 % of $t-1$ value	22 %	30 %	12,200	3,400
50 % of $t-1$ value	16 %	22 %	8,600	2,300
25 % of $t-1$ value	9.1 %	12 %	4,500	1,000
10 % of $t-1$ value	4.4 %	5.3 %	1,700	300

Table 6: Share of house buyers who reduce bank deposits to less than X % from year $t-1$ to year $t+1$.

House purchase probability - average effect The above results capture the balance sheet effects for households who purchase a house following the requirement, i.e. the balance sheet effects of the intensive margin. However, we know from Section 4 that there are also sizable extensive margin effects. These can also be studied in a similar difference in difference framework using predicted LTV-ratios. Specifically, we estimate equation (6) using an indicator variable for buying a house as our dependent variable. The results are reported in Table 7, and confirm that the probability of buying a house decreases following the reform.

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. 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 0.14 percentage points lower probability of purchasing a house following the new regulation – a decrease of three percent.

We know from the previous analysis that the extensive margin effect was substantially larger following the LTV-tightening in 2012, and so we expect larger effects on house purchase probabilities from the second requirement. The results in the last two columns of Table 7 confirm that this is indeed the case. 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 fall by 0.34 percentage points or 6.5 percent. Considering the reform year and the year *after* the reform yields similar results. Hence, the results from the difference in difference analysis supports the findings from the bunching analysis, in that especially the LTV-tightening in 2012 had important extensive margin effects.

	(1) House Purchase	(2) House Purchase	(3) House Purchase	(4) House Purchase
$\hat{LTV}^{high} \times Post^{2010}$	-0.0776	-0.143**		
	(0.0599)	(0.0717)		
$\hat{LTV}^{high} \times Post^{2012}$			-0.336***	-0.392***
			(0.0519)	(0.0758)
N	4,352,860	4,394,038	4,508,483	4,510,650
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 7: House purchase probability (%).

Notes: Results from estimating equation (6), 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.

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 7 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 constricting the sample to only considering (potential) first time buyers. As shown in Appendix Table B6, 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.

A potential concern - as in the bunching analysis - is that house price growth affects the outcomes considered in this section. The year fixed effects should capture any effect of house prices which is common to all groups. There is however a concern that individuals with high predicted LTV-ratios are differentially affected by house price growth through, for instance, changes in credit standards. A comparison of our results in 2010/2011 and 2012 suggests that this is not a big concern, however. Note that for the first LTV-limit, the pre-period is one of low house price growth, while the post-period is one of relatively high house price growth. For the second LTV-limit the situation is flipped, with relatively high house price growth in the pre-period and lower house price growth in the post-period. Despite this, the results are consistent for the two reforms, suggesting that our findings are not heavily influenced by house price growth.

House purchase probability - heterogeneous effects 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 7. Because we know that borrowers use more liquid savings to buy a home after the requirements, a natural hypothesis is that households with less bank deposits will have a larger probability of not buying a house in response to the regulation. As seen from Table 8, 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 8, 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.

	(1) House Purchase	(2) House Purchase
$\hat{LTV}^{high} \times Post^{2010}$	-0.692***	
	(0.111)	
$\hat{LTV}^{high} \times Post^{2010} \times Deposits^{high}_{t-1}$	1.76***	
	(0.363)	
$\hat{LTV}^{high} \times Post^{2012}$		-1.36***
		(0.228)
$\hat{LTV}^{high} \times Post^{2012} \times Deposits^{high}_{t-1}$		2.57***
		(0.544)
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 8: Heterogeneous effects along the extensive margin.

Notes: Results from estimating equation (6), 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.

5.2.2 The reaction to adverse shocks

The previous subsection documented how households which purchased houses after the reform reduced both LTV-ratios and liquid savings. In this section, we investigate further the implications of this adjustment for household's ability to withstand large, negative shocks. We focus on one very salient form of adverse shocks, namely unemployment.

We consider two different specifications. First, we condition on unemployment occurring no more than three years after the house purchase. This implies that we on average should expect roughly equal amounts of time between the house purchase and the unemployment spell for our control and treatment groups. However, it also implies that the control and treatment group could become unemployed at times of systematically different macro-economic conditions, which might again affect the outcome variables. We therefore also consider an alternative sample, in which we only condition on unemployment occurring after the house purchase, but with no time limit. That is, unemployment can occur until 2017, the last year in our sample. In this case, a large share of the unemployment spells for both control and treatment groups occurs post-2014, in relation to the oil price collapse of mid-2014 (see Juelsrud and Wold (2019) for the employment effects of the 2014 oil price collapse in Norway).

Focusing on the 2012 requirement first, the estimated impact of becoming unemployed for affected households relative to non-affected households is shown in Table 9. Starting with the first column, we see that affected households – that is, households with a high predicted LTV-ratio who purchased a house *after* the 2012 LTV-cap - have an *increased* likelihood of selling their house when becoming unemployed. This is the case both when conditioning on unemployment occurring within three years of the house purchase (*Short*), and when considering the full sample (*Full*). That is, in response to unemployment, these households are 1.5-2.4 percentage points more likely to liquidate their housing wealth following the new regulation. This effect is large and constitutes an increase of 25-40 percent.

The gains from the house sale shows up in somewhat higher bank deposits, although this effect is only significant when considering the full sample, and then only at the ten percent level. As seen from the final two columns, we do not find any significant effect on imputed consumption. That is, the consumption level in response to unemployment does not differ systematically across households who purchased their home right before or right after the LTV-tightening. Overall, the results appear consistent with affected households liquidating their illiquid assets in order to smooth consumption.

	(1) House sale	(2) House sale	(3) Deposits	(4) Deposits	(5) Imp. cons.	(6) Imp. cons.
$HP^{post} \times U$	2.43**	1.48***	2,271	1,636*	-8,873	-2,725
	(0.986)	(0.565)	(1600)	(925)	(10183)	(5590)
N	38,937	58,641	38,937	58,641	38,937	58,641
Clusters	404	406	404	406	404	406
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Short	Full	Short	Full	Short	Full

Table 9: Household vulnerability, 2012 requirement.

Notes: Results from estimating equation (8), with dependent variables house sale probability (%), bank deposits (USD) and imputed consumption (USD). $HP^{post} = 1$ if the household purchased a house in 2012-2014 and zero otherwise. $U = 1$ if the household received unemployment benefits after the house purchase, and zero otherwise. Sample: Households who purchase one house in the sample period, and for which $L\hat{T}V > 85$. Sample period: *short* conditions on unemployment occurring within 3 years of house purchase, *full* includes unemployment up until 2017. Standard errors are clustered at the municipality level. * p<0.1, ** p<0.05, ***p<0.01.

In Table 10, we redo the estimation for the 2010 requirement. We have fewer observations in this case and the results are less conclusive. We note, however, that the house sale coefficient has the same sign as in Table 9. However, it is imprecisely estimated and we fail to reject the null hypothesis. The same holds for bank deposits and imputed consumption.

	(1) House sale	(2) House sale	(3) Deposits	(4) Deposits	(5) Imp. cons.	(6) Imp. cons.
$HP^{post} \times U$	0.947	0.525	-5,104	-4,143	2,243	13,139
	(1.30)	(0.856)	(5,657)	(4,371)	(16,140)	(11,817)
N	15,776	21,291	15,776	21,291	15,776	21,291
Clusters	380	380	380	380	380	380
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Short	Full	Short	Full		Full

Table 10: Household vulnerability, 2010 requirement.

Notes: Results from estimating equation (8), with dependent variables house sale probability (%), bank deposits (USD) and imputed consumption (USD). $HP^{post} = 1$ if the household purchased a house in 2010-2011 and zero otherwise. $U = 1$ if the household received unemployment benefits after the house purchase, and zero otherwise. Sample: Households who purchase one house in the sample period, and for which $L\hat{T}V > 90$. Sample period: *short* conditions on unemployment occurring within 3 years of house purchase, *full* includes unemployment up until 2017. Standard errors are clustered at the municipality level. * p<0.1, ** p<0.05, ***p<0.01.

The findings in this section highlight a potentially unintended consequence of implementing borrower-based mortgage regulation in the form of LTV-caps. To the extent that it drains liquid savings, and given that households do not rebuild their liquid buffers, LTV-caps increase the likelihood that households in response to an adverse shock need to liquidate their housing wealth. To the extent that adverse shocks are idiosyncratic, this may be unproblematic for the macro economy. In response to a systemic shock, however, the lack of liquid savings and associated increased likelihood of liquidating housing wealth can potentially contribute to house price depreciation. This might in turn affect the consumption of other homeowners, implying a potentially amplifying effect on economic downturns.

6 Aggregate effects

The introduction of (tighter) mortgage regulation was partly motivated by a concern for high aggregate household credit growth. Our results so far show that the regulation led to both a reduction in LTV-ratios for observed mortgages (the intensive margin), and to a reduction in the number of mortgages (the extensive margin). In order to estimate the impact on *aggregate* credit growth, further assumptions are needed. Our results in Section 4 only apply to the sample used, i.e. to wage-taking households who purchase a new home in the given year and have an LTV-ratio between 60 and 110 percent. In order to provide some back of the envelope calculations on the impact on total credit growth, we need to make further assumptions on how households not in our sample were affected.

We first note that households not in our sample are a diverse group. Some of these are home buyers with business income, or home buyers with LTV-ratios outside the interval 60-110 percent. It seems plausible that these households were also affected by the regulation to some extent. Similarly, some of these households are borrowers who refinanced their existing mortgage, and might also have been affected by the new regulation. Finally, the remaining group consists of households who did not take up a new mortgage or refinance an existing mortgage in the given year. These households are less likely to be directly affected. In addition to the direct effects however, there are also potentially important indirect effects. For instance, if parents are more likely to co-sign a mortgage with their adult children, they may be less likely to increase their own personal debt. Also general equilibrium effects working through house prices, interest rates etc. may affect aggregate credit growth through several channels.

As our baseline, we assume that all house buyers are affected the same way that our in-sample house buyers are affected. Moreover, we assume no effect on non-home buyers. This implies that there is no effect on households who refinance their mortgage for instance, which might lead to a downward bias. However, there could also be sources of upward bias, for example caused by parents co-signing a mortgage with their adult children and therefore reducing their own private credit growth. Still, we view our baseline assumptions as relatively conservative.

	2011	2012
Extensive margin:		
Eliminated loans	947	6,149
$\Delta\text{Debt}\|_{b\in[B,b_{max}]}$ (1000 USD)	285	339
Eliminated credit (1000 USD)	269,895	2,084,511
Intensive margin:		
Shifted loans	779	751
$\Delta\text{Debt}\|_{b\in[B,b_{max}]}$ - $\Delta\text{Debt}\|_{b\in[b_{min},B]}$ (1000 USD)	36	46
Eliminated credit (1000 USD)	28,044	34,546
Total:		
Eliminated credit (1000 USD)	297,939	2,119,057

Table 11: Eliminated credit from LTV-caps in 2011 and 2012 – Extensive and intensive margin.

In 2011, almost 1,000 loans were eliminated due to the extensive margin. On average, these loans – which by definition had LTV-ratios above the bunching cap B – led to a debt increase of about USD 285,000. As a result, the extensive margin effect led to a reduction in debt uptake of about USD 270 million. At the same time, almost 800 loans had a reduced LTV-ratio due to the intensive margin. On average, the reduction in LTV-ratios on these loans implied that debt uptake was on average about 36,000 USD less. As a result, the intensive margin effect led to a reduction in debt uptake of about USD 28 million. Note that because the intensive margin effect does not eliminate a new mortgage, only reduce it, the extensive margin effect has a much larger impact on aggregate credit growth. Summing up the two effects, the total reduction in credit resulting from the 2011 LTV-cap was roughly USD 298 million, as outlined in the first column of Table 11.

In 2012, a substantially higher amount of loans was eliminated due to the extensive margin response. As a result, the extensive margin effect resulted in a reduction in debt uptake of USD 2,085 million. The intensive margin effect was modest and similar to that in the previous year, implying a total reduction in credit of USD 2,119 million, as seen from the second column of Table 11.

On average across 2011 and 2012, our sample captures roughly 40 percent of home buyers. The remaining share of home buyers are either self-employed, or have LTV-ratios outside the considered range. Scaling up the amount of eliminated credit, we thus find that – given our assumptions – credit was reduced by USD 745 million in 2011 and USD 5,298 million in 2012.

In 2011, aggregate household credit increased by USD 26,384 million or 7.3 percent. Without the introduction of the LTV-requirement, we estimate that the increase would have been USD 27,129 million, or 7.5 percent. Hence, our findings suggest that aggregate credit growth would have been 0.2 percentage points higher had the 2011-cap not been introduced. This modest effect on total credit growth reflects the relative importance of the intensive margin in the 2011 reform. Although the intensive margin effect is efficient in reducing debt burdens among affected households, its impact on aggregate credit growth is modest compared to the extensive margin.

In 2012, aggregate household credit increased by USD 20,170 million, or 5.2 percent. Our calculations suggest that without the introduction of the new LTV-cap, the increase would have been USD 25,468 million or 6.5 percent. Hence, we find that the 2012-requirement reduced aggregate credit growth by 1.3 percentage points. This constitutes a 20 percent reduction in the growth rate. The relatively large impact on aggregate credit growth of the 2012 reform reflects the large number of eliminated loans resulting from this reform. We thus conclude that especially the LTV-tightening in 2012 had substantial dampening effects on aggregate household credit growth.

7 Conclusion

Using administrative household level data, we have studied the impact of loan-to-value limits on household leverage and household balance sheets more generally. Our results indicate that LTV-limits have important impacts both on the intensive and extensive margin, with the latter implying sizable effects also on aggregate credit growth. We find moderate, but significant, price effects of the introduction of these limits. Moreover, our results highlight a trade-off for the impact of borrower-based macro-prudential regulation on financial vulnerability at the household level. While the limits are effective in reducing household leverage and interest expenses, affected borrowers also respond to the regulation by depleting more of their liquid savings – leaving them with smaller financial buffers after a house purchase. As a result, when subject to unemployment spells, affected households are more likely to liquidate their housing wealth.

While our main focus has been on the impact of LTV-limits on financial vulnerability, such policies may also have important welfare implications. As discussed in DeFusco et al. (2020), households who choose to carry high levels of debt may be doing so simply to smooth expected increases in future income. For these households, restricting leverage could potentially be welfare decreasing. Similarly, as found in Bailey et al. (2019), house price beliefs may be an important driver of household leverage. The welfare implications of policies that limit mortgage leverage may then depend on the extent to which such beliefs are based on fundamental versus behavioral factors. These are important considerations to bear in mind in the future when evaluating the effects of policies that limit mortgage leverage.

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Appendix A: Additional Figures

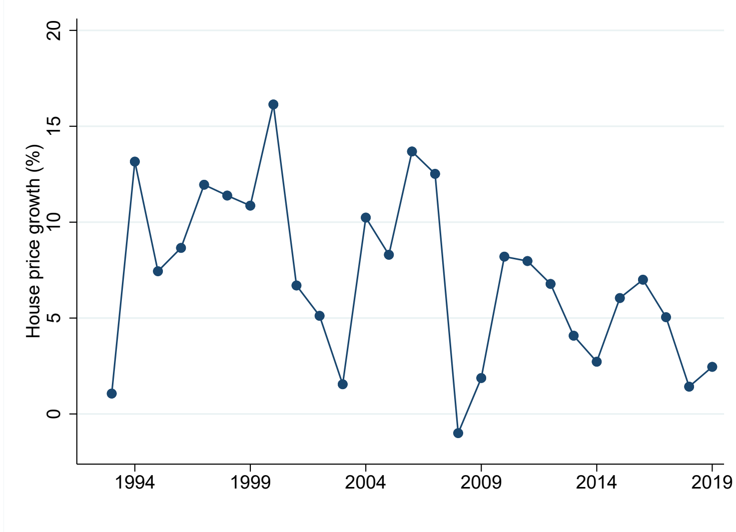

Figure A1: Annual house price growth (%).

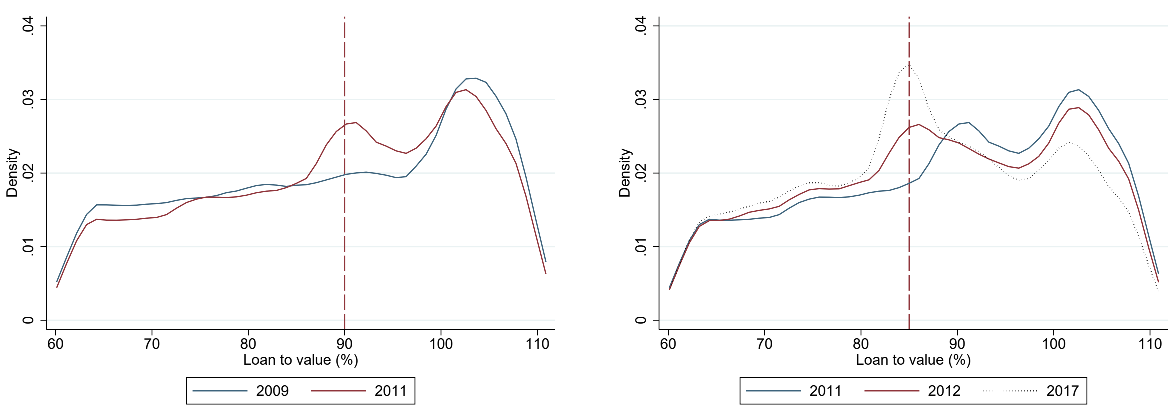

Figure A2: Kernel density plot of LTV-ratios for house buyers with LTV-ratios $\in [60, 110]$ by year, using the definition $LTV_{it} = \frac{Total\,debt_{it} - Student\,debt_{it}}{House\,purchase\,price_{it}}$ (i.e. not adjusted for unsecured debt).

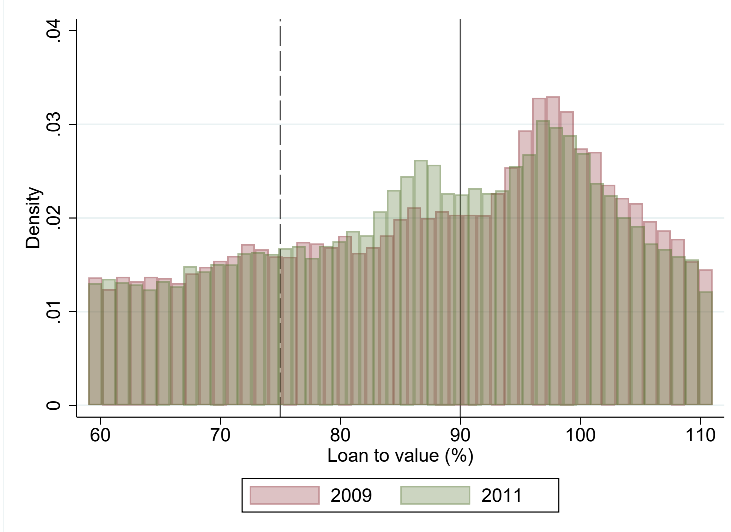

Figure A3: Histogram density plot of LTV-ratios for house buyers with LTV-ratios $\in [60, 110]$ in 2009 and 2011.

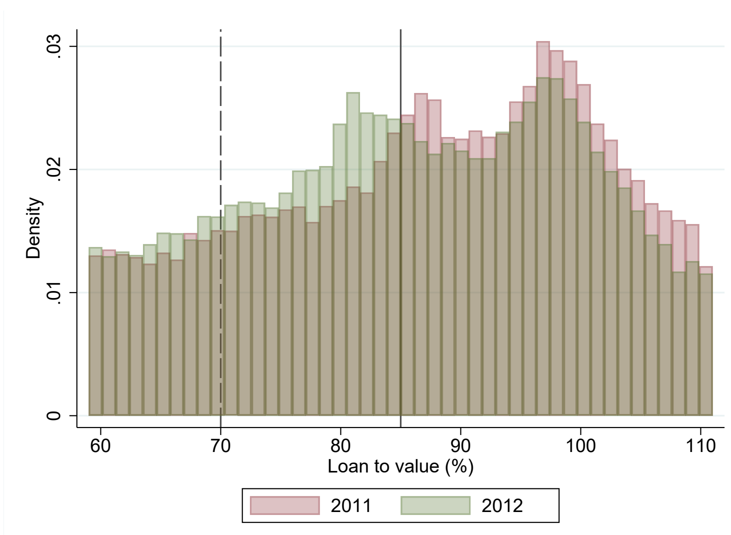

Figure A4: Histogram density plot of LTV-ratios for house buyers with LTV-ratios $\in [60, 110]$ in 2011 and 2012.

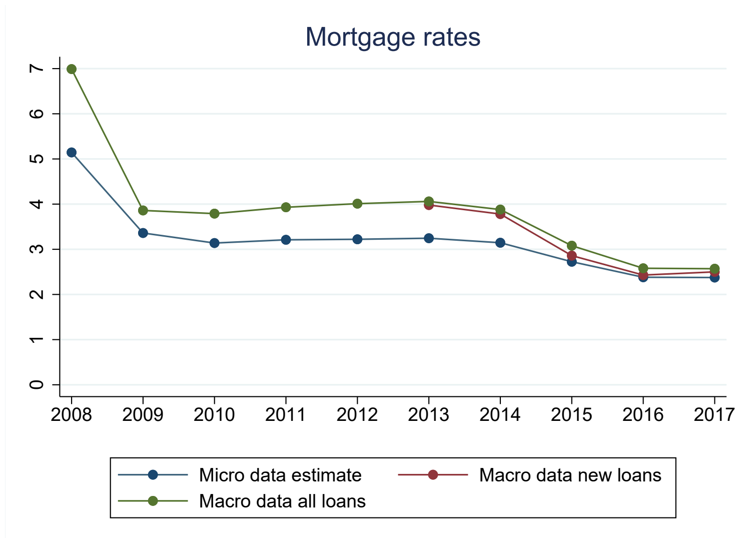

Figure A5: Official mortgage rates and computed, aggregate interest rate measure from micro data for house buyers with LTV-ratios $\in [60, 110]$. The micro data estimate includes all debt.

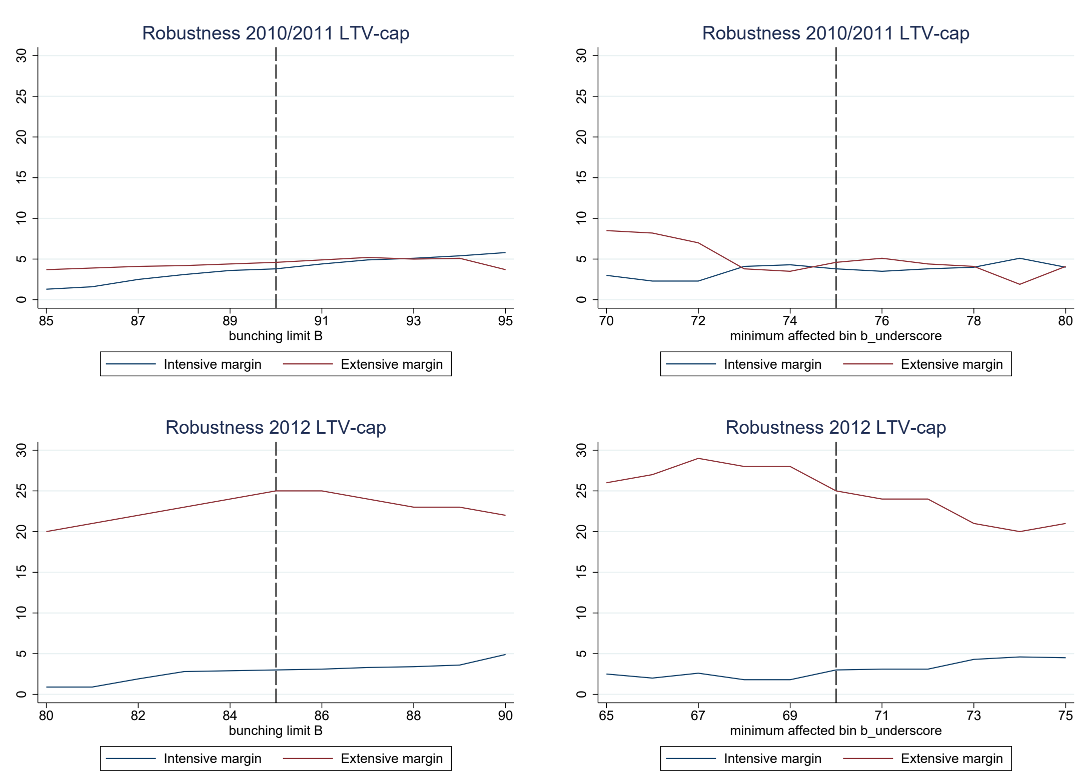

Figure A6: Sensitivity of bunching analysis to changing $\underline{b}$ and B for 2010/2011 requirement and 2012 requirement.

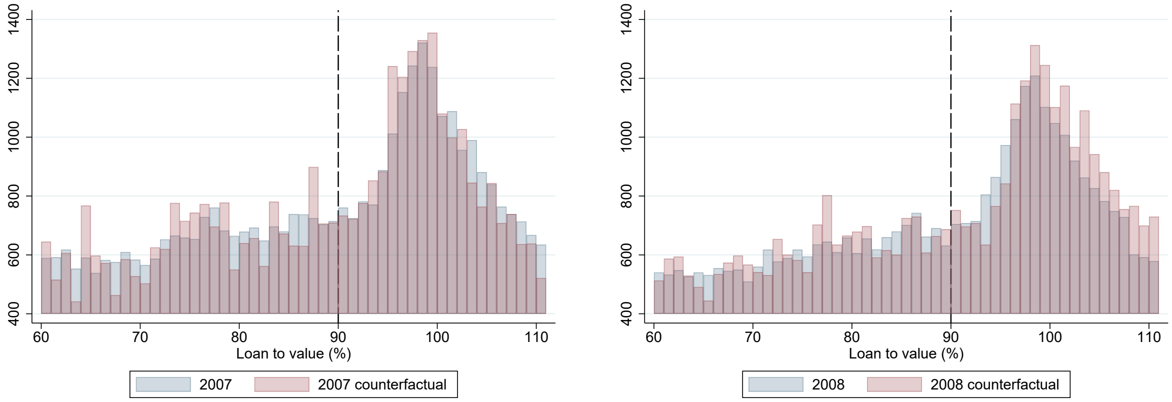

Figure A7: Placebo test. Histogram frequency plot of LTV-ratios in 2007 (left panel) and 2008 (right panel) for house buyers with LTV-ratios $\in [60, 110]$ – observed and counterfactual.

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

	(1) Interest Rate	(2) Interest Rate	(3) Interest Rate	(4) Interest Rate
$LTV^{high} \times Post^{2006}$	-0.189***	-0.188***		
	(0.0244)	(0.0246)		
$LTV^{high} \times Post^{2007}$			-0.179***	-0.180***
			(0.0287)	(0.0292)
N	111,157	111,157	121,301	121,301
Clusters	437	437	431	431
Mean interest rate	2.99	2.99	3.24	3.24
Controls	no	yes	no	yes
Sample period	2004-2007	2004-2007	2005-2008	2005-2008
Year FE	Yes	Yes	Yes	Yes

Table B2: Placebo test. Interest rates.

Notes: Results from estimating equation (5), with dependent variable computed interest rate (%). $LTV^{high} = 1$ if $\hat{LTV} > 90$ zero otherwise. $Post^{2006} = 1$ if year$\geq$ 2006 and zero otherwise. $Post^{2007} = 1$ if year$\geq$ 2007 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.

	(1) GFW	(2) Deposits	(3) Deposits t+1	(4) Deposits 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 B3: Balance sheet effects financial wealth, 2010 requirement.

Notes: Results from estimating equation (6), with dependent variables gross financial wealth (GFW) (USD), bank deposits (USD), bank deposits one year ahead and bank deposits 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) Deposits	(3) Deposits t+1	(4) Deposits 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 B4: Balance sheet effects financial wealth, 2012 requirement.

Notes: Results from estimating equation (6), with dependent variables gross financial wealth (GFW) (USD), bank deposits (USD), bank deposits one year ahead and bank deposits 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) Debt	(2) House price	(3) Deposits	(4) Debt	(5) House price	(6) Deposits
$\hat{LTV}^{high} \times Post^{2006}$	-938	9,889*	2,113			
	(3,862)	(5,404)	(1,445)			
$\hat{LTV}^{high} \times Post^{2007}$				-6,028	-2,064	530
				(4,150)	(5,798)	(1,527)
N	116,802	116,802	116,802	127,545	127,545	127,545
Clusters	438	438	438	432	432	432
Mean	280,777	359,677	30,556	280,777	359,677	30556.3
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	2004-2007	2004-2007	2004-2007	2005-2008	2005-2008	2005-2008

Table B5: Placebo test. Balance sheet.

Notes: Results from estimating equation (6), with dependent variables non-student debt (USD), house purchase price (USD) and bank deposits (USD). $LTV^{high} = 1$ if $\hat{LTV} > 90$ zero otherwise. $Post^{2006} = 1$ if year$\geq$ 2006 and zero otherwise. $Post^{2007} = 1$ if year$\geq$ 2007 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.

	(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 B6: House purchase probability - (potential) first time buyers

Notes: Results from estimating equation (6), 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.