

CAMP Working Paper Series  
No 7/2021

# The household effects of mortgage regulation

Knut Are Aastveit, Ragnar Enger Juelsrud and  
Ella Getz Wold



© Authors 2021 This paper can be downloaded without charge from the CAMP website [bi.no/camp](http://bi.no/camp)



# The household effects of mortgage regulation\*

Knut Are Aastveit<sup>†</sup>, Ragnar Enger Juelsrud<sup>‡</sup> and Ella Getz Wold<sup>§</sup>

December 2021

## Abstract

We evaluate the impact of mortgage regulation on child and parent household balance sheets, highlighting important trade-offs in terms of financial vulnerability. Using Norwegian tax data, we show that loan-to-value caps reduce house purchase probabilities, debt and interest expenses – thereby improving household solvency. Moreover, parents of first-time buyers also reduce their debt uptake, suggesting that concerns about regulatory arbitrage are unwarranted. However, the higher downpayment requirement also leads to a persistent deterioration of household liquidity. We show that this reduction in liquid buffers coincides with larger house sale propensities given unemployment, as households become more vulnerable to adverse income shocks.

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

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

---

\*This working paper should not be reported as representing the views of Norges Bank. The views expressed are those of the authors and do not necessarily reflect those of Norges Bank. We gratefully acknowledge comments and suggestions from Henrik Borchgrevink, Andreas Fagereng, Martin Blomhoff Holm, Torbjørn Hægeland, Stephanie Johnson (discussant), Erling Røed Larsen, Kjersti-Gro Lindquist, Nina Larsson Midthjell, Patrick Moran, Plamen T. Nenov, Kasper Roszbach, Kjetil Storesletten, Bjørn Helge Vatne, as well as seminar and conference participants at the Nordic Junior Macro Seminar Series, the CEBRA 2021 Annual meeting, Norges Bank and Oslo Macro Group. The manuscript has also been greatly improved by the comments and suggestions of an anonymous referee for the Norges Bank Working Paper Series, to whom we are grateful. Finally, we thank Mikkel Irving Fiksdal Riiser for excellent research assistance on this project. This paper is part of the research activities at the Centre for Applied Macroeconomics and Commodity Prices (CAMP) at the BI Norwegian Business School.

<sup>†</sup>Norges Bank & BI Norwegian Business School. Email: knut-are.aastveit@norges-bank.no

<sup>‡</sup>Norges Bank. Email: ragnar.juelsrud@norges-bank.no

<sup>§</sup>BI Norwegian Business School. Email: ella.g.wold@bi.no

# 1 Introduction

Following the financial crisis, several papers have documented the potential risks of rapid house price growth and household debt for macroeconomic outcomes.<sup>1</sup> In response to these concerns, a broad range of countries have implemented borrower-based macroprudential policies, aimed at reducing household indebtedness. An important component of these policies are Loan-to-Value (LTV) restrictions, imposing an upper bound on mortgage debt. While there is now a growing literature investigating the impact of LTV restrictions on various outcomes, the empirical evidence on the effects of such policies at the *household* level is still limited.

Understanding the impact of LTV restrictions at the household level is important for at least two reasons. First, households can adjust to LTV restrictions through several channels, and the way in which households respond is crucial for understanding how such policies affect their financial resilience. We discuss these different adjustment channels thoroughly below, with focus on the implications for leverage and liquidity. While borrowing restrictions were implemented mainly to reduce household *leverage*, stricter down payment requirements can also reduce household *liquidity*.

Second, because households are linked through family relationships, the LTV-restrictions might affect not only the (main) house buyers, but also their parents. We know from survey data that parents play a quantitatively important role in helping their children enter the housing market, and that this assistance often implies substantial debt uptake at the parent level. [Brandsaas \(2021\)](#) shows that around 30 % of American first-time homebuyers received downpayment assistance from their parents between 2009 and 2016, while a survey by [creditcards.com](#) found that 1/6 of US adults had co-signed a loan - and that among the co-signers aged 50 and above more than half had co-signed in order to help a child or step-child enter the housing market. In our administrative Norwegian data, parental debt uptake constitutes approximately 15 % of the total debt uptake related to a first-time house purchase. Hence, a comprehensive analysis of the impact of LTV restrictions on aggregate credit growth should account for the impact on parental balance sheets.

Because the existing literature relies mainly on either aggregate data or loan level data, the impact on household balance sheets – including *parental* balance sheets – is largely missing from the literature. We fill this gap by using administrative Norwegian tax data, merged with housing transaction data from the Land Registry to study the impact of LTV restrictions at the household level. Crucially, for our purpose, the tax data contains information on family ties between households, allowing us to identify the parent household(s) of first-time buyers.

In this paper we make two contributions. First, we show that households respond to the regulation by reducing house purchase probabilities, and by reducing debt uptake and interest expenses conditional on purchase. However, we also observe a persistent decline in liquid assets, as households deplete more of their savings in order to satisfy the LTV restriction and meet the new

---

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

downpayment requirement. Further, we show that the reduction in liquid financial buffers make households more likely to sell their house upon unemployment – driven by periods of negative house price growth. Such an increase in house sale propensities is disconcerting, as it could potentially amplify house price declines, see e.g. [Shleifer and Vishny \(2011\)](#). This is an important and perhaps surprising finding, as LTV-caps are intended to increase household resilience and make them less vulnerable to economic distress. Our results suggest that although LTV-caps do have positive leverage effects, there is a trade-off in terms of a negative impact working through lower liquidity.

Our second contribution is to analyze the impact of LTV restrictions on parental debt uptake. Note that it is ex ante unclear how parental housing support and debt uptake responds to LTV caps. On one hand, debt uptake could shift from child to parent, dampening the impact of LTV restrictions on aggregate debt and thereby representing a form of regulatory arbitrage. This concern has been raised by both policy makers, banks and the real estate sector.<sup>2</sup> On the other hand, LTV restrictions could bind for parents as well, leading to larger impacts on aggregate debt. We find no evidence of debt spillovers to parents, suggesting that regulatory arbitrage should not be a big concern. Instead, we show that the parents of first-time buyers also respond to the regulation by reducing debt uptake, and that this is driven by high-debt parents likely to themselves be constrained by the regulation. Our findings imply that focusing solely on first-time buyers would lead us to substantially understate the aggregate credit effects. In fact, rough back-of-the-envelope calculations suggest that the parental response accounts for more than half of the total credit effect related to first-time house purchases, due to a large extensive margin effect at the parental level.

Our data contains detailed information on income and wealth, and lets us study the balance sheet effects of the regulation. In addition, since we observe the full income statement and balance sheets of all households, we can impute consumption at the household level. The tax data further contains family links, allowing us to identify the parents of adult child households. We restrict the parent-child analysis to only include adult children with at least one living parent observable in the tax-data. To identify house buyers and measure housing values, we rely on data from the Land Registry. Loan-to-value ratios are defined as non-student debt relative to the house purchase price in the year the house is purchased. Because only collateralized debt is supposed to enter the LTV calculations, we adjust for average holdings of unsecured debt. All data is aggregated to the household level, and we restrict the sample to exclude the self-employed. The Norwegian Financial Supervisory Authority introduced a maximum LTV-level of 90 percent in the end of 2010, and then lowered this to 85 percent in 2012. We study the effect of both of these policies, in which the former constituted a new requirement whereas the latter was a tightening of an existing requirement.

We start by documenting the balance sheet responses of house buyers and parents at the time of a house purchase. Not surprisingly, debt and housing wealth increases dramatically at the time of purchase. Liquid savings increase sharply in the year before purchase, and then falls after the

---

<sup>2</sup>See among others, [KLP press release 2019](#), [Johannesen \(2019\) op-ed](#) and [Kaski \(2021\) op-ed](#).

purchase has taken place. Interestingly, parents of first-time buyers also experience large increases in debt and housing wealth at the time of an adult child's house purchase, indicating that co-signing a mortgage is an important channel for parental support in the housing market. The parental response is quantitatively important, as the increase in parent debt accounts for 15 % of the total debt increase in response to a first-time house purchase.

After having documented the balance sheet effects of a house purchase, we move on to evaluating the impact of LTV-regulation. We predict LTV-ratios in order to obtain a treatment indicator, and test our prediction in non-reform years. Specifically, we use pre-reform data to estimate LTV-ratios, and then define our treatment indicator to capture households with high predicted LTV-ratios as these are the households most likely to be affected by the reform. This allows us to rely on cross-sectional variation to estimate the impact of the regulation.

Starting with the extensive margin, we find that the house purchase probability falls by 3-6 percent. This is driven by households with low liquidity. As could perhaps be expected, households with high liquid asset holdings do not experience a reduction in purchase probability as a result of the reform. Moving on to the intensive margin, we document a decrease in LTV-ratios, debt, house purchase prices and interest expenses. These effects imply that the regulation is making households less leveraged, and therefore less vulnerable to fluctuations in asset values (Mian and Sufi (2011)). However, we also document a reduction in bank deposits, implying that households affected by the reform are left with smaller financial buffers. This response is intuitive, as the LTV-caps imply that a higher downpayment is needed for a given house purchase. We use an event study setup to show that the decrease in liquid savings is persistent, showing no sign of convergence even four years after the purchase.

Given that the reform leaves households with smaller financial buffers, a natural concern is that affected households are more vulnerable to adverse income shocks (Kaplan and Violante, 2014; Fagereng et al., 2021). In order to investigate this, we consider households who become unemployed after the house purchase. We find that affected households are two percentage points more likely to liquidate their housing wealth, enabling them to maintain their consumption levels despite their lower buffers. Given that house price growth has generally been positive in the period we consider, the increase in house sale propensities might seem surprising. Afterall, with positive house price growth, most households should be able to extract liquidity from their housing wealth. However, we show that the increase in house sale propensities is driven by the oil-price collapse of 2014, when house price growth in the South-West of Norway was negative as the result of a local oil-driven recession. Our results thus suggest that, in periods of macroeconomic distress, LTV-caps might contribute to higher house sale propensities, which could lead to (further) house price depreciations thereby amplifying the economic downturn.

Next, we turn to the parental level. We consider the concern that regulating home buyers may lead to a shift in debt uptake towards parents, i.e. a form of regulatory arbitrage. While this is a

relevant theoretical concern, we do not find any support for this hypothesis in the data. Rather, using the parent-child links in the tax data, we show that the regulation has a quantitatively large *dampening* effect on the debt uptake of first-time buyer *parents*. This large effect is likely the result of several first-time buyer parents choosing not to co-sign a mortgage with their adult child in response to the reform, i.e. the parental response is explained by a large extensive margin effect. Reassuringly for our causal interpretation of these results, the effect is driven entirely by high-debt parents – many of whom were likely to be constrained by the regulation themselves. First-time buyer parents with below median debt do not adjust their behavior in response to the reform. Due to the substantial debt response of parents, drawing conclusions on the impact of aggregate credit growth based on first-time buyers only would lead us to underestimate the dampening credit effect of the regulation.

Finally, although we emphasize that our results are based on cross-sectional variation and do not map directly into aggregate effects, we provide some simple back of the envelope calculations to get a rough sense of the magnitudes. We find that in absence of the regulation, credit growth would have been 0.4 percentage points or 6 % higher. To better understand if this effect is small or large, we compare it to the credit impact of a monetary policy shock, as estimated in the VAR literature (see Table 3 in [Robstad \(2018\)](#) for an overview of estimates from the literature). We find that the dampening effect on aggregate credit growth from the regulation is roughly of the same magnitude as would be expected from a 33 basis point increase in the policy rate. We view this effect as relatively modest, but non-trivial.

Our paper contributes to the empirical literature on the consequences of macroprudential policies by focusing on household behavior, and by being the first to use parent-child links to study the effects of such regulation. Until recently, the literature mainly used aggregate data to evaluate the benefits on house prices, household debt and bank lending.<sup>3</sup> Recently however, a handful of papers have used micro data to study the impact of borrower-based macro-prudential policy.

[Acharya, Bergant, Crosignani, Eisert, and McCann \(2022\)](#) use loan level data from Ireland to study the impact of loan-to-value and loan-to-income requirements. They show that mortgage credit is reallocated from more constrained areas to less constrained areas, inducing a dampening effect on house price growth. [Peydró, Rodriguez-Tous, Tripathy, and Uluc \(2020\)](#) use loan level from the UK and also find dampening impacts on credit and house price growth.<sup>4</sup> While our findings in this paper are certainly consistent with a dampening effect on credit and house price growth, we focus on the impact on household behavior, documenting significant responses in household debt, parental debt, interest expenses, liquid buffers and the reaction to adverse income shocks.

The two papers most similar to ours are [DeFusco, Johnson, and Mondragon \(2020\)](#) and [Van Bakkum,](#)

---

<sup>3</sup>See [Corbae and Quintin \(2015\)](#); [Greenwald \(2018\)](#); [Claessens, Ghosh, and Mihet \(2013\)](#); [Vandenbussche, Vogel, and Detragiache \(2015\)](#); [Kuttner and Shim \(2016\)](#); [Cerutti, Claessens, and Laeven \(2017\)](#); [Akinci and Olmstead-Rumsey \(2018\)](#); [Borchgrevink and Torstensen \(2018\)](#); [Morgan, Regis, and Salike \(2019\)](#).

<sup>4</sup>[Epure, Mihai, Minoiu, and Peydró \(2018\)](#) also use loan level data to study the impact of LTV-regulation, but their focus is mainly on foreign exchange and local currency loans in an emerging economy setting.

Gabarro, Irani, and Peydró (2019). DeFusco, Johnson, and Mondragon (2020) use loan level data and a bunching design to study the impact of a US debt-to-income requirement on credit volumes and prices. They find modest price effects, combined with relatively large quantity effects along both the intensive and extensive margin. Consistent with these findings, we document a reduction in house purchase probabilities and a reduction in household leverage conditional on a house purchase. Further, we use our household level data to also identify a negative impact on household liquidity and explore the implications for households’ resilience to adverse income shocks.

In ongoing work, Van Bakkum, Gabarro, Irani, and Peydró (2019) study the impact of a Dutch LTV-cap of 106% using household level data, and find a dampening effect on LTV-ratios, debt and liquidity. We show that with a substantially stricter LTV-cap and with higher house price growth, the negative liquidity effect is long-lived, showing no signs of convergence even four years after the house purchase. This raises concerns about households’ vulnerability to sudden income falls, and we further contribute to the literature by documenting an increase in house sale propensities upon unemployment for affected households – at least during periods of macroeconomic distress, which is when the benefits of LTV-regulation are most needed.

Finally, to the best of our knowledge, we are the first to study the impact of borrower-based mortgage regulation on parent debt uptake. This is possible due to our unique data, which identifies parent-households in the tax records. Doing so is important, as it allows us to also capture the quantitatively large parental responses seen in the data, and address concerns about regulatory arbitrage.

## 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 reduced from 90 to 85 percent. This is comparable to other advanced economies where the LTV-limits mostly range from 80 to 95 percent.<sup>5</sup> 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

---

<sup>5</sup>One exception, however, is the Netherlands where the LTV-limit was set to 106 percent.

existed additional collateral, or ii) if the bank had undertaken an extraordinary risk assessment. As in the initial guidelines, failure to comply could lead to higher capital requirements for the given bank.

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

Alongside the requirements levied on loan-to-values, the guidelines and the following regulation also outlined some other requirements relevant for the mortgage market. The guidelines issued in 2010 stated that banks had to ensure that their customers had a sufficient payment capacity, and that loans with a "high" LTV-ratio, should normally not be *interest only*. In the updated guidelines from 2011, the former requirement was specified to mean that interest only loans should normally have an LTV-ratio of 70 percent or below. A further specification was introduced into the regulation in 2016, when banks were required to evaluate their customers payment capacity in the event of a five percentage point increase in the lending rate. Finally, the December 2016 amendments also introduced a debt-to-income (DTI) requirement of 5, stating that loans should not be granted if the customers total debt exceeded five times gross annual income.<sup>6</sup> Key elements of the regulation 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 is cleaner. 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.

---

<sup>6</sup>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 data from Norway’s administrative tax records, covering the universe of tax filers in the period 2003-2017. Since Norway levies both income and wealth taxes, the data from the tax registry provides a complete and precise account of household income and balance sheets over time. Moreover, most of the data is provided by third parties, such as employers and banks. The tax data is merged with housing transaction data from the Land Registry, allowing us to precisely identify 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.<sup>7</sup> 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.<sup>8</sup> Specifically, we define Mortgage debt $_{it}$  = Total debt $_{it}$  – Student debt $_{it}$  –  $\overline{\text{Unsecured debt}}_t$ . While mismeasurement of mortgage debt could potentially affect our LTV-ratio results, it should not directly affect our main results related to house purchase probabilities, debt uptake, liquid asset holdings, and the reaction to adverse income shocks.

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.<sup>9</sup> The estimated market value of housing is available since 2010 only. Prior to 2010 only the tax value of real wealth is available.

When investigating how affected households respond to adverse income shocks, we are interested in identifying potential consumption effects. In order to do so, we need to impute consumption.

---

<sup>7</sup>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.

<sup>8</sup>Fagereng, Guiso, Malacrino, and Pistaferri (2020) show that the fraction of unsecured debt is fairly constant among high-leveraged households.

<sup>9</sup>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.

We follow Fagereng and Halvorsen (2017) and Fagereng, Holm, and Natvik (2021) and impute consumption based on income and balance sheet data. By observing balance sheet components and incomes we can exploit the accounting identity that consumption is equal to income less savings (see Fagereng and Halvorsen (2017) for more details on the exact imputation procedure). As highlighted by Fagereng, Holm, and Natvik (2021), imputation of consumption from the Norwegian tax records gives a fairly accurate aggregate consumption level. We do, however, acknowledge that imputed consumption will be associated with measurement error at the household level, primarily due to lack of granularity on data on individual equity holdings. As a result, most of our analysis focuses on more precisely measured outcomes such as bank deposits and house purchases.

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.

When analyzing the impact on parent debt uptake we restrict our attention to first-time buyers and their parents, as this is where the existing literature has documented that parental support is quantitatively important (Halvorsen and Lindquist (2017), Brandsaas (2021)). Moreover, we only include first-time buyers who have at least one parent alive for each household member. That is, in a household consisting of a married/co-habiting couple (and potentially some under-age children), we require both adults to have at least one identifiable parent in the tax data. The reasoning being that if both parents are deceased, it will not be possible to observe an increase in debt. Moreover, it is likely that some (implicit) parental support has already been given through inheritance.

For our analysis we rely on different samples of the data. First, our household balance sheet results rely on all home buyers in a given year, resulting in a repeated cross-section. For the event study results we rely on the panel structure of the data, restricting the sample to households with only one house purchase in a given time frame. When investigating the impact on house purchase probabilities, we need to expand the sample to include all households, i.e. we no longer condition on a purchase taking place. For the parent-child links, assembling the data requires substantial capacity, as household structures can be complicated and change over time, and we need to identify (all) the parent households on an individual basis. We therefore use a 50 % random sample, thereafter restricting the sample to first-time buyers for which all (tax-filing) household members have at least one identifiable (i.e. non-deceased) parent.

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 prices, ages, 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.<sup>10</sup>

	2009				2013			
	Mean	25th	50th	75th	Mean	25th	50th	75th
LTV (%)	88	76	90	99	85	75	85	96
DTI	3.5	2.4	3.1	4.0	3.8	2.6	3.4	4.3
Non-student debt	331,000	218,000	284,000	387,000	430,000	281,000	369,000	502,000
House purchase price	373,000	233,000	303,000	431,000	496,000	310,000	414,000	578,000
Interest expenses	11,000	5,000	9,000	14,000	13,000	6,000	11,000	17,000
Bank deposits	33,000	5,000	15,000	35,000	41,000	7,000	20,000	45,000
Other financial assets	47,000	0	2,000	7,000	54,000	0	2,000	7,000
Pre-tax income	115,000	70,000	101,000	144,000	136,000	82,000	118,000	168,000
Age (years)	36	27	33	42	36	27	33	43
First-time buyers (%)	54	0	100	100	43	0	0	100
N	36,993				47,112			

Table 1: Summary statistics 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 85 percent. Likewise, LTVs for the 25th and 75th percentile also fall, suggesting that the new restrictions had a broad impact on the distribution. Interestingly, we also observe that the fraction of first-time buyers falls from 54 to 43 percent. Furthermore, we observe an increase in house purchase prices. House price growth in Norway has generally been quite strong, with average annual growth rates exceeding six percent over the past twenty years, see Appendix Figure A1.<sup>11</sup> There has however been substantial regional variation, especially related to the oil price collapse of 2014. We use this variation in the data when addressing household reactions to adverse income shocks in Section 5.2.2. 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 debt-to-income levels.

## 4 Balance sheet adjustments at house purchase

Before investigating the impacts of the LTV-regulation, we start by describing how important balance sheet variables adjust at the time of a house purchase. We first consider house buyers in general, before moving on to first-time buyers and their parents in Section 4.1. To investigate the balance sheet adjustments we use an event study setup, in which we estimate equation (1).

<sup>10</sup>5.8 was the average exchange rate in 2012, see <https://www.dnb.no/bedrift/markets/valuta-renter/valutakurser-og-renter/historiske/hovedvalutaer/2012.html>. Note, however, that there have been substantial fluctuations in the exchange rate over the sample period.

<sup>11</sup>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.

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

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

As shown in Figure 1, primary housing wealth increases by roughly USD 170,000 in the year of a house purchase. This number captures the average effect over a heterogenous group of home buyers, including households who are just entering the housing market as well as households who have been in the housing market for some time – some of whom might be downsizing. The increase in debt is approximately as large as the increase in primary housing wealth, telling us that the average house purchase is mostly debt-financed.

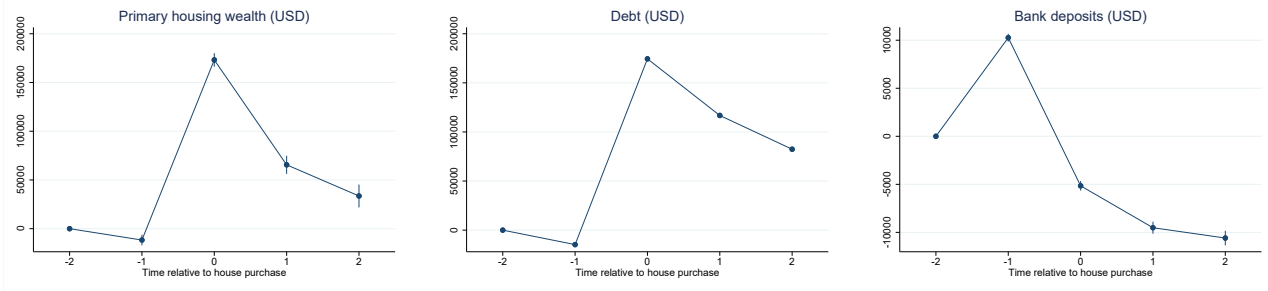


Figure 1: Event study: evolution of primary housing wealth, debt and bank deposits (USD) around house purchase ( $t = 0$ ). Year  $t = -2$  is used as the base level and normalized to zero. Vertical bars correspond to 95 % confidence intervals.

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

To summarize, we find that households increase liquid savings prior to a house purchase, and that this increase can only partly be explained by higher income. In the year of the house purchase,

liquid savings fall by more than the initial saving increase, leading to a net reduction in bank deposits. At the same time, housing wealth and debt increases sharply, the magnitudes of the relative increases being consistent with high average LTV-ratios.

#### 4.1 Parental balance sheet adjustments

Survey data from several countries indicate that parental support related to especially first-time house purchases is becoming increasingly common (see for instance Halvorsen and Lindquist (2017) for evidence from Norway and Brandsaas (2021) for evidence from the US). In this section we document large balance sheet effects for parents of first-time buyers.

Parents can help their adult children in the housing market through at least three different channels. First, parents can transfer funds to their child, either as gifts, loans or inheritance. This transfer could be debt-financed, in which case we should see an increase in parent debt, or could be financed by other assets, in which case we should see a reduction in other financial assets or in real wealth. Second, parents can co-sign the mortgage with their child, and hence co-own the house/apartment. In this case, we would expect to see an increase in both debt and housing wealth for parents. Finally, parents could act as mortgage guarantors for their child. This third option is hard to identify in the data, as it means that parents are accountable for (parts of) the mortgage only in case of default. If the child does not default on the mortgage, observed parent debt would not be affected. Survey data indicates that all of these three channels are important, and that the different mechanisms are not mutually exclusive.

Figure 2 depicts the evolution of primary housing wealth for first-time buyers and total housing wealth for their parents. There is a sharp increase in housing wealth for both groups upon the house purchase, with the parental increase accounting for more than 15 % of the total increase. Figure 3 shows a similar development in debt. Debt increases sharply for both first-time buyers and their parents upon purchase, with the parental debt increase accounting for roughly 15 % of the total increase in debt.

How do we interpret the fact that both parent housing wealth and parent debt increases by roughly USD 30,000 when an adult child buys a house? Note that this is not consistent with debt-financed transfers, as that should not have a (positive) impact on parent housing wealth. Nor is it consistent with parents acting as mortgage guarantors, as this would not lead to any observable balance sheet impacts for parents. Hence, we interpret the increase in debt and housing wealth to mean that parents help their adult child to enter the housing market by co-signing a mortgage with them. This does of course not rule out that transfers and guarantorships are also important mechanisms for parental support, but indicates that the balance sheet movements we identify are mostly driven by co-signing.

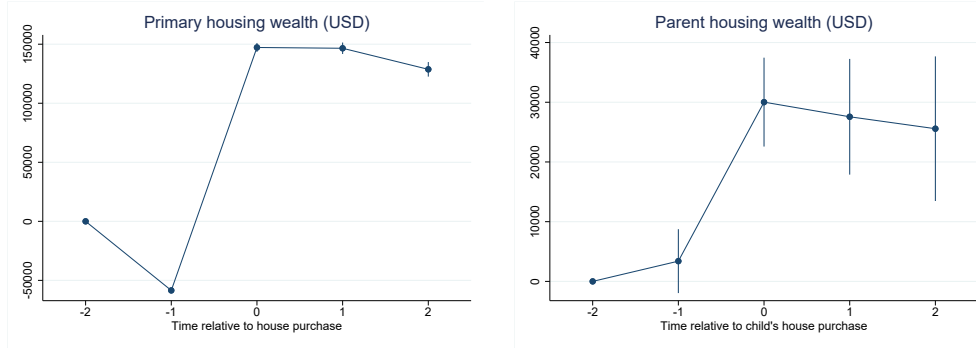


Figure 2: Event study: evolution of primary housing wealth and parent housing wealth (USD) around adult child’s house purchase ( $t = 0$ ). Year  $t = -2$  is used as the base level and normalized to zero. Sample: first-time buyers with at least one identifiable parent for every household member. Vertical bars correspond to 95 % confidence intervals.

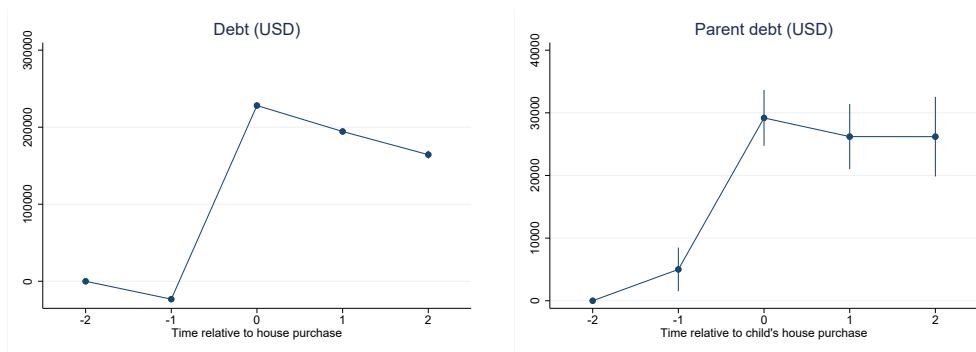


Figure 3: Event study: evolution of debt and parent debt (USD) around adult child’s house purchase ( $t = 0$ ). Year  $t = -2$  is used as the base level and normalized to zero. Sample: first-time buyers with at least one identifiable parent for every household member. Vertical bars correspond to 95 % confidence intervals.

As shown in Figure 4, the evolution of parent bank deposits is similar – although much less dramatic – to the evolution of bank deposits for the first-time buyers. While first-time buyers increase bank deposits by almost USD 10,000 prior to the purchase, parents increase bank deposits by USD 2,000. Moreover, there is no net decline in parent bank deposits once the house purchase has taken place. We do not find any significant effect on other financial assets for first-time buyers nor their parents, but the data is noisy and we cannot rule out that the increase in parent bank deposits is caused by a reduction in other financial assets for instance.

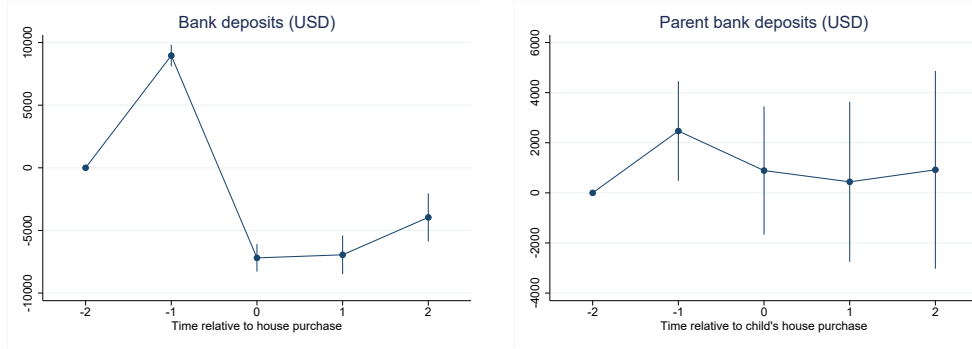


Figure 4: Event study: evolution of bank deposits and parent bank deposits (USD) around adult child’s house purchase ( $t = 0$ ). Year  $t = -2$  is used as the base level and normalized to zero. Sample: first-time buyers with at least one identifiable parent for every household member. Vertical bars correspond to 95 % confidence intervals.

The balance sheet adjustments confirm that parental support for first-time buyers is common and should be part of the analysis. In terms of credit growth, 15 % of the total debt uptake related to a first-time house purchase is in fact done by parents. This means that in order to evaluate the impact of a policy such as LTV-regulation on total credit growth, it is not sufficient to consider first-time buyers only – also the parental effects should be taken into account.

## 5 Effects of LTV-regulation

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

In terms of intensive margin effects, we find that affected households respond to the regulation by reducing LTV-ratios, debt and interest expenses – all of which are likely to reduce financial vulnerability at the household level. However, we also find a persistent reduction in liquid assets, potentially making households more vulnerable in the case of adverse income shocks. To explore this further, we consider households who become unemployed after a house purchase and show that households affected by the regulation are more likely to sell their house upon unemployment.

Finally, we consider the impact on first-time buyers and their parents. One concern policy makers might have is that restricting the debt uptake of the young could lead to an increase in debt for their parents, representing a form of regulatory arbitrage. However, we do not find any evidence of this. Instead, we see that parental debt uptake also falls in response to the regulation, and that this is driven entirely by parents with high debt – likely to themselves be constrained by the LTV-caps.

## 5.1 Methodology

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

We start by using past data to predict which households are likely to take up a mortgage with an LTV-ratio in excess of the maximum level. 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.

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. The exercise shows that 70 % of all house buyers are classified correctly, that is, they have both actual and predicted LTV-ratios above the (assumed) LTV-cap of 90 percent. Of the households which are falsely classified as treated (i.e. they have predicted LTV-ratios above the cap, but actual LTV-ratios below the cap), 25 % have LTV-ratios at most 5 percentage points below the cap and close to 50 % have LTV-ratios at most 10 percentage points below the cap. In other words, most of the households that are falsely assigned as treated, have observed LTV-ratios that are in fact high and "close" to the cap. Overall, we judge our treatment indicator to be acceptable in terms of precision. We have explored using standard machine learning methods such as LASSO to do the classification, which provides a comparable predictive performance to our baseline model.

Once we have the predicted LTV-ratios, we use  $L\hat{T}V_i^{high}$  as our treatment indicator and estimate both the extensive and the intensive margin effects according to equation (2). For the extensive margin effects,  $y_{i,t}$  is an indicator variable equal to one if a house purchase takes place. In this case we use the full sample. For the intensive margin effects, we use a repeated cross-section of house buyers and estimate the impact on LTV-ratios, debt, interest expenses, house purchase prices and bank deposits in the year of purchase. When considering parent outcomes,  $y_{i,t}$  is the balance sheet outcome of the parent. Year fixed effects  $\delta_t$  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.

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

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.  $L\hat{T}V_i^{high} = 1$ , as these are the households likely to be affected by the LTV-caps. 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

$$y_{i,t} = \alpha_i + \delta_t + \beta HP_i^{post} \times U_{i,t} + \gamma U_{i,t} + \epsilon_{i,t} \quad (3)$$

where  $y_{i,t}$  is either an indicator variable equal to 1 if the households sells their home, bank deposits or imputed consumption.<sup>12</sup>  $U_{i,t}$  is an indicator variable equal to 1 if someone in the 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  $\alpha_i$  and  $\delta_t$  are included to make sure that we only consider within-household and within-year variation.

## 5.2 Results

We start by reporting extensive margin results, before moving on to intensive margin results and the reaction to adverse income shocks. Finally, we report the parental responses and discuss the overall implications for aggregate credit growth and financial vulnerability.

### 5.2.1 Balance sheet effects

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

We start by considering the initial LTV-cap introduced in 2010. In the first column, we compare the house purchase probability in the year prior to the reform to the house purchase probability in

---

<sup>12</sup>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, Holm, and Natvik \(2021\)](#) and [Eika, Mogstad, and Vestad \(2020\)](#) and impute consumption based on household's balance sheets, disposable income and capital gains.

the reform year. In this case, the coefficient estimate is negative but not statistically significant. Note however, that this implies comparing 2010 to 2011, which might be a noisy comparison as the initial LTV-cap could have had some effect in 2010 as well. 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.

Interestingly, we find larger extensive margin effects following the LTV-tightening in 2012. The results are reported in the two last columns of Table 2. Considering first the year prior to the reform and the reform-year, we see that the house purchase probability for households with high predicted LTV-ratios 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 differences analysis suggests that especially the LTV-tightening in 2012 had important extensive margin effects.

	(1)	(2)	(3)	(4)
	House Purchase	House Purchase	House Purchase	House Purchase
$L\hat{T}V^{high} \times Post^{2010}$	-0.0776 (0.0599)	-0.143** (0.0717)		
$L\hat{T}V^{high} \times Post^{2012}$			-0.336*** (0.0519)	-0.392*** (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 2: House purchase probability (%).

*Notes:* Results from estimating equation (2), with dependent variable house purchase probability (%).  $L\hat{T}V^{high} = 1$  if  $L\hat{T}V > 90$  (85) for the 2010 (2012) requirement and zero otherwise.  $Post^{2010} = 1$  if year  $\geq 2010$  and zero otherwise.  $Post^{2012} = 1$  if year  $\geq 2012$  and zero otherwise. Standard errors are clustered at the municipality level. \* p<0.1, \*\* p<0.05, \*\*\*p<0.01.

Why are the extensive margin results from the subsequent LTV-tightening in 2012 larger than those of the initial LTV-cap introduced in 2010/2011? While we do not have a definitive answer to this, we offer some potential explanations. First, it is possible that banks and households adjusted to the new regulation over time, and that the initial guidelines were not immediately fully incorporated. In fact, part of the motivation for the tightening of the guidelines in 2012 was the result of the FSAs monitoring of the bank sectors response to the initial guidelines in 2010. Second, one could imagine that households responded to the first regulation mainly by adjusting along the intensive margin, i.e. by buying less expensive housing or by depleting more of their liquid assets. When the second and more restrictive LTV-cap was introduced shortly thereafter, this option may

have seemed less attractive or attainable, causing more households to cancel or delay their home purchases. Supportive of this explanation, we note that while 49 percent of new mortgages had LTV-ratios in excess of 90 percent in the year before the initial guidelines were introduced, 59 percent of new mortgages had LTV-ratios in excess of 85 percent in the year before the revised guidelines were introduced. Hence, the 2012 guidelines were more restrictive. This implies that the strictness of the mortgage regulation may affect the response, including the relative importance of the intensive and extensive margin effects.

Does the reduction in the house purchase probability indicate a transitory or permanent effect? If households are simply postponing their house purchase one year, the effects on aggregate credit growth will be smaller compared to a state of the world in which the house purchase probability is permanently lower. Identifying the long-term effects are more challenging, and so we have focused our analysis on a short time window around the introduction of the new requirements. The results in Table 2 suggest that the negative effect on house purchases is not limited to the reform-year, but seems to persist at least into the following year as well. Interestingly, we find different effects if restricting the sample to only considering (potential) first-time buyers – see Appendix Table B2. For households who have not yet entered the housing market, the negative effect on purchase probabilities is limited to the year of the reform. That is, in the following year, there is no significant impact on the purchase probabilities of potential first-time buyers. The data is thus consistent with there being at least a somewhat more persistent effect on housing transactions in general, compared to the impact on those not yet in the housing market.

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

To summarize, our results indicate that affected households are 3-6 percent less likely to purchase a new house immediately following the regulation. This negative effect is entirely driven by households with relatively low liquid wealth, meaning that households with sufficiently high liquid asset holdings are not affected by the regulation – at least along the extensive margin.

	(1)	(2)
	House Purchase	House Purchase
$L\hat{T}V^{high} \times Post^{2010}$	-0.692*** (0.111)	
$L\hat{T}V^{high} \times Post^{2010} \times Deposits_{t-1}^{high}$	1.76*** (0.363)	
$L\hat{T}V^{high} \times Post^{2012}$		-1.36*** (0.228)
$L\hat{T}V^{high} \times Post^{2012} \times Deposits_{t-1}^{high}$		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 3: Heterogeneous effects along the extensive margin.

*Notes:* Results from estimating equation (2), with dependent variable house purchase probability (%).  $L\hat{T}V^{high} = 1$  if  $L\hat{T}V > 90$  (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.  $Deposits_{t-1}^{high} = 1$  if deposits are above median and zero otherwise. Standard errors are clustered at the municipality level. \* p<0.1, \*\* p<0.05, \*\*\*p<0.01.

**The intensive margin** The intensive margin results for the 2010 regulation, i.e. the balance sheet effects conditional on purchase, are reported in Table 4. As seen from the first column, affected borrowers respond to the regulation by reducing their LTV-ratios, as could be expected. On average, LTV-ratios fall by 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. As a result of these changes, household solvency increases.

In the final column in Table 4, we report the results for household liquidity. Affected borrowers respond to the regulation 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 no sign of convergence.

	(1)	(2)	(3)	(4)	(5)
	LTV	Debt	Int.Expenses	House price	Deposits
$\hat{LTV}^{high} \times Post^{2010}$	-0.847*** (0.207)	-21,536*** (3,386)	-329*** (104)	-26,045*** (4,850)	-3,390*** (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 (2), 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 from 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 years following the house purchase – see Appendix Table B4.

	(1)	(2)	(3)	(4)	(5)
	LTV	Debt	Int.Expenses	House price	Deposits
$\hat{LTV}^{high} \times Post^{2012}$	-2.232*** (0.173)	-44,320*** (4,047)	-1,975*** (192)	-46,883*** (5,359)	-4,340*** (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 (2), 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. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

A potential issue is that house price growth could affect the outcomes considered. Note that 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 unlikely to be 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.

In Appendix Table B5 we report results from a placebo test for the balance sheet results. We use years prior to the reform for the placebo tests, as there appears to be continued adaption to the new regulation in the years following the reform. That is, when we look at the LTV-distributions we see that the bunching around the LTV-limits increase over time suggesting that the reform effects might not be contained to the year of implementation, but rather continue over time as households and banks adjust to the new regulatory framework. Reassuringly, we find no significant effect on debt uptake, house purchase prices or bank deposits prior to the reform.<sup>13</sup> 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 given that it entails a downward shift in households liquid asset positions. 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.

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

---

<sup>13</sup>The house purchase price coefficient is statistically significant at the ten percent level - but *positive* - in one of the two placebo tests.

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.

The results are qualitatively similar when considering first-time buyers only. This suggests that the increase in liquid savings prior to a house purchase is not (only) due to households selling an existing home before purchasing a new one. Four years after the house purchase, first-time borrowers who purchased their house following the reform had roughly USD 14,000 less in bank deposits – compared to a slight increase for those who purchased their home prior to the reform.

Our results differ from those in [Van Bakkum, Gabarro, Irani, and Peydró \(2019\)](#), who use Dutch data and find that liquid savings quickly converge after the house purchase. While we cannot with certainty say 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 savings in the form of bank deposits. The 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. In fact, in most institutional setups, an LTV-cap well above 100 % should be non-binding for the vast majority of households.

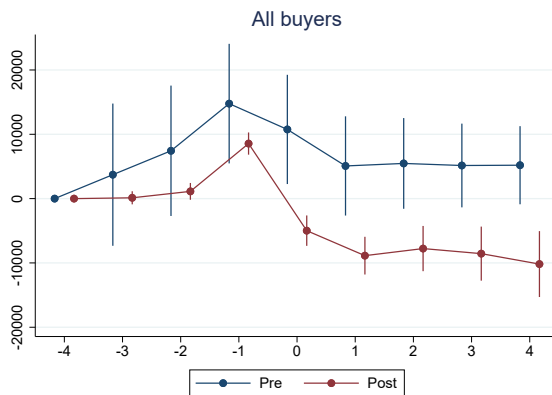


Figure 5: Bank deposits event study (USD). Year  $t = -4$  is used as the base level and normalized to zero. Households with high predicted LTV-ratios who undertake one house purchase in the period 2008-2015. Vertical bars correspond to 95 % confidence intervals.

How important is the decline in bank deposits quantitatively? 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.

Share who reduce bank deposits to less than:			Deposits at time $t + 1$ (USD)	
	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  $t + 1$ .

### 5.2.2 The reaction to adverse income shocks

The previous subsection documented how households which purchased houses after the reform reduced both LTV-ratios and liquid savings. The reduction in liquid savings can in principle affect households' responses to negative income shocks, either through an increased propensity to liquidate illiquid assets or by a larger marginal propensity to consume out of income. The former effect implies that LTV-caps can contribute to larger declines in house prices during economic downturns, as more households choose to sell their house in the event of adverse income shocks. The latter effect on the other hand, implies that household demand itself is more sensitive to this type of shock.

In this section, we investigate further the implications of lower liquid savings for household's ability to withstand large, negative shocks. We focus on one very salient form of adverse shocks, namely unemployment.<sup>14</sup> 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 macroeconomic 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

<sup>14</sup>In terms of institutional background on unemployment insurance, OECD data on 2015 replacement rates from the *Tax and Benefit Systems: OECD Indicators* shows that out of the 40 countries included, Norway is ranked as number 18, i.e. close to the OECD median. For comparison, the US is ranked as number 37.



especially, a large share of the unemployment spells for both control and treatment groups occur 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) and hence unemployment status is more likely to be driven by the same factors for households purchasing homes before and after the requirements.

Focusing first on the 2012 requirement, the estimated impact of becoming unemployed for affected households relative to non-affected households is shown in Table 7. 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 relative to the low baseline propensity of house sale, potentially reflecting the fact that many households experience unemployment in periods of macroeconomic distress, when house prices growth is less likely to be positive. Specifically, a large share of the unemployment occurs following the oil price collapse in 2014, in which house price growth was negative in the hardest affected areas. We explore this issue further below.

The gains from the house sale are reflected in somewhat higher bank deposits, although this effect is only significant when considering the full sample, and then only at the ten percent level. Note that this is not particularly surprising. Households in our sample have relatively short time periods between their house purchase and unemployment, so the house price appreciation should be relatively modest. Given that these households sell their homes, potentially during periods of house price declines, and then perhaps relocate and purchase a new home, we would not expect to see large increases in liquid assets. 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)	(2)	(3)	(4)	(5)	(6)
	House sale	House sale	Deposits	Deposits	Imp. cons.	Imp. cons.
$HP^{post} \times U$	2.43** (0.986)	1.48*** (0.565)	2,271 (1600)	1,636* (925)	-8,873 (10183)	-2,725 (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 7: Household vulnerability, 2012 requirement.

*Notes:* Results from estimating equation (3), 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  $LTV > 85$ . 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 8, 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 7. However, it is imprecisely estimated and we fail to reject the null hypothesis. The same holds for bank deposits and imputed consumption.

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 macroeconomy. In response to a systemic shock to income, however, the lack of liquid savings and associated increased likelihood of liquidating housing wealth can potentially contribute to house price depreciations (Shleifer and Vishny (2011)). This might in turn affect the consumption of other homeowners, implying a potentially amplifying effect on economic downturns.

	(1)	(2)	(3)	(4)	(5)	(6)
	House sale	House sale	Deposits	Deposits	Imp. cons.	Imp. cons.
$HP^{post} \times U$	0.947 (1.30)	0.525 (0.856)	-5,104 (5,657)	-4,143 (4,371)	2,243 (16,140)	13,139 (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	Short	Full

Table 8: Household vulnerability, 2010 requirement.

*Notes:* Results from estimating equation (3), 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  $LTV > 90$ . 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. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Adverse income shocks in periods of falling house prices** The increase in the house sale propensity documented in Table 7 might seem large given that house price growth has been positive over this time period. As long as house prices are increasing, the LTV restriction should not keep households from refinancing their mortgage in order to extract liquidity upon unemployment. However, although national house price growth has been positive in all years, this is not the case for all regions. Most noticeably, house price growth was negative in the Southwest of Norway following the oil price collapse of 2014. We show that the increase in the house sale propensity is largely driven by individuals becoming unemployed as a result of the oil crisis, highlighting the importance of macroeconomic conditions for the importance of adverse income shocks at the household level.

We focus here on the 2012 requirement, as for the 2010 requirement our sample restriction implies that we are excluding unemployment which occurs after 2013, i.e. after the oil price collapse. Note that this may also be why we found stronger effects for the 2012-requirement than for the 2010-requirement.

The first two columns of Table 9 simply reproduce the first two columns of Table 7, showing that affected households are more likely to sell their house upon unemployment. Columns 3 and 4 restrict the sample to only including households who become unemployed after the oil price collapse in 2014, and who at the time of unemployment are residing in the oil region. The effects for this subset is 2-3 times larger than for the full sample, and more precisely estimated. The final two columns are estimated based on the remaining sample, that is households who either became unemployed prior to the oil price collapse in 2014, and/or who reside outside of the oil region at the time of unemployment. For this sample, the impact on the house sale propensity is quantitatively

small and not statistically significant.

	(1)	(2)	(3)	(4)	(5)	(6)
	House sale	House sale	House sale	House sale	House sale	House sale
$HP^{post} \times U$	2.43** (0.986)	1.48*** (0.565)	8.18*** (1.45)	3.49*** (0.942)	1.55 (0.978)	0.977* (0.587)
N	38,937	58,641	8,351	12,619	30,586	46,022
Clusters	404	406	92	112	396	399
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Short	Full	Short	Full	Short	Full
Subset	All	All	Oil crisis	Oil crisis	Non oil crisis	Non oil crisis

Table 9: Household vulnerability - oil crisis, 2012 requirement.

*Notes:* Results from estimating equation (3), 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  $LTV > 90$ . 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. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

With the exception of the oil crisis, house price growth following the financial crisis has generally been positive in most areas. We thus interpret the small and insignificant estimates in the last two columns of Table 9 to imply that LTV-regulation has very limited effects on house sale propensities in normal times. This is intuitive, as positive house price growth means that affected households can always tap into their home equity in response to an adverse income shock. The reduction in liquid financial assets is therefore not a binding concern.

Once house price growth is no longer positive however, the situation is very different. Without house price growth, households who were constrained by the regulation at the time of purchase will continue to be so, thereby limiting their ability to extract liquidity from their housing wealth. The oil price collapse and the large impacts on house sale propensities identified in columns 3-4 highlights the importance of macroeconomic conditions for financial stability concerns. The reduction in liquid assets caused by the regulation may only be detrimental given a negative shock to the macroeconomy. In this case, more households will end up selling their house, potentially amplifying the downturn through a negative house price spiral.

### 5.2.3 Parental balance sheet effects

Finally, we investigate the impact on the balance sheets of first-time buyer's parents. Policy makers and industry representatives have expressed concern that mortgage regulation could lead to a form

of regulatory arbitrage, in which debt uptake is shifted onto parent households. For instance, the private bank KLP writes that "they see a trend that credit-demand is shifting from children to parents", and that some parents have increased their own mortgage in order to assist their children in the housing market.<sup>15</sup> However, we show that parental debt uptake is in fact reduced by the reform, and that this is driven entirely by parents with high debt levels – many of whom are likely to be constrained by the regulation themselves. As with the bank deposit event study in the previous section, we increase precision by considering the two reforms jointly in this section.

Consider first the event study plots in Figure 6. The plots depict the evolution of parent real wealth and parent debt for parents with above median (lagged) debt - prior to and following the reform. Note that we cannot use the imputed market value of housing wealth, as this is only available since 2010. Instead we must rely on the tax value of real wealth, which is known to severely underreport the market value of housing. We thus expect to see smaller impacts and smaller magnitudes when considering this variable.

Although not statistically significant, we do see that high-debt parents seem to have a somewhat lower increase in real wealth upon their adult child's house purchase following the reform. This is corroborated by the evolution of debt, in which the increase in parental debt prior to the reform is about USD 50,000, compared to about USD 25,000 following the reform. Hence, the event studies indicate that, if anything, the regulation seems to be dampening parental debt uptake as well.

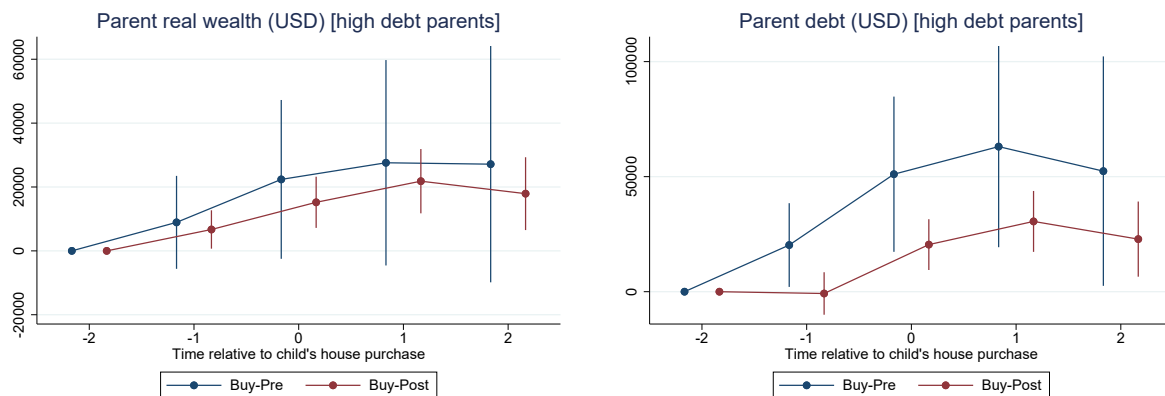


Figure 6: Event study: evolution of parent real wealth and parent debt (USD) around adult child's house purchase ( $t = 0$ ). Year  $t = -2$  is used as the base level and normalized to zero. Sample: first-time buyers with at least one identifiable parent for every household member. Vertical bars correspond to 95 % confidence intervals.

To formally investigate the impact on parent debt uptake, we again rely on predicted LTV-ratios as our treatment indicator, and estimate equation (2) as before. The results are reported in Table 10. First, we report results using debt for first-time buyers as our dependent variable, as we

<sup>15</sup>KLP press release 2019

are now using a different sample than in the previous section (i.e. only first-time buyers with at least one living parent per household member). As before, those with high predicted LTV-ratios who purchase a house following the regulation decrease their debt uptake relative to those with low predicted LTV-ratios. As seen from the second column, there is also a decrease in house purchase prices as previously documented.

In the third column of Table 10 we report results using parent debt holdings as the outcome variable. In this case the coefficient estimate is relatively small in size and statistically insignificant. We know however, that parent debt is dominated by their own mortgage, and by simply comparing the total debt level of parents whose adult children purchase a house prior to or following the requirement, we risk picking up other factors that determine their overall debt level. To better isolate the debt uptake in response to a child's house purchase, we estimate the impact on the change in debt,  $\Delta Parent\ debt = Parent\ debt - Parent\ debt_{-1}$ , in the year of purchase. This is done in column four, and we see that the coefficient estimate becomes much larger and is now statistically significant. Hence, using predicted LTV-ratios supports the notion that, if anything, LTV-caps seem to be dampening parent debt growth in response to the first-time house purchase of adult children.

	(1)	(2)	(3)	(4)	(5)
	Debt	House Price	P.debt	$\Delta$ P.debt	$\Delta$ P.debt
$L\hat{T}V_i^{high} \times I_t^{post}$	-14,765** (7,359)	-19,405** (9,455)	-1,372 (30,422)	-50,872** (50,872)	-4,983 (7,528)
$L\hat{T}V_i^{high} \times I_t^{post} \times P.debt_i^{high}$					-75,459*** (27,658)
N	5,821	5,821	5,821	5,692	5,692
Clusters	321	321	321	317	317
Mean	291,884	323,822	225,866	33,877	33,877
Year FE	Yes	Yes	Yes	Yes	Yes

Table 10: Parent debt uptake upon child house purchase.

*Notes:* Results from estimating equation (2), with dependent variables debt (USD), house purchase price (USD), parent debt (USD) and the first difference in parent debt (USD).  $I_t^{post} = 1$  if the household purchased a house in year  $t \geq 2011$ .  $P.debt_i^{high} = 1$  if parents have above median debt in the year of the (child) house purchase. Sample: First-time buyers with at least one living parent for each tax-filing household member. Sample period: 2009-2015. Standard errors are clustered at the municipality level. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

Intuitively, we would expect that parents with higher debt burdens themselves would be more likely to reduce their debt uptake related to a child's house purchase in response to the reform. We therefore add a triple interaction term,  $L\hat{T}V_i^{high} \times I_t^{post} \times P.debt_i^{high}$ , to the estimation, in which  $P.debt_i^{high} = 1$  if household  $i$  have parents with above median debt holdings in the year of purchase.

The regression results are reported in the final column of Table 10. Interestingly, there is no effect on households with low-indebted parents. But for households whose parents have relatively high debt burdens, parental debt uptake is significantly lower after the reform. Moreover, the estimated debt impact is quantitatively large, and larger than that of the household. To understand why this is the case, note that for the household we are estimating only the intensive margin effect. That is, this is the impact on debt conditional of a house purchase taking place. For parents however, we are picking up both an intensive margin *and* an extensive margin effect. That is, given that an adult child purchases a house, the parents can choose to adjust the size of their (debt-financed) financial support, or withdraw it altogether.

Are these high-debt parents who are responding to the regulation likely to be constrained by the LTV-caps themselves? While we do not have the purchase value of the houses the parents own, we do have an estimate of their primary housing wealth (post-2009). Using primary housing wealth to calculate an estimate of the parent LTV-ratio, we find that among parents with above median debt, the median LTV-ratio is 56 %. This compares to only 12 % for the parents with low debt burdens. Moreover, for the parents with high debt burdens, 25 % of them have LTV-ratios in excess of 85 %. We thus find it very likely that many of the highly indebted parents, are in fact themselves constrained by the regulation. Hence, reducing debt uptake related to an adult child's house purchase is a logical response.

What does our results imply about parental support in the housing market? We documented in Section 4.1 that parents increased both housing wealth and debt in response to an adult child's house purchase, allowing us to conclude that co-signing a mortgage was an important component of parental support. The reduction in parental debt (along with the noisy and insignificant reduction in the tax value of parents real wealth) suggests that co-signing a mortgage has become less common in response to the reform, and that this is driven by high-debt parents. It does not speak to the prevalence of mortgage guarantorship, which we cannot observe in the data. However, given that the reduction in debt uptake is driven by high-debt parents we would expect their opportunity to act as mortgage guarantors to be limited by the banks willingness to accept their collateral. According to the largest private bank in Norway, LTV-ratios is one of the most important factors in determining whether a household is suitable to be a mortgage guarantor. Given that high-debt parents have high LTV-ratios which seem to be restricting them from co-signing a mortgage, we expect their ability to act as a mortgage guarantor to also be negatively affected.

Given that parents also reduce their debt uptake in response to the reform, how important is the parental margin for the overall impact of LTV-caps on debt? In order to get a sense of the magnitude, we do some simple calculations. First assume that for a potential first-time house purchase, total debt growth  $g_{ft}$  is governed by equation (4), in which  $P$  is the purchase probability,  $c$  denotes child and  $p$  denotes parent.

$$g_{ft} = P_{ft}(g_{ft}^c|_{buy} + g_{ft}^p|_{buy}) + (1 - P_{ft})(g_{ft}^c|_{nobuy} + g_{ft}^p|_{nobuy}) \quad (4)$$

Assuming that there is no change in debt growth when a purchase does *not* take place, i.e.  $\Delta g_{ft}^c|_{nobuy} = \Delta g_{ft}^p|_{nobuy} = 0$ , we have that  $\Delta g_{ft} = \Delta g_{ft}^c + \Delta g_{ft}^p$ , in which  $\Delta g_{ft}^c = \Delta P_{ft}g_{ft}^c|_{buy} + P_{ft}\Delta g_{ft}^c|_{buy}$  and  $\Delta g_{ft}^p = \Delta P_{ft}g_{ft}^p|_{buy} + P_{ft}\Delta g_{ft}^p|_{buy}$ . We know debt uptake and the change in debt uptake for parents and children from Table 10, and we can use the results in Table B2 to get the change in purchase probability for first-time buyers. Specifically, averaging over the contemporaneous results for the two reforms in columns 1 and 3, we get that  $P^{purch} = 0.054$ , and  $\Delta P^{purch} = 0.0019$ . We then have that  $\Delta g_{ft}^c = 0.0019 \times 291,900 + 0.054 \times 14,800 \approx 1,400$  and  $\Delta g_{ft}^p = 0.0019 \times 33,900 + 0.054 \times 50,900 \approx 2,800$ .

Our simple calculations suggest that parents in fact account for more than 60 % of the debt reduction related to a first-time house purchase. We interpret this to mean that a substantial share of parents who would previously have co-signed a mortgage with their first-time buyer children, now chose not to do so. Because they can adjust both along the intensive and the extensive margin, the debt reduction is large. Reassuringly in terms of our identification, the reduction in parental debt is driven exclusively by high debt parents likely to be constrained by the regulation. For parents with below median debt levels, there is no impact on their balance sheet adjustments when their adult children purchase a house. This again highlights the important distributional impacts of this type of regulation, where not only the buyers own financial position becomes more important, but also the financial position of their parents. This type of distributional impact may be especially important to policymakers, if advantages in the housing market resulting from parental wealth are deemed more "unfair" than advantages in the housing market resulting from own wealth (see for instance Dworkin (1981a), Dworkin (1981b) and Roemer (2002)).

#### 5.2.4 Implications for financial stability and aggregate credit growth

We have documented several effects of LTV-regulation which are likely to impact financial stability. First, as intended, the regulation has led to lower debt burdens and lower interest expenses among house buyers. Lower leverage on illiquid wealth makes households more solvent and improves financial resilience against fluctuations in asset values (Mian and Sufi (2011)). Given that borrower based mortgage regulation such as LTV-caps were introduced in the aftermath of the financial crisis, this seems like an important outcome of the regulation.

As the quantitative importance of parental support in the housing market has increased over time, policy makers have expressed concern that any reduction in credit growth from the introduction of LTV-caps could potentially be met by an increase in parental debt. That is, this kind of regulation might simply shift debt burdens from younger first-time borrowers, on to their parents. Whether or not this is an improvement in financial stability would then depend on several variables, among others the balance sheets of the first-time buyers and their parents. However, we have shown



that the data does not indicate that such a form of regulatory arbitrage is taking place. Instead, there seems to be a reduction in debt also for the parents – driven by high-debt parents likely to themselves be constrained by the regulation.

At the same time as household leverage is reduced, so is liquid assets. LTV-regulation leads to a higher required downpayment for a given house, and so induces buyers to deplete more of their liquid assets. This leaves households with smaller financial buffers due to the regulation, potentially making households more vulnerable to fluctuations in income (Kaplan and Violante, 2014; Fagereng et al., 2021). To explore this issue further, we show that affected households who become unemployed after the house purchase are more likely to sell their house as a response. This entails non-trivial transaction costs, as houses are highly illiquid, but allows households to maintain their consumption level in the absence of large liquid savings. We show that the increased propensity for house sales upon unemployment is driven by time-location instances in which house price growth is negative. This is intuitive, as positive house price growth means that most home owners will be able to extract liquidity from their house. However, it implies that in times of bad macroeconomic conditions, a higher house sale propensity may contribute to house price depreciations, potentially amplifying the economic downturn.

Although our cross-sectional estimates do not necessarily map directly into aggregate effects, we still find it useful to report some back of the envelope calculations for the impact on total credit growth to get a sense of the magnitudes. For this exercise we rely on the results from the full sample. Note that using the first-time buyer estimates would lead to a large downwards bias, as the parental debt responses would be excluded.<sup>16</sup> However, to the extent that the parental responses are driven by co-purchasing, their responses should be captured in the full sample estimates. We focus here on the 2012-reform. Considering instead the 2010-reform would yield smaller effects, as especially the extensive margin results were smaller in response to the initial regulation. As before, we assume that total credit growth is governed by equation (5), and that the reform does not affect debt growth if a purchase does not take place, so that  $\Delta g|_{nobuy} = 0$

$$g = Pg|_{buy} + (1 - P)g|_{nobuy} \tag{5}$$

The per household effect of the reform is then given by  $\Delta g = \Delta P g|_{buy} + P \Delta g|_{buy} = 0.0034 \times 385,650 + 0.052 \times 44,320 \approx 3,600$  USD, in which  $\Delta P$  and  $P$  are from Table 2 and  $g|_{buy}$  and  $\Delta g|_{buy}$  are from Table 5. Multiplying this figure by the number of households in 2012, and adding this to total household debt, we find that in the absence of the reform, credit growth would have been 6.2 %. This compares to an observed household credit growth of 5.8 %. Hence, our simple calculations

---

<sup>16</sup>Note that this is often done in the literature, as first-time buyers are attractive to look at for other reasons. They do not have any mortgage debt prior to the house purchase, and they do not sell a house before buying a new one. This makes it easier to precisely measure their (new) mortgage debt and LTV-ratios. However, our findings highlight a downside of considering only first-time buyers, given that one does not have information about their parents' balance sheets.

suggest that the LTV-regulation reduced aggregate credit growth by 0.4 percentage points, or 6 %.

Is the reduction in credit growth caused by the reform small or large? While this is a subjective matter, we can compare the effect to the impact of a monetary policy shock from the VAR-literature. [Robstad \(2018\)](#) estimates the impact of a monetary policy shock on credit growth, and includes a table with other estimates from the literature. Averaging over all the estimates reported in the table, we find that the average estimate suggests that a one percentage point increase in the policy rate reduces household credit by 1.1 percent. In our case, this number translates into a reduction in credit growth of 1.2 percentage points. Hence, this dampening effect on credit is three times larger than what we estimate as the result of the reform. Or in other words, the dampening effect on credit growth from the LTV-regulation seems to be about the same magnitude as one could expect from a 33 basis points increase in the policy rate. We interpret this as saying that the dampening effect on aggregate credit growth from the regulation is non-trivial, but at the same time not very large. It is worth noting however, that this only captures the immediate impact. Looking at LTV-distributions over time indicate that households and banks continue to adjust to the regulation in the years following the reform, meaning that the full effect on credit growth is likely to be larger.

## 6 Summary

We have shown that the LTV-regulation introduced in the aftermath of the financial crisis lead to a reduction in house purchase probabilities of 3-6 percent. The reduction was driven entirely by low liquidity households. Intuitively, households with high liquid wealth holdings – who should be more able to meet the higher downpayment requirement – did not experience any reduction in purchase probability.

In terms of intensive margin effects, we showed that house buyers affected by the regulation had lower LTV-ratios, lower debt, lower purchase prices and lower interest expenses. These effects improve household solvency, and make the households more resilient against large fluctuations in asset values. At the same time however, we also documented a reduction in liquid assts. For a given house purchase, the LTV-caps imply that a higher downpayment is required, inducing households to deplete more of their liquid assets at the time of purchase. We showed in an event study setup that this effect was persistent, showing no sign of convergence even four years after the purchase.

The reduction in liquid assets could make households more vulnerable to adverse income shocks. To explore this hypothesis, we studied households who experienced job loss after purchasing a house. We showed that households affected by the regulation were two percentage points more likely to sell their house upon unemployment, allowing them to maintain their consumption level despite low liquid asset holdings. This effect however, was driven by instances with negative (regional) house price growth. Our results thus illustrate that the regulation may lead to an increase in the house sale propensity, but only given weak macroeconomic conditions. One might worry therefore, that

the increase in the house sale propensity could contribute to further house price depreciations in such events, potentially amplifying the economic downturn.

Using the parent-child links in the tax data, we showed that the regulation had large effects on the debt uptake of first-time buyer parents. The large effect likely reflects that several first-time buyer parents chose not to co-sign a mortgage with their adult child in response to the reform. Reassuringly for our causal interpretation of these results, the effect was driven entirely by high-debt parents – many of whom were likely to be constrained by the regulation themselves. first-time buyer parents with below median debt did not adjust their behavior in response to the reform.

Our results underline the complexity of borrower-based mortgage regulation, and its effect on financial vulnerability. In addition to having important distributional impacts and substantial intra-generational effects, the regulation causes a fundamental trade-off in its positive effect on household solvency and its negative effect on household liquidity. The net gain from this kind of regulation is therefore challenging to evaluate, and likely to depend on factors such as distributional preferences, initial household balance sheets and macroeconomic conditions.

## References

- Acharya, V. V., K. Bergant, M. Crosignani, T. Eisert, and F. J. McCann (Forthcoming 2022). The anatomy of the transmission of macroprudential policies. *Journal of Finance*.
- Akinci, O. and J. Olmstead-Rumsey (2018). How effective are macroprudential policies? An empirical investigation. *Journal of Financial Intermediation* 33(C), 33–57.
- Borchgrevink, H. and K. N. Torstensen (2018). Residential mortgage loan regulation. Economic Commentaries 2018/1, Norges Bank.
- Brandsaas, E. E. (2021). *Essays in Macroeconomics and Household Finance*. The University of Wisconsin-Madison.
- Browning, M. and S. Leth-Petersen (2003). Imputing consumption from income and wealth information. *Economic Journal* 113(488), 282–301.
- Cerutti, E., S. Claessens, and L. Laeven (2017). The use and effectiveness of macroprudential policies: New evidence. *Journal of Financial Stability* 28(C), 203–224.
- Claessens, S., S. R. Ghosh, and R. Mihet (2013). Macro-prudential policies to mitigate financial system vulnerabilities. *Journal of International Money and Finance* 39(C), 153–185.
- Corbae, D. and E. Quintin (2015). Leverage and the Foreclosure Crisis. *Journal of Political Economy* 123(1), 1–65.
- DeFusco, A. A., S. Johnson, and J. Mondragon (2020). Regulating household leverage. *The Review of Economic Studies* 87(2), 914–958.
- Dworkin, R. (1981a). What is equality? part 1: Equality of welfare. *Philosophy & public affairs*, 185–246.
- Dworkin, R. (1981b). What is equality? part 2: Equality of resources. *Philosophy & public affairs*, 283–345.
- Eggertsson, G. B. and P. Krugman (2012). Debt, deleveraging, and the liquidity trap: A fisher-minsky-koo approach. *The Quarterly Journal of Economics* 127(3), 1469–1513.
- Eika, L., M. Mogstad, and O. L. Vestad (2020). What can we learn about household consumption expenditure from data on income and assets? *Journal of Public Economics* 189, 104163.
- Epure, M., I. Mihai, C. Minoiu, and J.-L. Peydró (2018). Household credit, global financial cycle, and macroprudential policies: credit register evidence from an emerging country.

- Fagereng, A., L. Guiso, D. Malacrino, and L. Pistaferri (2020). Heterogeneity and persistence in returns to wealth. *Econometrica* 88(1), 115–170.
- Fagereng, A. and E. Halvorsen (2017). Imputing consumption from norwegian income and wealth registry data. *Journal of Economic and Social Measurement* 42(1), 67–100.
- Fagereng, A., M. B. Holm, and G. J. Natvik (2021). Mpc heterogeneity and household balance sheets. *American Economic Journal: Macroeconomics* 13(4), 1–54.
- Farhi, E. and I. Werning (2016). A theory of macroprudential policies in the presence of nominal rigidities. *Econometrica* 84(5), 1645–1704.
- Greenwald, D. (2018). The mortgage credit channel of macroeconomic transmission. Technical Report 5184-16, MIT Sloan Research Paper.
- Halvorsen, E. and K.-G. Lindquist (2017). Getting a foot on the housing ladder: The role of parents in giving a leg-up.
- Juelsrud, R. E. and E. G. Wold (2019). The saving and employment effects of higher job loss risk. Working Paper 2019/17, Norges Bank.
- Kaplan, G. and G. L. Violante (2014). A model of the consumption response to fiscal stimulus payments. *Econometrica* 82(4), 1199–1239.
- Korinek, A. and A. Simsek (2016). Liquidity trap and excessive leverage. *American Economic Review* 106(3), 699–738.
- Kuttner, K. N. and I. Shim (2016). Can non-interest rate policies stabilize housing markets? Evidence from a panel of 57 economies. *Journal of Financial Stability* 26(C), 31–44.
- Mian, A. and A. Sufi (2011). House prices, home equity-based borrowing, and the us household leverage crisis. *American Economic Review* 101(5), 2132–56.
- Mian, A., A. Sufi, and E. Verner (2017). Household debt and business cycles worldwide. *The Quarterly Journal of Economics* 132(4), 1755–1817.
- Morgan, P. J., P. J. Regis, and N. Salike (2019). LTV policy as a macroprudential tool and its effects on residential mortgage loans. *Journal of Financial Intermediation* 37(C), 89–103.
- Peydró, J.-L., F. Rodriguez-Tous, J. Tripathy, and A. Uluc (2020). Macroprudential policy, mortgage cycles and distributional effects: Evidence from the UK. Bank of England working papers 866, Bank of England.
- Robstad, Ø. (2018). House prices, credit and the effect of monetary policy in norway: evidence from structural var models. *Empirical Economics* 54(2), 461–483.

- Roemer, J. E. (2002). Equality of opportunity: A progress report. *Social Choice and Welfare* 19(2), 455–471.
- Shleifer, A. and R. Vishny (2011). Fire sales in finance and macroeconomics. *Journal of Economic Perspectives* 25(1), 29–48.
- Van Bakkum, S., M. Gabarro, R. M. Irani, and J.-L. Peydró (2019). Take it to the Limit? The Effects of Household Leverage Caps. Working Papers 1132, Barcelona Graduate School of Economics.
- Vandenbussche, J., U. Vogel, and E. Detragiache (2015, March). Macroprudential Policies and Housing Prices: A New Database and Empirical Evidence for Central, Eastern, and Southeastern Europe. *Journal of Money, Credit and Banking* 47(S1), 343–377.

## Appendix A: Additional Figures

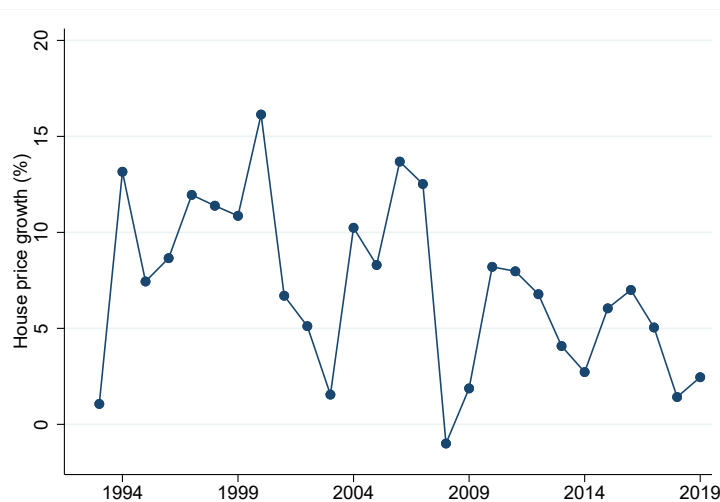


Figure A1: Annual house price growth (%).

## 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)	(2)	(3)	(4)
	House Purchase	House Purchase	House Purchase	House Purchase
$L\hat{T}V^{high} \times Post^{2010}$	-0.0494 (0.0793)	-0.0319 (0.109)		
$L\hat{T}V^{high} \times Post^{2012}$			-0.324*** (0.115)	-0.0649 (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 B2: House purchase probability - (potential) first-time buyers

*Notes:* Results from estimating equation (2), with dependent variable house purchase probability (%).  $L\hat{T}V^{high} = 1$  if  $L\hat{T}V > 90$  (85) for the 2010 (2012) requirement and zero otherwise.  $Post^{2010} = 1$  if year  $\geq 2010$  and zero otherwise.  $Post^{2012} = 1$  if year  $\geq 2012$  and zero otherwise. Standard errors are clustered at the municipality level. \* p<0.1, \*\* p<0.05, \*\*\*p<0.01.

	(1)	(2)	(3)	(4)
	GFW	Deposits	Deposits t+1	Deposits t+2
$L\hat{T}V^{high} \times Post^{2010}$	-20,276 (13,658)	-3,390*** (1,163)	-2,475*** (511)	-2,186*** (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 (2), with dependent variables gross financial wealth (GFW) (USD), bank deposits (USD), bank deposits one year ahead and bank deposits two years ahead.  $L\hat{T}V^{high} = 1$  if  $L\hat{T}V > 90$  zero otherwise.  $Post^{2010} = 1$  if year  $\geq 2010$  and zero otherwise. Sample: house buyers with  $L\hat{T}V \in [60, 110]$ . Sample period: 2007-2011. Standard errors are clustered at the municipality level. \* p<0.1, \*\* p<0.05, \*\*\*p<0.01.



	(1)	(2)	(3)	(4)
	GFW	Deposits	Deposits t+1	Deposits t+2
$L\hat{T}V^{high} \times Post^{2012}$	14,898 (19,267)	-4,340*** (1,616)	-3,294** (1,633)	-5,160*** (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 (2), 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  $L\hat{T}V > 85$  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)	(2)	(3)	(4)	(5)	(6)
	Debt	House price	Deposits	Debt	House price	Deposits
$L\hat{T}V^{high} \times Post^{2006}$	-938 (3,862)	9,889* (5,404)	2,113 (1,445)			
$L\hat{T}V^{high} \times Post^{2007}$				-6,028 (4,150)	-2,064 (5,798)	530 (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 (2), with dependent variables non-student debt (USD), house purchase price (USD) and bank deposits (USD).  $LTV^{high} = 1$  if  $L\hat{T}V > 90$  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.

Centre for Applied Macroeconomics and Commodity Prices (CAMP)

will bring together economists working on applied macroeconomic issues, with special emphasis on petroleum economics.

BI Norwegian Business School  
Centre for Applied Macro - Petroleum economics (CAMP)  
N-0442 Oslo

[www.bi.no/camp](http://www.bi.no/camp)