The impact of mortgage regulation on homeownership and household leverage [*]

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Abstract

This paper examines the impact of a limit on loan-to-value (LTV) ratios implemented in Finland in 2016. Focusing on households that buy an apartment for the first time, we evaluate how the regulation influenced household leverage and the decision to become a homeowner. According to our bunching estimates, the reform reduced transitions into homeownership by 17% among borrowers potentially affected by the reform. This corresponds to an 8% reduction in the total amount of first-time apartment buyers. The reduction in transitions to homeownership is found to be driven by households with below median income, suggesting that the regulation may have important consequences for the distribution of wealth. An additional 8% of potentially affected households reduced their LTV ratios to below the limit. Differences-in-differences analysis, comparing those expected to have LTV ratios above the limit to those below the limit, indicates that the reduction in mortgage debt was accompanied by an increase in other debt.

Key words: macroprudential policy; mortgage regulation; loan-to-value ratio, household leverage; homeownership; housing demand
JEL codes: G28; G51; R21; R28

1 Introduction

After the financial crisis, many countries introduced macroprudential policies to reduce household indebtedness. The measures directly related to housing markets can be divided into those that influence the supply of credit and those affecting the demand for credit through borrower-based measures. The latter work through their effects on households' budget constraints and therefore directly affect housing choices of credit constrained households.[1]

The main rationale for these measures is related to financial stability. By reducing household leverage, macroprudential measures improve households' ability to deal with adverse shocks and to service debt. Consequently, such measures contribute to both the resilience of the credit market and the aggregate economy.

However, while targeted at reducing aggregate risks, the measures are likely to have direct efficiency and distributional effects at the household level. The potential negative welfare effects for households come in the form of reduced possibilities to smooth consumption over the life cycle and reduced transition from renting to owner-housing. As housing wealth constitutes a large part of household wealth, these effects are also important in thinking about the role of housing in the household wealth formation and more generally in wealth inequality (see, e.g. Piketty and Zucman, 2014). The quantitative importance of these effects depends on the tax system: For instance, in countries with sizable tax advantages to owner-housing, the effects on wealth distribution are likely larger.

Most research on the effects of borrower-based macroprudential measures examines aggregate outcomes such as house prices and credit stock using cross-country comparisons. Recently Aastveit et al. (2022) examine the effects of 90 and 85 LTV limits in Norway and van Bekkum et al. (2022) the effects of a 106 LTV limit in the Netherlands using detailed household-level register data. Both studies identify a range of channels through which the regulation may operate and show that the LTV limits may also have some adverse effects because reduction in leverage is in part obtained by depleting liquidity. Kinghan et al. (2017) analyze the effect of LTV limit for first time buyers above a house price threshold, and find that this kind of regulation affects leverage in higher income groups that are more likely to buy in the affected price range.

We add to the emerging body of micro level evidence on the impacts of macroprudential policies. Relative to the previous papers, our advantage is that in Finland the regulation was imposed during quite stable housing market conditions in 2016 (as opposed to soon after the financial crisis) and no other borrower-based regulation was at place nor has been introduced since. Also, the data allows us to measure LTV ratios

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[1]On the use of different types of supply and demand measures especially after the financial crisis, see e. g. Cavalleri et al. (2019).

more precisely than some earlier papers.

We use population wide individual level data to evaluate how household debt and tenure choices reacted to the introduction of a loan-to-value (LTV) limit in Finland in 2016. We focus on first-time-buyers whose LTV limit was 95% and take two approaches to analyse the reform. In the first approach, we examine the effects of the LTV limit through bunching analysis, in essence by comparing distributions of LTV ratios before and after the reform. In the second approach, we conduct a difference-in-differences analysis where the treatment group is defined as those households that have predicted LTV ratios above the reform's limit, based on regressions in the pre-reform period. In the analysis, we disentangle the effects along two different margins: the extensive margin concerning the decision to take up a mortgage and buy an apartment and the intensive margin concerning the size of the mortgage, housing and non-housing consumption as well as household balance sheet.

Thus far, the LTV limit is the only binding borrower-based macroprudential measure in place in Finland. This facilitates the interpretation of the estimated treatment effect and stands in contrast to the wide range of measures adopted in Finland's neighbouring countries (see Table 1). Compared to these countries, Finland was also a late adopter of the LTV limit. In particular, the adaptation of the regulation did not take place right after the financial crisis, but instead in relatively stable housing market conditions.

We find clear evidence that the reform constrained LTV ratios of apartment buyers both on the extensive and intensive margin. Our bunching estimates indicate that the reform reduced transitions into homeownership by 17% among borrowers potentially affected by the reform. This corresponds to an 8% reduction in the total amount of first-time apartment buyers. If the effect persists in later years, the regulation can lead to a sizeable reduction in homeownership rate, which is currently above 60% in Finland. The reduction in transitions to homeownership is entirely driven by households with below median income. This suggests that the LTV regulation may have important consequences for the distribution of wealth as the regulation limits the opportunities of lower income households to benefit from the tax advantages of homeownership. An additional 8% of potentially affected households reduced their LTV ratios to below the limit. Our differences-in-differences estimates indicate that the reduction in mortgage debt was accompanied by an increase in other debt (e.g. consumer loans), which suggests that as a consequence of being able to borrow less, households eat up more liquidity which they seek to compensate with other debt.

The structure of the paper is as follows: In Section 2, we discuss related literature. In Section 3, we provide contextual information regarding the Finnish housing and mortgage market and macroprudential policy. In Section 4 we present our data, fol-

lowed by some descriptive statistics in Section 5. Section 6 explains our first approach to analysing the reform and presents the associated results, while section 7 details our second approach and reports the results. Finally, we conclude in Section 8.

2   Related literature

Most studies on macroprudential measures and housing markets are cross-country comparisons focusing on aggregate outcomes like house price appreciation or credit growth.[2] Following the wave of borrower-based macroprudential measures in the 2010s, also an empirical literature using micro data and with research designs aiming for causal inference is emerging.

Two papers most similar to ours are Aastveit et al. (2022) and van Bekkum et al. (2022). Both use detailed administrative household tax data linked with housing transactions data. In both papers, the analysis is based on a comparison of individuals predicted to have a high LTV ratio before and after the reform in a difference-in-differences setting.

Aastveit et al. (2022) study the effects of a 90% LTV limit and a further 85% limit introduced in 2010 and 2012 in Norway. The authors show that the affected households were 2–6% less likely to purchase a new house during the year the regulation was imposed. In addition, conditional on purchase, the average LTV ratios were reduced by three percent and average debt holdings by 11%. The authors also document a 10% reduction in liquid assets. This negative effect on liquid assets was highly persistent, showing no sign of weakening four years after the purchase of the house.

van Bekkum et al. (2022) in turn assess the effects of a 106% LTV limit introduced in 2011 in the Netherlands. The authors focus exclusively on first-time buyers and find a sizeable extensive margin response (8.9%) in the two years after the reform. In addition, conditional on buying, the households substantially reduced leverage and debt servicing costs. The LTV ratios among more-affected households drop by 6.4 percentage points. The main margin of adjustment was liquid wealth, not cheaper homes or increased lightly-regulated loans.

Also Kinghan et al. (2017) analyze the effect of LTV limits in a difference-in-differences setting in slightly different regulation system where the LTV limit depends on house price. They find that LTV limit changes above a house price threshold affect leverage in higher income groups that are more likely to buy in the affected price range.

Finally, interestingly for our focus on first-time buyers, Carozzi (2020) studies the evolution of housing markets in the UK in 2008-2009. During the housing bust, house prices dropped across all housing types, but transaction volumes fell more for units at

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[2]See, e.g. Morgan et al. (2019) for a review.

the lower end of the market. The author interprets the results through the lens of an overlapping-generations housing ladder framework and shows that steady states with tighter lending conditions have fewer first-time buyers, a bigger decrease in transactions of lower quality units and higher levels of renting. Tighter credit also implies more buy-to-let (households that keep their starter houses and rent these when trading up). The results are driven by the pricing out of young buyers by wealthier, older households when credit is tighter.

Our study is also related to the large literature on household tenure choices and the housing ladder. For reviews on home ownership and consumption over the life cycle we refer the reader to Piazzesi and Schneider (2016) and Davis and Van Nieuwerburgh (2015). Here, we will highlight the studies that seem relevant when thinking about the effects of credit conditions and the different behavioral margins when focusing on first-time buyers.

The main questions of interest are (i) what factors explain home ownership rate over the life cycle and in particular of young adults, (ii) what forces determine the interaction of housing markets and labour markets. Macroprudential regulation is a potentially important factor contributing to both.

Especially after the financial crisis, the downward trend in homeownership rates of young adults has been documented in many countries. Several potential explanations have been proposed in the literature, including increased earnings uncertainty, reduced residential mobility, changes in household formation, and tighter credit conditions (see, e.g. Fisher and Gervais, 2011; Sommer et al., 2013; Mnasri, 2015; Ma and Zubairy, 2021; Cribb et al., 2018; Carozzi, 2020). Especially in large cities with high house prices, young adults' transitions from renting to owning may also depend on tenure, wealth or other characteristics of their parents (see, e.g. Engelhardt and Mayer, 1998; Enström Öst, 2012; Kolodziejczyk and Leth-Petersen, 2013). Finally, the tax treatment of owner-housing relative to rental housing or other assets may influence housing consumption, tenure choice as well as household leverage (see, e.g. Gervais, 2002; Saarimaa, 2011; Hilber and Turner, 2014; Gruber et al., 2021).

The interaction of housing and labour markets is most clearly reflected in household location choices. In a spatial equilibrium, house prices adjust to accommodate residential mobility to local productivity and amenity shocks. Tight down payment requirements may induce households to choose locations with lower house prices and thereby reduce house price dispersion across locations. The inability to borrow against future earnings reduces the desire of young high ability households to move to expensive locations (see e.g. Halket and Vasudev (2014)).

Acharya et al. (2020) find evidence consistent with this mechanism when studying house prices and mortgage credit development after an introduction of LTV and DTI

limits in Ireland in 2015. They show that after the reform mortgage credit is reallocated from low-income to high-income borrowers and from urban to rural counties.

In general the evidence on macroprudential measures and household responses highlight the complexity of the regulation in the housing market. The results indicate that borrower-based regulation affects both extensive and intensive margin decisions at least in the short run. But the margins of adjustment seem to vary and the quantitative importance of different channels depends on the institutional details. The fact that household leverage and tenure choices are also affected by (or jointly determined with) geographic mobility, earnings risks, taxation, and household formation makes it difficult to pin down the long run effects of the policies.

3 Institutional setting and the reform

3.1 Finnish housing market

Roughly 60% of Finnish households and 70% of population live in owner-occupied housing and some 30% live in rental housing.[3] The prevalence of owner-occupied housing varies somewhat geographically, being most common in rural areas and small towns. For instance, in the capital city of Helsinki the share of owner-occupiers is slightly less than 50%.[4]

Homeownership is subsidised through the tax code in several ways. Firstly, imputed rent of owner-occupied housing is not taxed, while rental income is treated as capital income and taxed accordingly.[5] Secondly, owner-housing is not subject to capital gains taxation upon sale of the unit, if the unit has been the permanent residence of the owner for at least two years. Thirdly, until 2012, mortgage interest payments were fully tax deductible at the capital income tax rate. Since 2012, the degree of tax deductibility has been gradually reduced and in 2023 it will have phased out completely.

The Ministry of Finance estimates that the non-taxation of imputed rental income lead to a 4.3 bn loss in tax revenue in 2021, and the tax expenditure due to the exemption of capital gains from owner-occupied housing was approximately 1.5 bn. The total tax expenditure is annually roughly 1500 euros per person in owner-occupier households. This implies that access to homeownership is likely an important driver of economic inequality.

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[3]The Finnish rental market can be divided into the private rental market (70% of rental units) where rents can be freely set, and social housing (30% of units) where rents and tenant selection are regulated.

[4]For details, see Statistics Finland (2022a).

[5]In 2022, capital income is subject to a 30% tax rate and 34% if taxable income exceeds an annual threshold of 30,000 euros. The tax rate has been slightly increasing during the recent decades, and progressivity was introduced in 2012.

Real estate transfers are subject to a tax, which is either 2% or 4% of the purchase price depending on the type of the housing unit. Young first-time buyers are exempt from the housing transfer tax. More specifically, buyers and transactions that satisfy the following requirements are exempt:

- Buyer is between 18 and 39 years old at the time of the transaction.

- After the sale, the buyer owns 50% or more of the dwelling.

- The dwelling is bought as permanent residence and the buyer moves into it within 6 months of the transaction.

- The buyer has not previously owned 50% or more of any dwelling.

Consistently with high homeownership rate, housing is the single most important form of wealth for Finnish households. According to the Statistics Finland Wealth Survey, in 2016 roughly 50% of net wealth of the households was in the form of owner-housing (principal residence), 6% in secondary houses and 10% in other real estate.

Homeowner households either own a house or an apartment. The ownership of apartments in multi-unit buildings is arranged through housing co-operatives that own the building (or sometimes multiple buildings on the same lot) and often also the lot under the building. Owning the shares to a specific housing unit in practice implies owning the unit.[6] Housing co-operatives often have outstanding loans obtained during the construction of the building or later for renovation purposes. A fraction of the housing co-operative loan is linked to the shares of each unit. The loan is formally a debt of the housing co-operative, but in practice the repayment of a housing co-operative loan is a joint responsibility of the shareholders, managed through monthly fees.

In the analysis, we will focus on owner-occupied apartments and omit single family detached housing units. This is because we do not have data on single family house transactions needed to calculate LTV ratios.

Figure 1 shows the real price development for resales of apartments in multi-unit buildings in Finland, and separately for the Helsinki Metropolitan Area and the rest of Finland. In the 2000's the overall price development has been more stable in Finland than in many other European countries and the US. However, regional price divergence of apartments has increased. In real terms, resale prices of apartments have been increasing in Helsinki Metropolitan Area and decreasing in the rest of Finland.[7]

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[6]In the case of detached housing units, the ownership structure is simpler: one directly owns the structure and typically also the lot under the structure.

[7]Eerola et al. (2020) analyse in more detail the regional divergence of housing prices after the financial crisis.

Figure 1: Real prices - apartments, 2000/1-2021/4.
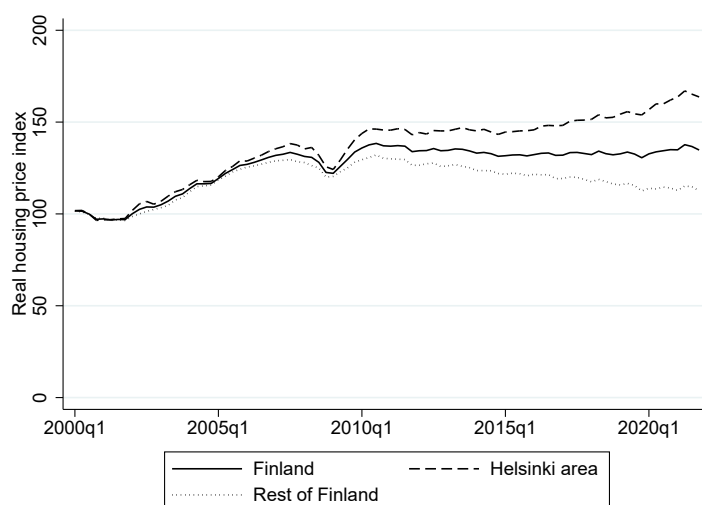
Notes: The house price indeces are quality adjusted and based on dwelling resales in apartment buildings. Helsinki area consists of four cities (Helsinki, Espoo, Vantaa and Kauniainen. Quarterly data. 2000=100. Source: Statistics Finland.

3.2 Mortgage markets and the LTV reform

Household indebtedness has increased in Finland during the 2000's. When looking at mortgages, the main reason is substantially larger loans. In 2021, the average mortgage was roughly 110,000 euros which is double the size of the average mortgage in the early 2000's. The share of households with a mortgage has in turn remained relatively constant at around 30% of all households (Statistics Finland, 2022b).[8] Also, despite rapid increase in household indebtedness, credit losses on mortgages have remained very low (Bank of Finland, 2022).

In Finland, mortgages are always full recourse loans, and a typical maturity for mortgages is around 25 years. The interest rates on mortgages are almost exclusively variable and mostly tied to the 12-month Euribor rate. Up until the early 2022, the average interest rate of new mortgages has been declining after the financial crisis. During the time period we focus on, the average interest rate of new and renegotiated mortgages declined from 1.80% in 2014 to 0.89% in 2018.

First-time buyers are entitled to government support in buying their first owner-housing through the *Act on Bonus for Home Savers* (ASP). An ASP savings account can be opened by young adults who are between 15 and 39 years old and have not previously owned an apartment. The interest on the savings are tax-exempt. After

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[8]These figures do not include households' share of the housing co-operative loans. Taking into account the housing co-operative loans shows that the structure of housing-related debt has changed. In particular, the share of housing co-operative loans of households' all housing-related loans has risen from less than 5% in the early 2000's to about 17% in 2021.

having saved at least 10% of the purchase price of the apartment, the applicant may be granted an ASP interest subsidy loan by the bank. The interest rate of the ASP loan is lower than the interest rates of other similar mortgage loans granted by the bank for first-time buyers. In addition, the ASP loan features a government interest subsidy for ten years and a government guarantee. The house to be bought serves as security for up to 75% of the purchase price, but with the state guarantee the amount of the loan may be 90% of the purchase price.[9]

The ASP system was developed in the early 1980s. In the early 1990s, more than 10,000 ASP loans were granted annually. After that, the number of loans granted gradually decreased. Increased house prices and the decrease in the interest rate reduced the attractiveness of the ASP loans. In 2009, the system was reformed and conditions made more favourable, after which the number of new ASP contracts increased substantially.

Macroprudential policy is conducted by the Finnish Financial Supervisory Authority (FIN-FSA). Before 2016, the FIN-FSA steered mortgage lending only through recommendations.[10] In July 2016, a loan-to-value (LTV) limit was introduced for mortgages.

The LTV limit was set to 95% for first-time buyers, and to 90% for other buyers. The LTV limit for first-time buyers has remained unchanged ever since. However, the limit for other than first-time buyers has been changed several times. It was lowered to 85% in July 2018 and raised back to 90% in July 2020, and lowered again to 85% in October 2021.[11]

As explained above, first-time buyers between 18 and 39 years old are exempt from the real estate transfer tax. This tax exemption has created a well-established practice on who can be considered a first-time buyer for those below the age of 40. According to FIN-FSA, the banks use this definition of first-time buyers also in their own risk management guidelines. As our data are based on transaction tax registers they contain information on whether the buyer is exempt from the transaction tax. This information allows us to directly and reliably infer who is considered to be a first-time buyer in the LTV regulation.

The main components of the LTV ratio formula are the mortgage, purchase price of the housing unit, and the housing co-operative loan associated with the unit. For example, suppose a household pays 260 000 euros for an apartment which is burdened

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[9]The maximum amount of the ASP interest subsidy loan is determined based on the location of the apartment. The maximum state guarantee per apartment is 50,000 euros.

[10]For example in 2010, the FIN-FSA recommended a maturity limit of 25 years and the use of hypothetical 6% interest to assess solvency.

[11]The decision on the LTV limit for mortgages is taken quarterly by the FIN-FSA Board. Ministry of Finance, Ministry of Social Affairs and Health and Bank of Finland are consulted prior to taking the decision.

with 40 000 euros of co-operative debt and takes on a mortgage of 170 000 euros. The LTV ratio would then be (170 000 + 40 000)/(260 000 + 40 000) = 0.7. However, additional collateral provided by the borrower can be included in the denominator of the LTV ratio. These include other housing pledges (including leisure time residences), deposit collateral, other real collateral (e.g. equity shares, fund units) and third party pledges. Thus, the LTV ratio is defined as the ratio of the mortgage loan to the current value of collateral posted at loan approval.

To support financial stability, additional borrower-based measures have also been discussed. Based on a recent committee report (Ministry of Finance, 2019), a new government proposal is currently being reviewed by the Parliament. The provisions to be added to current legislation concern the housing co-operative loans of new construction and include i) a 60% limit to the ratio of co-op debt to the debt-free price of new housing shares for sale, ii) a maximum repayment period of 30 years, and iii) a requirement that the loan contract should not contain repayment holidays during the first five years after construction.

Many countries have implemented several different measures in parallel, which might make it difficult to disentangle the effect of each individual measure (see Table 1). In Finland, the LTV limit is the only binding borrower-based macroprudential measure in place. In addition, the introduction of the limit did not take place right after the financial crisis, but instead in relatively stable housing market conditions.

Table 1: Borrower-based macroprudential measures in Nordic and Baltic countries.

Instrument	Country	Description
Loan-to-value (LTV)	Denmark	95%
	Estonia	85%
	Finland	85%, 95% for first-time buyers
	Iceland	80%, 85% for first-time buyers
	Latvia	90-95%
	Lithuania	85%, 70% for second and subsequent loans
	Norway	85%
	Sweden	85%
Amortisation requirements	Norway	For LTV > 60%: 2.5% p.a.
	Sweden	For LTV > 70%: 2% p.a.
		For 70% > LTV > 50%: 1% p.a.
		1% extra p.a. if LTI > 4.5 times gross income
Debt service-to-income (DSTI) limit	Estonia	50% while using at least 6% interest rate in calculations
	Iceland	35%, 40% for first-time buyers
	Latvia	40% (to net income)
	Lithuania	40%
	Norway	interest stress test requirement of 3 pp increase
Maximum loan maturity	Estonia	30 yr
	Latvia	30 yr (mortgage loans) / 7 years (consumer loans)
	Lithuania	30 yr
Other measures	Denmark	If DTI>4 and LTV>60%, fixed interest rate mandatory for at least 5 yr
	Norway	DTI: 5 times gross income
	Latvia	DTI: 6 times net income

Notes: The table shows the borrower-based macroprudential measures in the Nordic and Baltic countries in 2022. LTI refers to loan-to-income. Source: Nordic-Baltic Macroprudential Forum.

3.3 Impact of LTV regulation - potential channels and behavioral margins

The behavioural effects of LTV regulation at the household level may operate through several different channels. Some households may decide not to borrow at all (extensive margin) while others may still borrow but adjust their LTV by other means (intensive margin). We set out to identify the impact of the LTV reform along both margins.

Throughout the analysis we assume that the decision to take up a mortgage is jointly made with the decision to buy. As we only consider first-time buyers, the extensive margin response then equates with reduced transitions from rental housing to owner housing. Given the tax advantages associated with homeownership, the extensive margin response is important for the wealth distribution. In addition, to the extent that rental and owner housing are not viewed as perfect substitutes, the extensive margin response also has direct welfare consequences.

When looking at the intensive margin, there are several margins of adjustment. Conditional on buying a house, households taking up a smaller mortgage may reduce housing consumption (size, location and other characteristics), non-housing consumption and/or other assets. There may also be other balance sheet or labour market effects. If first-time buyers respond to the regulation through the intensive margin, they can meet the new down payment requirement through an increased use of savings. This could generate an adverse effect for financial stability by reducing the liquidity of first-time buyers. Alternatively they can choose to buy a less expensive apartment and take up a smaller mortgage.

In our first identification approach, we estimate these effects by analysing changes in the distribution of LTV ratios. Missing observations are interpreted to reflect the extensive margin response of not taking up a mortgage (and buying an apartment). A shift in the LTV distribution in turn shows the intensive margin response which may reflect a change both in the mortgage and the purchase price.

The bunching analysis does not allow us to disentangle the different margins of adjustment at the intensive margin. Therefore, in our second identification approach, we focus on the intensive margin. To this end, we examine how borrowers predicted to have high LTV ratios react to the reform. In this setting, we can separate the response into an effect on borrowing and an effect on purchase prices. This should provide an indication of whether households choose to deplete liquid assets or reduce non-housing consumption to meet the new down payment requirement.

4 Data

We use register data from Statistics Finland on the total Finnish population combined with register data on the universe of mortgages as well as data on transactions of apartments (that is, dwellings in housing co-operatives). Transaction data on detached houses are not available to us.[12] The data cover years 2006–2018 but we focus on years 2013-2018 surrounding the reform. The household data contain a rich set of socio-economic characteristics and housing characteristics, including the location of the housing unit. In Appendix A.1 we provide an overview of the datasets used, references to their source and detailed definitions of the main variables we examine.

For each transaction, the data contain the personal identification code of the buyer(s) which enables us to link the transaction to the household data. The data also contain exact timing of the transaction and information on the liable transfer tax, which allows us to identify first-time buyers as explained above.

We exclude households that buy multiple apartments. Additionally we focus on households in dwellings with only one household. Some dwellings are inhabited by individuals that do not constitute a household and others by multiple households or a combination of the two. In these cases, we cannot allocate debt or income to the household-level and therefore leave these dwellings out of our sample.

For all households in our sample, we aggregate debt and gross income at the household level. We use the characteristics of the household head (age, education level, socio-economic status) when analysing leverage and tenure choice by socio-economic groups. When allocating the households into income groups, we adjust for household size in order to take into account economies of scale in consumption.[13] Note finally that each quantile consists of the same number of households, not individuals. That is, if household size varies systematically with age or income, the number of individuals may vary from one group to the other.

Although our data include a rich set of socio-economic characteristics, we are not able to link parents to adult children. This means that we are not able study the relationship between, for instance, parents' tenure or wealth and their children's tenure choices. In fact, we have no comprehensive data on wealth, so we cannot examine the role of available collateral other than that produced by the purchase of an apartment.

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[12]Above 40% of homeowner households occupy an apartment, for first-time buyers the share is much higher at 70%.

[13]This adjustment is made using the OECD's adjusted consumption unit scale.

5 Descriptive statistics

Table 2 presents summary statistics on the main variables used in our analysis, one year before and one year after the reform.

Our data are restricted to apartment owners. The sample used in our analysis further zooms in on first-time buyers between 18 and 39 years old with an LTV ratio between 50 and 140 i.e. excluding the bottom and top percentile. The LTV ratio is calculated as the sum of a household's mortgages and the co-operative loan associated with the apartment bought, over the "debt-free" purchase price of the apartment.[14] The LTV ratio is calculated only once, at the time of purchase.

The data do not include information on other collateral used. This implies that some of the buyers with LTVs above the limit may comply with the limit if they have put forth additional collateral. Nevertheless, the apartment bought is likely the only hard collateral for a large share of buyers. Therefore, we would expect to observe bunching in the LTV distribution below the limit, if the limits were successful in constraining leverage.

As can be observed in Table 2, the number of first-time buyers increased by almost 20% between 2015 and 2017. Loan-to-value ratios decreased on average, with house prices (8.6%) increasing slightly faster than mortgages (7.1%) and outpacing average earnings growth (5.5%). Simultaneously, the interest expenses and the simulated interest rate have come down.[15]

Finally, because the sample consists of first-time buyers, the households are mainly relatively young singles or couples. A large share of the households in the sample do not have capital income or other debt holdings.

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[14]Debt-free refers to the price including the apartment owner's share of the housing co-operative loan as explained in Section 3.

[15]The interest rate is imputed from interest expenses as the ratio of interest expenses to mortgage in first full year of interest payments.

Table 2: Summary statistics - first-time buyers

	2015				2017			
	mean	p25	p50	p75	mean	p25	p50	p75
LTV	94	87	96	102	93	87	93	100
Mortgage debt	137163	93567	127893	169611	145323	97541	134291	181625
House price	153396	103211	141509	187676	164860	111155	151846	207424
Surface (m^2)	65	51	62	78	64	51	62	78
Interest expenses	1764	1203	1638	2196	1402	915	1255	1734
Interest rate	1	1	1	2	1	1	1	1
Capital Income	805	0	4	40	1486	0	10	102
Wage Income	46624	30515	42124	59909	48602	31687	43803	62221
Disposable income	38520	25711	36378	48631	40484	26768	37816	50629
Other debt	6493	0	1092	9183	8816	0	3556	12514
Age	28	25	28	32	29	25	28	32
Family size	2	1	2	2	2	1	2	2
N	8821				10416			

 Notes: This table presents summary statistics on first-time buyers below the age of 40 a year before and after the reform. Amounts are real values expressed in 2016 euros.

Figure 2 shows the distribution of LTV ratios before the reform in years 2013–2015 and after the reform in 2017. For each year the figure presents the number of first-time buyers with LTV ratios between 50 and 140 (98% of the data) in bins of one percentage point width.

Several observations can be made. First, there are clear spikes in the distribution at LTV ratios 90 and 100 before the reform in years 2013–2015. Banks typically accept only 70–75% of the purchase price as collateral. This means that the buyer needs additional collateral to get a loan corresponding to the LTV limit. This might include other assets or guarantees by the state or banks. As discussed above, all young first-time buyers are entitled to a government guarantee through the ASP program. However, in order to qualify for the ASP interest subsidy loan, the total amount of the loan cannot exceed 90% of the purchase price. Another institutional factor explaining the bunching is FIN-FSA guidelines which recommended banks not to grant loans with LTV ratios exceeding 90%.[16]

Second, from the 2017 distribution it is apparent that following the introduction of a 95% LTV regulation in 2016, bunching now occurs also at LTVs of 95%. In addition, there are substantially less cases with LTV ratios above 100.

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[16]For example: "FIN-FSA invites the banks to abstain from financing home purchases for own use involving a higher than 90% LTV ratio relative to the market value of the property to be financed, with the exception of certain rare cases." (Finnish Financial Supervisory Authority, 2010) and "[…] if loan decisions involve LTV ratios higher than those mentioned in this recommendation, such decisions should be preceded by a particularly thorough assessment, along with the applicant, of the applicant's repayment capacity and guarantees." (Finnish Financial Supervisory Authority, 2010).

We do not have data on household or loan characteristics that would allow us to analyse in more detail why a some first-time buyers have a very high LTV ratio both before and after the reform. For instance, we do not observe possible additional collateral or other financial support from parents. Survey evidence on all households (not only first-time buyers) from the time before the reform, suggests that main reasons for LTV ratios exceeding 100% were 1) other collateral provided by the customer or his/her parents, 2) housing change situation, where the old housing is still unsold, and 3) significant renovation/extension that increases the value of the apartment (Finnish Financial Supervisory Authority, 2011).

Figure 2: The evolution of LTV distribution
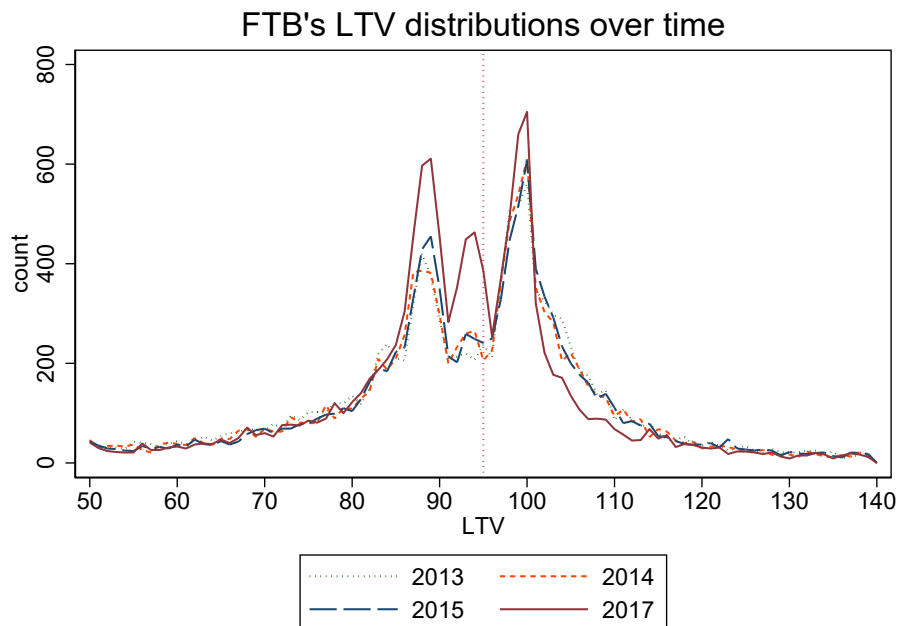
6   Bunching analysis

6.1   Research design

6.1.1   Counterfactual LTV distribution

In the spirit of DeFusco et al. (2020), we first analyse the impact of LTV regulation by comparing the observed distribution of LTV ratios to a distribution that would have prevailed in the absence of the regulation. For this comparison, we construct a counterfactual LTV distribution based on pre-reform distributions and a range of unaffected borrowers.

First, to use the LTV distribution in a pre-reform year as a baseline, we assume that the regulation had no effects prior to its implementation. Second, we assume that

part of the distribution is unaffected by the regulation. The idea is that borrowers well below the LTV limit would have borrowed at low LTV ratios even in the absence of regulation. The mass of these borrowers before and after the reform allows us to account for changes in the total number of borrowers that is independent of the regulation. Finally, to allow for changes in the distribution over time, we assume a constant time trend, which we capture by taking the difference between the normalized distributions of two years prior to the regulation. Our counterfactual distribution is then the result of adding this difference to the normalized baseline and scaling it with the bottom end of the post-reform distribution assumed to be unaffected by the regulation.

Formally, let $n_{b,t}$ be the actual number of first-time buyers and $\hat{n}_{b,t}$ the counterfactual number of first-time buyers in bin b at time t. In addition, let $\bar{b}$ denote the implemented LTV limit and $\underline{b}$ the threshold for unaffected borrowers.

The above assumptions can then be expressed as follows:

Assumption 1. There is no anticipation in years prior to the introduction of the regulation in year t:

$$\widehat{n}_{b,t-k} = n_{b,t-k} \text{ for k } > 0, \tag{1}$$

Assumption 2. The bottom end of the LTV distribution is unaffected by the regulation:

$$\sum_{b_{min}}^{\underline{b}} \hat{n}_{b,t} = \sum_{b_{min}}^{\underline{b}} n_{b,t} \equiv N_{\underline{b},t} \text{ for some } \underline{b} < \bar{b}, \tag{2}$$

where b_{min} is the bin with the lowest observed LTV ratio in the sample.

Assumption 3. There is a constant time trend:

$$\frac{\hat{n}_{b,t}}{N_{\underline{b},t}} = \frac{\hat{n}_{b,t-1}}{N_{\underline{b},t-1}} + \left(\frac{\hat{n}_{b,t-1}}{N_{\underline{b},t-1}} - \frac{\hat{n}_{b,t-2}}{N_{\underline{b},t-2}} \right) \equiv \hat{\pi}_{b,t}. \tag{3}$$

This implies that the counterfactual number of borrowers per bin is determined by $\hat{n}_{b,t} = \hat{\pi}_{b,t} \times N_{\underline{b},t}$.

To calculate the counterfactual, note first that given our sample (see Table 2) we have that $[b_{min}, b_{max}] = [50, 140]$. We assume that borrowers with LTV ratios below 90 are not affected by the regulation and set $\underline{b} = 90$. Given $\bar{b} = 95$, this implies that we expect borrowers affected by the regulation to be located in the interval or bunching window $[\underline{b}, \bar{b}] = [90, 95]$ after the reform.

The resulting counterfactual distribution is shown in Figure 3, where the 2015 LTV distribution is used as the pre-reform baseline and the time trend was captured from the change between 2014 and 2015. Because the regulation was implemented in the middle of 2016, the reform year is disregarded and the counterfactual was made for 2017.

Figure 3: Counterfactual LTV distribution of first-time buyers.
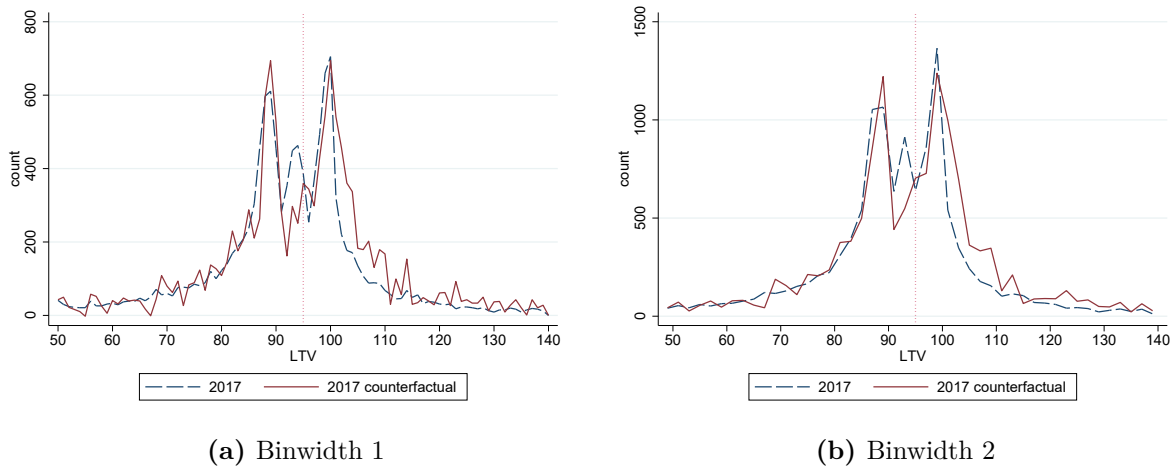
(a) Binwidth 1

(b) Binwidth 2

Notes: Actual (dashed line) and counterfactual (solid line) LTV distributions of first-time buyers in 2017. In panel (a) the sample is divided into 100 bins and in panel (b) into 50 bins. Summary statistics of the sample shown in Table 2.

As can be observed from Figure 3a, the counterfactual tracks the observed distribution of LTV ratios quite nicely in the lower end of the distribution, even beyond the ratio of 85 up to 90. The bunching at 90, already observed in pre-reform years for reasons discussed in the previous section, was entirely predicted by the normalized time trend. At the same time, it is apparent that no bunching was predicted in the LTV ratio interval of 90–95. Also there appears to be missing mass between 100–110.

Due to the limited number of observations per bin outside the bunching window, the counterfactual distribution appears quite jagged. In addition to the bunching for institutional reasons, the limited number of observations is partly because we focus on apartments, not houses, and only consider first-time buyers. Widening the bins to two percentage points results in a less precise but smoother distribution as displayed in Figure 3b.

A few comments on the underlying assumptions are in order. First, as the reform had been announced and discussed beforehand, some anticipation effects are plausible. However, because the reform was implemented in July, most anticipation effects are likely to be excluded as we drop 2016 from the analysis. Second, while a constant time trend is a strong assumption, it seems intuitive to use lagged growth to predict short run changes. However, this also means that the bunching analysis may not be suitable for estimating the long-run effects of the regulation, even if we had data from later years.

6.1.2 Effect estimation and inference

To calculate the extensive margin and intensive margin responses, we use the estimated excess mass below and the missing mass above the LTV limit. For the excess mass, we sum over the difference between the observed and counterfactual loans in the bins between the minimum and maximum bunching level, $\underline{b}$ and $\overline{b}$:

$$\hat{B} = \sum_{\underline{b}}^{\overline{b}} (n_{b,t} - \hat{n}_{b,t}). \tag{4}$$

Similarly, to calculate the missing mass, we sum over the difference between the counterfactual and observed loans in the bins that exceed the maximum bunching level $\overline{b}$ up to the bin of the highest LTV ratio in the sample b_{max}:

$$\hat{M} = \sum_{\overline{b}+1}^{b_{max}} (\hat{n}_{b,t} - n_{b,t}), \tag{5}$$

where $\sum_{\overline{b}+1}^{b_{max}} \hat{n}_{b,t}$ is the number of potentially affected borrowers.

Finally, we calculate an intensive margin effect using the number of potentially affected borrowers and our estimate of the excess mass (equation 4):

$$\text{IM} = \frac{\hat{B}}{\sum_{\overline{b}+1}^{b_{max}} \hat{n}_{b,t}} \tag{6}$$

This effect represents the share of potentially affected borrowers induced to reduce their debt take-up.

Finally, the extensive margin effect is the difference between the missing mass above the LTV limit (equation 5) and the excess mass in the bunching window relative to the amount of potentially affected borrowers:

$$\text{EM} = \frac{\hat{M} - \hat{B}}{\sum_{\overline{b}+1}^{b_{max}} \hat{n}_{b,t}} \tag{7}$$

This effect represents the share of potentially affected borrowers that refrained from borrowing all together because of the regulation. To provide confidence intervals for these effects, we bootstrap the procedure to calculate them, drawing 100 samples from the data, stratified by year.

6.2 Results

6.2.1 Main findings

Table 3 presents the estimated effects as well as their main components, the number of excess, missing and potentially affected loans. Estimates were obtained for two

years post-reform, 2017 and 2018. The first two columns show the effect estimated for a binwidth of 1 percentage point difference in LTV ratio while the last two columns show the effect for a binwidth twice that size.

The binwidth choice does not matter for the estimated effects and only slightly affects their precision. Note finally that the effect is estimated for a bunching interval encompassing LTV ratios between 90 and 95. Considering that bunching below the LTV of 90 occurred already before the regulation, for institutional reasons that are still in place today, there is no reason to think that people who would otherwise bunch at 90 would now bunch at 95 or people who would otherwise have an LTV above 95 would now bunch at 90 rather than 95. This is quite apparent from the observed LTV distribution.

Table 3: Bunching analysis: main estimation outcomes by binwidth and post-reform year

	Binwidth 1		Binwidth 2	
	2017	2018	2017	2018
Intensive margin	0.08**	0.11*	0.08**	0.11*
	(0.03)	(0.05)	(0.03)	(0.05)
	[0.03,0.14]	[0.01,0.20]	[0.03,0.13]	[0.02,0.19]
Extensive margin	0.16*	0.18	0.16*	0.18
	(0.07)	(0.10)	(0.07)	(0.10)
	[0.03,0.30]	[-0.02,0.39]	[0.03,0.30]	[-0.01,0.38]
Affected loans	6289.49	6399.91	6294.35	6415.59
Excess loans	512.56	680.68	510.25	674.17
Missing loans	1542.49	1838.91	1547.35	1854.56
N	11703	11883	11703	11883
Binwidth	1	1	2	2
Window	[90 - 95]	[90 - 95]	[90 - 95]	[90 - 95]

* denotes $P < 0.05$, ** $P < 0.01$ and *** $P < 0.001$ (bootstrapped)

Notes: Standard errors in parentheses obtained through bootstrap, stratified by year, 100 reps. 95% confidence intervals in brackets. Significance is denoted by asterisks: * $P < 0.05$, ** $P < 0.01$ and *** $P < 0.001$

For 2017, the results in Table 3 show an intensive margin effect suggesting that 8% of affected borrowers reduced their LTV ratio. The extensive margin effect suggests that in 2017, 16% of affected borrowers decided not to borrow and as a result did not become homeowners that year. For 2018, the effects are quantitatively similar, though the estimates lose some precision. The extensive margin effect is no longer statistically significant at the 5% level but it remains significant at the 10% level. At any rate, the results for 2018 do not suggests the impact was only temporary.

In order to quantify the possible implications of the extensive margin response for

homeownership rate, we relate the extensive margin response to the total amount of first-time apartment buyers. In 2017, the sample size was 11703. Taking our bunching estimates at face value, we infer that in the absence of the reform there would have been 1030 first-time buyers more (Missing loans – Excess loans). Thus, we estimate that the extensive margin effect corresponds to a roughly 8% reduction in the total amount of first-time apartment buyers $(1030/(11703 + 1030) = 0.081)$. For 2018, the estimate is close to 9%. If this effect persists also in later years, it would imply a sizable shift in the tenure type distribution away from owning to renting.

6.2.2 Heterogeneity

To further examine how the affected households reacted to the regulation, we next estimate the effects for different income groups. Because splitting the sample in quartiles or tertiles would leave too few observations for a meaningful analysis, we split the sample in households with above or below median income (adjusted for household size). The counterfactual distributions estimated for these groups are presented in Figure 4a and 4b.

From Figure 4b it appears that bunching occurred not just at the 95 LTV ratio limit but also at the 100 LTV ratio. This is likely due to high-income households being able to offer more collateral than the value of the purchased property. If so, this would lead us to underestimate the intensive margin effect.

Figure 4: Bunching analysis by income group
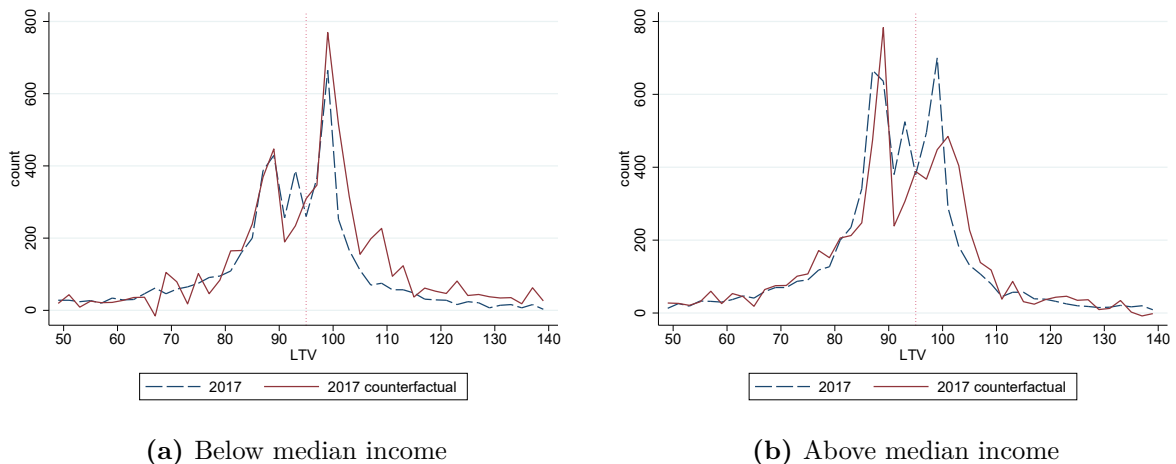
(a) Below median income (b) Above median income

Notes: Actual (dashed line) and counterfactual (solid line) LTV distributions of first-time buyers in 2017. Panel (a) shows first-time buyers below median income and panel (b) first-time buyers above median income. In both panels, the binwidth is 2.

Table 4 shows the estimated effects for both groups in the two post-reform years we observe. For the low-income group, the intensive margin effect is similar to the one

found in the main analysis. The extensive margin effect on the other hand is much more pronounced and suggests that 30% of affected low-income first-time buyers were dissuaded from taking up a mortgage to buy a house and become homeowners. This corresponds to 17% of all low-income first-time buyers in 2017 and 18% in 2018. For the group of first-time buyers with above median income, the intensive margin effect is the same, but just not significant at the 5% level. At the extensive margin however, we find a close to zero effect for affected high-income first-time buyers. Overall, these findings suggest an important role for income in determining how first-time buyers respond to LTV regulation in terms of leverage and homeownership.

Table 4: Bunching analysis: effects by income group and post-reform year

	Below Median		Above Median	
	2017	2018	2017	2018
Intensive margin	0.08**	0.10**	0.09	0.13
	(0.03)	(0.05)	(0.06)	(0.09)
	[0.02,0.13]	[0.01,0.19]	[-0.02,0.20]	[-0.06,0.31]
Extensive margin	0.30***	0.31***	-0.01	-0.01
	(0.07)	(0.11)	(0.15)	(0.23)
	[0.17,0.44]	[0.08,0.54]	[-0.30,0.28]	[-0.46,0.44]
Affected loans	3487.61	3436.67	2798.27	2865.59
Excess loans	265.48	333.61	248.76	359.01
Missing loans	1319.61	1400.67	219.27	340.59
N	5190	4897	6513	6986
Binwidth	2	2	2	2
Window	[90 - 95]	[90 - 95]	[90 - 95]	[90 - 95]

Notes: Standard errors in parentheses obtained through bootstrap, stratified by year, 100 reps. 95% confidence intervals in brackets. Significance is denoted by asterisks: * $P < 0.05$, ** $P < 0.01$ and *** $P < 0.001$

We conclude that the LTV limits reduced risky leverage measured in terms of LTV ratio across income groups. In addition, the regulation has substantially affected the probability of low-income households to become homeowners. It should be noted though that these are short-run results. Although the results did not change between 2017 and 2018, it is possible that the longer-run effect is smaller as these households may have decided to postpone borrowing and save for the new down payment requirement.

6.2.3 Robustness checks

To check to what extent our results are sensitive to the time trend we calculate, we consider two alternative specifications. We re-estimate the main effects using the change between 2013 and 2015 as well as the change between 2013 and 2014. The first approach may actually be better at capturing a trend in borrowing behaviour. The second approach may be more appropriate should there be concerns about anticipation (but note that we still use 2015 as our baseline). Figure 5 shows the resulting counterfactual distributions. In both cases, the counterfactuals seem to track the observed LTV ratio distributions quite well below the bunching window as well as the pre-existing peaks.

The estimation results are presented in Table 5. The first column shows results when using the change between 2013 and 2015 as time trend, while the second column shows results when we use just the change between 2013 and 2014. For both counterfactuals the results are quantitatively similar with regard to the extensive margin effect. With regard to the intensive margin effect, we get a very similar and precisely estimated effect using the 2013–2015 time trend but a somewhat smaller effect using only the change between 2013 and 2014 which saw an increase of borrowers in the bunching window.

Figure 5: Bunching analysis using different time trends
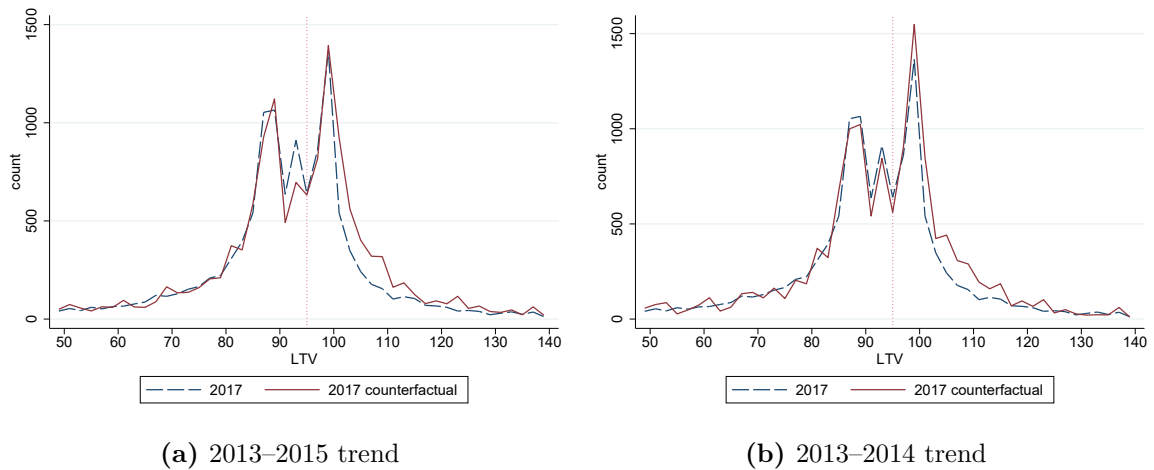
(a) 2013–2015 trend   **(b)** 2013–2014 trend

Notes: Actual (dashed line) and counterfactual (solid line) LTV distributions of first-time buyers in 2017. In panel (a) time trend based on change between 2013–2015 and in panel (b) on change in 2013–2014. In both panels, the binwidth is 1.

**Table 5:** Bunching analysis: effects estimated using alternative time trends

	2013-2015	2013-2014
Intensive margin	0.07***	0.05*
	(0.02)	(0.02)
	[0.03,0.10]	[0.01,0.10]
Extensive margin	0.17***	0.18**
	(0.04)	(0.06)
	[0.09,0.26]	[0.06,0.30]
Affected loans	6256.16	6222.82
Excess loans	419.80	327.04
Missing loans	1509.16	1475.82
Year	2017	2017
Binwidth	1	1
Window	[90 - 95]	[90 - 95]

Notes: Standard errors in parentheses obtained through bootstrap, stratified by year, 100 reps. 95% confidence intervals in brackets. Significance is denoted by asterisks: * $P < 0.05$, ** $P < 0.01$ and *** $P < 0.001$.

Placebo tests are another check to make sure that the estimation procedure is not picking up something else instead of the reform. Using 2014 as a baseline and the change between 2013 and 2014 to capture the time trend, Figure 6 shows a counterfactual for the LTV ratio distribution of 2015, which seems to be very similar to the observed distribution. Indeed, the estimated effects in Table 6 are close to zero and statistically insignificant.

Figure 6: Bunching Analysis: Placebo test
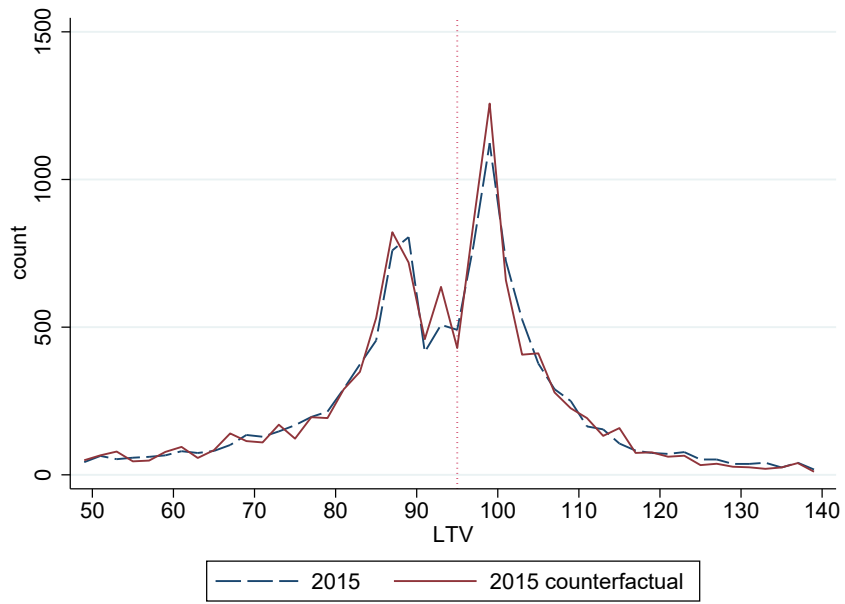
Notes: Actual (dashed line) and counterfactual (solid line) LTV distributions of first-time buyers in 2015. Time trend based on change between 2013 and 2014. The binwidth is 1.

Table 6: Bunching analysis: placebo test

	Binwidth 1	Binwidth 2
Intensive margin	-0.02	-0.02
	(0.02)	(0.02)
	[-0.05,0.02]	[-0.05,0.02]
Extensive margin	0.01	0.01
	(0.06)	(0.06)
	[-0.10,0.12]	[-0.10,0.13]
Affected loans	5317.09	5332.77
Excess loans	-80.44	-85.19
Missing loans	-28.91	-13.23
Year	2015	2015
Binwidth	1	2
Window	[90 - 95]	[90 - 95]

Notes: Standard errors in parentheses obtained through bootstrap, stratified by year, 100 reps. 95% confidence intervals in brackets.

7 Difference in differences

7.1 Research design

In a second approach to evaluating the impact of a limit on LTV ratios, we follow Aastveit et al. (2022) and van Bekkum et al. (2022) in conducting a difference-in-differences analysis where we examine how borrowers predicted to have high LTV ratios react to the reform.

First, we fit a predictive model for LTV ratios to data on first-time buyers in pre-reform years (2010–2015) using the household head's age, family type and size, highest education level and socio-economic status, second degree polynomial of the household's capital income and lagged capital income, disposable income percentile and lagged non-mortgage debt.[17] In addition, the model accounts for a linear time trend and fixed effects for 1km by 1km grid cells.

$$LTV_{i,t} = \alpha + \beta X_{it} + \epsilon_{i,t}, \tag{8}$$

where X_{it} consists of the aforementioned predictor variables.

Then, we estimate the effect of the policy (δ) on a range of outcomes ($y_{i,t}$) including LTV and its components, from the following DiD regression specification for the first-time buyers between 2014 and 2018 (excluding 2016):

$$y_{i,t} = \alpha + \lambda_t + \delta \widehat{LTV}_i^{high} \times I_t^{post} + \gamma \widehat{LTV}_i^{high} + \zeta X_i + \epsilon_{i,t}, \tag{9}$$

where $\widehat{LTV}_i^{high} := \begin{cases} 1 & \text{if } \widehat{LTV}_i > 95 \text{ ,} \\ 0 & \text{if } \widehat{LTV}_i \leq 95 \text{ .} \end{cases}$ and $I_t^{post} := \begin{cases} 1 & \text{if } t > 2016 \text{ ,} \\ 0 & \text{if } t < 2016 \text{ .} \end{cases}$

Predicting within the pre-reform sample, 64% of first-time buyers are correctly predicted to be below or above the LTV limit. This means that the treatment status is measured with error. The measurement error in the treatment status implies that our DiD estimates are biased towards zero. In other words, we estimate an Intent to Treat (ITT) parameter. An approximate Average Treatment Effect on the Treated estimate (ATT) can be obtained by rescaling the ITT estimates with estimates of the share of compliers in our treament and control groups.

Following Bloom (1984), $ATT = ITT/($Compliance rate in treatment group $-$ Compliance rate in control group). We do not know the true compliance rates in the post-reform period but we can use within sample predictions with pre-treatment data to get rough estimates. The estimated compliance rate is 70.4% in the treatment group and 28.3% in the treatment group. This suggests that ATT = ITT/(0.704 - 0.283 =

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[17]Family type is a categorical variable distinguishing between singles, married and cohabiting couples, with or without children. Socio-economic status is a variable related to occupation.

2.37*ITT). We report the ITT estimates in the tables, but note that ATT is likely more than double the size of ITT.

7.1.1 Main results

The results of the analysis are presented in Table 7. The top panel shows results for a linear specification and the bottom panel the results for a log-linear specification, facilitating interpretation. As before we find a negative effect on LTV ratios suggesting the reform reduced leverage. In addition, column 2 shows that this effect is mainly driven by a reduction in mortgage debt and less so by a reduction in housing consumption (proxied by purchase price in column 3). This is also evident from the effect on surface area (column 4) which is not statistically significant and close to zero. As mortgage debt goes down, we find that interest expenses follow (see columns 5 and 6).

A last interesting finding relates to non-mortgage debt. The result shown in the last column was estimated differently from those in the other columns. Before, non-mortgage debt was included in the prediction of LTV ratios. Now, to estimate the effect of the LTV limit on the take-up of other debt, non-mortgage debt was excluded from the prediction. While most households have no other debt than mortgage debt after the LTV limit was imposed, affected households take up more debt in addition to mortgage debt.[18] This other debt includes consumer loans, which may suggest that as a consequence of being able to borrow less, households eat up more liquidity which they seek to compensate with other debt.

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[18]We do not estimate the log-linear specification for non-mortgage debt due to a significant number of zero observations.

Table 7: DiD analysis: main estimation outcomes

				y			
	LTV	Mortgage	Price	Surface	Interest expenses	Interest rate	Non-mortgage debt
$\widehat{LTV}_i^{high} \times I_t^{post}$	-6.55***	-10500.54***	-3798.7*	-0.13	-50.11	-0.5*	1374.56***
	(0.46)	(1555.05)	(1564.65)	(0.52)	(26.85)	(0.02)	(413.27)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Area FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	20081	20081	20081	20025	19245	19245	20081
R^2	0.27	0.74	0.78	0.64	0.46	0.56	0.54
Adj. R^2	0.13	0.69	0.74	0.57	0.35	0.47	0.46
				log(y)			
	LTV	Mortgage	Price	Surface	Interest expenses	Interest rate	
$\widehat{LTV}_i^{high} \times I_t^{post}$	-0.07***	-0.08***	-0.01	-0.01	-0.06***	0.02	
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	
Area FE	Yes	Yes	Yes	Yes	Yes	Yes	
N	20081	20081	20081	20025	19174	19174	
R^2	0.26	0.75	0.82	0.64	0.41	0.41	
Adj. R^2	0.12	0.71	0.78	0.58	0.30	0.33	

Notes: Results from estimating equation (9) with dependent variable LTV, mortgage, house price, surface area, interest expenses and imputed interest rate. Amounts are real values expressed in 2016 euros. $\widehat{LTV}_i^{high} = 1$ if $\widehat{LTV}_i > 95$ and zero otherwise. Controls include year and grid cell fixed effects as well as age, family size and type, education, socio-economic status, 2nd degree capital income and lagged capital income polynomial, disposable income percentile and lagged non-mortgage debt (except for column 6). Standard errors in parentheses bootstrapped with 200 iterations over the two-stage estimation procedure. Significance is denoted by asterisks: * $P < 0.05$, ** $P < 0.01$ and *** $P < 0.001$

7.1.2 Heterogeneity

As before, it may be interesting to split the sample into low and high income groups. For this part of the analysis, prediction of LTV_i^{high} is still based on data for the whole sample but to accommodate bootstrapping and estimation of the effect for the two income groups, income percentiles are replaced with a second degree income polynomial and capital income is dropped from the estimation as there are few non-zero observations for low-income households.

The results in Table 8 confirm earlier findings that the reform had a negative impact on LTV ratios for both types of households. In absolute numbers, the estimated effects are quite similar across all outcomes, with magnitudes close to those reported in Table 7. The bottom line in each sub-table shows the pre-reform average for the borrowers predicted to have high LTV's. From this we can derive the relative effects, which reveal more negative effects on mortgage debt and interest expenses for low-income households (see also Table A2 in Appendix A.2).

Table 8: DiD analysis: effects by income group

	Below median disposable income					
	LTV	Mortgage	Price	Surface	Interest expenses	Interest rate
$\widehat{LTV}_i^{high} \times I_t^{post}$	-6.08***	-7922.41***	-2577.61	0.75	-1.61	-0.00
	(1.02)	(2329.07)	(2283.29)	(0.84)	(41.94)	(0.03)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Area FE	Yes	Yes	Yes	Yes	Yes	Yes
N	8834	8834	8834	8813	8345	8345
R^2	0.38	0.74	0.78	0.71	0.57	0.59
Adj. R^2	0.13	0.63	0.69	0.60	0.39	0.42
Baseline average	99.47	113358	118698	65.90	1951.52	1.75
	Above median disposable income					
	LTV	Mortgage	Price	Surface	Interest expenses	Interest rate
$\widehat{LTV}_i^{high} \times I_t^{post}$	-6.48***	-12506.89***	-5282.72*	-1.64*	-82.78	-0.05*
	(0.67)	(2206.24)	(2281.29)	(0.70)	(45.34)	(0.02)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Area FE	Yes	Yes	Yes	Yes	Yes	Yes
N	11247	11247	11247	11212	10900	10900
R^2	0.31	0.71	0.76	0.65	0.46	0.57
Adj. R^2	0.13	0.64	0.70	0.56	0.32	0.46
Baseline average	98.33	158130	166976	69.75	2295.44	1.52

Notes: Results from estimating equation (9) with dependent variable LTV, mortgage, house price, surface area, interest expenses and imputed interest rate. Amounts are real values expressed in 2016 euros. $\widehat{LTV}_i^{high} = 1$ if $\widehat{LTV}_i > 95$ and zero otherwise. Controls include year and grid cell fixed effects as well as age, family type, education, socio-economic status, 2nd degree polynomial of lagged and current disposable income and non-mortgage debt. Standard errors in parentheses bootstrapped with 200 iterations over the two-stage estimation procedure. Significance is denoted by asterisks: * $P < 0.05$, ** $P < 0.01$ and *** $P < 0.001$

8   Conclusions

The LTV regulation implemented in Finland in 2016 led to substantial short-run responses by banks and home buyers. In the analysis, we focused on first-time buyers and transactions in multi-unit buildings. We find that the LTV limit substantially reduced risky leverage measured in terms of the LTV ratio. This happened both on the extensive and intensive margin.

The extensive margin response is almost fully driven by less transitions from renting to owning for households with lower than median income. Some 30% of the potentially affected low-income borrowers withdraw from the owner-occupied housing market because of bigger down payment needed to obtain a mortgage. For households with higher income levels, the extensive margin effect is close to zero. These results pertain to two years right after the regulation was implemented. Given our data, we are not able to distinguish between a persistent effect on the transition into homeownership

and a more transitory delay in borrowing that allows households to save to meet the new borrowing constraint. However, the fact that the extensive margin effect does not diminish from 2017 to 2018 suggests that the LTV regulation can have longer run implications for homeownership.

The large extensive margin effect on among less well-off homebuyers means that LTV regulation likely increases economic inequality. Homeownership is subsidised through the tax code annually by roughly 1500 euros per person in homeowner households, and the LTV limit implies that the benefits of this subsidy are more strongly concentrated among high income households.

Our analysis on the intensive margin effects indicates that reduction in LTV ratios is quite similar for high-income and low-income households. The reduction in LTV ratios is mainly driven by reduced mortgage. This suggests that households used their liquid assets to meet the new down payment requirement. Consistently with this, we find evidence that the LTV limit increased the take-up of non-mortgage debt.

In general, the evidence on macroprudential measures and household responses highlight the complexity of the regulation in the housing market. The results indicate that borrower-based regulation affects both extensive and intensive margin decisions. But the margins of adjustment seem to vary and the quantitative importance of different channels depends on the institutional details. For instance, the tax advantages of owner-housing relative to renting and financial assets influence the incentive to transition from renting to owning. Likewise, the degree of mortgage interest deductibility influences the incentives for debt vs. equity financing of owner-housing.

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Appendix

A Appendix

A.1 Data definitions

The data used in this report are obtained from Statistics Finland. Two of the datasets are ready-made research data available as so-called FOLK-modules, covering households' debt and income statistics and individual socio-economic characteristics, see links and descriptions in table A1. In addition, we requested three custom-made datasets which include dwelling characteristics, links between dwellings and occupants and mortgage debt data at the individual level. The main outcome variables of interest are leverage and income, aggregated over members of a household.

Regarding leverage, we distinguish between a household's total level of debt and debt from housing loans. In addition to housing loans, total debt consists of debts from trade and business activities (except between 2007 and 2013), debts relating to agriculture, debts from business groups (until 2005) and other liabilities, including student loans and consumer credit (from 2012 onward the latter only consists of consumer loans and no longer includes continuous credit). Due to the inconsistency in some series of debt categories we focus mainly on mortgage debt in this report. Unless explicitly mentioned, mortgage debt only includes debt held by the household, not debt held by the housing cooperative, the main reason being that we only have this data since 2006. More information on Statistics Finland's debt data can be found here, here and in the data description referenced in table A1.

Regarding income, we calculate gross income as the sum of entrepreneurial income, property income, earned income and income from current transfers received. Entrepreneurial income aggregates agricultural and forestry entrepreneurial income (MATU and METU), entrepreneurs' entrepreneurial income (MUUYRTU), business groups' entrepreneurial income (YHTYRTU) and entrepreneurial income from copyrights, patents etc. Property income includes rental, interest and dividend income and pensions based on private insurance. Transfers received cover earnings-related and national pensions and other social security benefits and social allowances (child benefits, housing allowances etc.). Finally, when we compare households across income quartiles, we adjust for household size using the OECD's adjusted consumption unit scale, made available by Statistics Finland. In doing so we take into account shared consumption benefits (economies of scale in consumption), see this page for more details on this adjustment. More information on Statistics Finland's gross income data and related concepts can be found here and in the data description referenced in table A1.

Table A1: Dataset descriptions and sources

Name	Short description	Source
Askun	FOLK dataset on household level debt and income	Statistics Finland (more info here)
Perus	FOLK basic dataset with individuals' socio-economic characteristics	Statistics Finland (more info here)
Asunto	Dataset on dwelling locations, characteristics, and mortgage debt	Statistics Finland (custom-made)
Henkilo paikkatiedot	Dataset connecting dwellings to their respective occupants	Statistics Finland (custom-made)
Henkilo asuntovelat	Individual mortgage debt dataset	Statistics Finland (custom-made)
Varainsiirtovero	Transfer tax dataset containing purchase prices and cooperative-held debt for apartments since 2006	Statistics Finland (custom-made)

A.2 Results appendix

Table A2: DiD analysis: effects by income group, log-linear regression output

	Below median disposable income					
	LTV	Mortgage	Price	Surface	Interest expenses	Interest rate
$\widehat{LTV}_i^{high} \times I_t^{post}$	-0.07***	-0.07***	-0.01	0.01	-0.05	0.03
	(0.01)	(0.02)	(0.02)	(0.01)	(0.03)	(0.02)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Area FE	Yes	Yes	Yes	Yes	Yes	Yes
N	8834	8834	8834	8813	8311	8311
R^2	0.37	0.75	0.81	0.70	0.54	0.50
Adj. R^2	0.12	0.64	0.73	0.58	0.34	0.29
Baseline average	99.47	113358	118698	65.90	1951.52	1.75
	Above median disposable income					
	LTV	Mortgage	Price	Surface	Interest expenses	Interest rate
$\widehat{LTV}_i^{high} \times I_t^{post}$	-0.07***	-0.08***	-0.02	-0.03**	-0.05	0.04
	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.03)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Area FE	Yes	Yes	Yes	Yes	Yes	Yes
N	11247	11247	11247	11212	10863	10863
R^2	0.30	0.74	0.81	0.65	0.40	0.43
Adj. R^2	0.12	0.67	0.76	0.56	0.24	0.28
Baseline average	98.33	158130	166976	69.75	2295.44	1.52

Notes: Results from estimating equation (9) with logged dependent variable LTV, mortgage, house price, surface area, interest expenses and imputed interest rate. Amounts are real values expressed in 2016 euros. $\widehat{LTV}_i^{high} = 1$ if $\widehat{LTV}_i > 95$ and zero otherwise. Controls include year and grid cell fixed effects as well as age, family type, education, socio-economic status, 2nd degree polynomial of lagged and current disposable income and non-mortgage debt. Standard errors in parentheses bootstrapped with 200 iterations over the two-stage estimation procedure. Significance is denoted by asterisks: * $P < 0.05$, ** $P < 0.01$ and *** $P < 0.001$