Down Payments, Deferred Homes: How LTV Restrictions Reshape Household Consumption

Yann Cerasi
Swiss Finance Institute
University of Zurich

Gazi Kabaş
Tilburg University

Steven Ongena
Swiss Finance Institute
University of Zurich

Kasper Roszbach
Norges Bank
Lund University

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Abstract

We study how households adjust consumption following the introduction of a loan-to-value (LTV) restriction using population-wide electronic expenditure records and administrative data. We find that renters increase consumption as a result of the policy, at odds with the policy's objective to further financial stability through greater down payments and household savings. As mandatory down payments make home purchases more difficult, prospective buyers delay on both home-ownership and the savings required for a down payment. This effect is robust to within-renter comparisons and is strongest in regions where the LTV restriction binds more. In contrast, households that purchase under the constraint reduce expenses before buying and keep them persistently lower thereafter. Our findings show that borrower-based macroprudential policies can reshape intertemporal behavior in unintended ways, with important implications for home ownership, aggregate demand, and the transmission of housing regulation to the real economy.

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Ongena is also affiliated with KU Leuven, NTNU Business School, and CEPR. Cerasi: yannjoel.cerasi@df.uzh.ch. Kabaş: g.kabas@tilburguniversity.edu. Roszbach: kasper.roszbach@norges-bank.no. Steven Ongena: steven.ongena@df.uzh.ch The authors would like to thank Manuel Adelino, Francisco Amaral, Martin Brown, Kristoph Kleiner, Winfried Koeniger, Vincenzo Pezone, and Ramana Nanda, as well as the participants at the Banca d'Italia/Collegio Carlo Alberto/Norges Bank conference on "Monetary Policy and Heterogeneity in Households, Firms, and Financial Intermediaries", Ghent University's Empirical Macroeconomics Workshop, the Liverpool Workshop on Micro Data and Macro Policy, Norges Bank Housing Workshop, Swiss Society for Financial Market Research, Swiss Economists Abroad Conference, Tilburg University, University of Groningen, Central Bank of Ireland, HYRCE Liège and University of Zurich for their helpful conversations and comments. The authors thank Sigurd Holmsen for his excellent research assistance. The authors gratefully acknowledge financial support from the Swiss National Science Foundation. This 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.

1 Introduction

Wealth effects and borrowing constraints create a powerful link between the housing market and aggregate consumption. When house prices fall, collateral values decline, collateral-based borrowing capacity tightens, and constrained households reduce spending disproportionately. This mechanism amplifies downturns and deepens recessions in models with collateral constraints and borrowing limits tied to asset values, and is central to leading explanations of business-cycle fluctuations and credit-driven booms (Bernanke and Gertler, 1986; Kiyotaki and Moore, 1997; Iacoviello, 2005). Because this amplification channel is stronger when household leverage is high, countries around the world expanded the use of borrower-based macroprudential tools in the aftermath of the Global Financial Crisis, such as restrictions on loan-to-value (LTV) and debt-to-income (DTI) ratios, to restrain excessive leverage and strengthen financial stability (Cerutti et al., 2017; Alam et al., 2019). By construction, these policies limit borrowing against housing collateral and are intended to reduce leverage by requiring households to finance a larger share of home purchases out of their own resources, which, in turn, implies higher saving and lower consumption.

Despite their central role in macroeconomic policy, however, there is no consensus in the theoretical literature on how such borrowing restrictions will affect the consumption-saving decisions as well as timing of prospective homebuyers. Consistent with the policy objectives, in standard life-cycle models with binding borrowing constraints, tighter borrowing limits require renters to increase their savings to meet down-payment requirements, implying a reduction in consumption prior to purchase (Artle and Varaiya, 1978; Slemrod, 1982; Jappelli and Pagano, 1994). In contrast, models that emphasize consumption smoothing and endogenous housing tenure choice predict that tighter constraints may instead delay both home purchase and the start of targeted saving, thereby increasing current consumption (Balke et al., 2024). These opposing predictions imply fundamentally different macroeconomic consequences of borrower-based restrictions: while the former mechanism reinforces the contractionary effects of credit tightening, the latter may attenuate or even temporarily reverse the impact on aggregate demand. Yet, empirical evidence remains scarce, in part because studying these dynamics requires jointly observing consumption, saving and housing transactions at high frequency to avoid measurement error in imputing consumption around home purchase events.

This paper provides new evidence on how borrower-based mortgage restrictions affect consumption dynamics and savings behavior over the life cycle by combining debit card and electronic payments data with tax records and administrative registers for the entire

Norwegian population. We exploit the introduction of an LTV restriction as a macroprudential borrowing restriction and study how renters are affected by tighter down-payment requirements when planning to enter the housing market.

Our main finding is that renters increase consumption following the introduction of the borrowing constraint, contrary to the standard life-cycle prediction. Quantitatively, the *consumption/income* ratio rises by 2.76 percentage points, corresponding to a 4 percent increase relative to the sample mean. Consistent with models in which tighter constraints delay both home purchase and the start of targeted saving, we find that renters postpone entry into homeownership and defer the accumulation of down-payment savings. The increase in consumption is strongest among renters for whom the borrowing constraint is most binding and among households with higher expected income growth, further supporting this interpretation. In contrast, renters who transition into homeownership under the LTV constraint exhibit the opposite pattern: they reduce consumption prior to purchase to meet the higher down-payment requirement and maintain lower consumption thereafter. These findings show that borrower-based macroprudential regulation can have fundamentally different effects on consumption across households and over time. As a result, policies designed to curb leverage and dampen demand may instead increase consumption among marginal renters by delaying both home purchase and saving, thereby altering, and potentially weakening, the intended transmission of these policies to aggregate demand.

We measure household consumption by using electronic payment data that is available for the entire Norwegian population. Ahn et al. (2024) document that, thanks to payments via banks being the dominant payment method in Norway, this data covers 80 percent of all card-based transactions. In addition, it is highly correlated with total consumption in national accounts, even within subcategories, and has similar coverage rates across different age groups, mitigating concerns about possible selection issues. We supplement our consumption data with housing transaction data and several population registers and tax filings, enabling us to observe housing tenure, household balance sheet items, and individual characteristics.

We exploit our rich data in a difference-in-differences design, where the introduction of the LTV ratio restriction serves as the treatment. The restriction is a result of financial stability concerns driven by strong growth in home prices and household debt levels in the years following the GFC. The Norwegian government started to implement the restriction in the fall of 2010, with a threshold set at 90 percent.[1] As the borrowing restriction is only applied to all new mortgages and leaves existing mortgages unaffected, we use renters as the

[1]The threshold is reduced to 85 percent starting in 2012. We provide the details of the LTV ratio restriction in Section 2.

treatment group and homeowners who purchased their homes at least five years before the introduction of the restriction as the control group.

To identify the effect of the borrowing restriction on renters' consumption, we need to assume that renters and homeowners would have followed the same trend over time in the absence of the restriction and that the restriction does not affect homeowners. We assess this identifying assumption with a battery of tests. First, we visually show that renters and homeowners exhibit parallel trends in their consumption before the introduction of the restriction. Second, we control for differences in observable characteristics between renters and homeowners by including granular fixed effects, such as age×year or debt bins×year. Our results remain robust when we interact all of the relevant fixed effects, comparing renters and homeowners with similar balance sheets and demographic characteristics.[2] Third, considering that the restriction may limit the borrowing capacity of homeowners when purchasing an additional home or obtaining a second mortgage, we perform a heterogeneity test based on homeowners' current LTV ratios. If our estimates were driven by constrained homeowners, removing those with high current LTV ratios should attenuate the effect. Yet, the estimates remain virtually unchanged when such homeowners are gradually excluded from the sample. Fourth, we address the possibility of a housing wealth effect by including municipality×year fixed effects, which absorb heterogeneity in local housing market conditions and location-specific house price expectations. Finally, we perform a within-renter comparison in which the housing wealth channel cannot play a role because both the treatment and control groups consist of renters. To construct this design, we first predict renters' probability of becoming homeowners using characteristics as of 2006 and then compare renters with lower predicted homeownership probabilities, who are more likely to be affected by the restriction, to renters with high predicted probabilities who appear to have sufficient ownership capacity even before the policy change. The results from this within-renter comparison confirm our main findings.

After addressing concerns about identification, we examine the underlying mechanism. Our findings suggest that, due to the LTV ratio restriction, renters delay saving and postpone home purchases, although macroprudential borrowing restrictions are intended to reduce leverage and therefore increase household savings. The mechanism begins with the observation that tightening down payment requirements raises the savings required to purchase a home. Accumulating this additional down payment would require a costly deviation from consumption smoothing. As a result, renters optimally delay both saving and home purchases until their income rises, allowing them to accumulate the required down payment

[2]Namely, we include age×deposit bins×financial asset bins×total wealth bins×parental wealth bins×year fixed effects.

with a smaller reduction in consumption (Balke et al., 2024). Supporting this mechanism, we document that the age of first-time homebuyers increases after the introduction of the restriction. Importantly, the age distribution of first-time buyers is stable prior to the policy, indicating that the subsequent increase reflects the introduction of the LTV restriction rather than pre-existing trends. To further examine the role of expected income growth in this decision, we construct an earnings potential gap, defined as the difference between a household's current income and the average income of the top decile within cells defined by education, labor market region, and immigration background. This measure captures how far a household is from the upper end of the income distribution among comparable individuals and therefore proxies for the scope for future income growth. Including income bin×year and cell×year fixed effects, the identifying variation in this exercise comes from differences in earnings potential among households with similar current income and within the same education–region–immigration cell. Consistent with the proposed mechanism, the increase in consumption is strongest for renters with larger earnings potential gaps and is insignificant for renters in the top decile, suggesting that households with greater expected income growth are more likely to postpone saving and maintain higher consumption.

Additional heterogeneity tests reinforce this interpretation. First, when we examine heterogeneity with respect to income levels, we find an inverted U-shaped relationship between income and the treatment effect. The consumption response is small for low-income renters, who are unlikely to become homeowners, and for high-income renters, who can more easily satisfy down payment requirements. The effect is largest for middle-income renters, who are the most likely prospective homebuyers but are also the most exposed to borrowing constraints, making the LTV restriction particularly binding for them. Second, exploiting regional variation in borrowing conditions, we show that the consumption response is stronger in municipalities with higher pre-restriction LTV ratios, where the restriction is more likely to bind. To further characterize the nature of the consumption response, we classify consumption categories according to their expenditure elasticities, estimated from category-specific Engel curves and described in detail in the appendix. The increase in renters' consumption is concentrated in more elastic categories, indicating that the response reflects greater discretionary spending rather than only higher necessary expenditures. We also use labor-market outcomes to distinguish delayed saving from a giving-up response in which renters abandon the goal of homeownership and reduce effort. The evidence points against this interpretation: renters become more likely to earn labor income, less likely to receive unemployment benefits, and more likely to receive performance-related compensation, suggesting that the increase in consumption is not accompanied by weaker labor-market attachment. We further show that renters become less likely to relocate after the restriction,

consistent with delayed transitions into homeownership. Although they remain slightly more likely to live in higher-income municipalities, the implied effect of local price differences explains only a small share of the consumption increase, indicating that the response is driven primarily by delayed home purchases and saving rather than locational sorting.

We provide additional evidence on how the LTV restriction affects renters' broader financial behavior and the components underlying the consumption response. First, we examine how other balance sheet items evolve following the introduction of the LTV restriction. Consistent with increased consumption, renters exhibit higher debt-to-income ratios and lower deposit and financial asset holdings than homeowners, resulting in lower net savings. These findings suggest that the increase in consumption is accompanied by adjustments in other parts of renters' balance sheets, indicating that households partly finance their higher consumption through a combination of additional borrowing and reductions in liquid asset holdings. Second, we examine whether the increase in the consumption-to-income ratio is driven primarily by consumption or income. Estimating specifications with the logarithm of consumption and the logarithm of income as dependent variables reveals that both consumption and income increase for renters after the restriction. However, the increase in consumption is considerably larger, implying that the rise in the ratio is primarily driven by higher spending rather than a decline in labor supply. Finally, we conduct a range of robustness checks to address concerns that differential exposure to macroeconomic conditions between renters and homeowners may drive our results. Allowing renters' consumption to respond differently to changes in GDP growth, inflation, monetary policy rates, financial market volatility, and unemployment leaves the estimated effect of the LTV restriction virtually unchanged, suggesting that differential responses to macroeconomic shocks cannot explain the observed increase in renters' consumption.

How do renters who purchase a home under a borrowing constraint adjust their consumption? To answer this question, we analyze households that transition from renting to homeownership after the introduction of the LTV restriction, whom we refer to as homebuyers. Studying consumption around a home purchase presents an empirical challenge because purchasing a home itself generates substantial consumption responses (Benmelech et al., 2023). To isolate the effect of the borrowing restriction from the direct effect of homeownership, we use a design similar to a triple-differences design. Specifically, we compare households that purchase a home after the introduction of the restriction in 2013 with a cohort that purchased their homes in 2009, before the policy was implemented. The time difference between the LTV restriction and 2013 allows us to observe consumption dynamics before the home purchase under a borrowing constraint. The earlier cohort captures the

baseline consumption dynamics associated with home purchases in the absence of the restriction. We account for selection effects that the LTV restriction induces by performing a matching between the 2013 and 2009 homebuyer cohorts based on characteristics associated with saving capacity, including income, parental wealth, debt, and demographics. To control for aggregate time effects, we further use homeowners as an additional control group. This design allows us to identify how the borrowing restriction alters consumption behavior around the home purchase while controlling for both the mechanical consumption response to homeownership and common macroeconomic shocks.

Our results show that homebuyers under the restriction begin adjusting their consumption well before the home purchase. After the implementation of the LTV restriction, the 2013 cohort reduces consumption relative to the purchase year significantly more than the 2009 cohort in the three years preceding the purchase, indicating that these households increase savings to accumulate the higher down payment required by the restriction. After the purchase, the 2013 cohort continues to exhibit significantly lower consumption, even before accounting for mortgage payments; incorporating mortgage payments into household cash outflows leaves this conclusion unchanged. This persistence suggests that households purchasing under the restriction do not use the lower mortgage payments generated by larger down payments to raise consumption. Instead, post-purchase balance sheet evidence points to a liquidity-buffer motive: relative to the 2009 cohort, the 2013 cohort rebuilds deposits and financial assets more rapidly after purchase. We find no corresponding evidence of faster repeat purchases, greater mobility, or sorting into systematically cheaper municipalities, suggesting that lower post-purchase consumption primarily reflects the rebuilding of liquid wealth rather than housing-ladder adjustment or location sorting. Overall, these findings suggest that macroprudential borrowing restrictions influence not only leverage at the time of purchase but also the intertemporal consumption behavior of homebuyers, with effects that persist beyond the home purchase itself.

Literature review Our paper builds mainly on three strands of the literature. The first one studies how housing markets affect household consumption. House prices can influence spending either by changing household wealth directly or by relaxing collateral constraints, and empirical estimates of these effects are economically meaningful, typically ranging from roughly 0.05 to 0.30 (e.g., Campbell and Cocco, 2007; Mian et al., 2013; Aladangady, 2017; Guren et al., 2021).[3] A more recent literature shows that housing transactions themselves

[3]Related work emphasizes the collateral channel more directly by showing that higher home equity expands borrowing capacity and debt extraction (Mian and Sufi, 2011; DeFusco, 2018), while recent theory shows that large house-price effects on consumption are consistent with incomplete-markets models with leverage and illiquid housing (Guerrieri and Iacoviello, 2017; Berger et al., 2018; Kaplan et al., 2020). Also,

are an important driver of expenditure dynamics: home purchases induce sizable increases in spending on home-related durables, renovations, and repairs, implying that transaction volume is itself a distinct source of aggregate demand variation (Best and Kleven, 2018; Benmelech et al., 2023; Favara et al., 2025).[4] We contribute by showing that borrower-based mortgage regulation changes the consumption dynamics around home purchase, implying that the spending profile around purchase is not policy-invariant but depends on the financing regime governing access to mortgage credit. This is important because the housing–consumption literature increasingly emphasizes the role of housing-market activity for aggregate demand; our results show that this link depends not only on house prices and transaction volume, but also on the credit constraints faced by prospective buyers.

The second one is about the borrower-based macroprudential policies that have gained prominence in recent years. Theory suggests that such policies can mitigate externalities associated with excessive leverage, collateral constraints, and aggregate-demand spillovers (Farhi and Werning, 2016; Korinek and Simsek, 2016; Dávila and Korinek, 2018). Empirically, a growing body of literature documents that these policies enhance financial stability by reducing leverage, curbing risky lending, and lowering default risk (Cerutti et al., 2017; Peydró et al., 2024). At the same time, they generate unintended effects on household behavior, including lower transition rates into homeownership and changes in housing choices, such as shifts toward cheaper or lower-quality properties (Acharya et al., 2022; Carozzi, 2020; Tzur-Ilan, 2023; Bolliger et al., 2025; Kvaerner et al., 2024; Tracey and Van Horen, 2025; Hintermaier et al., 2026; Coulier et al., 2026). This literature has also emphasized a potential trade-off between lower leverage and reduced liquidity: tighter borrowing constraints require higher down payments, which may deplete households' liquid assets and weaken their resilience to shocks (Aastveit et al., 2024; Bekkum et al., 2024). We contribute to this literature along two dimensions. First, we show that the effects of borrower-based regulation extend beyond realized homebuyers to prospective buyers who remain renters. By delaying both home purchase and the accumulation of down-payment savings, tighter borrowing constraints increase renters' consumption, implying an aggregate-demand channel of macroprudential policy that has been largely overlooked in the existing literature. Second, we refine the leverage–liquidity trade-off by showing that the adjustment to higher down-payment requirements does not operate solely through the depletion of pre-existing liquid assets. Instead, affected homebuyers actively reduce consumption prior to purchase and maintain lower cash outflows afterward, suggesting that higher down payments are financed

see Agarwal et al. (2017); Di Maggio et al. (2017); Best et al. (2020); Agarwal et al. (2023) for the role of mortgages for consumption.

[4]Closely related work by Sodini et al. (2023) shows that homeownership itself affects consumption and household welfare by altering balance sheets and access to collateral

in part through intertemporal spending adjustment.

Third, we contribute to the discussions about the central role of housing markets in business-cycle fluctuations. Models with collateral constraints show that movements in real estate prices affect credit availability, as real estate serves as collateral for households, and amplify macroeconomic fluctuations (Bernanke and Gertler, 1986; Bernanke et al., 1999; Kiyotaki and Moore, 1997; Iacoviello, 2005; Liu et al., 2013), while a large empirical literature documents that increases in household leverage are associated with subsequent declines in consumption and deeper recessions (Schularick and Taylor, 2012; Mian et al., 2013, 2017; Garriga and Hedlund, 2020).[5] In this literature, tightening borrowing constraints typically arise from adverse shocks, such as declines in house prices—that reduce collateral values and force households to deleverage, leading to a contraction in consumption (Eggertsson and Krugman, 2012; Mian and Sufi, 2014; Guerrieri and Lorenzoni, 2017; Justiniano et al., 2017). A key question is how households respond to tighter borrowing constraints when the tightening is policy-driven rather than induced by negative shocks. In standard life-cycle models with binding down-payment requirements, tighter borrowing limits can induce households to increase savings and reduce consumption in order to meet the constraint, in line with the policy's aim (Artle and Varaiya, 1978; Slemrod, 1982; Jappelli and Pagano, 1994).[6] In contrast, models that emphasize consumption smoothing and endogenous tenure choice predict that tighter constraints may delay both home purchases and the start of saving, thereby increasing current consumption (Balke et al., 2024).[7] We contribute by providing empirical evidence on this margin. Our results show that renters delay their home purchases and increase consumption when borrowing constraints tighten, consistent with a delay in saving, while households who proceed to buy compress consumption around purchase. These findings reconcile competing theoretical predictions and provide new evidence on how households adjust their behavior under policy-induced borrowing constraints, with important implications for the macroeconomic effects of credit-market regulation.

The rest of the paper proceeds as follows. Section 2 provides relevant background information and describes our main data sources. Section 3 explains our empirical strategy. Section 4 provides our results with robustness checks and evidence regarding the underlying mechanism. Section 5 concludes.

[5]Several studies refine key facts about the distribution of mortgage borrowing and defaults across households during the boom and bust, see Mian and Sufi (2009, 2011); Adelino et al. (2016, 2018); Foote et al. (2021).

[6]In Chopra et al. (2025), exogenous shifts in long-run home-price expectations affect renters' spending but do not significantly shift reported expectations about the timing of future purchases, suggesting muted timing adjustment in that setting.

[7]Also see Stein (1995); Ortalo-Magne and Rady (2006) for how down payment constraints shape housing transitions by delaying entry and mobility when households cannot finance the required equity share.

2 Background information and data

In this section, we provide background information about the housing market and macroeconomic conditions in Norway. Then, we explain our data sources and main variables.

Housing market Norway has high homeownership rates, with approximately 80 percent of households owning their primary residence. Several institutional features contribute to this pattern. Mortgage interest payments are tax deductible, and capital gains on primary residences are exempt from taxation if the owner has lived in the property for at least one year. Property is taxed at the municipal level, and households are also subject to a wealth tax. For wealth tax purposes, the taxable value of the primary residence is assessed at approximately 25 percent of its market value, whereas secondary housing is assessed at higher rates. In contrast, other asset classes, such as deposits and equities, do not receive similar discounts.

The mortgage market in Norway is dominated by adjustable-rate mortgages.[8]. Borrowers typically obtain amortizing loans with maturities of 20–30 years. Prior to the introduction of the loan-to-value (LTV) restriction, mortgage lending standards were largely determined by banks' discretion, allowing households to finance housing purchases with relatively high leverage. Unlike in countries with extensive rent control systems, such as Sweden, rents in Norway are largely determined by market conditions. Most rental units are supplied by private households, typically through the rental of secondary dwellings or parts of owner-occupied homes, while institutional landlords play a relatively small role outside major cities. Rental contracts typically last between one and three years, and although rents for sitting tenants can only be adjusted once per year in line with the consumer price index, rents are typically reset to market levels when a new contract is signed. Overall, the Norwegian rental market exhibits relatively high mobility and flexibility.

Macroeconomic conditions Norway experienced strong house price growth in the years preceding the Global Financial Crisis (GFC), followed by a temporary correction in 2008 when house prices declined by approximately 9%. The Norwegian economy was only mildly affected by the crisis and recovered relatively quickly compared to many other advanced economies. For example, GDP per capita declined slightly in 2009 and 2010 but had already surpassed its 2008 level by 2011. The policy rate set by Norges Bank increased from around 2% in 2006 to above 5% in 2008 before being sharply reduced in response to the financial crisis and stabilizing at low levels thereafter. The only other notable macroeconomic shock occurred in 2014, when oil prices fell sharply, disproportionately affecting regions with strong

[8]Fixed-rate mortgages are also available

exposure to the petroleum sector. Overall, macroeconomic conditions remained relatively stable during our sample period.

Data sources Our analysis combines several administrative datasets that jointly cover the universe of Norwegian residents. The data are collected by the Norwegian tax authorities and maintained by Statistics Norway, and our analysis uses the period 2006–2018.[9] Thanks to the fact that a substantial share of the underlying information is reported directly by third parties, in particular financial institutions, and pre-filled in tax records, the data quality is high. In addition to the standard population and tax registers, which provide rich information on household balance sheets and individual characteristics, we use two additional sources that are central to our analysis: detailed electronic payment-based consumption data and housing transaction records.

The population and tax registers allow us to observe demographic characteristics, such as age, immigration status and education, household composition, income, deposits, financial assets, housing wealth, and liabilities, while the housing transaction registry records the universe of housing transactions and contains information on transaction dates, prices, location, housing characteristics, and the identities of buyers and sellers. Using this information, we can identify homeowners, renters, and first-time homebuyers and observe housing purchases and sales over time. The housing transaction registry is linked to the administrative registers through the same anonymized identifiers. The data are therefore particularly useful for studying housing decisions of households over time.

Our consumption measure is constructed from the Norwegian electronic payments database and also covers the entire Norwegian population (Ahn et al., 2024). The underlying sources are debit card transactions and bank wire transfers, including invoice payments and direct transfers. Credit card payments are observed as a separate category.[10] The database excludes payments made abroad, online payments, mobile payments, and cash transactions. We do not view the omission of cash as a major limitation, however, because Norway is one of the least cash-intensive economies, and cash usage is stable over our sample period. Importantly, the growth rate of our consumption measure is highly correlated with consumption in national accounts, as it captures about 80 percent of card payments in a stable way over time, documented by Ahn et al. (2024). The data are aggregated at the household-year level and classified into consumption types using the Classification of Individual Consumption According to Purpose (COICOP) categories based on shop industry or merchant category

[9] Tax filings data start from 1993.

[10] Because the data record payments to banks rather than the ultimate credit card purchases themselves, we follow the algorithm by Ahn et al. (2024) and remove mortgage payments from payments to banks and impute credit card spending accordingly.

codes; the details of this classification are provided in the appendix.[11] Overall, the comprehensive coverage of the administrative registers and payments data allows us to follow households over time and observe both their housing decisions and consumption behavior without any attrition or selection biases.

Variable construction We classify households into three groups: renters, homeowners, and homebuyers. Renters are households that report no housing wealth in the tax registry and have no recorded housing transactions prior to the introduction of the LTV restriction.[12] This definition excludes individuals who may have received property through inheritance. Homeowners are households that purchased a home before 2006, the first year of our consumption data. These households may transact again during the sample period, allowing us to observe repeat purchases and the ownership of secondary properties. Homebuyers are renters who transition to homeownership during the sample period.[13] To ensure a consistent sample, we exclude self-employed individuals and individuals who appear intermittently in the tax records.

Table 1 reports the summary statistics of our main variables and three types of households. In the renters sample, we report summary statistics from our treated group, which includes renters who purchase after the LTV policy.[14] In the homebuyers column, we report both 2009 and 2013 homebuyers, which we use in our homebuyers exercise in Section 4.4. As expected, homeowners have higher income levels than the other two groups. Renters, on average, consume less than homeowners, which likely reflects the influence of the life cycle effects. The highest debt holdings and the lowest net savings are observed among homebuyers. This is expected, as they are likely to obtain a mortgage to finance their home purchase.

[11] We cannot observe account ownership for accounts closed before 2012, which creates a break in the aggregate level of the raw series. The effect of this is negligible because account closures are mostly driven by death or emigration, hence not creating within-individual breaks. Moreover, we exclude individuals without observed consumption records prior to 2012 from the analysis, as these individuals do not have pre-restriction observations.

[12] An increase in housing wealth in the tax filings without a corresponding transaction may occur due to inheritance. We therefore impose a minimum threshold of 50,000 NOK (in 2010 values) when identifying housing wealth in the tax data.

[13] The tax filings and housing transaction registry may record different dates for housing purchases. In such cases, we rely on the transaction registry.

[14] We exclude renters over 65, for whom we cannot rule out prior homeownership. This affects only a small share of observations.

Table 1: **Summary statistics**

This table provides summary statistics of the main variables.

	Renters Sample		Homeowners		Homebuyers	
	Mean	SD	Mean	SD	Mean	SD
Total Consumption	223,059	152,698	344,320	206,427	307,092	187,895
Durables	35,189	58,968	80,258	97,784	61,558	86,498
Non-Durables	63,173	51,744	90,122	63,328	83,966	56,624
Income	374,073	204,978	526,923	250,524	510,939	239,317
Consumption/Income	0.60	0.27	0.67	0.32	0.61	0.27
Deposits	159,876	330,548	391,522	596,377	205,085	342,892
Financial Assets	197,427	437,278	529,244	815,924	270,223	498,592
Debt	517,330	958,340	959,627	1,080,000	1,330,000	1,220,000
Net Savings	−354,000	1,010,000	−450,000	1,410,000	−1,140,000	1,260,000
Age	41.72	11.80	51.24	15.26	35.38	10.28
Share Immigrant	0.17	0.36	0.08	0.24	0.20	0.37
Years of Education	14.06	3.47	15.41	3.25	16.24	3.30
College (=1)	0.33	0.47	0.46	0.50	0.57	0.50
Obs.	484,520		7,116,669		29,264	

3 Empirical strategy

The goal of this section is to describe our empirical strategy for estimating how a macroprudential borrowing restriction affects the consumption behavior of households that are not homeowners at the time the policy is introduced. The policy we study is the introduction of a loan-to-value (LTV) ratio restriction in Norway. Prior to the Global Financial Crisis (GFC), house prices in Norway increased rapidly, as illustrated in Figure A6, and resumed their upward trajectory shortly after the temporary slowdown during the crisis. This strong growth in house prices was accompanied by rising household leverage, raising concerns among policymakers about risks to financial stability. In response to these developments, Finanstilsynet (the Financial Supervisory Authority of Norway) issued guidelines for new residential mortgages in March 2010. These guidelines stipulated that, starting in the fall of 2010, new mortgages should not exceed a loan-to-value ratio of 90 percent. The guidelines were tightened in December 2011 and implemented in January 2012, lowering the maximum LTV ratio to 85 percent. Banks that failed to comply with the guidelines faced higher capital requirements.[15]

[15]The LTV regulation in Norway allowed for limited deviations in special cases, permitting banks to make discretionary adjustments for a small number of households. As a result, the distribution of observed LTV

We empirically investigate the effects of this LTV ratio restriction on non-homeowner households by estimating the following regression model

$$y_{it} = \beta \; Renter_i \times Post_t + \alpha_i + \alpha_t + \varepsilon_{it} \tag{1}$$

where y_{it} is the dependent variable, such as the consumption-to-income ratio. $Renter_i$ is an indicator equal to one if the household is a renter in 2009 and zero if the household is a homeowner. We define renters as households that did not report any housing transaction or housing value in tax filings prior to 2009, ensuring that these households did not own residential property before the introduction of the restriction.[16] Importantly, we define $Renter_i$ based on households' housing status prior to the policy and keep households in the renter group even if they purchase a home after the introduction of the LTV restriction. This approach avoids conditioning on post-policy housing choices that may themselves be affected by the policy. In particular, the LTV restriction may discourage or delay home purchases for some renters, and defining treatment based on post-policy homeownership would therefore mechanically change the composition of the renter group and introduce selection bias. $Post_t$ is an indicator equal to one for the years following the introduction of the LTV restriction in 2010. The vector of $controls_{it}$ includes household and year fixed effects. We cluster the standard errors at the labor market region level to account for location-specific characteristics of housing markets.[17]

The coefficient of interest in Equation 1 is β. Interpreting β causally requires that, in the absence of the LTV ratio restriction, renters and homeowners would have followed parallel trends in the outcome variables. In addition, homeowners must not be directly affected by the restriction. These assumptions may be violated for three main reasons. First, renters and homeowners differ in observable and unobservable characteristics, and these differences may begin to affect the outcome variables differently around the time of the policy introduction. Second, although the restriction applies only to new mortgages, it may still influence homeowners if it constrains their borrowing capacity when refinancing or when purchasing an additional home. Third, the LTV restriction could affect house prices and thereby influence homeowners' behavior through a housing wealth channel. In Section 4,

ratios still contains some mass above the constraint. Borrowers could also exceed the limit by pledging additional collateral, for example, by using parental property. During 2011–2015, approximately 28% of homebuyers received parental assistance in the form of gifts, loans, early inheritance, or collateral pledged by parents.

[16] Our tax filings and housing transaction data start in 1999.

[17] Labor market regions are defined by Statistics Norway as functional economic areas with a common labor market, where inhabitants can commute to work without relocating. There are 47 labor market regions in our data.

we conduct a series of tests to assess these potential concerns and find that our results are unlikely to be driven by these channels.

In addition to renters, we analyze households that transition from renters to homeowners under the LTV ratio restriction, whom we refer to as homebuyers. This exercise presents four empirical challenges. The first challenge is that we must observe households sufficiently long before and after the home purchase in order to capture the consumption adjustments associated with it. To address this issue, we focus on households that purchased a home in 2013, referred to as the 2013 cohort, which serves as the treatment group. This cohort allows us to observe three years before the home purchase, which provides sufficient time to capture the pre-purchase consumption adjustments made by these households. The second challenge is that purchasing a home has a direct effect on consumption. Benmelech et al. (2023) document that home purchases generate substantial consumption responses. This mechanical consumption response may affect both the pre-purchase and post-purchase periods, making it difficult to isolate the effect of the borrowing restriction. We address this issue by using the 2009 homebuyer cohort as an additional control group. Because these households purchased their homes before the introduction of the LTV restriction, they allow us to capture the baseline consumption dynamics associated with a home purchase in the absence of the policy. Moreover, the 2009 cohort also provides three years of pre-purchase observations. In addition, we restrict the post-purchase period to three years. Using a longer post-purchase window would generate overlap between the post-purchase periods of the two cohorts, which would complicate the comparison between them. The third challenge is that the two cohorts purchase their homes in different calendar years, so a direct comparison between them would confound cohort differences with aggregate time effects. Similar to the renter analysis, we address this concern by using homeowners as an additional control group, which allows us to difference out common time effects.

Lastly, as indicated by our renter analysis, the LTV restriction can induce a selection into the housing market, as it delays home purchases for some renters. Consequently, households that succeed in purchasing a home after the restriction may systematically differ from earlier cohorts. For instance, they may have a greater capacity to increase their savings by reducing consumption prior to purchase. Such selection could influence the estimated consumption dynamics. To account for this selection effect, we follow papers that investigate homebuyers around a policy introduction and perform a matching procedure between the 2013 and 2009 cohorts based on characteristics associated with saving capacity and use the matched sample in this analysis (Tzur-Ilan, 2023; Bernstein and Koudijs, 2024). In particular, we use income, parental wealth, and debt used as percentiles of the population, and individual

characteristics such as age, immigration status, and household size.[18] The balance statistics are reported in Table A2 and show that the matching procedure substantially reduces differences in observable characteristics across the two cohorts.

To implement this strategy, we estimate the following models separately for the pre-purchase and post-purchase periods:

$$\begin{aligned} y_{it} =& \beta\ Homebuyer_i \times Prepurchase_{it} \times Post_t + \alpha_1\ Homebuyer_i \times Prepurchase_{it} + \\ & \alpha_2\ Homebuyer_i \times Post_t + \alpha_3\ Prepurchase_{it} \times Post_t + \alpha_i + \alpha_t + \varepsilon_{it} \end{aligned} \tag{2}$$

$$\begin{aligned} y_{it} =& \beta\ Homebuyer_i \times Postpurchase_{it} \times Post_t + \alpha_1\ Homebuyer_i \times Postpurchase_{it} + \\ & \alpha_2\ Homebuyer_i \times Post_t + \alpha_3\ Postpurchase_{it} \times Post_t + \alpha_i + \alpha_t + \varepsilon_{it} \end{aligned} \tag{3}$$

where y_{it} is the dependent variable, and $Homebuyer_i$ is an indicator equal to one for homebuyers and zero for homeowners. $Prepurchase_{it}$ equals one for the three years before the home purchase and zero in the purchase year, while $Postpurchase_{it}$ equals one for the three years after the home purchase and zero in the purchase year. $Post_t$ is an indicator equal to one for years after 2010 in the pre-purchase analysis and equal to one for years after 2013 in the post-purchase analysis. Household and year fixed effects are included in both specifications, and standard errors are clustered at the labor market region level.

The coefficient of interest in both equations is β. Intuitively, these specifications compare how the consumption gap between the pre-purchase (or post-purchase) period and the purchase year differs between the 2013 and 2009 homebuyer cohorts, relative to homeowners. The identifying variation can be understood as a sequence of three differences. First, we compare homebuyers to homeowners, which isolates the consumption dynamics associated with purchasing a home. Second, we compare the pre-purchase (or post-purchase) period to the purchase year, which captures the consumption adjustments surrounding the purchase. Third, we compare the post-restriction cohort (2013) to the pre-restriction cohort (2009), which isolates how these consumption dynamics change after the introduction of the LTV restriction. By combining these three differences, the specification differences out both the

[18] We use 2006 information for the 2009 cohort, and 2009 information for the 2013 cohort. Due to the time difference, we use balance sheet items as population percentiles to limit the time effects on balance sheet items. The matching is a one-to-one matching, done with replacement, and a caliper of 0.05.

direct consumption response to home purchase documented by Benmelech et al. (2023) and aggregate time effects.

4 Results

In this section, we analyze how the renters adjust their consumption when an LTV ratio restriction is introduced. Section 4.1 presents our findings regarding how renters adjust their consumption and assesses the validity of the identifying assumptions. Section 4.2 explores the underlying mechanisms. Section 4.3 provides additional robustness checks. Finally, Section 4.4 studies the consumption reaction of homebuyers who purchase a home under the LTV ratio restriction.

4.1 Consumption adjustment by the renters

We report our main result in Table 2, in which we use the *consumption/income* ratio as the dependent variable in our difference-in-differences model. In column (1), we estimate the model without any controls or fixed effects. We find that *Renter* × *Post* has a positive and significant coefficient, indicating that renters increase their consumption relative to homeowners after the introduction of the LTV ratio restriction. In this model, we also estimate negative coefficients for *Renter* and *Post* dummies, suggesting that renters and the years after the restriction have lower consumption. Therefore, in the following columns, we include year and household fixed effects to control for year and household-level characteristics. Our preferred model, which includes both of these fixed effects, reveals that renters increase their consumption by 2.76 pp after the restriction. As the sample mean of the *consumption/income* ratio is 66 pp, this magnitude indicates a 4 percent increase in consumption relative to the mean value.

As explained in Section 3, to interpret our results causally, we must assume that the parallel trends assumption holds and the LTV ratio restriction does not affect the homeowners. In what follows, we evaluate these assumptions in detail. We start by considering the differences between renters and homeowners. As shown in Table 1, homeowners have a higher *consumption/income* ratio, income, and financial assets, and they are older than renters. Note that the differences in characteristics would violate the parallel trends assumption only if they affect the outcome variables differently before and after the restriction.

Our first observation regarding the differences in characteristics is from column (3) in Table 2. Including household fixed effects increases R^2 to 41 percent from 0.6 percent,

Table 2: **Consumption reaction of renters**

This table documents how renters react to the LTV ratio restriction with their consumption. Each column uses household-level data. The dependent variable is the consumption-to-income ratio. *Renter* takes the value of one for renters and zero for homeowners. *Post* equals one for the years after 2010. Control variables and fixed effects are indicated at the bottom of each column. Standard errors are clustered at the municipality level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

	Consumption/Income		
	(1)	(2)	(3)
Renter × Post	0.0318***	0.0296***	0.0276***
	(0.00349)	(0.00374)	(0.00340)
Renter	-0.0837***	-0.0814***	
	(0.00286)	(0.00271)	
Post	-0.0168***		
	(0.00615)		
Fixed Effects:			
Year FE		✓	✓
Household FE			✓
Obs.	7,601,189	7,601,189	7,601,189
R^2	0.003	0.006	0.412
Mean(Dependent Var.)	0.662		

while decreasing the magnitude of the effect modestly—to 2.76 pp from 2.96 pp. Oster (2017) develops a simple formula to assess the magnitude of the omitted variable bias to make the effect insignificant (Altonji et al., 2005). Plugging our estimates into this formula yields a bias-adjusted coefficient of 0.0247, assuming selection on unobservables is equal in magnitude to selection on observables. To put it differently, this finding suggests that selection on unobservable household characteristics must be more than nine times as strong as selection on observables to make the coefficient zero.[19]

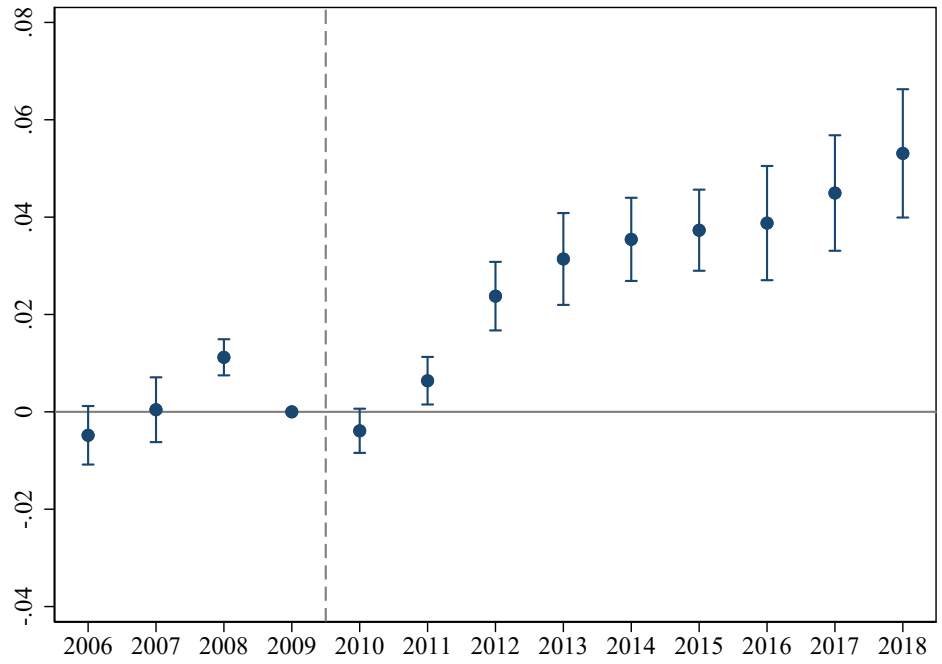
Armed with this evidence, we assess the parallel trends assumption by estimating a dynamic version of the model in Equation 1. Using 2009 as the benchmark year, Figure 1

[19]Specifically, we use the formula $\beta^* = \tilde{\beta} - \delta(\hat{\beta} - \tilde{\beta})\frac{R_{max} - \tilde{R}}{\tilde{R} - \hat{R}}$ where parameters with tildes are from the model with household fixed effects and the parameters with hats are from the model without household fixed effects. We plug in one for R_{max} and one for δ to calculate the respective values.

depicts that the difference between renters and homeowners is stable before the introduction of the LTV ratio restriction, supporting the assumption. The only slight deviation from parallel trends occurs in 2008, amid the GFC. Although the GFC is arguably the most volatile period over the last three decades, the magnitude of the deviation is still small relative to the estimated treatment effect. The renters' consumption gradually increases in the post-restriction years, and the difference stabilizes around 4 pp.

Figure 1: **Dynamic consumption reaction of renters**

This figure shows the dynamics of how renters react to the LTV ratio restriction with their consumption. The dependent variable is the *consumption/income* ratio. The y-axis shows the βs of the regression model $consumption/income_{it} = \sum_{k=-4}^{8} \beta_k \; D_k \times Renter_i + controls_{it} + \epsilon_{ht}$. Baseline event period is $k = -1$. The regression model includes household-and year-fixed effects. Standard errors are clustered at the labor market region level, and the bars indicate 95% confidence intervals.



We exploit the richness of our data and refine the comparison between renters and homeowners with granular fixed effects in Table 3. As noted before, renters are younger than homeowners. Thus, renters may be at different stages of their life cycle, which influences their consumption patterns. To control for life cycle effects, we include age $\times$ year fixed effects in the model in column (1), where age is measured in 2009. The next three columns consider the differences in household balance sheet items between renters and homeowners. We first create deciles by using deposits, total financial assets, and total wealth based on 2009 values. Then, to compare renters and homeowners in the same bins over the years, we include bins $\times$ year fixed effects. In the next column, we control for parental wealth using the same

method, as households may use parental wealth to afford a down payment and circumvent the restriction when purchasing a home. The last column further refines the comparison by including the interaction of all fixed effects introduced in this table, mimicking a matching estimator that compares renters and homeowners in the same age-deposit-financial assets-total wealth-parental wealth cells.[20] In all of these models, the treatment effect remains stable and is highly statistically significant, indicating that the rich set of characteristics considered in this table does not account for the increase in renters' consumption.

Table 3: **Controlling for differences between renters and homeowners**

This table controls for differences between renters and homeowners using granular fixed effects. Each column uses household-level data. The dependent variable is the *consumption/income* ratio. *Renter* takes the value of one for renters and zero for homeowners. *Post* equals one for the years after 2010. Control variables and fixed effects are indicated at the bottom of each column. Age is the rounded mean age of the household as of 2009. Bins are deciles of the respective variable in 2009. Standard errors are clustered at the labor market region level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

	Consumption/Income					
	(1)	(2)	(3)	(4)	(5)	(6)
Renter × Post	0.0316***	0.0293***	0.0261***	0.0412***	0.0282***	0.0385***
	(0.00451)	(0.00383)	(0.00374)	(0.00441)	(0.00361)	(0.00412)
Fixed Effects:						
Household FE	✓	✓	✓	✓	✓	✓
Age × Year FE	✓					
Deposit bins × Year FE		✓				
Financial Asset bins × Year FE			✓			
Total wealth bins × Year FE				✓		
Parental wealth bins × Year FE					✓	
Full Interacted FE						✓
Obs.	7,601,189	7,601,189	7,601,189	7,601,189	7,601,189	7,480,133
R^2	0.413	0.412	0.412	0.413	0.412	0.445
Mean(Dependent Var.)	0.660					

Next, we address the concern that the relative increase in renters' consumption could be influenced by homeowners' consumption if the LTV ratio restriction also affects homeowners. The restriction could possibly influence homeowners via two channels. First, as the restriction applies to all new mortgages, it could limit homeowners' borrowing capacity when they purchase a second home. Yet, the importance of this channel is likely to be limited since homeowners can use the home equity they have accumulated in their primary homes. Nevertheless, we again use the richness of our data to test the importance of this channel. The tax filings in Norway provide information on the current debt levels and housing wealth,

[20] We plot the dynamic difference in differences graph of the model in column (6) in Figure A4. We find insignificant pre-restriction coefficients, supporting the assumption of parallel trends.

enabling us to calculate the current LTV ratios of homeowners.[21] If our estimates are influenced by homeowners who are constrained by the restriction, removing such homeowners should reduce the magnitude of our estimates. To analyze this, we gradually remove homeowners from our sample whose current LTV ratios are above 95, 85, 75, and 65 percent, and then estimate the main regression model. In the first four columns of Table 4, we report the coefficients and demonstrate that the magnitude of the coefficients stays the same.[22]

The second channel is that the LTV restriction could reduce home prices or expected home prices, generating a wealth effect and lowering homeowners' consumption. The lack of a decline in Norway's house price index and regional house price growth rates, as illustrated in Figure A6 and Figure A7, suggests that this channel has limited empirical support. In addition, Chopra et al. (2025) document that homeowners do not reduce their consumption when their home price expectations are reduced exogenously. We assess the effect of home prices by accounting for heterogeneity in home prices with municipality $\times$ year fixed effects in the last column of Table 4. If this channel influences our results, absorbing the heterogeneity should have significantly affected them. Yet we again find a strong and positive effect and conclude that, despite the possibility, our results are not driven by the LTV ratio restriction on homeowners.

To further address the concern that our main estimates may reflect differential effects on homeowners rather than a genuine response by renters, we implement an alternative design that relies exclusively on heterogeneity within renters. In this exercise, both the treatment and control groups consist of renters, thereby eliminating any direct role for homeowners' borrowing capacity, housing wealth, or refinancing behavior. The key idea is to exploit heterogeneity in renters' ex-ante likelihood of becoming homeowners. Using information available as of 2006, the beginning of our sample period, we estimate a homeownership prediction model using an XGBoost algorithm, explained in Section A1. The model is trained on the full population using demographic characteristics and balance sheet information as of 2006. We then apply this model to renters and compute a predicted probability of being a homeowner. Because these probabilities are constructed using only 2006 information, they capture differences in renters' ex-ante ownership capacity and observable characteristics prior to the introduction of the LTV restriction. Intuitively, renters with a high predicted probability resemble homeowners in terms of income, assets, and other observables, while renters with a lower predicted probability are less similar along these dimensions. Importantly,

[21] Note that this is a conservative estimate, as the debt might include debt other than mortgages, and the housing values in tax filings are typically lower than the market values.

[22] We support this finding by depicting the second home purchase probabilities of homeowners in Figure A5. In line with the lack of effect on homeowners, there is no reduction in their purchases of second homes after the introduction of the LTV restriction.

Table 4: **Heterogeneity with respect to homeowners' current LTV ratios**

This table shows the heterogeneous effects with respect to homeowners' current LTV ratios. Each column uses household-level data. The dependent variable is the *consumption/income* ratio. *Renter* takes the value of one for renters and zero for homeowners. *Post* equals one for the years after 2010. The first four columns exclude homeowners whose current LTV ratios exceed the thresholds indicated in the column headers. Control variables and fixed effects are indicated at the bottom of each column. Standard errors are clustered at the labor market region level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

	Consumption/Income				
	(1)	(2)	(3)	(4)	(5)
	LTV<95%	LTV<85%	LTV<75%	LTV<65%	
Renter × Post	0.0258***	0.0252***	0.0242***	0.0230***	0.0308***
	(0.00314)	(0.00302)	(0.00289)	(0.00274)	(0.00309)
Fixed Effects:					
Household FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	
Municipality × Year FE					✓
Obs.	7,230,463	7,062,467	6,809,292	6,444,576	7,601,189
R^2	0.411	0.411	0.413	0.415	0.417
Mean(Dependent Var.)	0.656				

renters with high predicted probabilities appear, based on their observable characteristics, to have sufficient ownership capacity even before the policy change. As a result, their housing decisions are less likely to be directly constrained by the introduction of the LTV cap, making them a natural control group for renters whose ownership capacity is more likely to be affected by the restriction.

We use one minus the predicted probability measure to construct treatment and control groups within renters and estimate a difference-in-differences specification analogous to our baseline model. Table 5 reports the results. In the first two columns, we interact the one minus the continuous predicted probability with the post-restriction indicator, allowing the consumption response to vary with renters' ex-ante ownership capacity. In the next two columns, we define a treatment dummy equal to one for renters whose predicted probability is below the 80^{th} percentile of the distribution and zero otherwise, so that renters with relatively high predicted probabilities serve as the control group. Because these two groups differ in observables by construction, we further perform a matching based on pre-policy consumption-to-income ratios to enhance comparability across groups in columns (2) and (4). Across all specifications, the interaction between exposure to the policy and the post indicator remains positive and statistically significant, indicating that renters who are more likely to be affected by the LTV restriction increase their consumption relative to renters who

Table 5: **Within renter comparison**

This table shows the results of a within-renter comparison, where the control group consists of renters who have similar observable characteristics as of 2006. Each column uses household-level data. The dependent variable is the *consumption/income* ratio. *Homeowner prob.* is the $1 - estimated\ probability$, where estimated probability is a measure of a renter's similarity to homeowners in 2006, estimated by the method explained in Section A1. *Post* equals one for the years after 2010. The first two columns use *Homeowner prob.* in the continuous form. The last two columns use $\mathbb{1}$(Homeowner prob.$<80_{pct}$) as the treatment variable, which is a dummy variable that takes the value of one if the *Homeowner prob.* is smaller than 80^{th} percentile of the *Homeowner prob.* values. The second and fourth columns use the matched sample. Control variables and fixed effects are indicated at the bottom of each column. Standard errors are clustered at the municipality level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

	Consumption/Income			
	(1)	(2)	(3)	(4)
Homeowner prob. × Post	0.0489***	0.0320***		
	(0.00650)	(0.00743)		
$\mathbb{1}$(Homeowner prob.$<80_{pct}$) × Post			0.0299***	0.0173***
			(0.00523)	(0.00523)
Obs.	433,427	146,809	484,520	155,020
R^2	0.444	0.440	0.448	0.441
Mean(Dependent Var.)	0.606			

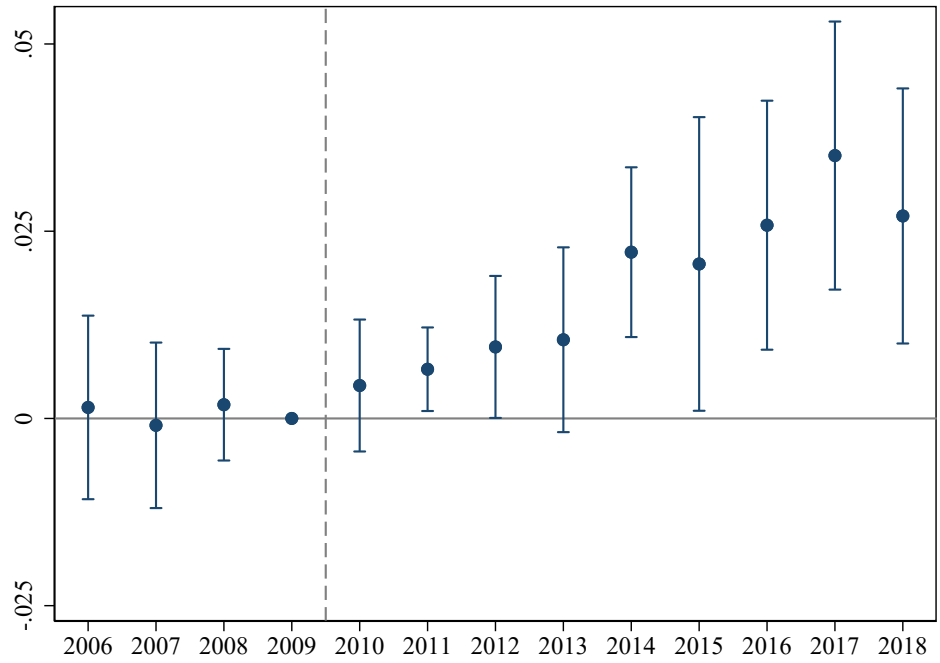
are less likely to be constrained. Figure 2 presents the dynamic version of this within-renter analysis based on the matched sample. The event-study coefficients show no evidence of differential trends prior to the introduction of the LTV restriction, supporting the parallel trends assumption in this setting. The divergence between the two groups emerges only after 2010 and persists in subsequent years. Taken together, these results demonstrate that the increase in renters' consumption does not hinge on using homeowners as the control group, but also arises when identification is based solely on ex-ante heterogeneity within renters.

4.2 Underlying mechanism

After establishing the causal effect of the LTV ratio restriction on renters' consumption, we investigate the underlying mechanism in this section. An LTV ratio restriction increases the amount of the down payment that households need to provide. Households may decide to increase their savings, which lowers their consumption, to afford the down payment. Alternatively, as demonstrated by Balke et al. (2024) with a heterogeneous-household incomplete-markets model, households may delay their savings when they delay their home purchase, suggesting an increase in *consumption/income* ratio. The reason is that a higher savings

Figure 2: **Dynamics of the within renter comparison**

This figure shows the dynamics of the within renter comparison. The dependent variable is the *consumption/income* ratio. $\mathbb{1}$(Homeowner prob.$>80_{pct}$) is a dummy variable that takes the value of one if the *Homeowner prob.* is smaller than 80^{th}. The y-axis shows the βs of the regression model $consumption/income_{it} = \sum_{k=-4}^{8} \beta_k \; D_k \times \mathbb{1}(Homeowner\ prob. > 80_{pct} i + controls_{it} + \epsilon_{ht}$. Baseline event period is $k = -1$. The sample is the matched sample. The regression model includes household-and year-fixed effects. Standard errors are clustered at the labor market region level and the bars indicate 95% confidence intervals.



rate induces a deviation from consumption smoothing, which is costly for the households. While households may prefer homeownership to renting, they may delay saving if the cost of deviation from consumption smoothing is sufficiently high. In this section, we present empirical evidence that supports the latter mechanism.

To assess the mechanism, we first examine whether the LTV ratio restriction induced a delay in transitioning to homeownership. Utilizing the universe of linked tax filings and housing transaction registry, we identify all first-time homebuyers. Figure 3 plots the age distribution of these households, comparing the pre-restriction cohort (2006–2009) to the post-restriction cohort (2013–2015). The histograms show a pronounced rightward shift in the age of first-time buyers following the policy's implementation. Notably, the mean age at purchase, indicated by the dashed lines, rises by approximately two and a half years. This systematic delay across the entire distribution provides direct evidence that the borrowing constraint forces prospective buyers to postpone their entry into the housing market.

Figure 3: **Age distribution of first-time homebuyers**

This figure shows the histogram of the ages of first-time homebuyers before and after the introduction of the LTV ratio restriction. First-time homebuyers are determined by using tax filings and the housing transactions registry. The dashed lines represent the average age of first-time homebuyers in the pre- and post-period, respectively.

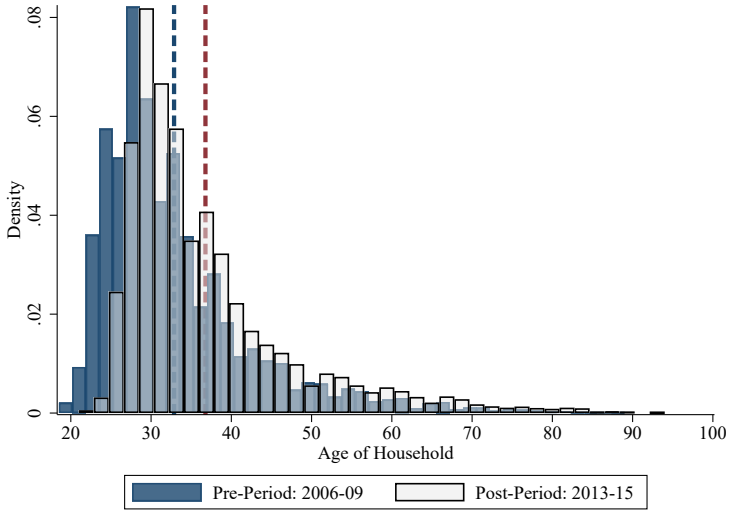


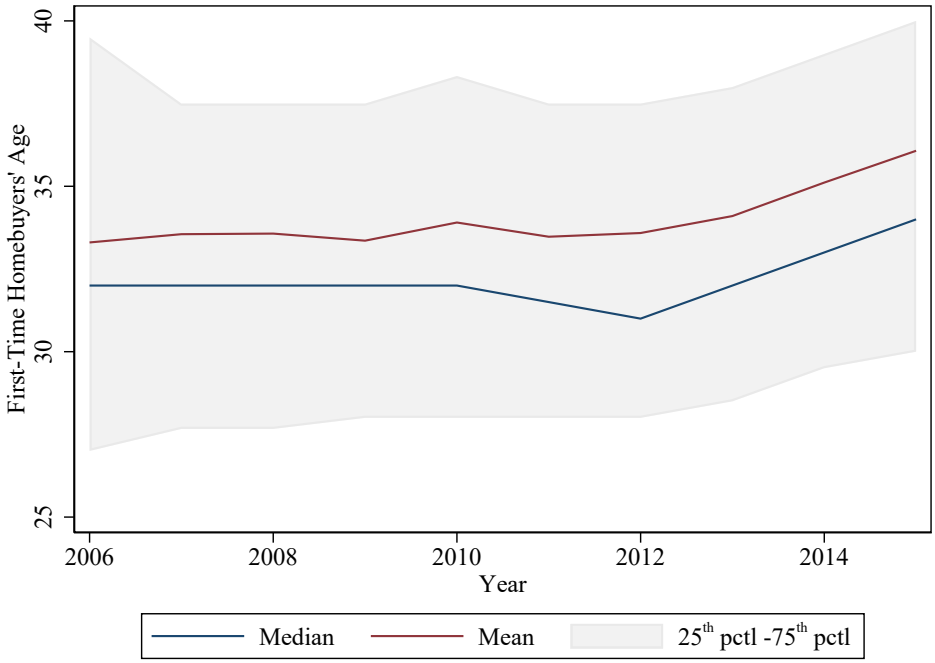
Figure 4 demonstrates the age distribution of first-time homebuyers over time. Importantly, the age distribution prior to the restriction is stable, suggesting that the subsequent increase is driven by the introduction of the LTV ratio restriction. Notably, the increase is gradual rather than a discontinuous jump. This gradual increase implies a dynamic selection into the housing market. Following the introduction of the restriction, the pool of realized homebuyers is heavily skewed toward unconstrained households, such as those with high baseline wealth or access to parental transfers, whose timing and age at purchase remain largely unaffected. Conversely, constrained renters temporarily delay their purchase to accumulate the required down payment. As these constrained renters accumulate enough savings to enter the market in the subsequent years, the age of first-time homebuyers increases. This shifting composition of buyers naturally generates the gradual, continuous upward drift in the average purchase age observed in the data.[23]

Even though the delay in transitioning to homeownership is evident, this delay does not by itself establish that renters postpone saving. A delayed purchase could also arise if renters respond to the higher down-payment requirement by saving more aggressively and

[23]This observed change in ages likely underestimates the true magnitude of the policy's effect. Because the transaction data only captures those who eventually succeed in buying a home, it mechanically excludes renters whose purchases are not just delayed but completely discouraged by the LTV restriction. If these permanently sidelined households were accounted for, the aggregate delay in homeownership would be even more severe.

Figure 4: **Age distribution of first-time homebuyers over the years**

This figure shows the distribution of the ages of first-time homebuyers over the years. First-time homebuyers are determined by using tax filings and the housing transactions register. The blue line is the median, and the red line is the mean of ages in corresponding years. The gray areas illustrate the 25^th and 75^th percentiles of the age distribution.



reducing consumption, but still need additional time to accumulate the required equity. To distinguish between these two responses, we examine whether the consumption increase is stronger among renters for whom delaying saving is most attractive: households with greater scope for future income growth. The mechanism predicts that these households have a stronger incentive to postpone down-payment saving. When the LTV restriction increases the required down payment, saving immediately would require a costly reduction in current consumption. For renters who expect their income to rise, however, waiting allows the same down payment to be accumulated later with a smaller sacrifice in consumption. Thus, a household that is still far from its likely earnings frontier can smooth consumption by delaying both the home purchase and the intensive saving period until income is higher.

To capture this idea empirically, we construct an earnings potential gap prior to the reform. We partition the population into cells defined by immigration background, labor market region, and education, and compute the potential highest income as the mean income of the top 10 percent of earners within each cell. We then define the earnings potential gap as the difference between this potential income and the household's 2009 income. The

measure thus captures how far a household is from the upper end of the income distribution among individuals at later stages in their careers but with similar education, labor-market location, and immigration background. For instance, among teacher-school graduates, the earnings potential gap is 354,000 NOK, whereas it is 595,000 NOK for business administration graduates and 615,000 NOK for dental-school graduates. These differences are intuitive: some education groups have relatively compressed earnings profiles, while others offer steeper earnings trajectories and higher upper-tail income. The measure therefore captures not only a household's current income position but also the income opportunities associated with its labor-market track.

A concern with this exercise is that the earnings potential gap may mechanically reflect current income. Since the gap is defined as potential income minus 2009 income, households with larger gaps may simply be lower-income households. In that case, the heterogeneity could reflect differences in current income rather than differences in future earnings potential. To address this concern, we include income bins $\times$ year fixed effects, where income bins are deciles of household income in 2009. A second concern is that cells may differ in time-varying ways unrelated to earnings potential. For example, education groups and labor market regions may experience different post-crisis recoveries or local labor-demand shocks. We therefore also include cell $\times$ year fixed effects. With both sets of fixed effects included, we compare two households with similar pre-reform income, but different scope for future income growth, while absorbing time-varying shocks to their education-region-immigration cells. Under the proposed mechanism, the renter in the cell with the higher income frontier should have a stronger incentive to delay saving and maintain higher current consumption.

We use the earnings potential gap in Table 6. In column (1), we use the households whose 2009 income already exceeds the top-decile in their cells and find no consumption response after the restriction. This is consistent with the view that renters at or near their earnings peak have limited incentive to postpone savings. We split the households into quartiles based on the earnings potential gap in the remaining four columns. These columns reveal a strong monotonic pattern: the magnitude of the coefficient increases from 0.86 pp in the lowest quartile to 4 pp in the highest quartile, indicating that renters farther from their earnings potential frontier respond more strongly to the LTV restriction by increasing their consumption. This is again in line with the proposed mechanism, as renters with greater anticipated income growth appear more willing to delay the costly deviation from consumption smoothing required to meet higher down payment requirements.

In the next exercise, we examine heterogeneity with respect to households' income levels, which provide a natural proxy for both liquidity constraints and the likelihood of transi-

Table 6: **Earnings potential gap**

This table reports how renters' consumption reaction changes with respect to their earnings potential gap. Earning potential is the average of the top ten percent income earners in each cell, and the earnings potential gap is the difference between earnings potential and earnings in 2009. Cells are created by using the immigration background, labor market regions, and education information (five-digit Norwegian Standard Classification of Education codes). Column (1) uses the households whose incomes are above the mean of the top 10 percent earners in their cells. The remaining columns use the quartiles of the earnings potential gap, calculated by excluding households with a negative gap. *Renter* takes the value of one for renters and zero for homeowners. *Post* equals one for the years after 2010. The dependent variable is *consumption/income*. Household and year fixed effects are included for all regressions. Standard errors are clustered at the labor market region level. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	Above top 10%	$1^{st}Quart.$	$2^{nd}Quart.$	$3^{rd}Quart.$	$4^{th}Quart.$
	(1)	(2)	(3)	(4)	(5)
renterXpost	-0.00160	0.00860***	0.0169***	0.0317***	0.0401***
	(0.00822)	(0.00297)	(0.00441)	(0.00494)	(0.00356)
Fixed Effects:					
Household FE	✓	✓	✓	✓	✓
Cell × Year FE	✓	✓	✓	✓	✓
Income bins × Year FE	✓	✓	✓	✓	✓
Obs.	544,254	1,690,597	1,866,771	1,822,874	1,668,886
R^2	0.424	0.407	0.434	0.465	0.470
Mean(Dependent Var.)	0.662				

tioning into homeownership. Figure 5 plots the estimated treatment effects across income bins and reveals an inverted U-shaped relationship between the consumption response and income. The effect is insignificant for low-income renters, which is intuitive because these households are less likely to become homeowners and therefore less directly affected by tighter down payment requirements. The effect increases for middle-income households and reaches its peak in this segment of the distribution. This pattern is in line with the proposed mechanism: middle-income renters are the most likely prospective homebuyers but are also the most exposed to borrowing constraints, making the LTV restriction particularly binding for them. In contrast, the effect becomes smaller again for high-income households, who are less likely to be constrained by the LTV rule because they can more easily afford the required down payment. Together with the earnings potential gap results, this analysis strengthens the mechanism interpretation. While the income-level heterogeneity identifies the group for whom the borrowing constraint is most binding, the earnings potential gap exercise shows that renters with greater scope for future income growth are more likely to delay savings and maintain higher consumption, reflecting the costs associated with deviation from consumption smoothing.

Figure 5: **Heterogeneity with respect to renters' income levels**

This figure shows the heterogeneous effects with respect to renters' income levels. Each data point uses household-level data and illustrates the coefficient of Renter × Post, where the dependent variable is *consumption/income* ratio. The x-axis illustrates the households that remain in the sample with respect to their income levels, grouped by income percentiles. Regression models include household-, and year-fixed effects. Standard errors are clustered at the labor market region level and the bars indicate 95% confidence intervals.

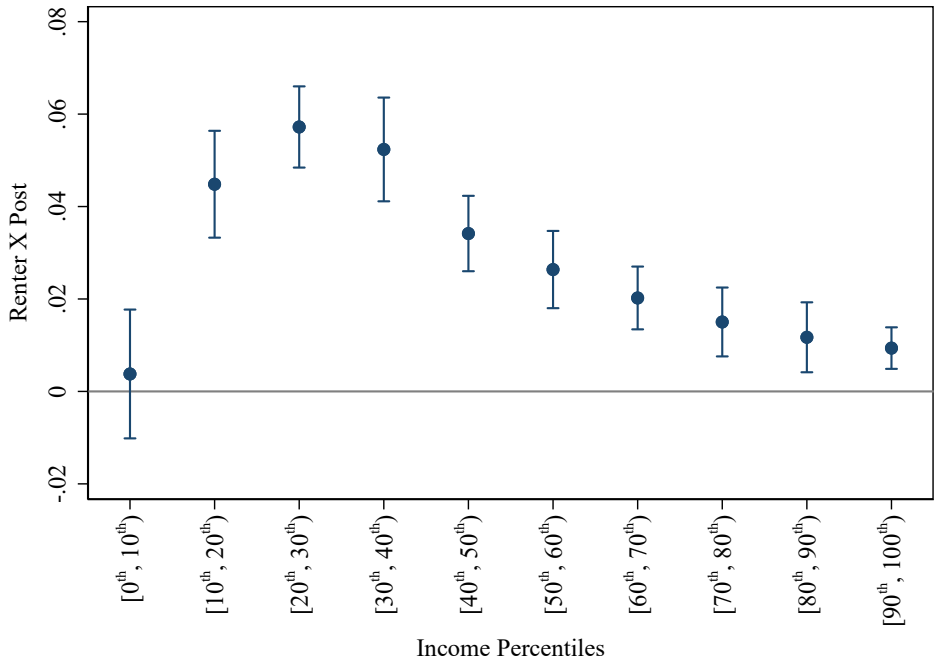


Table 7 provides additional support for the proposed mechanism by exploiting regional variation in pre-restriction leverage. We classify municipalities according to the average LTV ratios of first-time homebuyers in the pre-restriction period and find that the increase in renters' consumption after the introduction of the LTV cap is stronger in municipalities with higher pre-restriction LTV ratios. In the continuous specification reported in column (1), a one standard deviation increase in the municipality-level LTV ratio raises the magnitude of the consumption response by 0.36 percentage points. This pattern is consistent with a binding borrowing-constraint mechanism. In regions where borrowers were already financing home purchases with high leverage prior to the policy, the LTV cap represents a larger tightening of required down payments. For renters considering homeownership, this implies a sharper increase in required savings and thus a larger deviation from consumption smoothing. In line with the mechanism, renters in these high-LTV municipalities respond more strongly by increasing current consumption and postponing savings.

We perform a sample-split analysis in the remaining four columns, where column (2) uses

Table 7: **Regional heterogeneity**

This table reports heterogeneity in renters' consumption responses by municipality-level pre-policy average LTV, computed among first-time homebuyers in 2006–2009. The municipality-level LTV variable is standardized to have zero mean and unit standard deviation. Column (1) interacts *Renter* × *Post* with the municipality-level LTV variable; columns (2)–(4) split the sample into groups by percentiles. The dependent variable is *consumption/income*. Household and year fixed effects are included for all regressions. Standard errors are clustered at the labor market region level. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	MuniLTV Interaction	MuniLTV <25th perc.	MuniLTV 25th–75th perc.	MuniLTV >75th perc.
	(1)	(2)	(3)	(4)
Renter × Post× Municipality LTV	0.0036**			
	(0.00177)			
Renter × Post	0.0306***	0.0206***	0.0289***	0.0377***
	(0.00257)	(0.00422)	(0.00399)	(0.00314)
Post × Municipality LTV	0.0134***			
	(0.00214)			
Fixed Effects:				
HH FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
Obs.	7,058,502	1,701,527	3,496,391	2,403,271
R^2	0.413	0.415	0.412	0.410
Mean(Municipality LTV)	0.923	0.810	0.921	1.030
Diff. to MuniLTV > 75th Coeff		0.0171***	0.0087*	
Diff. to MuniLTV < 25th Coeff			0.0084	

municipalities whose LTV ratios are below 25^{th} percentile, column (3) uses the ones between 25^{th} and 75^{th} percentiles, and column (4) uses the ones above 75^{th} percentile. Households with missing municipality-level data are dropped from the sample. We find that the estimated effect in the top quartile of municipalities exceeds that in the bottom quartile by 1.7 percentage points, and this difference is statistically significant. Notably, the mean pre-restriction LTV ratio differs by 20 percentage points between these two groups. This sizeable gap in the LTV ratios translates into a materially stronger consumption response, reinforcing the interpretation that the policy operates through the tightening of down payment constraints. Importantly, we still find a positive and significant effect even in municipalities in the lowest LTV quartile. This does not imply that the policy was non-binding in those areas. Rather, the comparison is relative: even in low-LTV municipalities, there were first-time mortgages with LTV ratios above the new cap prior to the reform. As illustrated in Figure A8, the LTV distributions in both low- and high-LTV regions exhibit mass above the threshold, implying that the restriction tightened borrowing conditions across all regions, albeit more strongly where pre-restriction leverage was highest.

An additional implication of the mechanism is that the increase in renters' consumption should be concentrated in discretionary spending rather than in categories that are close to subsistence. If tighter LTV restrictions induce prospective buyers to postpone both home

Table 8: **Expenditure elasticities**

This table reports difference-in-differences estimates for consumption categories with estimated expenditure elasticities below one and above one. Columns (1) and (2) use the ratio of group-level consumption to household income as the dependent variable. Columns (3) and (4) use the ratio of group-level consumption to total household consumption. Categories are grouped according to whether their estimated expenditure elasticity, reported in Table A1, is below or above one. *Renter* takes the value of one for renters and zero for homeowners. *Post* equals one for the years after 2010. All models include household and year fixed effects. Standard errors are clustered at the labor market region level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

	Cons. type/Income		Cons. type/Total Cons.	
	(1)	(2)	(3)	(4)
	$\epsilon < 1$	$\epsilon > 1$	$\epsilon < 1$	$\epsilon > 1$
Renter × Post	0.00324	0.0199***	-0.0192***	0.0211***
	(0.00252)	(0.00122)	(0.00310)	(0.00363)
Fixed Effects:				
Household FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Obs.	7,601,189	7,601,189	7,601,189	7,601,189
R^2	0.579	0.444	0.547	0.542
Mean(Dependent Var.)	0.147	0.380	0.301	0.687

purchase and the start of targeted saving, then affected renters should not merely consume more in the aggregate; they should also tilt their spending toward goods whose demand rises more than proportionally with household resources. This prediction follows directly from the consumption-smoothing channel emphasized above. Households that respond to the LTV restriction by delaying the costly adjustment needed to accumulate a higher down payment are unlikely to compress discretionary spending immediately. Instead, they continue to allocate a relatively large fraction of their budgets to categories that are more income- or expenditure-elastic, that is, to goods that the demand-system literature classifies as luxuries rather than necessities. Following the Engel-curve literature, we therefore classify consumption types using estimated expenditure elasticities, with categories whose elasticity exceeds one interpreted as luxuries and those whose elasticity falls below one interpreted as necessities (Houthakker, 1957; Deaton and Muellbauer, 1980; Hall and Jones, 2007; Browning and Crossley, 2009; Aguiar and Bils, 2015; Comin et al., 2021; Bachas et al., 2024). As in this literature, we estimate category-specific Engel curves using households' budget shares and recover the corresponding expenditure elasticities from the estimated Engel-curve slopes.[24]

[24]Section A2 provides a detailed description of both the consumption categorization and the elasticity

The results strongly support the mechanism. In columns (1) and (2) of Table 8, where the dependent variable is category-level consumption relative to income, renters do not increase spending on necessity categories relative to income, but they do increase spending on luxury categories. In columns (3) and (4), where the dependent variable is the share of each group in total consumption, the same pattern becomes even sharper: the budget share of necessity spending falls, whereas the budget share of luxury spending rises. Taken together, these findings indicate that the post-restriction increase in renters' consumption is concentrated in the more discretionary part of the consumption basket, exactly as one would expect if affected households respond to tighter borrowing constraints by postponing down-payment saving rather than by immediately reducing current consumption.

Table 9: **Delayed savings or giving-up?**

This table reports difference-in-differences estimates for various labor market outcome variables. Column (1) uses an indicator variable for labor income as the outcome variable. Column (2) uses an indicator variable for business income as the outcome variable. Column (3) uses an indicator variable for unemployment insurance payments as the outcome variable. Column (4) uses an indicator variable for bonus payments as the outcome variable. Column (5) uses the logarithm of bonus payments as the outcome variable. Column (6) uses an indicator variable for irregular payments as the outcome variable. Column (7) uses the logarithm of irregular payment as the outcome variable. Column (8) uses an indicator variable for overtime payments as the outcome variable. Column (9) uses the logarithm of overtime payment as the outcome variable. *Renter* takes the value of one for renters and zero for homeowners. *Post* equals one for the years after 2010. All models include household and year fixed effects. Standard errors are clustered at the labor market region level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

	$\mathbb{1}(Labor)$	$\mathbb{1}(Business)$	$\mathbb{1}(UI)$	$\mathbb{1}(Bonus)$	ln(bonus)	$\mathbb{1}(Irregular)$	ln(irregular)	$\mathbb{1}(Overtime)$	ln(overtime)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Renter × Post	0.0286***	0.00117***	-0.00251***	0.0327***	0.0499**	0.0556***	0.109***	-0.0105	0.0262
	(0.00371)	(0.000293)	(0.000652)	(0.00191)	(0.0226)	(0.00494)	(0.0130)	(0.0109)	(0.0182)
Fixed Effects:									
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Household FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Obs.	7,601,189	7,601,189	7,601,189	7,601,189	641,786	7,601,189	2,196,029	7,601,189	1,606,616
R^2	0.749	0.395	0.282	0.418	0.675	0.549	0.624	0.494	0.588
Mean(Dependent Var.)	0.655	0.008	0.020	0.093	7.305	0.298	7.375	0.220	6.969

The evidence so far is consistent with renters postponing targeted down-payment savings in anticipation of higher future income. However, an alternative interpretation is also possible. If the LTV restriction makes homeownership appear unattainable for a subset of renters, these households may abandon the goal of purchasing a home altogether. In that case, the increase in consumption would not reflect a delay in saving toward a still-valued future purchase, but rather a *giving-up* response: households would stop saving for homeownership, increase current consumption, and reduce effort in the labor market because the return to additional earnings in terms of reaching the housing market has fallen sharply.

estimation procedure.

This distinction is important, because the two mechanisms have opposite implications for labor-market behavior. Under the delayed-saving mechanism, affected renters should continue to invest in their future earnings capacity, since higher future income makes it less costly to accumulate the required down payment later on. Under the giving-up mechanism, by contrast, renters should weaken their labor-market attachment and reduce effort once they no longer perceive homeownership as a realistic objective. This prediction is closely related to the recent framework of Lee and Yoo (2025), in which households that lose hope of future homeownership consume more and work less, whereas households that remain on track exert higher labor effort and continue to save toward the goal.

To distinguish between these two mechanisms, we exploit the detailed labor-market information in the Norwegian administrative data. Specifically, we examine three extensive-margin outcomes: indicators for whether any member of the household receives labor income, whether the household reports business income, and whether the household receives unemployment insurance benefits.[25] We then turn to several compensation components that proxy for labor-market effort and performance. In particular, the earnings registers allow us to observe whether households receive bonuses, irregular additional pay, and overtime compensation, as well as the corresponding payment amounts conditional on receipt.[26] While these variables are not direct measures of effort, they provide informative evidence on margins along which labor-market intensity and performance-related compensation may adjust following the reform.

Table 9 reports the results. The first three columns provide no support for the giving-up interpretation. If anything, renters appear more attached to the labor market after the introduction of the LTV restriction: the probability that the household earns labor income increases, the probability of reporting business income rises slightly, and the probability of receiving unemployment insurance falls. These patterns are difficult to reconcile with a broad withdrawal from the labor market. Columns (4)–(9) reinforce this conclusion. After the restriction, renters become more likely to receive bonus payments and irregular additional pay, and conditional on receipt, the corresponding payment amounts also increase. By contrast, the coefficient on overtime receipt is negative but statistically insignificant, and the estimated effect on overtime amounts is likewise imprecisely estimated. Thus, not every

[25]These variables are constructed at the household-year level using the mean values of the underlying individual indicators within the household.

[26]Bonuses include commissions, profit sharing, production allowance and gratuities. Variable additional allowances include allowances such as shift allowance, allowances for working evenings and nights, call-out allowance, dirty conditions allowance, offshore allowance and other allowances that occur irregularly. Finally, overtime pay includes the sum of cash compensation for work carried out beyond contractual working hours, and is a calculated average per month over the period 1 January to the time of the census.

margin moves in the same direction. Nevertheless, the overall pattern is clearly inconsistent with a reduction in labor-market effort or attachment.

Taken together, these findings do not support the view that renters respond to the LTV restriction by giving up on homeownership and disengaging from the labor market. Instead, the labor-market evidence aligns more closely with the delayed-saving mechanism emphasized above. Renters appear to maintain—and in some dimensions even strengthen—their attachment to earnings-generating activities after the reform, which is precisely what one would expect if they continue to value future homeownership but postpone the start of targeted saving until their income rises further. In this sense, the labor-market evidence complements the earlier heterogeneity results based on the earnings potential gap and income bins: the increase in consumption is not accompanied by a withdrawal from future-oriented behavior, but rather by a postponement of when the costly saving adjustment takes place.

Table 10: **Renters' location choices and neighborhood-level income levels**

This table investigates renters' mobility choices. Column (1) uses Relocation prob. as the outcome variable, which is a dummy variable that takes the value of one when the household switches to another neighborhood. Column (2) uses the difference between log of the mean neighborhood income at the rental unit and the new homepurchase location for homebuyers prior to the LTV restriction in 2010. Column (3) uses the log of the median income at the neighborhood level between 2006 and 2009 as the outcome variable. Column (4) instead uses the mean income at the municipality level between 2006 and 2009 as the outcome variable. Household and year fixed effects are included for all regressions. Standard errors are clustered at the labor market region level. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

	Pr(Relocation)	Δ Neighb income | Reloc.	Neighb.level Income-Median	Neighb.level Income-Mean
Renter X Post	-0.0318***		0.00584***	0.00235*
	(0.00224)		(0.000768)	(0.00138)
Homebuyers (<2010)		-0.0112***		
		(0.00132)		
Fixed Effects:				
HH FEs	✓		✓	✓
Year FEs	✓		✓	✓
Adj. R^2	0.071		0.964	0.923
Obs.	7,589,952	12,236	331,079	7,566,261

We next examine whether the LTV restriction affects renters' mobility and location choices, which provides additional evidence for the delayed homeownership mechanism. If the policy makes purchasing a home more difficult, renters who were previously planning to transition to ownership may postpone this decision and remain in their current rental units longer. In Norway, similar to many other countries, rent increases for sitting tenants are typically tied to inflation, whereas rents can be reset when a tenant moves. This institutional feature reduces renters' incentives to relocate. Consistent with this reasoning, column (1) of Table 10 shows that renters' relocation probability declines by 3.2 percentage points after the introduction of the LTV cap. The reduction in mobility suggests that renters delay housing

transitions and remain longer in their current rental housing when homeownership becomes harder to attain. Importantly, we find that the renters who transitioned into homeowners have moved to neighborhoods with lower average income levels in column (2).

Reflecting the findings in the previous two columns, after the restriction, renters become slightly more likely to reside in municipalities with higher income levels. Columns (3) and (4) of Table 10 indicate that the average municipality income of renters increases by approximately 0.6 percent when measured using the median income and 0.2 percent when measured using the mean income. One interpretation is that renters who postpone home purchases remain longer in urban or high-amenity municipalities, rather than moving to suburban locations that are more typical of owner-occupiers. This channel could explain the increase in consumption because higher-income municipalities tend to have higher local price levels, thereby increasing consumption. To evaluate whether this location-sorting channel can account for the observed increase in consumption, we estimate the elasticity of municipality-level household consumption with respect to municipality-level income in a log–log specification and obtain an elasticity close to one. Combining this elasticity with the increase in municipality income implies a consumption increase of only 0.3–0.6 pp. This magnitude is small relative to our baseline consumption response of 3.3 pp, indicating that location sorting can explain only a minor share of the effect. Instead, the decline in mobility primarily reinforces the interpretation that the LTV restriction delays homeownership and alters renters' housing trajectories, consistent with the proposed mechanism.

4.3 Additional exercises

This section discusses our further findings. First, we decompose $consumption/income$ ratio by using $ln(income)$ and $ln(consumption)$ as dependent variables in Table 11. Renters may react to the LTV ratio restriction by changing their labor supply. In line with this, we find that renters have higher incomes relative to homebuyers after the restriction in the first three columns. However, the last three columns document that the increase in consumption is significantly higher, indicating that the effects on $consumption/income$ ratio is dominated by the consumption effect.

Next, we explore renters' other balance sheet items in Table 12. An increase in consumption without a comparable increase in income may increase renters' debt levels and decrease their stock of savings, relative to the homeowners. In columns (1)-(3), we use $debt/income$, $deposits/income$, and $financial\ assets/income$ ratios as dependent variables. We find that renters have higher debt and lower deposits and financial asset holdings after the LTV ratio

Table 11: *consumption/income* **decomposition**

This table shows the decomposition of *consumption/income* ration in consumption and income.. Each column uses household-level data. The first three columns use *ln*(*income*) as the dependent variable and the last three columns use *ln*(*consumption*) as the dependent variable. *Renter* takes the value of one for renters and zero for homeowners. *Post* equals one for the years after 2010. Control variables and fixed effects are indicated at the bottom of each column. Standard errors are clustered at the labor market region level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

	Log(Income)			Log(Consumption)		
	(1)	(2)	(3)	(4)	(5)	(6)
Renter × Post	0.0680***	0.0717***	0.0545***	0.115***	0.116***	0.0948***
	(0.00673)	(0.00702)	(0.00671)	(0.00483)	(0.00497)	(0.00558)
Renter	-0.403***	-0.408***		-0.525***	-0.528***	
	(0.00603)	(0.00596)		(0.00642)	(0.00656)	
Post	0.226***			0.217***		
	(0.00175)			(0.0101)		
Fixed Effects:						
Year FE		✓	✓		✓	✓
Household FE			✓			✓
Obs.	7,601,189	7,601,189	7,601,189	7,601,189	7,601,189	7,601,189
R^2	0.076	0.094	0.855	0.050	0.058	0.747
Mean(Dependent Var.)	0.662			0.662		

restriction. To see the overall effect on net savings, we first define net savings as the difference between total financial assets and debt and use *net savings/income* ratio as the dependent variable in the last column. In line with the previous columns, we find that renters have lower net savings.

Renters and homeowners differ systematically in wealth, leverage, and housing exposure, which may cause them to respond differently to macroeconomic conditions. If such differential responses coincide with the introduction of the LTV restriction, they could contribute to the consumption differences we observe. To address this concern, we allow renters' consumption to respond differently to macroeconomic shocks by interacting the renter indicator with changes in key macroeconomic variables, including GDP growth, inflation, the monetary policy rate, the VIX index, and the unemployment rate. These interaction terms capture the possibility that renters and homeowners have different sensitivities to macroeconomic fluctuations over time. The results, reported in Table 13, show that the estimated effect of the LTV restriction on renters' consumption remains positive and statistically significant across all specifications. This stability indicates that differential exposure to macroeconomic conditions cannot explain our findings, suggesting that the increase in renters' consumption is driven by the borrowing restriction rather than by macroeconomic shocks that may affect

Table 12: **Other balance sheet items of renters**

This table shows how other balance sheet items of renters evolve after the LTV ratio restriction. Each column uses household-level data. The dependent variables are indicated in each column. *Renter* takes the value of one for renters and zero for homeowners. *Post* equals one for the years after 2010. Control variables and fixed effects are indicated at the bottom of each column. Standard errors are clustered at the labor market region level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

	Debt/Income	Deposits/Income	Fin. Assets/Income	Net Savings/Income
	(1)	(2)	(3)	(4)
Renter × Post	0.309***	-0.114***	-0.128***	-0.436***
	(0.0152)	(0.00676)	(0.00512)	(0.0190)
Fixed Effects:				
Year FE	✓	✓	✓	✓
Household FE	✓	✓	✓	✓
Obs.	7,601,189	7,601,189	7,601,189	7,601,189
R^2	0.763	0.788	0.801	0.827
Mean(Dependent Var.)	1.724	0.818	1.079	-0.649

renters and homeowners differently.

4.4 Homebuyers

So far, our results show that renters respond to the LTV ratio restriction by increasing their consumption as they delay their home purchases and postpone savings. This finding contrasts with the intended objective of macroprudential borrowing restrictions, which is to reduce household leverage and encourage more conservative balance sheet behavior. Consistent with this objective, existing empirical studies find that households purchasing homes under LTV restrictions tend to exhibit lower leverage (Aastveit et al., 2020; Bekkum et al., 2024). However, these studies primarily focus on the leverage implications of borrowing constraints and provide limited evidence on how such policies affect household consumption behavior. In this section, we therefore examine the consumption dynamics of renters who eventually purchase a home under the restriction—whom we refer to as homebuyers—and investigate how their consumption evolves around the home purchase. Examining consumption dynamics around the home purchase is particularly informative in this context, as it allows us to assess whether borrowing restrictions influence not only households' leverage at the time of purchase but also their intertemporal consumption behavior. To do so, following the empirical strategy described in Section 3, we compare a matched 2013 homebuyer cohort to a matched 2009 cohort, while using homeowners as an additional control group to absorb common time effects.

Table 13: **Controlling for macroeconomic conditions**

This table shows the robustness of the increase in consumption when renters are allowed to be affected by macroeconomic conditions differently. Column (1) includes Renter × Δ GDP growth as a control. Column (2) includes Renter × Δ Inflation as a control. Column (3) includes Renter × Δ Policy rate as a control. Column (4) includes Renter × Δ VIX as a control. Column (5) includes Renter × Δ Inflation as a control. *Renter* takes the value of one for renters and zero for homeowners. *Post* equals one for the years after 2010. Fixed effects are indicated at the bottom of each column. Standard errors are clustered at the labor market region level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

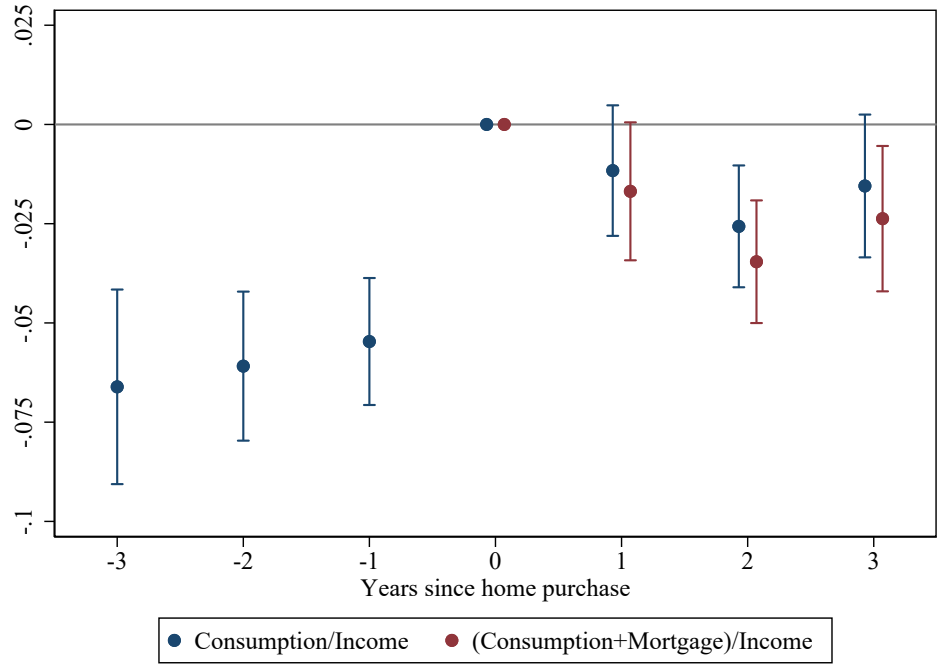
	Consumption/Income				
	(1)	(2)	(3)	(4)	(5)
Renter × Post	0.0346***	0.0349***	0.0378***	0.0398***	0.0443***
	(0.00398)	(0.00404)	(0.00437)	(0.00439)	(0.00481)
Renter × Δ GDP growth	-0.00560***	-0.00587***	-0.00788***	-0.00480***	-0.00283***
	(0.000441)	(0.000515)	(0.000813)	(0.000798)	(0.000591)
Renter × Δ Inflation		-0.000483**	-0.00238***	-0.00200***	0.00135***
		(0.000224)	(0.000486)	(0.000473)	(0.000325)
Renter × Δ Policy rate			0.00339***	0.000511	-0.00563***
			(0.000797)	(0.000819)	(0.000633)
Renter × Δ VIX				0.00102***	0.00134***
				(0.000102)	(0.000107)
Renter × Δ Unemployment rate					-0.0145***
					(0.00196)
Fixed Effects:					
Year FE	✓	✓	✓	✓	✓
Household FE	✓	✓	✓	✓	✓
Obs.	7,601,189	7,601,189	7,601,189	7,601,189	7,601,189
R^2	0.428	0.428	0.428	0.428	0.428
Mean(Dependent Var.)	0.660				

We begin by examining consumption dynamics in the years preceding a home purchase. Figure 6 presents an event-study specification that plots the coefficients on the interaction term $\sum_{k=-3}^{-1} \beta_k \left(Homebuyer_i \times D_{it}^k \times Post_t \right)$, using the home purchase year as the benchmark period. The figure shows that households that purchase a home after the introduction of the LTV restriction reduce their consumption more strongly in each of the three years leading up to the purchase than households that purchased before the restriction. Rather than displaying a pronounced gradual decline toward the purchase year, the estimates indicate a broadly stable negative gap throughout the pre-purchase period, suggesting that households purchasing under the restriction maintain lower consumption well in advance of the transaction in order to accumulate the higher down payment required by the LTV cap. This pattern is in line with forward-looking behavior in incomplete-markets models with borrowing constraints, in which households begin adjusting their spending before the purchase and sustain that adjustment over multiple years. Column (1) of Table 14 confirms

this result in the regression framework. The estimated triple interaction term implies that, relative to homeowners and relative to the purchase year, households in the 2013 cohort reduce their cash outflows-to-income ratio by an additional 6 percentage points in the three years preceding a home purchase compared to the 2009 cohort.

Figure 6: **Consumption reaction of homebuyers**

This figure shows the dynamics of how homebuyers adjust their consumption around a home purchase before and after the introduction of the LTV ratio restriction. The figure plots the coefficients of $\sum_{k=-3}^{-3} \beta_k \left(Homebuyer_i \times D_{it}^k \times Post_t \right)$ obtained from the regression models Equation 2 and Equation 3 in an event study design. The x-axis indicates the years relative to the home purchase, which is 2009 for the 2009 cohort and 2013 for the 2013 cohort. The dependent variable is *consumption/income* ratio. The figure uses the matched sample. Standard errors are clustered at the labor market region level, and the bars indicate 95% confidence intervals.



We next turn to the post-purchase period. Figure 6 and columns (2) and (3) of Table 14 show that households purchasing a home after the introduction of the LTV restriction maintain lower spending after the home purchase than households that purchased before the restriction. This pattern is already visible when we use the consumption-to-income ratio, indicating that the two cohorts do not converge after the purchase. Because the LTV restriction requires a higher down payment, however, households purchasing after the policy also obtain smaller mortgages and therefore face lower mortgage payments. To account for this difference, we construct annual mortgage payments using mortgage interest rates reported by Statistics Norway (Table 07200). We assume a standard mortgage maturity of 25 years with full amortization and compute annual mortgage payments based on the initial mort-

gage amount, which we proxy using the change in total household debt in the year of home purchase.[27]

Table 14: **Consumption reaction of homebuyers**

This table shows how homebuyers adjust their consumption around a home purchase before and after the introduction of the LTV ratio restriction. The table uses household-level data and the dependent variable is *consumption/income* ratio. $Homebuyer_i$ is a dummy variable that takes the value of one for households who purchase their first home in 2009 or in 2013. $Prepurchase_{it}$ is a dummy variable that takes the value of one for the three years before the home purchase and zero in the purchase year. $Postpurchase_{it}$ is a dummy variable that takes the value of one for the three years after the home purchase and zero in the purchase year. $Post_t$ is a dummy variable that takes the value of one for years after 2010 in the pre-purchase analysis and equal to one for years after 2013 in the post-purchase analysis. Fixed effects and estimation sample are indicated at the bottom of each column. Standard errors are clustered at the labor market region level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

	Consumption/Income		(Cons.+Mor.)/Income
	(1)	(2)	(3)
Homebuyer × Prepurchase × Post	-0.0602***		
	(0.00844)		
Homebuyer × Prepurchase	0.000228		
	(0.00398)		
Homebuyer × Post	0.0459***		
	(0.00785)		
Homebuyer × Postpurchase × Post		-0.0177**	-0.0250***
		(0.00734)	(0.00753)
Homebuyer × Postpurchase		-0.0149***	0.0164***
		(0.00468)	(0.00473)
Homebuyer × Post		0.0450***	0.0722***
		(0.00721)	(0.00751)
Fixed Effects & Matching:			
Year FE	✓	✓	✓
Household FE	✓	✓	✓
Matching	✓	✓	✓
Obs.	4,434,946	4,340,866	4,340,866
R^2	0.483	0.500	0.500
Mean(Dependent Var.)	0.658		

[27]We adjust consumption only for homebuyers, since homeowners in our control group purchased their homes before the beginning of our sample, and their mortgage information is therefore not observed. Because our empirical specification uses a triple-difference design, the absence of mortgage information for homeowners should not affect the identification of the coefficients of interest.

Column (3) of Table 14 reports the results when the dependent variable is defined as cash outflows, measured as consumption plus mortgage payments divided by income. Once mortgage payments are incorporated, the post-purchase effect remains negative and statistically significant. Thus, although the higher down payment requirement creates additional consumption capacity through lower mortgage payments, households purchasing under the restriction do not use this capacity to raise spending. Instead, they continue to exhibit lower spending rates after the purchase, consistent with a more persistent increase in savings behavior. The fact that the post-purchase gap is already visible in consumption alone, and remains when mortgage payments are added, suggests that the lower spending of the 2013 cohort reflects a broader behavioral adjustment rather than merely the mechanical effect of lower debt service.

Table 15: **Why do homebuyers after the restriction have lower consumption?**

This table investigates the mechanisms for lower consumption of homebuyers purchasing under the LTV restriction. Colum (1) uses the change in deposits and financial assets to income ratio as the dependent variable. Column (2) uses the change in the deposits to income ratio as the dependent variable. Column (3) uses the change in financial assets to income ratio as the dependent variable. Column (4) uses an indicator variable for municipality change as the dependent variable. Column (5) uses an indicator variable for home purchases as the dependent variable. Column (6) uses the municipality-level income between 2006 and 2009 as the dependent variable. *Post* is a dummy variable that takes the value of one for homebuyers in 2013. The sample period is the post-home purchase period. The estimation sample is indicated at the bottom of each column. Standard errors are clustered at the labor market region level and reported in parentheses. *, **, and *** indicate significance at 10% level, 5% level, and 1% level, respectively.

	Δ (Dep.+Fin. Assets)/Income$_{t-1}$	Δ Dep./Income$_{t-1}$	Δ Fin. Assets/Income$_{t-1}$	Δ Municipality	$\mathbb{1}$(Home purch.)	log(Muni. income$_{06\text{-}09}$)
	(1)	(2)	(3)	(4)	(5)	(6)
Post	0.0681**	0.0252**	0.0429***	-0.00122	-0.00186	0.00360
	(0.0264)	(0.0123)	(0.0143)	(0.00296)	(0.00609)	(0.00312)
Sample:						
Matching	✓	✓	✓	✓	✓	✓
Obs.	6,254	6,254	6,254	4,584	6,712	6,712
R^2	0.001	0.001	0.002	0.000	0.000	0.000
Mean(Dependent Var.)	0.062	0.026	0.036	0.012	0.121	12.495

Why do homebuyers purchasing under the LTV restriction continue to exhibit lower post-purchase consumption? Several mechanisms may account for this behavior. First, a tighter down-payment requirement may induce households to rebuild liquid wealth after purchase, because a larger equity contribution leaves them with thinner liquidity buffers even if lower borrowing reduces subsequent mortgage payments. Second, these households may save more in order to climb the housing ladder after being forced to purchase a smaller or lower-quality home. Third, they may purchase in cheaper locations, which could mechanically reduce their subsequent consumption. To distinguish between these mechanisms, Table 15 compares several post-purchase outcomes across the two homebuyer cohorts. We find the strongest support for the liquidity-buffer channel. Relative to the 2009 cohort, homebuyers

in 2013 exhibit significantly larger increases in deposits and financial assets after purchase, indicating a faster rebuilding of liquid wealth. By contrast, we find no evidence that the post-restriction cohort is more likely to change municipality, purchase another home, or locate in municipalities with systematically different income levels. These results suggest that the persistently lower post-purchase consumption of homebuyers under the LTV restriction primarily reflects the rebuilding of liquidity buffers after making a larger down payment, rather than faster housing-ladder adjustment or differential sorting into cheaper locations. At the same time, the absence of significant effects on mobility or repeat purchases should be interpreted with caution, as our post-purchase window is limited to three years.

Taken together, these results show that the effects of macroprudential borrowing restrictions extend beyond their immediate impact on household leverage at the time of home purchase. While the restriction mechanically reduces mortgage borrowing through higher down-payment requirements, it also reshapes households' intertemporal consumption and saving decisions. Prospective homebuyers compress consumption before purchase in order to meet the tighter equity requirement, and this more conservative spending pattern persists after purchase even though lower mortgage balances create additional room for consumption. The post-purchase evidence indicates that households use part of this room not to raise spending, but to rebuild liquid wealth: relative to the pre-restriction cohort, homebuyers purchasing under the LTV cap accumulate deposits and financial assets more rapidly after purchase, with no corresponding evidence of faster housing-ladder adjustment or sorting into cheaper locations. These findings imply that borrower-based macroprudential policy can affect household consumption over a considerably longer horizon than is typically emphasized. They also contribute to the debate on liquidity depletion by suggesting that, while tighter down-payment requirements may erode liquid wealth on impact, households actively rebuild those buffers after purchase, so that the longer-run depletion effect may be smaller than previously thought (Aastveit et al., 2020; Bekkum et al., 2024). By changing both the timing of homeownership and the spending profile around purchase, these policies may therefore have broader consequences for aggregate demand and for the transmission of housing-market regulation to the macroeconomy.

5 Discussion

In this paper, we study how households who are not homeowners react to mortgage borrowing restriction with their consumption. Using an LTV ratio restriction and detailed data from Norway, we find that renters increase their consumption relative to the homeowners after the

introduction of the restriction. Our analysis regarding the underlying mechanism suggests that renters delay their home purchase as well as savings after the restriction. The reason is that purchasing a home under the LTV ratio restriction requires an increase in savings, which entails a costly deviation from consumption smoothing. To reduce this cost, renters wait until their income levels to be high enough to start their savings. On the contrary, the renters who purchase a home under the restriction reduce their consumption before the home purchase to accumulate savings for down payment. After the purchase, they keep their consumption low despite a lower mortgage payments thanks to the down payment.

These findings help to understand how macroprudential policies, one of the most popular financial stability tools across the world, influence the economy. So far, the discussions among the academicians and policymakers focus on the financial stability aspects of these policies. Yet, our results reveal that borrowing restrictions on households have far-reaching effects on the economy by altering the consumption decisions of households. The discussions about macroprudential borrowing restrictions should consider the effects on consumption documented in this paper as these effects could matter for welfare implications and financial stability concerns.

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Online Appendix for
"Down Payments, Deferred Homes: How LTV Restrictions Reshape Household Consumption"

Yann Cerasi Gazi Kabaş Steven Ongena Kasper Roszbach

A1 Detecting renters who do not prefer to purchase a home-XGBoost algorithm

In this section, we explain how and why we implement an XGBoost model for classifying whether households are likely to be future homebuyers or not. First, we justify our choice of classification model. Second, we go over the details of data collecting and model training. Last, we evaluate the model's performance.

XGBoost is an abbreviation of Extreme Gradient Boosting, and it is a popular supervised ML model thanks to its performance and versatility. The idea of gradient boosting in ML is to combine many weak models to create a collectively strong model. The XGBoost model is a special case of gradient boosting where the weak models are decision trees, and each new tree is trained to correct the errors of the previous trees. This iterative approach reduces overall errors and improves predictive accuracy. The XGBoost model excels at capturing complex and nonlinear interactions between independent variables. This is particularly beneficial for tabular data with many variables, as it can uncover patterns and relationships that standard regression models might miss.

In the data selection, we include as many variables as possible. Boosting models excel in handling extensive datasets with numerous features (independent variables), offering a significant advantage over standard regression models, as such models can suffer from issues like multicollinearity. We gather data from several population registers: tax records that includes wage, capital income, unemployment benefits, debt, bank deposits, financial assets and vehicle value. Family statistics include mean household age, immigration status, parental wealth, education and household size. All the data are aggregated to the household level.

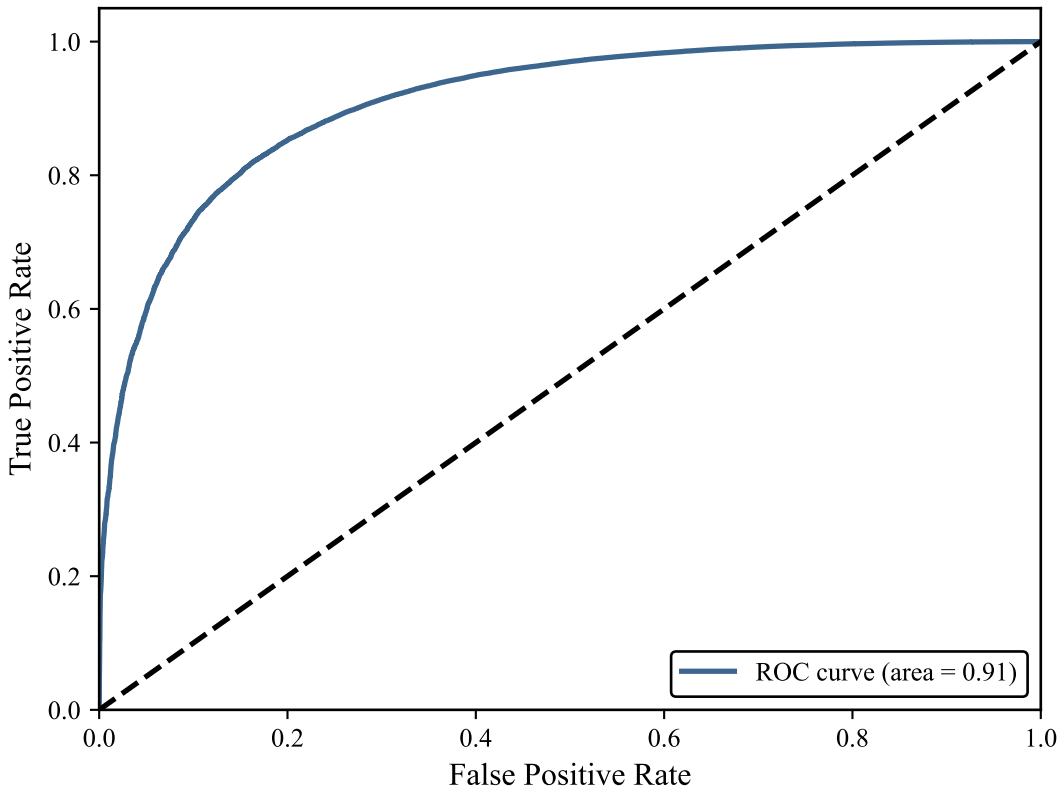
The main purpose of this exercise is to estimate renters willingness to become homeowners. To estimate this willingness clean from the influence of the LTV ratio restriction, we use the feature (independent) variables measured in 2006. The target variable is the binary variable, which takes the value of one for homeowners who are already homeowners in 2006, and zero for renters. 80% of

this data is used for estimating the XGBoost model, and the rest is later used for evaluation.

The following hyperparameters were selected based on test data accuracy from their respective parameter ranges: $learning\_rate = 0.1$ out of $\{1, 0.1, 0.01\}$, $n\_estimators = 100$ out of $\{10, 100, 1000\}$ and $max\_depth = 15$ out of $\{5, 15, 30\}$. Given these parameters, we fit the model to the training data.

Figure A1: **Receiver Operating Characteristic curve**

ROC (Receiver Operating Characteristic) curve plotting True Positive Rate against False Positive Rate for different model thresholds. A higher ROC curve area indicates better discriminative power.

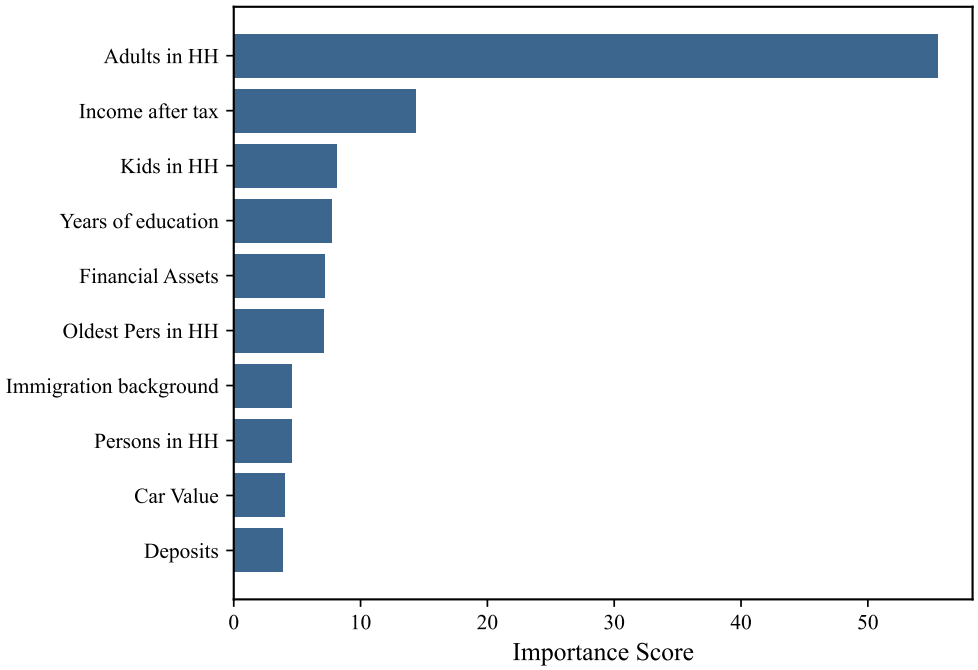


To evaluate the model's performance, we produce the ROC (Receiver Operating Characteristic). Given a binary classifier, this curve plots the true positive rate on the x-axis and the false positive rate on the y-axis at various classification thresholds. This metric shows how well the model distinguishes between classes across all thresholds. A successful classification model maintains a

higher true positive rate relative to the false positive rate as the probability threshold is lowered. In other words, the closer the ROC curve is to the top-left corner of the plot, the more successful the model is. The area under the ROC curve (AUC) provides a single measure of performance, with higher values indicating better discrimination. The curve is produced using test data, i.e., data that the model has not been directly fitted. In this way, the ROC curve evaluates how well the model generalizes to unseen data. The ROC curve generated using the XGBoost model in Figure A1 has an area of 0.91. This is considered a strong score in the ML literature.

Figure A2: **Feature importance curve**

Feature importance plot generated using the XGBoost model, highlighting the relative contribution of each feature to the model's predictions. Higher bars indicate greater importance of the corresponding feature.



Unlike regression models, an XGBoost model does not have parameters that can be directly interpreted. However, we can gain insights into the model's behavior by calculating feature importance, which highlights the most influential features in making predictions. Figure A2 confirms that the model behaves in line with our expectations. Figure A2 shows the feature importance for the

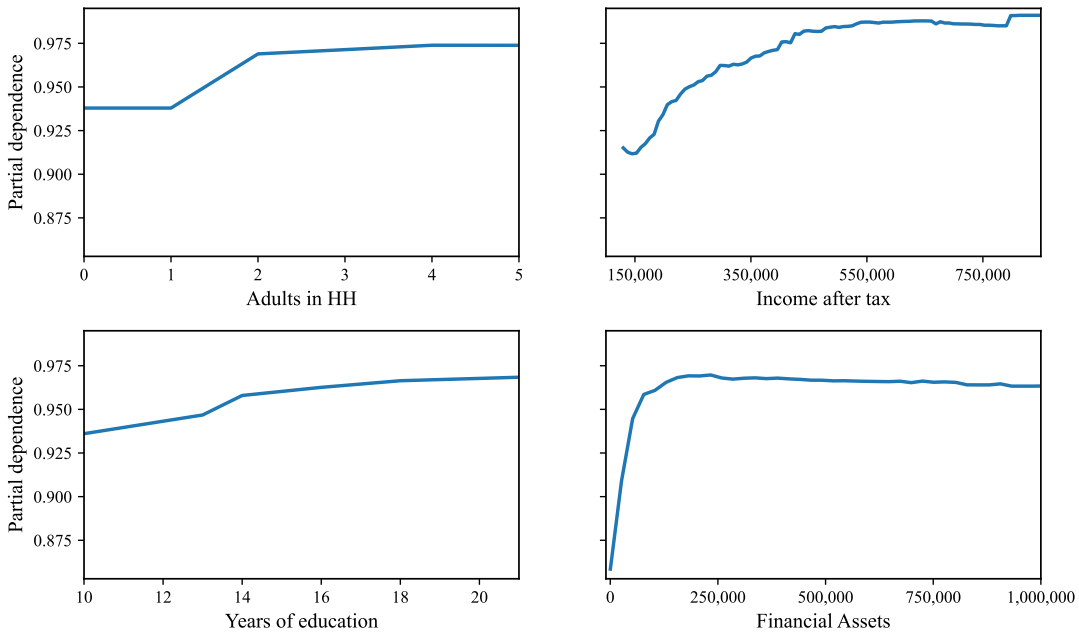
boosting model, which is computed by evaluating how much each feature improves the model's accuracy when this variable is used to split the data. These metrics collectively provide a comprehensive view of the relative significance of each feature in the model's predictions. Household size, income, financial assets and education are the most important variables. After fitting and evaluating the model, we apply it to all renters in our sample. We estimate the probabilities as of 2006 and have one probability per household.

Additionally, we can depict partial dependence plots to visualize the relationship between specific features and the predicted outcome, helping us understand how the model makes its decisions. In line with economic intuition, the probability increases with the number of adults in a household, income, years of education, and financial assets.

a

Figure A3: **ML Partial Dependence Plots**

This figure shows the partial dependence plots of four of the most important features from the ML model.



A2 Expenditure elasticities

In this section, we explain how consumption is categorized and how we estimate elasticities using these categories.

Consumption categorization We classify household expenditures into consumption types using the United Nations' 1999 *Classification of Individual Consumption According to Purpose* (COICOP). The construction follows the procedure in Ahn et al. (2024). Our electronic payments data contain two main underlying transaction sources: point-of-sale card payments and bank wire transfers. For point-of-sale transactions, the primary input is the Merchant Category Code (MCC) assigned to the payment terminal by the data provider. The MCC is a four-digit code used in retail financial services to classify merchants according to the main type of goods or services they provide. When the MCC is unavailable, we infer the consumption category from additional terminal metadata, most importantly the merchant's industry classification (typically the NACE code) or, when necessary, the merchant name. For bank wire transfers, we classify transactions using the industry code of the payment recipient. We then map these classifications into COICOP expenditure categories and aggregate them to the household-year level.

Measurement implications This classification procedure has two important implications. First, because merchants are assigned a single primary activity, expenditures made at multi-product retailers may be measured with error at the category level. For example, purchases of cleaning products at a grocery store are classified under food-related expenditure if the merchant is categorized primarily as a food retailer. This type of error is therefore most likely to arise when merchants sell a broad mix of goods that span multiple COICOP categories. Second, the underlying data do not contain the final merchant-level category of credit card purchases. Instead, credit card spending is observed only as payments to financial institutions and is therefore recorded as a separate category. Although this limitation is mitigated by the fact that debit cards are the dominant payment instrument in Norway, it may introduce measurement error for spending types that are disproportionately likely to be paid by credit card, such as travel-related or foreign expenditures. These features imply that category-level expenditure measures are inevitably noisier than total expenditure. For this reason, we estimate expenditure elasticities directly from the data rather than imposing ex ante

assumptions about which consumption categories should be classified as necessities or luxuries.

Estimating expenditure elasticities across consumption types To characterize how the composition of spending varies with household resources, we estimate category-specific Engel curves using budget shares. Following the demand-system literature (e.g., Deaton and Muellbauer, 1980; Aguiar and Bils, 2015; Bachas et al., 2024), we run the following regression separately for each consumption category c:

$$share_{ic} = \beta_c \log(C_i) + X_i' \Gamma_c + \varepsilon_{ic}, \tag{4}$$

where $share_{ic}$ denotes category c's share in household i's total consumption, C_i is total household consumption, and X_i includes household-size, municipality, age, and education fixed effects. In this specification, β_c captures the slope of the Engel curve for category c. We then convert the estimated Engel-curve slope into an expenditure elasticity using

$$\varepsilon_c = 1 + \frac{\beta_c}{\bar{s}_c}, \tag{5}$$

where $\bar{s}_c$ is the average budget share of category c in the relevant estimation sample. Intuitively, categories with $\varepsilon_c > 1$ receive a more than proportional increase in spending as total household consumption rises and therefore behave as luxuries, whereas categories with $\varepsilon_c < 1$ behave as necessities.

We estimate these elasticities both in the pre-restriction cross-section, using 2009 only, and in the full sample over 2006–2018, and report the results in Table A1. Two patterns stand out. First, the estimated shares and elasticities are highly stable across the two samples, indicating that the cross-sectional ranking of consumption types is not driven by a particular year. Second, the classification of categories is economically intuitive for most expenditure groups. Spending on clothing, vehicles, hotels, personal care, and several recreation categories exhibits elasticities at or above one, consistent with these goods being more discretionary. By contrast, categories such as communications, education, social services, transport services, and public-sector payments have elasticities below one, in line with necessity-type expenditure.

One notable difference relative to the existing survey-based literature is the food category, whose

estimated elasticity is slightly above one in our data. A likely explanation is that our category assignment is based on merchant-level information rather than product-level scanner or diary records. In particular, large grocery stores and supermarkets in Norway are classified primarily as food retailers even though they also sell a broad set of non-food items, including household goods, personal-care products, small durables, seasonal items, and other higher-income purchases. Expenditures on these goods are therefore recorded under the food category whenever they are purchased at merchants whose primary classification is food retail. This generates a composition effect that mechanically raises the estimated food elasticity relative to studies based on finer product-level expenditure records, in which food purchased for home consumption is typically isolated more cleanly and tends to have an elasticity below one. In our setting, the food category should therefore be interpreted as spending at food-oriented retailers rather than as a narrow measure of food-at-home expenditure. This distinction is important for interpreting the elasticity estimates, but it does not affect our broader objective, which is to use the observed transaction data to classify spending categories in a manner that is internally consistent with the structure of our consumption measure.

Table A1: **Estimated expenditure elasticities by consumption type**

This table reports estimated expenditure elasticities by consumption type. For each consumption category, the elasticity is obtained from a regression of the category's expenditure share on total household consumption, illustrated in Equation 4. The first two columns report the shares and elasticities using observations in 2009. The last two columns report the shares and elasticities using observations between 2006 and 2018. The control variables are the number of people in the household as fixed effects, municipality fixed effects, age fixed effects, and education fixed effects. Consumption categories are listed in alphabetical order.

	2009		All years	
Category	Share	Elasticity	Share	Elasticity
Alcohol&Tobacco	0.021	1.125	0.017	1.268
Clothing	0.059	1.056	0.054	0.966
Communications	0.072	0.471	0.065	0.453
Education	0.004	0.835	0.004	0.804
Food	0.241	1.066	0.244	1.041
Furnishing	0.064	1.116	0.060	1.031
Health	0.038	0.904	0.043	0.922
Hotels	0.005	1.030	0.006	1.051
Newspapers&Books	0.002	1.196	0.002	1.127
Personal Care	0.013	1.129	0.015	1.139
Personal Effect	0.010	1.040	0.010	0.930
Public Sector Payments	0.069	0.788	0.073	0.822
Recreation-Audio&Visual	0.036	2.041	0.038	1.980
Recreation-Other	0.007	0.910	0.009	0.922
Recreation-Outdoor	0.035	1.007	0.045	1.100
Recreation-Service	0.010	0.773	0.011	0.658
Restaurants	0.026	0.997	0.031	1.114
Social Services	0.005	0.538	0.005	0.604
Transport	0.039	0.906	0.040	0.909
Transport-Service	0.027	0.725	0.025	0.688
Utilities	0.159	1.130	0.151	1.085
Vehicles	0.040	1.151	0.037	1.102

A3 Additional tables and graphs

Figure A4: **Dynamic consumption reaction of renters**

This figure shows the dynamics of how renters react to the LTV ratio restriction with their consumption. The model includes age×deposit bins×financial asset bins×total wealth bins×parental wealth bins×year fixed effects, corresponding to the column (6) of Table 3. The dependent variable is the *consumption/income* ratio. The y-axis shows the βs of the regression model $consumption/income_{it} = \sum_{k=-4}^{8} \beta_k \; D_k \times Renter_i + controls_{it} + \epsilon_{ht}$. Baseline event period is $k = -1$. The regression model includes household-and year-fixed effects. Standard errors are clustered at the labor market region level, and the bars indicate 95% confidence intervals.

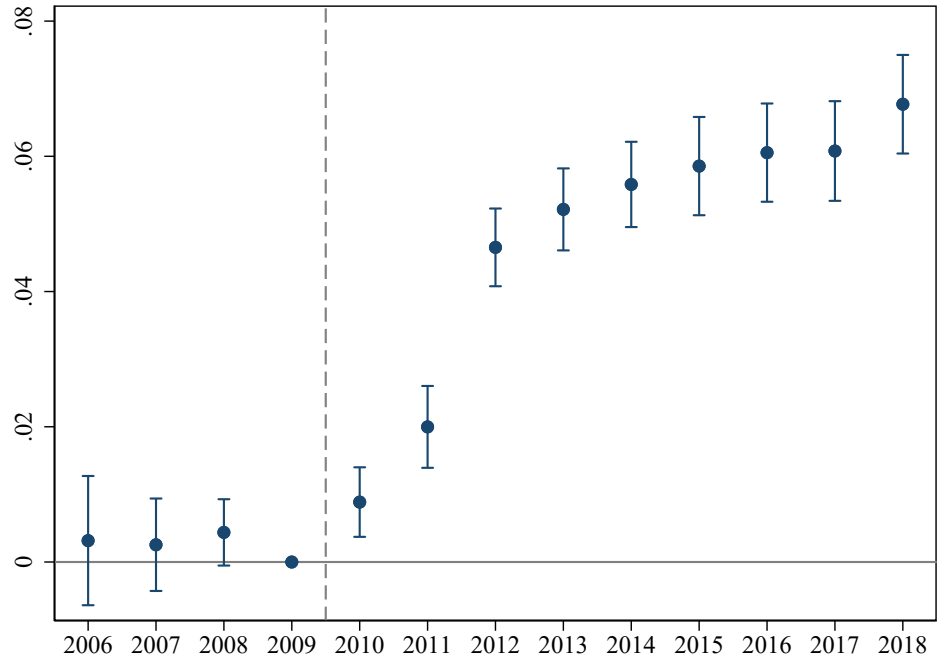


Figure A5: **Second home purchases of homeowners**

This figure shows the homeowners do not change their second home purchase probabilities after the LTV ratio restriction. The blue bars indicate the standard errors for each year. The horizontal red lines are mean values before and after the introduction of the LTV ratio restriction with standard errors.

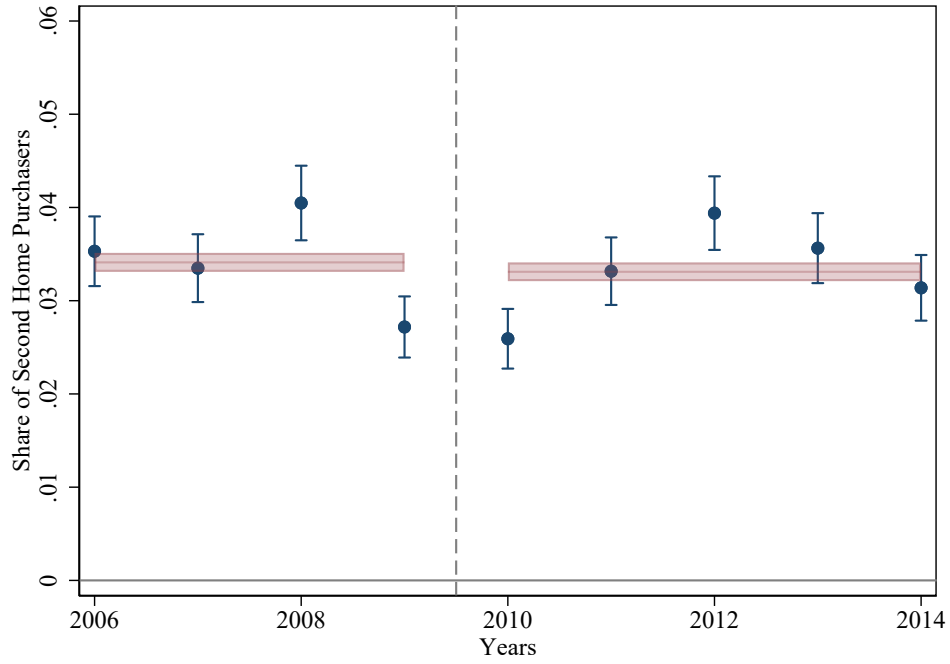


Figure A6: **House price index in Norway**

This figure shows the house price index in Norway.

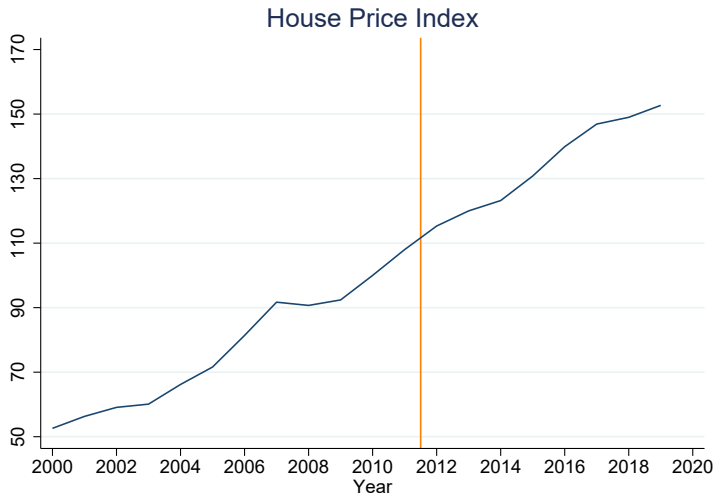


Figure A7: **Regional house prices in Norway**

This figure shows the growth rates of house prices in the largest nine regions in Norway.

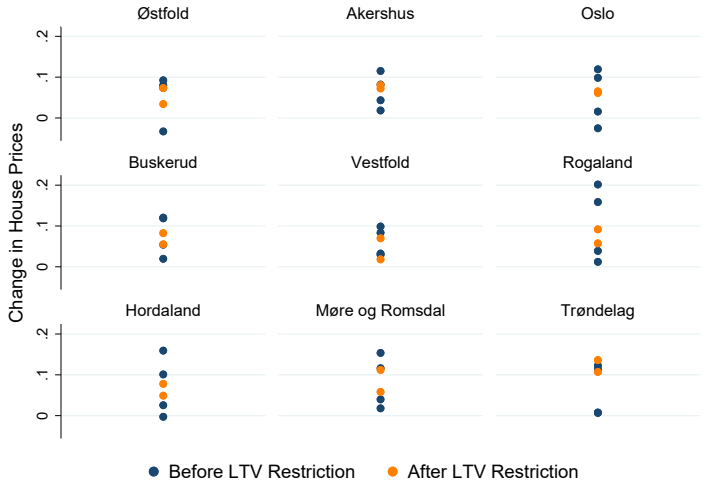


Figure A8: **Regional LTV distributions**

This figure shows the distribution of LTVs (%) $\in \{60, 110\}$ for Low mean LTV municipalities and High mean LTV municipalities.

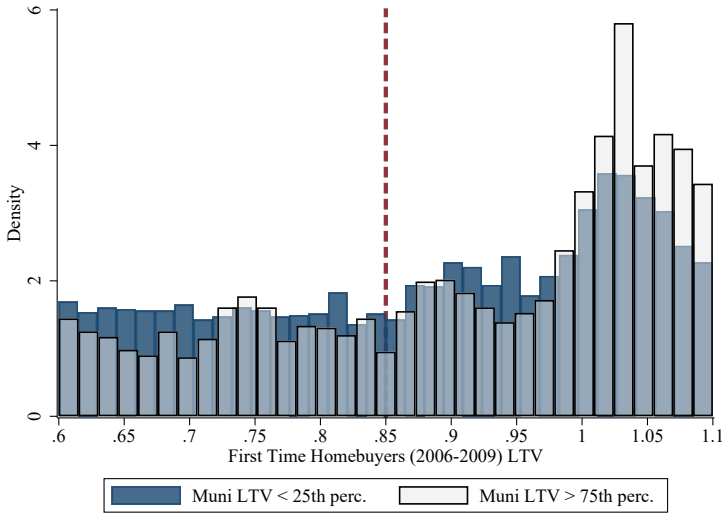


Table A2: **Balancedness table for homebuyers**

This table shows the performance of the matching for homebuyers in 2009 and 2013. $HB'09$ denotes the households who purchase their homes in 2009. $HB'13$ denotes the households who purchase their homes in 2013. The first four columns use the full sample. The last four columns use the matched sample. Income, parental wealth, and debt are used as percentiles, calculated by using the whole population. The values for $HB'09$ are as of 2006 and 2009 for $HB'13$.

	All sample				Matched sample			
	HB'09-Mean	HB'13-Mean	Diff.	p-value	HB'09-Mean	HB'13-Mean	Diff.	p-value
Income perc.	35.271	44.152	-8.881	0.000	42.099	44.152	-2.052	0.200
Parental Wealth perc.	38.814	41.579	-2.765	0.063	41.261	41.579	-0.318	0.878
Debt perc.	31.796	46.767	-14.972	0.000	42.497	46.767	-4.270	0.035
Age	32.171	35.569	-3.398	0.000	34.677	35.569	-0.892	0.223
Immigration st.	0.233	0.242	-0.009	0.663	0.210	0.242	-0.033	0.253
College	0.554	0.588	-0.035	0.161	0.554	0.588	-0.035	0.302
# of Children	0.191	0.240	-0.049	0.014	0.207	0.240	-0.033	0.240
# of Adults	1.611	1.563	0.047	0.205	1.594	1.563	0.031	0.509