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

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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 or give up 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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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 current 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), as well as alter locational choices (Tzur-Ilan, 2023). 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. We

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 invoicing transactions 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 3.3 percentage points, corresponding to a 6 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 bank 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. 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. Finally, we examine renters' mobility and location choices. We find that renters become less likely to relocate after the restriction, consistent with delayed transitions into homeownership. At the same time, renters become slightly more likely to reside in municipalities with higher average income levels, suggesting that renters who postpone home purchases remain longer in urban or higher-amenity locations. However, quantitative calculations indicate that differences in local price levels associated with this location sorting can explain only a small fraction of the increase in consumption. Taken together, these findings support the interpretation that the LTV restriction increases renters' consumption primarily by delaying home purchases and the associated savings, rather than through 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.

We further exploit the granularity of our consumption data to study how renters reallocate spending across consumption categories. We find that the share of non-durable consumption increases significantly, indicating that renters expand everyday consumption rather than shifting spending toward long-lived goods. In addition, the share of credit card expenditures rises, suggesting that renters partly finance higher consumption through short-term borrowing instead of increasing savings for the required down payment. 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. To account 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 their consumption relative to the purchase year significantly more than the 2009 cohort in the three years preceding the purchase, indicating that these households increase their savings to accumulate the higher down payment required by the restriction. After the purchase, the two cohorts initially appear to exhibit similar consumption levels. However, because households purchasing under the restriction make larger down payments and therefore face lower mortgage payments, they generate additional capacity for consumption. Once mortgage payments are incorporated into household cash outflows, we find that the 2013 cohort maintains significantly lower spending after the purchase, suggesting that these households do not use the additional capacity to increase consumption. Instead, they maintain higher savings rates even after becoming homeowners. In a robustness check, we address the selection effect induced by the LTV restriction by performing a matching exercise between the 2013 and 2009 homebuyer cohorts based on characteristics associated with saving capacity, including income, parental wealth, debt, and demographics. 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 are an important driver of expenditure dynamics: home purchases induce sizable increases in spending on home-related durables, renovations, and repairs, implying that transaction

[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, 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.

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 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

[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

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, mortgages, 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 can be reset to market levels when a new contract is signed. As a result, 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

[8]Fixed-rate mortgages are also available

occurred in 2014, when oil prices fell sharply, disproportionately affecting regions with strong exposure to the petroleum sector. Overall, macroeconomic conditions remained relatively stable during our sample period.

Data sources Our analysis combines several detailed administrative datasets covering the universe of Norwegian residents. The data are collected by the Norwegian tax authorities and maintained by Statistics Norway. Because Norway levies both an income tax and a wealth tax, residents are required to report detailed information on their income and asset holdings. Much of this information is automatically reported by financial institutions and other third parties and pre-filled in tax filings, which ensures a high degree of accuracy. The administrative registers also contain detailed information on individuals' demographic characteristics, income, assets, and liabilities, making them well suited to study household financial decisions.

The population registers span the years 1993–2018 and provide information on individuals' location, household composition, and demographic characteristics, including age, gender, education, immigration background, and marital status. The tax records include detailed balance sheet information such as income, wages, bank deposits, financial assets, housing wealth, and debt. These datasets can be linked using unique anonymized personal identifiers that allow us to follow individuals over time. In addition, the data include family and household identifiers that allow us to link individuals to their parents and construct households.

We complement the administrative records with data from the Norwegian housing transaction registry, which contains the universe of housing transactions between 1980 and 2016. The registry records transaction dates, prices, location, and detailed housing characteristics, as well as 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.

To measure household consumption, we use transaction data from the Norwegian payments database, which records debit card transactions processed through the national payment system as well as bank transfers cleared through the Norwegian interbank clearing system. The database covers approximately 80% of all card and bank wire transactions for Norwegian residents and therefore provides a comprehensive measure of household expenditure in a largely cashless economy (Ahn et al., 2024). Transactions are aggregated at the individual level and classified according to COICOP consumption categories, which allows us

to decompose spending across different consumption categories. The payments data span the period 2006–2018 and are aggregated to the yearly level in our analysis. 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.

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.[9] This definition excludes individuals who may have received a 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.[10]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. 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.

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, Finanstil-

[9]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.

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

Table 1: **Summary statistics**

This table provides summary statistics of the main variables.

	Renters		Homebuyers		Homeowners	
	Mean	SD	Mean	SD	Mean	SD
Total Consumption	180,000	120,000	315,000	191,000	344,000	207,000
Durables	26,497	43,396	63,521	87,088	80,228	97,937
Nondurables	50,142	44,698	85,941	57,615	89,763	63,300
Income	309,000	158,000	521,000	244,000	527,000	251,000
Consumption/Income	0.59	0.27	0.62	0.28	0.67	0.32
Deposits	191,000	380,000	207,000	360,000	397,000	612,000
Financial Assets	227,000	465,000	278,000	536,000	539,000	839,000
Debt	229,000	617,000	1,390,000	1,220,000	956,000	1,080,000
Net Savings	-19,400	772,000	-1,190,000	1,270,000	-438,000	1,420,000
Age	54.41	17.72	38.65	11.60	54.23	16.03
Immigrant share	0.13	0.33	0.19	0.36	0.08	0.24
Education (Years)	13.13	3.14	16.23	3.31	15.41	3.25
College Degree (=1)	0.23	0.42	0.57	0.50	0.46	0.50
Obs.	518,450		407,573		7,122,975	

synet (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.[11]

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. $Renter_i$

[11]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 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.

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.[12] 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.[13]

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, 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 the renters, we analyze the households who transition to homeowners from renters under the LTV ratio restriction, whom we refer to as homebuyers. This exercise presents three 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 who purchase a home in 2013, referred to as the 2013 cohort, which serves as the treatment group. This

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

[13] 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.

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.

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

A potential concern in this analysis is selection into homeownership. As shown in the renter analysis, the LTV restriction 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 assess the importance of this concern, we perform a matching procedure between the 2013 and 2009 cohorts based on characteristics associated with saving capacity, as described in subsection 4.4, and discuss the implications of the selection for our findings.

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 3.31 pp after the restriction. As the sample mean of *consumption/income* ratio is 66 pp, this magnitude indicates a 5 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 0.7 percent, while decreasing the magnitude of the effect modestly—to 3.31 pp from 3.52 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 indicates that the bias-adjusted coefficient is 0.028 under the assumption that 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 almost six times as strong as selection on observables.[14]

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 depicts that the difference between renters and homeowners is stable before the introduction

[14]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.

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.

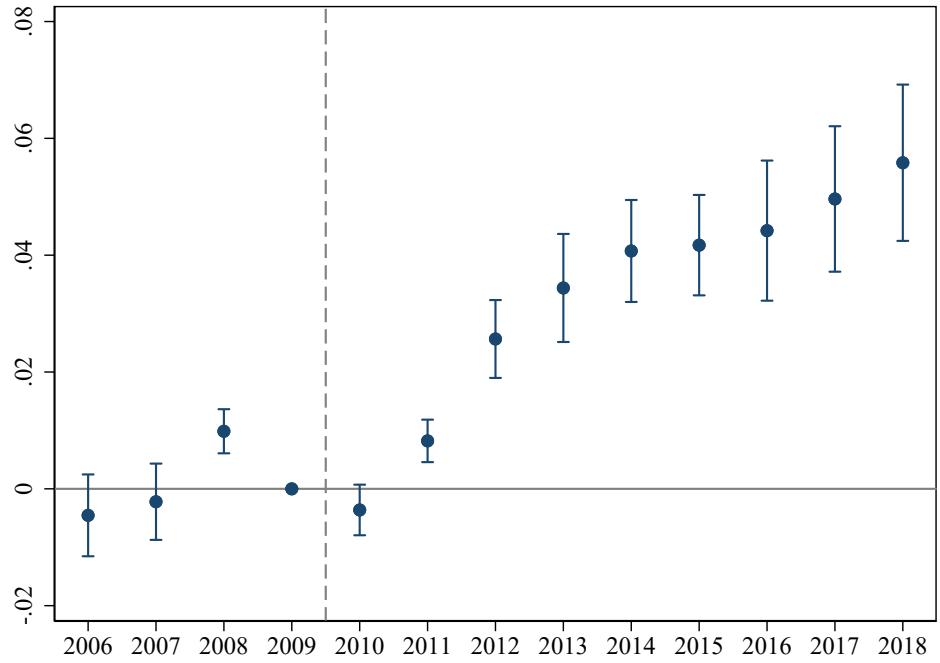
	Consumption/Income		
	(1)	(2)	(3)
Renter × Post	0.0370***	0.0352***	0.0331***
	(0.00379)	(0.00401)	(0.00369)
Renter	-0.0970***	-0.0951***	
	(0.00351)	(0.00371)	
Post	-0.0170***		
	(0.00620)		
Fixed Effects:			
Year FE		✓	✓
Household FE			✓
Obs.	7,707,415	7,707,415	7,707,415
R^2	0.004	0.007	0.414
Mean(Dependent Var.)	0.660		

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 slightly above 4 pp.

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 × 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

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.



values. Then, to compare renters and homeowners in the same bins over the years, we include bins × 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.[15] 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.

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

[15]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 parallel trends assumption.

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	0.0352***	0.0342***	0.0315***	0.0435***	0.0327***	0.0430***
	(0.00359)	(0.00393)	(0.00396)	(0.00422)	(0.00341)	(0.00341)
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.	7,707,415	7,707,415	7,707,415	7,707,415	7,707,415	7,585,892
R^2	0.415	0.415	0.415	0.416	0.415	0.447
Mean(Dependent Var.)	0.658					

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, enabling us to calculate the current LTV ratios of homeowners.[16] 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 90, 75, 60, and 50 percent, and then estimate the main regression model. In the first four columns of Table 4, we report the coefficients, demonstrating that the coefficients stay the same.[17]

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

[16]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.

[17]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.

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 × 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 effect of the LTV ratio restriction on homeowners.

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<90%	LTV<75%	LTV<60%	LTV<50%	
Renter × Post	0.0198***	0.0183***	0.0164***	0.0148***	0.0339***
	(0.00323)	(0.00300)	(0.00271)	(0.00260)	(0.00350)
Fixed Effects:					
Household FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	
Municipality × Year FE					✓
	6,163,551	5,748,093	5,172,700	4,717,126	7,707,415
R^2	0.438	0.445	0.454	0.461	0.419
Mean(Dependent Var.)	0.658				

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

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.

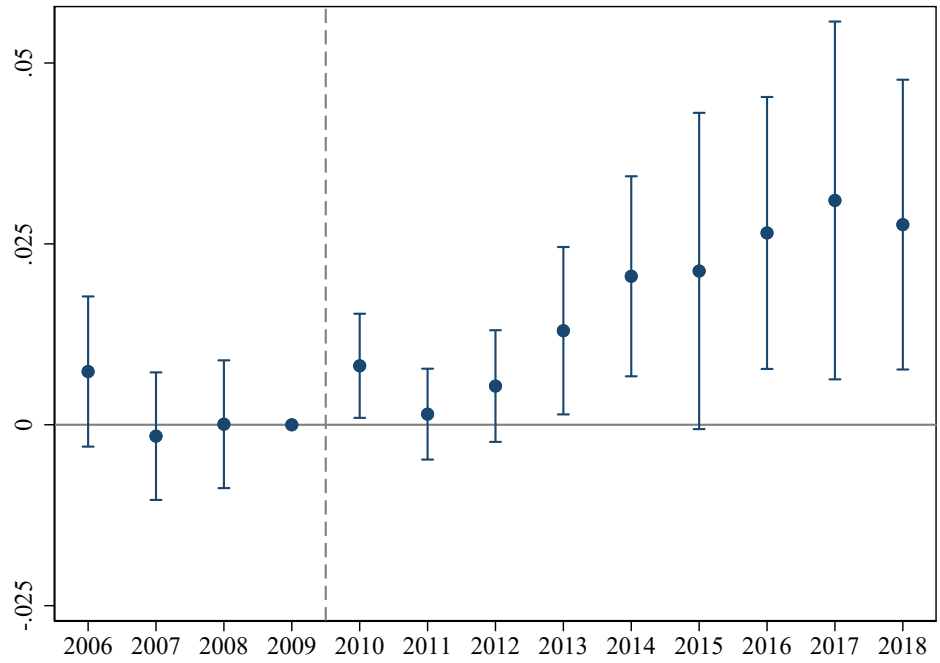
	Consumption/Income			
	(1)	(2)	(3)	(4)
1 - Homeowner prob. × Post	0.0409***	0.0308***		
	(0.00616)	(0.00759)		
$\mathbb{1}$(Homeowner prob.$<80_{pct}$) × Post			0.0343***	0.0159***
			(0.00444)	(0.00474)
Fixed Effects:				
Year FE	✓	✓	✓	✓
Household FE	✓	✓	✓	✓
Matching		✓		✓
Obs.	645,452	240,841	645,452	256,382
R^2	0.448	0.431	0.449	0.431
Mean(Dependent Var.)	0.599			

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, 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 the inverse of 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 inverse of the continuous predicted probability with the post-restriction indicator, allowing

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.



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 lies below the 80th 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 exercise 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 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

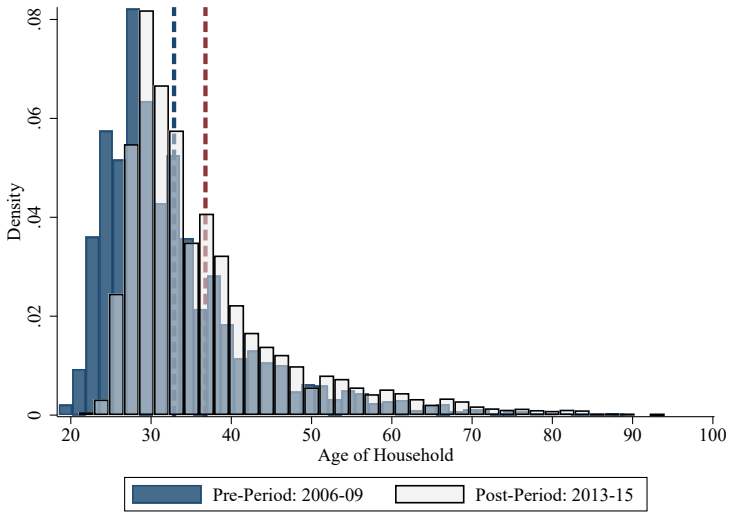
increase in renters' consumption does not hinge on comparisons with homeowners, but also arises when identification is based solely on ex-ante heterogeneity within renters.

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 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 consumption smoothing is sufficiently high. In this section, we present empirical evidence that supports the latter mechanism.

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.



To assess the mechanisms, 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.

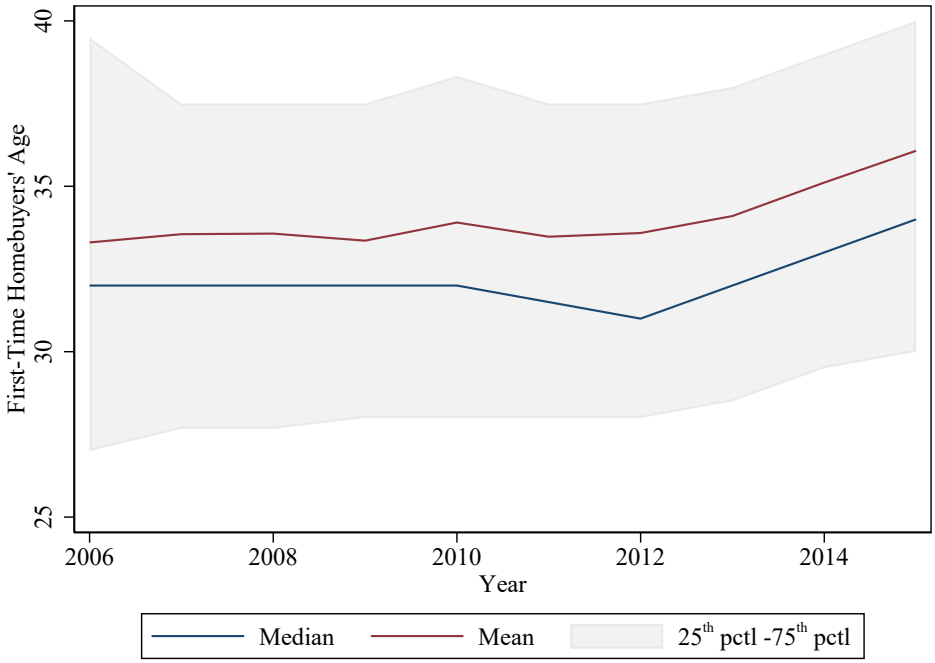
Figure 4 demonstrates the age distribution of first-time homebuyers over time. Importantly, the age distribution prior to the restriction is stable, suggesting that the subsequent increase is driven by the introduction of the LTV ratio restriction. Notably, the increase is gradual rather than a discontinuous 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.[18]

Even though the delay in transitioning to homeownership is evident, this delay does not necessarily indicate a delay in savings and higher consumption, as renters may increase their savings and decrease their consumption to reduce the delay in home purchase. Therefore, to further examine whether renters delay savings in anticipation of higher future income, we investigate heterogeneity with respect to households' earnings potential gap prior to the reform. The mechanism implies that renters who expect substantial income growth have a stronger incentive to postpone saving for a down payment, since waiting allows them to accumulate savings at a lower utility cost once income rises. To proxy for potential income, we partition the population into cells defined by immigration background, labor market region, and education, and compute the mean income of the top 10 percent earners within each cell. We define the earnings potential gap as the difference between potential income and the household's 2009 income. This measure captures how far a household is from the upper end of the income distribution among comparable individuals. For instance, the gap amounts to 354,000 NOK for teacher school graduates, 595,000 NOK for business

[18]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.



administration graduates, and 615,000 NOK for dental school graduates, suggesting that the measure reflects the occupations' earnings profiles quite well.

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 1.5 pp in the lowest quartile to 4.8 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 (four-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.

	Consumption/Income				
	(1)	(2)	(3)	(4)	(5)
	Above top 10%	1^{st} Quart.	2^{nd} Quart.	3^{rd} Quart.	4^{th} Quart.
Renter × Post	0.00321	0.0152***	0.0240***	0.0450***	0.0481***
	(0.00691)	(0.00329)	(0.00400)	(0.00568)	(0.00300)
Fixed Effects:					
Year FE	✓	✓	✓	✓	✓
Household FE	✓	✓	✓	✓	✓
Obs.	687,301	1,838,144	1,967,037	1,948,125	1,724,505
R^2	0.350	0.349	0.398	0.438	0.447
Mean(Dependent Var.)	0.660				

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 small 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.

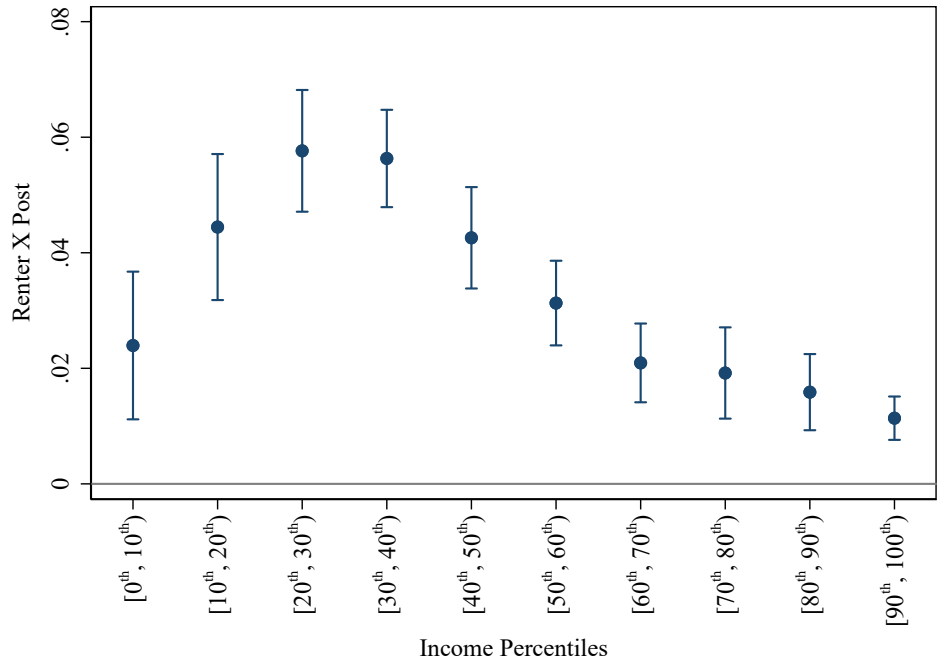


Table 7 provides additional support for the proposed mechanism by exploiting regional variation in pre-restriction leverage. We classify municipalities according to the average LTV ratios of first-time homebuyers in the pre-restriction period and find that the increase in renters' consumption after the 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**			
	(0.00171)			
Renter × Post	0.0348***	0.0257***	0.0324***	0.0423***
	(0.00295)	(0.00520)	(0.00436)	(0.00297)
Post × Municipality LTV	0.0134***			
	(0.00214)			
Fixed Effects:				
HH FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
Obs.	7,154,905	1,720,427	3,542,385	2,444,603
R^2	0.415	0.416	0.415	0.413
Mean(Municipality LTV)	0.923	0.810	0.921	1.030
Diff. to MuniLTV > 75th Coeff		0.0162**	0.0094*	
Diff. to MuniLTV < 25th Coeff			-0.0069	

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 1.6 percentage points, and this difference is statistically significant. Notably, the mean pre-restriction LTV ratio differs by 19 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.

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

Table 8: **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

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 8 shows that renters' relocation probability declines by 2.6 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.

We also find that, after the restriction, renters become slightly more likely to reside in municipalities with higher income levels. Columns (2) and (3) of Table 8 indicate that the average municipality income of renters increases by approximately 0.6 percent when measured using the median income and 0.3 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 9. 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.

Table 9: *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.

	ln(income)			ln(consumption)		
	(1)	(2)	(3)	(4)	(5)	(6)
Renter × Post	0.0528***	0.0559***	0.0348***	0.114***	0.114***	0.0890***
	(0.00534)	(0.00559)	(0.00565)	(0.00495)	(0.00513)	(0.00547)
Renter	-0.470***	-0.474***		-0.624***	-0.626***	
	(0.00916)	(0.00899)		(0.0137)	(0.0139)	
Post	0.225***			0.216***		
	(0.00187)			(0.0101)		
Fixed Effects:						
Year FE		✓	✓		✓	✓
Household FE			✓			✓
Obs.	7,707,415	7,707,415	7,707,415	7,707,415	7,707,415	7,707,415
R^2	0.099	0.116	0.861	0.072	0.080	0.754
Mean(Dependent Var.)	0.660			0.660		

Next, we explore renters' other balance sheet items in Table 10. 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 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.

Table 10: **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	0.242***	-0.125***	-0.165***	-0.407***
	(0.0159)	(0.00733)	(0.00515)	(0.0190)
Fixed Effects:				
Year FE	✓	✓	✓	✓
Household FE	✓	✓	✓	✓
Obs.	7,707,415	7,707,415	7,707,415	7,707,415
R^2	0.767	0.793	0.807	0.830
Mean(Dependent Var.)	1.689	0.834	1.101	-0.593

Lastly, we exploit the granularity of our consumption data and examine how renters reallocate their spending across consumption categories by using the share of each category in total consumption as the outcome variable. In Table 11, we find that renters increase their spending on non-durables, credit cards, and food significantly. The increase in the share of non-durable consumption suggests that renters adjust their short-run consumption, while the absence of an increase in durable spending suggests that the consumption increase is not driven by home-preparation expenditures, reinforcing the interpretation that renters postpone saving and home purchases. The higher food share suggests that renters increase routine consumption, consistent with the interpretation that they postpone saving and delay their home purchases. The increase in the share of credit card spending is also informative for the mechanism. In Norway, credit cards are not the dominant payment method and are typically used when households rely on short-term credit or installment payments. The higher credit card share therefore suggests that renters partly finance higher consumption through short-term borrowing rather than increasing savings for the down payment, reinforcing the interpretation that they postpone saving and delay home purchases.

Table 11: **Consumption types**

This table shows how renters adjust different types of consumption after the LTV ratio restriction. The dependent variables consumption category are indicated in each column and measured in shares of total consumption. *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.

	Durables	Non-durables	Furnishing	Vehicles	Luxury	Clothing	Restaurant	Credit Card	Food
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Renter × Post	-0.00164	0.0115***	0.000594	-0.00352***	-0.000862	-0.000125	-0.00195***	0.00400**	0.0142***
	(0.00276)	(0.00100)	(0.000440)	(0.000264)	(0.00174)	(0.000256)	(0.000375)	(0.00196)	(0.000910)
Fixed Effects:									
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Household FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Obs.	7,707,415	7,707,415	7,707,415	7,707,415	7,707,415	7,707,415	7,707,415	7,707,415	7,707,415
R^2	0.364	0.640	0.316	0.149	0.451	0.633	0.622	0.420	0.644
Mean(Dependent Var.)	0.209	0.262	0.047	0.034	0.185	0.042	0.023	0.063	0.204

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 12, 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 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.,

Table 12: **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	0.0412***	0.0417***	0.0444***	0.0465***	0.0514***
	(0.00433)	(0.00442)	(0.00472)	(0.00478)	(0.00522)
Renter × Δ GDP growth	-0.00629***	-0.00671***	-0.00859***	-0.00533***	-0.00321***
	(0.000522)	(0.000635)	(0.000867)	(0.000800)	(0.000576)
Renter × Δ Inflation		-0.000737**	-0.00250***	-0.00209***	0.00148***
		(0.000304)	(0.000456)	(0.000438)	(0.000284)
Renter × Δ Policy rate			0.00315***	0.000123	-0.00648***
			(0.000732)	(0.000814)	(0.000667)
Renter × Δ VIX				0.00107***	0.00142***
				(0.000123)	(0.000129)
Renter × Δ Unemployment rate					-0.0156***
					(0.00204)
Fixed Effects:					
Year FE	✓	✓	✓	✓	✓
Household FE	✓	✓	✓	✓	✓
Obs.	7,707,415	7,707,415	7,707,415	7,707,415	7,707,415
R^2	0.431	0.431	0.431	0.431	0.431
Mean(Dependent Var.)	0.658				

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.

Being an important event in household life cycles, purchasing a home has a direct effect on consumption. In the years preceding the purchase, households may postpone durable consumption and increase savings to prepare for move-related expenses. During the purchase year, households often incur additional expenditures related to moving or the replacement of durables, while after the purchase, they may invest in their homes through renovations

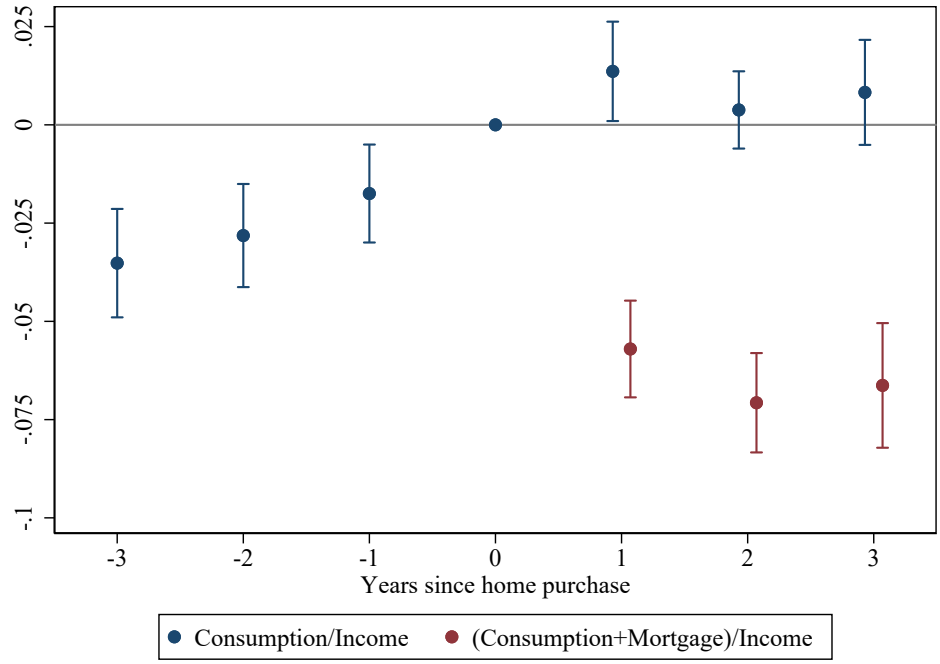
or furnishing (Benmelech et al., 2023). As a result, home purchase generates consumption dynamics that extend to the years surrounding the transaction. As our objective is to isolate the effect of the LTV ratio restriction rather than the direct effect of home purchase itself, our empirical strategy must account for these dynamics. To do so, we implement the triple-differences framework described in Section 3. In this framework, homebuyers who purchase in 2013, denoted as HB'13, serve as the treatment group, homebuyers who purchase in 2009, denoted as HB'09, serve as the first control group to absorb the consumption dynamics associated with home purchase, and homeowners serve as the second control group to difference out common time effects.

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 (HB'13) reduce their consumption more strongly in the years leading up to the purchase compared to households that purchased before the restriction (HB'09). The difference is largest three years prior to the home purchase and gradually declines as the purchase year approaches. This pattern suggests that households purchasing under the restriction begin adjusting their consumption well in advance in order to accumulate the higher down payment required by the LTV cap. Such gradual adjustments are consistent with forward-looking behavior in incomplete-markets models with borrowing constraints, where households smooth consumption by spreading the savings effort over multiple periods rather than concentrating it immediately before the purchase. As the purchase approaches, the gap narrows, potentially reflecting the emergence of move-related expenditures by the 2009 cohort. Column (1) of Table 13 confirms this finding in a 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 consumption-to-income ratio by an additional 2.7 percentage points in the three years preceding a home purchase compared to the 2009 cohort.

We next turn to the post-purchase period. Both Figure 6 and the column (2) of Table 13 show that the consumption levels of the two homebuyer cohorts are broadly similar once the home purchase has occurred. At first glance, this finding suggests that the consumption patterns of the two cohorts converge after the home purchase. However, this comparison does not account for differences in mortgage payments across the cohorts. Because the LTV restriction requires a higher down payment, households purchasing a home after the restriction obtain smaller mortgages and therefore face lower mortgage payments. To account

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 $Homebuyer_i \times Post_t \times Relative\ years_{it}$ 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 2009 for the 2009 cohort and 2013 for the 2013 cohort. The dependent variable is *consumption/income* ratio. Standard errors are clustered at the labor market region level and the bars indicate 95% confidence intervals.



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 mortgage amount, which we proxy using the change in total household debt in the year of home purchase.[19]

Column (3) of Table 13 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, we find that households purchasing under the restriction exhibit significantly lower cash outflows after the home purchase compared to the earlier cohort, around 6 pp. This suggests that although the higher down payment requirement generates additional capacity for consumption through lower mortgage payments, the 2013

[19]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.

Table 13: **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.

	Cons/Inc		(Cons+Mor)/Inc	Cons/Inc		(Cons+Mor)/Inc
	(1)	(2)	(3)	(4)	(5)	(6)
Homebuyer × Prepurchase × Post	-0.0265***			-0.0602***		
	(0.00530)			(0.00844)		
Homebuyer × Prepurchase	-0.0164***			0.000228		
	(0.00348)			(0.00398)		
Homebuyer × Post	0.0160***			0.0459***		
	(0.00546)			(0.00785)		
Homebuyer × Postpurchase × Post		0.00844*	-0.0646***		-0.0177**	-0.0250***
		(0.00491)	(0.00576)		(0.00734)	(0.00753)
Homebuyer × Postpurchase		-0.0271***	0.0553***		-0.0149***	0.0164***
		(0.00409)	(0.00424)		(0.00468)	(0.00473)
Homebuyer × Post		0.0163***	0.0910***		0.0450***	0.0722***
		(0.00535)	(0.00469)		(0.00721)	(0.00751)
Fixed Effects & Matching:						
Year FE	✓	✓	✓	✓	✓	✓
Household FE	✓	✓	✓	✓	✓	✓
Matching				✓	✓	✓
Obs.	4,445,253	4,351,295	4,351,295	4,434,946	4,340,866	4,340,866
R^2	0.483	0.500	0.500	0.483	0.500	0.500
Mean(Dependent Var.)	0.658					

cohort does not use this additional capacity to increase spending. Instead, these homebuyers maintain a higher savings rate even after purchasing a home, implying persistent effects on household behavior.

A potential concern in this analysis is selection into homeownership. As shown in our renter analysis, the LTV restriction delays home purchases for some households or even completely discourages some of them. Consequently, the households that succeed in purchasing a home after the restriction may be those with a greater ability to increase savings by reducing consumption. To assess the importance of this concern, we perform a matching exercise between the 2013 and 2009 homebuyer cohorts based on characteristics associated with saving capacity, such as income, parental wealth, and debt used as percentiles of the population, and

individual characteristics such as age, immigration status, and household size.[20] The balance statistics are reported in Table A1 and show that the matching procedure substantially reduces differences in observable characteristics across the two cohorts. Columns (4)–(6) of Table 13 report the corresponding regression results for the matched sample. The estimates remain similar to the baseline results, suggesting that selection into homeownership is unlikely to be the primary driver of the consumption patterns we document.

Taken together, these results suggest 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 induces households to adjust their intertemporal consumption and savings behavior. Prospective homebuyers increase savings prior to the purchase in order to accumulate the required down payment, and this higher saving behavior persists even after the home purchase, despite the additional consumption capacity generated by lower mortgage payments. This persistence implies that macroprudential borrowing restrictions may influence household consumption over a longer horizon than previously documented in the literature. By altering both the timing of home purchases and the consumption behavior surrounding them, these policies may therefore have broader implications for aggregate demand and the transmission of housing market regulations to the macroeconomy. In addition, these results suggest that the liquidity depletion effect of macroprudential borrowing restrictions may be limited as they increase the savings rate both before and after a home purchase (Aastveit et al., 2020; Bekkum et al., 2024).

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

[20] 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.

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

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.

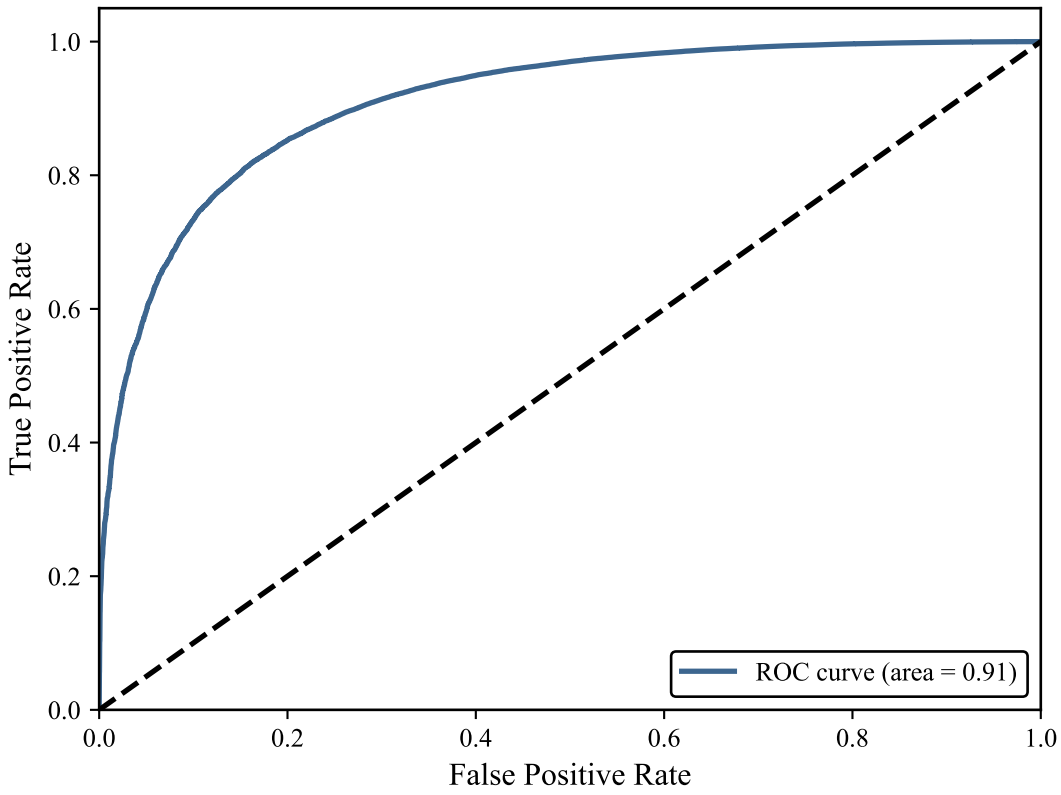
The main purpose of this exercise is to estimate renters willingness to become homeowners. To estimate this willingness clean from the influence of the LTV ratio restriction, we use the feature (independent) variables measured in 2006. The target variable is the binary variable, which takes the value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

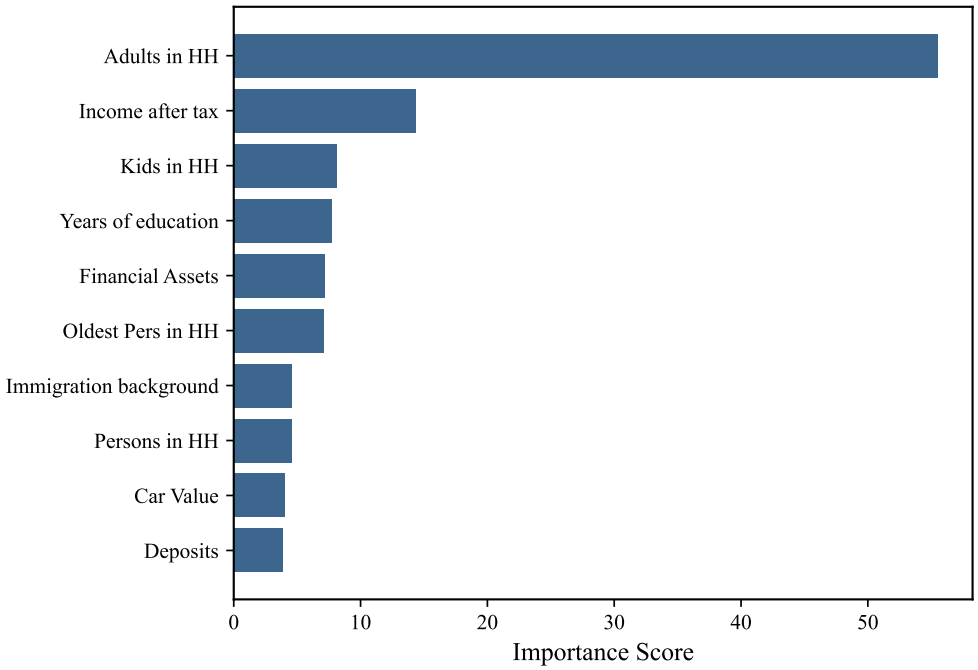


To evaluate the model's performance, we produce the ROC (Receiver Operating Characteristic). Given a binary classifier, this curve plots the true positive rate on the x-axis and the false positive rate on the y-axis at various classification thresholds. This metric shows how well the model distinguishes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

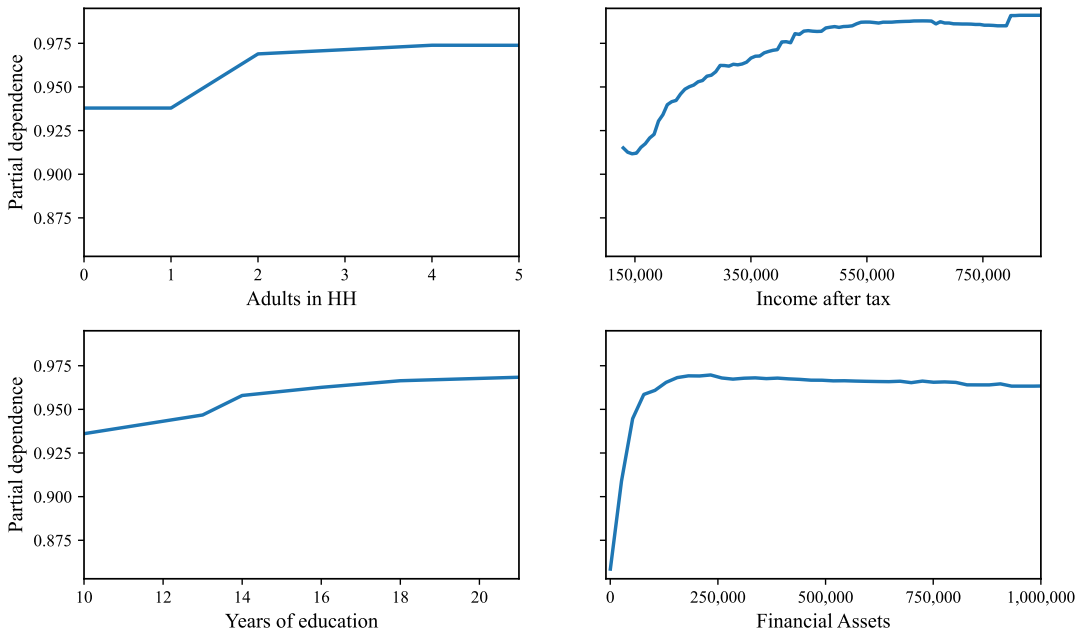
boosting model, which is computed by evaluating how much each feature improves the model's accuracy when this variable is used to split the data. These metrics collectively provide a comprehensive view of the relative significance of each feature in the model's predictions. Household size, income, financial 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.

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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 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.

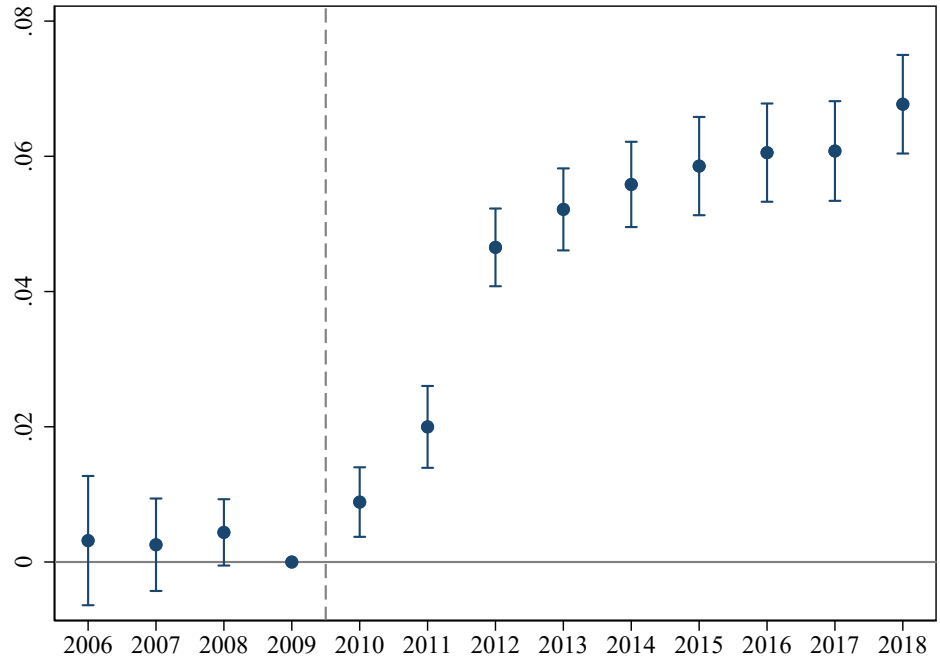


Figure A5: **Second home purchases of homeowners**

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.

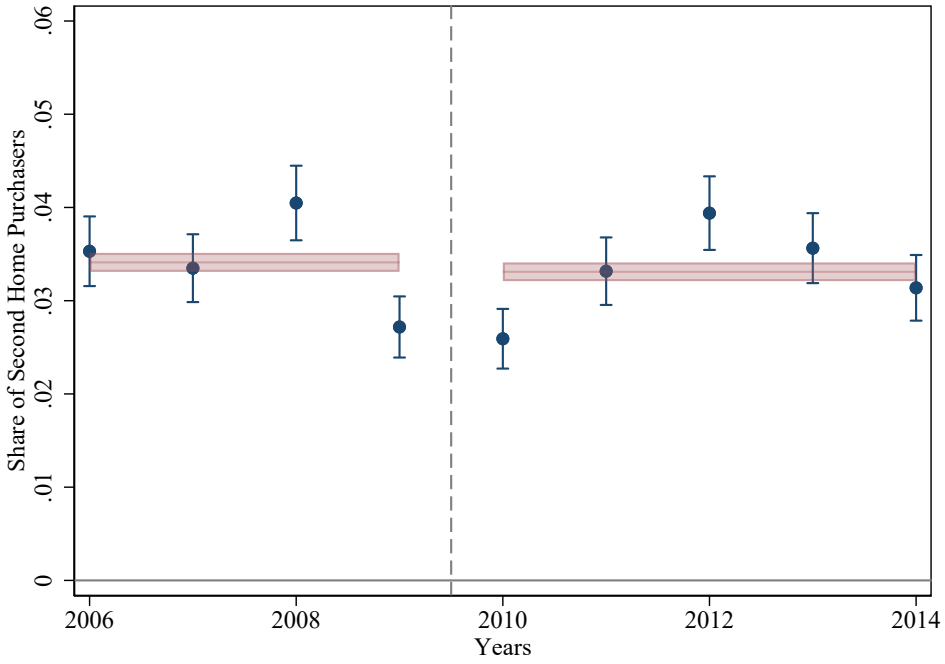


Figure A6: **House price index in Norway**

This figure shows the house price index in Norway.

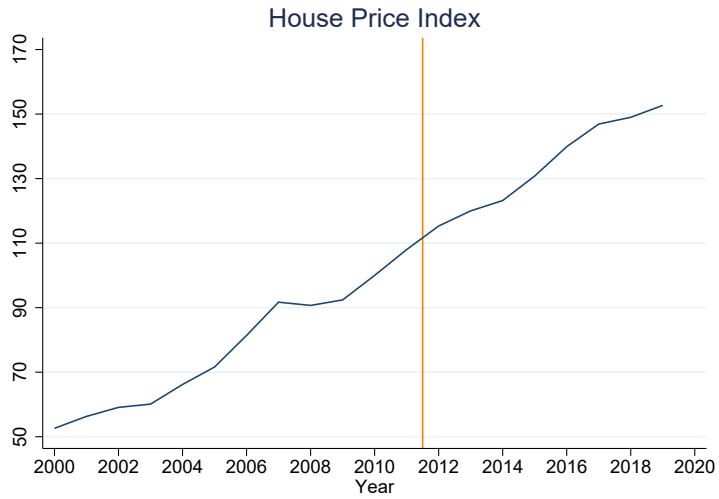


Figure A7: **Regional house prices in Norway**

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

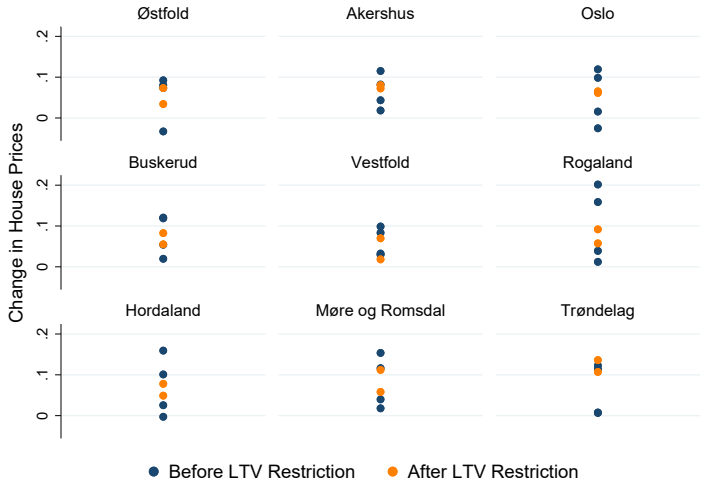


Figure A8: **Regional LTV distributions**

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

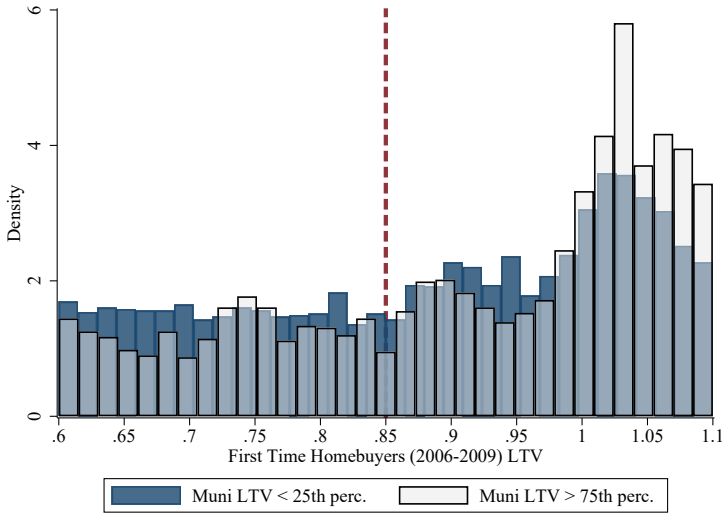


Table A1: **Balancedness table for homebuyers**

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