Norwegian University of Life Sciences
School of Economics and Business

Philosophiae Doctor (PhD)
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Regulations, government policies and the Norwegian housing market

Reguleringer, politikktiltak og det norske boligmarkedet

Jeanette Fjære-Lindkjenn

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I take full responsibility for the content of this thesis, and any errors are mine alone.

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Abstract

This thesis investigates the effects of specific regulations and policies in the Norwegian housing market at both national and regional levels. While grounded in the Norwegian context, the findings aim to contribute to the international literature on housing economics, particularly through the use of household-level microdata to analyze policy impacts on housing markets.

The first chapter analyzes the 2017 introduction of a debt-to-income limit for residential mortgages in Norway. Using a combination of house transaction and tax data, the study finds that the regulation had a modest impact on household balance sheets. However, affected households experienced higher interest rates post-regulation.

The second chapter (joint with Knut Are Aastveit, Ragnar Juelsrud, Markus Karlman, Karin Kinnerud, and Ella Getz Wold) reviews existing literature on mortgage regulations, highlighting their effectiveness in curbing household debt growth and house price inflation. Nonetheless, these tools entail significant costs, including reduced consumption smoothing, lower homeownership rates for certain groups, and increased dependence on parental wealth for home purchases.

The third chapter (joint with Nini Barth) examines a supply-side regulation aimed at increasing larger apartment construction in Oslo's inner districts. By restricting the proportion of small apartments and setting a minimum size of 35 square meters, the regulation sharply reduced the construction of smaller apartments, driving up their prices significantly.

The final chapter (joint with Nini Barth) explores house price dynamics across 14 Norwegian municipalities using a cointegrated vector error correction model (VECM). The analysis reveals substantial variation in the house price response to changes in user costs and indicates that larger deviations from model-implied prices increase the probability of housing market peaks and subsequent downturns.

Norsk sammendrag

Denne doktorgradsavhandlingen undersøker effektene av spesifikke reguleringer og politikk som påvirker det norske boligmarkedet, både på nasjonalt og regionalt nivå. Selv om studiene er forankret i en norsk kontekst, forsøker jeg å bidra til den internasjonale litteraturen knyttet til boligmarkedet, spesielt gjennom bruk av mikrodata på husholdningsnivå for å analysere hvordan enkelte reguleringer påvirker boligmarkedet.

Det første kapittelet analyserer kravet om maksimal gjeldsgrad på fem ganger inntekten ved kjøp av bolig som ble innført i Norge i 2017. Ved å kombinere boligtransaksjonsdata og skattestatistikk viser studien at reguleringen hadde en moderat effekt på husholdningenes boligverdi, gjeld og likvide sparemidler. Imidlertid opplevde berørte husholdninger høyere rentenivåer etter innføringen av kravet.

Det andre kapittelet (med Knut Are Aastveit, Ragnar Juelsrud, Markus Karlman, Karin Kinnerud og Ella Getz Wold) gjennomgår eksisterende litteratur om boliglånsreguleringer. Artikkelen konkluderer med at reguleringen til en viss grad bidrar til å dempe veksten i husholdningsgjeld og boligpriser. Samtidig fører reguleringen med seg betydelige kostnader knyttet til glatting av konsum, lavere eierandeler for visse grupper og økt avhengighet av foreldres formue ved boligkjøp.

Det tredje kapittelet (med Nini Barth) undersøker en tilbudssideregulering som hadde som mål å øke byggingen av større leiligheter i Oslo indre by. Ved å begrense andelen små leiligheter og innføre en minstestørrelse på 35 kvadratmeter, førte reguleringen til en kraftig reduksjon i byggingen av mindre leiligheter, og videre høyere boligprisvekst for disse leilighetene.

Det siste kapittelet (med Nini Barth) utforsker boligprisutviklingen i 14 norske kommuner ved bruk av en kointegrert feilkorreksjonsmodell (VECM). Analysen viser betydelig variasjon i hvordan boligprisene reagerer på endringer i brukerkostnaden (som i stor grad drives av endringer i realrenten) og indikerer at større avvik fra modellens estimerte priser øker sannsynligheten for at boligprisene har nådd en topp og at det dermed kommer en påfølgende nedtur i prisene.

0 Introduction to the thesis

0.1 Introduction

For most households, housing represents the largest financial investment made over a lifetime. Beyond being an investment, housing is also a form of consumption, as we derive value from the homes we live in daily. Furthermore, homeownership is closely tied to personal identity and social status, with maintaining a home often reflecting these aspects. Another important dimension of housing is its role in supporting local labor markets, as people generally prefer to live close to where they work. At a macroeconomic level, housing plays a critical role in financial stability, as housing is often highly leveraged and, therefore, closely interconnected with the banking sector.

In all these ways, the housing market is important for both the overall economy and individual households. Regulations and institutions that foster a well-functioning housing market can greatly enhance welfare. Government policies and institutional frameworks affect both the supply and demand sides of the housing market, influencing homeownership rates, house prices, and ultimately, economic welfare. These policies and regulations can vary between countries and even across regions within a country.

This thesis aims to examine the impact of specific regulations and policies in the Norwegian housing market, both at the national and regional levels. The goal is to offer insights that, while rooted in the Norwegian context, contribute to the broader international literature on housing economics. Additionally, the thesis aligns with the growing empirical research that uses household-level microdata to explore how government policies influence housing markets.

While there are many policies that affect homeownership, house prices, and the broader housing market, only a few of them are addressed in this thesis. For instance, housing taxation, transaction regulations, and technical building standards are all important but fall outside the scope of this work. Instead, the focus is on assessing the impact of a few specific policies.

The first paper investigates the effects of the debt-to-income limit for residential mortgages that was introduced in Norway in 2017 on homeownership, household debt, and property values. By combining house transaction data with household-level tax statistics, the study finds that the regulation had only a modest impact on household balance sheets. However, households affected by the regulation faced higher interest rates following its implementation.

The second paper offers a literature review on the broader impacts of residential mortgage regulations. The findings suggest that, while these tools are generally effective in curbing household debt growth and house price inflation, their macroeconomic effects are relatively limited. The regulations also carry substantial costs, such as reduced consumption smoothing, lower homeownership rates for certain demographics, and increased reliance on parental wealth in accessing housing.

In the third paper, a supply-side regulation is investigated. The apartment size regulation aimed at increasing the construction of larger apartments in Oslo by limiting the proportion of small apartments that could be built in the city's inner districts. The regulation also set a minimum apartment size of 35 square meters. The results indicate that the construction of smaller apartments sharply declined after the regulation was implemented, leading to a significant rise in their prices.

The final paper examines housing price cycles across 14 Norwegian municipalities using a cointegrated vector error correction model (VECM). The findings suggest that the house price response to a change in the user cost varies significantly between municipalities. Additionally, whether deviations between actual house prices and model-implied prices (the real house price gap) affect the likelihood of turning points in the housing market is explored. The results indicate that an increase in the price gap raises the probability of a housing market peak, followed by a downturn.

0.2 Background

0.2.1 The Norwegian housing market

Nearly eight out of ten Norwegian households own their homes, according to data from *Housing conditions, register-based* (2023). Achieving a high homeownership rate across all segments of society is also an explicit goal of the government (*Hurdalsplattformen*, 2021). The high ownership rate means that a significant portion of household wealth in Norway is tied up in housing assets. In addition, housing is largely financed through mortgage debt, and Norway's debt-to-disposable-income ratio is high compared to most other countries (Norges Bank, 2024).

During the 1980s, the Norwegian housing market underwent significant deregulation. After this, house prices were allowed to fluctuate according to supply and demand, and banks shifted from state ownership to privatization. Today, the Norwegian housing market is predominantly privatized. Private companies are responsible for housing construction, mortgage financing is provided by private banks, and home transactions are typically conducted through ascending-bid auctions facilitated by real estate brokers. In addition, there are certain programs designed to assist low-income households and other vulnerable groups, such as the start-up mortgage program.[1]

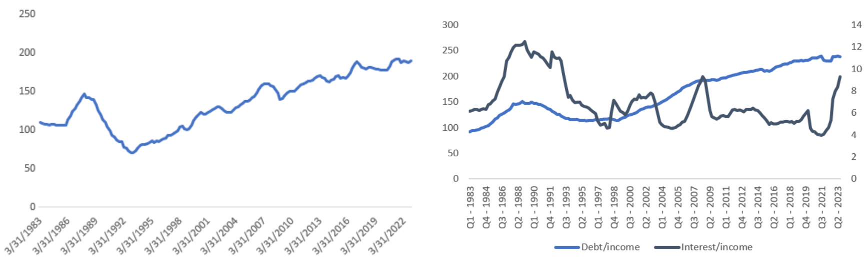
[1]According to The Norwegian State Housing Bank, nearly 8,000 households received start-up loans in

Despite the privatized nature of the Norwegian housing market, it is still heavily influenced by various policies and regulations. Housing taxes, monetary policy, and macroprudential measures—such as bank capital requirements and residential mortgage regulations—along with supply-side regulations, have significant effects on house prices, household debt, and homeownership rates. The policies and regulations analyzed in this thesis are described in detail in the following chapters. First, however, a brief overview of the historical trends in house prices and household debt is provided.

0.2.2 Norwegian house prices and household debt

Over the past few decades, Norwegian house prices have increased at a faster pace than disposable income per capita (see Figure 1, panel a). As a result, the average household now allocates a larger portion of its budget to housing compared to 40 years ago. While disposable income has also grown significantly during this period, it may seem reasonable that a larger share is spent on housing, as other expenses, such as food, have come to represent a smaller portion of household budgets.

Figure 1: House prices and household debt relative to income, 1983-2023.



(a) House prices/Disposable income per capita

(b) Household debt/Disposable income

Source: Norges Bank (Monetary policy report 4-23 and Financial stability report 1-23)

At the same time, household debt as a percentage of disposable income has surged to around 230 percent (see Figure 1, panel b). Debt in itself, is not inherently problematic since it enhances welfare by allowing households to smooth consumption over their life cycle. For example, young households with relatively low incomes can borrow to buy homes and repay the debt over time. However, a rapid rise in house prices and household debt has been shown to amplify economic downturns.[2] Consequently, these trends have become key concerns for monetary policy and other policymakers, especially in the wake of the global financial crisis of 2007-2009.

2023. See https://statistikk.husbanken.no/lan/startlan.

[2]See, e.g., Jorda et al. (2013), Mian and Sufi (2009), Mian et al. (2017), and Reinhart and Rogoff (2009).

Over the past two decades, mortgage rates have remained low, with the exception of the last few years. As a result, interest expenses relative to income have stayed modest, even as house prices and household debt have increased (see Figure 1, panel b, right axis). In response, macroprudential policy tools— such as capital buffer requirements for banks and borrower-based regulations like residential mortgage rules— have become the primary mechanisms to bolster the resilience of the banking sector and to dampen the growth of both house prices and household debt.

0.2.3 The residential mortgage regulation

Two chapters of this thesis examine the Norwegian residential mortgage regulations. By 2022, macroprudential policies targeting residential mortgages—such as limits on loan-to-value (LTV) ratios, debt-to-income (DTI) ratios, and amortization requirements—had been implemented in 26 European countries, according to The Financial Supervisory Authority (2024).

In Norway, guidelines for residential mortgage loans were first introduced in March 2010. In July 2015, these guidelines were formalized into regulation, with banks allowed a flexibility quota of 10 percent of their lending volume per quarter to deviate from the requirements. The regulations were further tightened in January 2017. An overview of the guidelines and regulations are provided in Table 1.

Table 1: Residential mortgage loan regulations in Norway.

	Guidelines		Regulation				
Year	2010	2011	2015	2017	2020	2020	2023
Maximum loan-to-value	0.90	0.85	0.85	0.85	0.85	0.85	0.85
Maximum debt-to-income	3			5	5	5	5
Maximum loan-to-value to avoid amortization		0.70	0.70	0.60	0.60	0.60	0.60
Interest rate stress		5pp	5pp	5pp	5pp	5pp	3pp*
Maximum loan-to-value secondary home	0.90	0.85	0.85	0.85	0.85	0.85	0.85
Maximum loan-to-value secondary home Oslo	0.90	0.85	0.85	0.60	0.60	0.60	0.85
Flexibility quota			0.10	0.10	0.20	0.10	0.10
Flexibility quota Oslo			0.10	0.08	0.20	0.08	0.08

Note: This table provides an overview of residential mortgage guidelines and regulations in Norway. Loan-to-value (LTV) is defines as the value of the mortgage relative to the value of the residence, while debt-to-income (DTI) is defined as total debt relative to income before taxes. *A required minimum stress interest rate of 7 per cent.

0.2.4 The apartment size regulation

Another regulation analyzed in this thesis is the apartment size regulation imposed by the local government in Norway's capital, Oslo. In 2008, the city introduced a regulation governing the construction of residential apartments in Oslo's inner city. Before this, the only restriction concerned the number of rooms in an apartment. The new regulation specified the proportion of different apartment sizes within a building project. It set a minimum size of 40 square meters and capped the share of apartments between 40 and 50 square meters at 20 percent. In addition, at least 50 percent of apartments had to be above 80 square meters. The aim was to promote high housing quality and ensure a variety of apartment sizes to accommodate different demographic and social groups. In particular, the regulation sought to ensure the construction of larger apartments suitable for families with children (Oslo Municipality, 2007).

The original regulation faced criticism for not aligning with the demand for housing in the inner city, where a large proportion of single-person households resides, and demand for small apartments has been both high and growing (Oslo Municipality, 2013). After almost six years, the regulation was relaxed in September 2013 to better reflect the demand for smaller apartments. This liberalization reduced the minimum apartment size from 40 to 35 square meters and allowed for 35 percent of newly built apartments to be under 50 square meters, up from the previous 20 percent. The minimum share of large apartments was reduced from 50 to 40 percent. Additionally, the local government emphasized that the regulation should be applied flexibly, allowing exceptions for technical reasons or challenges related to the existing housing stock.

0.3 Methodology and contribution to literature

0.3.1 Related theory

The primary contribution of this thesis is empirical, with all chapters building on existing theoretical frameworks. In this section, a few of the theoretical models that are particularly relevant to the empirical work in this thesis are briefly presented.

Theoretical life-cycle models of household consumption which incorporates housing in some form are particularly relevant to the three papers that focus on the demand side of the housing market. Notably, theoretical general equilibrium models with heterogenous agents incorporating borrowing constraints, such as those by Balke et al. (2023) and Fagereng et al. (2017), provide a foundation for understanding how residential mortgage regulations influence homeownership rates, household debt, house prices, and overall welfare. In the most simple life-cycle model presented in Balke et al. (2023) the dynamic problem is formulated as:

$$
\begin{aligned}
\max_{c_t, b_t} \int_0^T (u(c_t) + \mathbb{1}\Psi)dt \text{ s.t.}& \\
c_t + b_t = y_t& \\
\dot{b_t} >= 0& \\
\mathbb{1} = \begin{cases} 1 & \text{if } b_t >= \bar{b} \\ 0 & \text{otherwise} \end{cases}&
\end{aligned}
\tag{1}
$$

In Equation 1 households choose consumption c_t and savings b_t to maximize utility over the lifetime. The households live from period zero to T and face and upward-facing income profile y_t with age t. Income is the only source of heterogeneity in this model. The household gets utility from consumption and in addition can receive a utility bonus Ψ if it saves enough to buy a house. This minimum savings amount $\bar{b}$ to be able to buy a house is the credit constraint in the model that can be interpreted as a mortgage requirement, in addition to a classic credit constraint. Balke et al. (2023) demonstrate with this simplified model that stricter mortgage requirements lead to delays in both home purchases and savings. They further employ a more complex model, calibrated to U.S. data, to examine the impact of stricter LTV requirements on debt levels, housing prices, welfare, and marginal propensities to consume. Several findings are explored in greater detail in the second paper of this thesis.

The inverted demand approach, which explains house price formation (Meen, 1990, 1999), is also central to the work in this thesis. In this framework, a representative agent seeks to maximize lifetime utility by allocating resources between housing and other consumption goods. A rental sector is also present, where—under market efficiency—rent aligns with the user cost of housing. Because imputed rent cannot be directly observed, it is assumed to evolve proportionally with income and the housing stock, which are used as proxies for rental prices. In equilibrium, house prices are determined by the user cost of housing, household disposable income, and the supply of housing.

Other studies that establish theoretical links between the housing market, the macroeconomy, and household finance are highly relevant to this thesis. Notably, Davis and van Nieuwerburgh (2015) provide a comprehensive summary of the role of housing in the business cycle and its influence on household portfolio choices, which is particularly pertinent to the two papers analyzing residential mortgage regulations. Similarly, the overview by Piazzesi and Schneider (2016) offers several theoretical models that are instrumental in exploring the interplay between housing and the macroeconomy.

0.3.2 Data

Two chapters of this thesis are based on micro-level data, analyzing transactions, individuals, and households. The primary data sources are as follows:

1. Transaction Data from Eiendomsverdi: This dataset provides daily records of real estate transactions, including details such as sale and asking prices, property location, and characteristics like building year, square footage, building type, number of rooms, and floor level. It also includes a unique person ID for each party involved in the transaction (buyers and sellers), enabling linkage to other individual-level datasets.

2. Tax Register Data from Statistics Norway: This dataset contains annual information on individual and household characteristics, such as age, education, and household structure, along with balance sheet data on income, assets, and debt. It includes both individual and household identification numbers, allowing for integration with other datasets. In two chapters, these micro datasets are combined to provide a more comprehensive analysis. Additionally, one chapter utilizes macro-level data with quarterly frequency, mostly sourced from Statistics Norway. This dataset, aggregated at the municipality level, includes variables such as house prices, mortgage rates, disposable income, housing stock, and unemployment rates. These data are used to construct models of house prices at the municipal level.

0.3.3 Empirical frameworks

The empirical methods used in this thesis are primarily microeconometric techniques applied to the microdata described above. In one chapter, time-series econometrics is employed, utilizing macro-level time-series data. Below, the main futures of these two empirical frameworks are explained.

1. Difference-in-differences: In two chapters, a difference-in-differences (DiD) framework is applied to identify the effects of two distinct regulations. This approach involves comparing a treatment group, affected by the regulation, to a control group that is not, assessing both groups before and after the regulation. The difference in outcomes between the treatment and control groups after the regulation, relative to their difference before, is interpreted as the regulation's effect. This framework can be expressed as a fixed-effect regression in its most simple form, see for example Angrist and Pischke (2009).

$$y_{i,t} = \beta_0 + \beta_1 treat_i + \beta_2 post_t + \beta_3 treat_i post_t + \epsilon_{i,t} \tag{2}$$

In Equation 2, the outcome variable $y_{i,t}$ varies over group $i = 0, 1$ which is equal to 1 for the treatment group and 0 for the control group and over time $t = 0, 1$ which is equal to 1 post the regulation and 0 prior to the regulation. In the basic set-up with only two periods and two groups, this is equivalent to a two-way fixed effect regression with two periods and two groups. The effect of the regulation is β_3, which identifies the difference between the groups post the regulation relative to prior to the regulation.

The key identifying assumption underpinning this framework is that, in the absence of the regulation, the trends for both groups would have followed common/parallel trends. While this assumption cannot be directly tested, observing parallel trends prior to the regulation increases confidence in the validity of this assumption and that the control group is appropriately chosen. Researchers often evaluate this assumption by visually inspecting pre-treatment trends and conducting statistical tests such as the Wald pre-trend test. However, the Wald test typically has low power and cannot alone confirm parallel trends. A dynamic DiD analysis, which assesses differences between the groups both before and after treatment, can help detect potential violations. Beyond empirical analyses, it is important to theoretically justify the selection of an appropriate control group. Proper placebo and robustness tests can further increase confidence in a causal interpretation of results. Additionally, in the DiD framework, it is assumed that the treatment has no causal effect before implementation (no anticipation).

One advantage of the DiD framework is its ability to provide a clean identification strategy, allowing for causal interpretation of results, provided the identifying assumptions hold (parallel trends without treatment and no anticipation). It is also a relatively straightforward econometric method, making it easy to implement and understand. These advantages have contributed to an increasing use of the DiD framework in economics. However, the growing number of economic research articles that employ the DiD framework, coupled with the increasing complexity of its applications beyond the simple two-period setup with one treatment group and one control group, has sparked a discussion about its proper implementation. A significant contribution to this debate is provided by Roth et al. (2023), who summarize much of the recent work on this issue and explore the framework and its critical identifying assumptions across both the canonical two-period model and more complex scenarios. Other important contributions to this debate are, for example, Callaway and Sant'Anna (2021), de Chaisemartin and D'Haultfoeuille (2020), Goodman-Bacon (2021), and Sun and Abraham (2021). In this thesis, the fixest package in R Studio and the did package in Stata are applied, which incorporates the insights from Callaway and Sant'Anna (2021) and Sun and Abraham (2021).

2. Time-series methods: In the third paper, a cointegrated vector error correction model (VECM) is applied, initially proposed by Johansen (1988) and widely used in studies examining the long-run relationship between house prices and their fundamental drivers, such as Anundsen (2019). This method falls within the field of time series econometrics, which is more commonly used in macroeconomic research. The cointegration framework tests whether a linear combination of house prices and key fundamental drivers, household income, housing user cost, and housing stock, is stationary. While house prices, household income, and the housing stock are typically nonstationary individually, stationarity of their linear combination suggests the existence of an equilibrium relationship among these variables, implying that house prices revert to this equilibrium over time.

0.3.4 Contribution to literature

This thesis contributes to multiple strands of the literature on the housing market, particularly regarding the formation and evolution of house prices. In the first two chapters, the effects of residential mortgage regulations are examined, with a specific focus on the Norwegian DTI limit. These studies add to the empirical literature investigating the impact of mortgage regulations. For instance, Aastveit et al. (2020) empirically analyze the effects of Norway's LTV limit, finding that it successfully reduced leverage both through lower borrowing by home buyers and through canceled home purchases. However, they also found that home buyers depleted liquid assets to comply with the limit, increasing their vulnerability in cases of unemployment. Similarly, Van Bekkum et al. (2019) study the LTV limit in the Netherlands using a comparable methodology, concluding that it effectively reduced leverage, although the negative impact on liquid assets was temporary. Balke et al. (2023) use a life-cycle model with heterogeneous agents to demonstrate how LTV limits affect household debt, homeownership, and welfare over the long term, showing that these limits reduce leverage and homeownership rates while negatively impacting welfare, particularly for households with steep income trajectories. While most research focuses on LTV limits, there are fewer studies on DTI limits. Notable examples include Acharya et al. (2022) and Peydró et al. (2020), both of whom find that DTI limits disproportionately affect low-income groups. This thesis contributes to this field by summarizing existing studies and showing that, in the Norwegian case, the DTI limits had only a modest effect on household debt and house prices, although interest rates for affected households increased.

The third chapter relates to the literature on house price cycles and the fundamental drivers of house prices at the national and regional levels. Anundsen (2019) employ the cointegration framework of Johansen (1988) to establish a long-term relationship between house prices and their fundamental drivers at the national level. The analysis in the third chapter builds on this framework to estimate long-term models for house prices at the regional level. Important contributions to the literature on regional house price models include works by Meen (1990) and Meen (1999), as well as more recent studies by Oikarinen et al. (2018) and Plakandras et al. (2024).

The fourth chapter addresses the supply side of the housing market and the effect of government restrictions on housing supply on house prices, housing affordability, and demographics. While most studies document a positive association between strict housing supply regulations and rising house prices, we aim to establish a causal link by examining one specific regulation. Molloy (2020) provides a comprehensive overview of this literature. Particularly influential studies include Glaeser and Gyourko (2018) and Gyourko and Krimmel (2021).

0.4 Overview and synthesis of thesis

0.4.1 Overview of thesis

The central research question of this thesis is how government policies and regulations influence house prices and other aspects of the Norwegian housing market. In the first two papers, this is addressed through an analysis of the impact of Norwegian residential mortgage regulations on house values, household debt, and mortgage rates. The third paper investigates the long-term effect of monetary policy, through the real user cost, on real house prices at the regional level, using a co-integrated error correction model. These three papers primarily focus on the demand side of the housing market. The fourth paper shifts focus to the supply side, examining how a specific supply-side regulation in the inner city of Norway's capital affected house prices and the area's demographic composition. A conceptual framework for the thesis is illustrated in Figure 2.

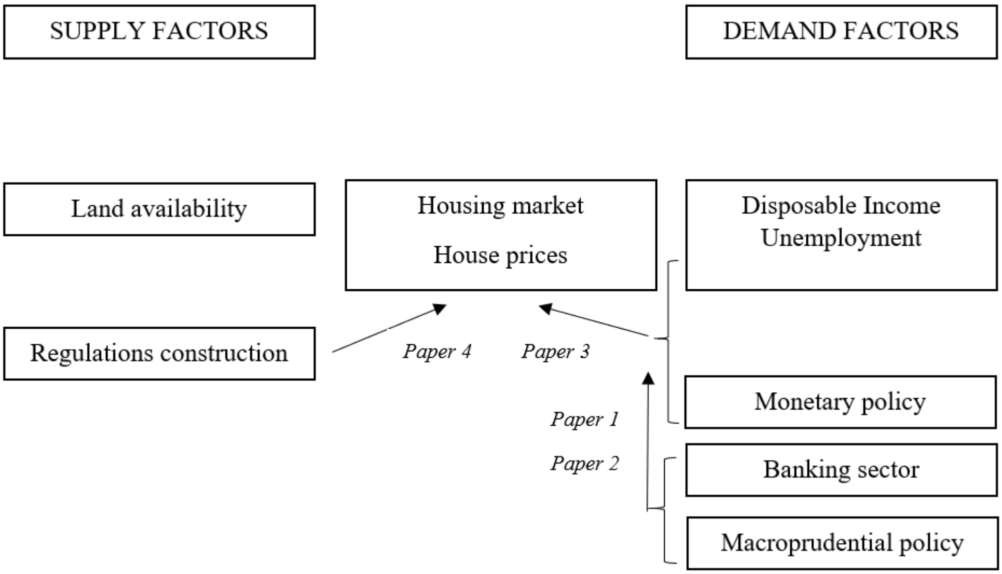
The first three papers in this thesis are unified by their focus on the demand side of the housing market, while the final paper shifts attention to a supply-side regulation. Despite this topical difference, paper 1 and 4 are methodologically aligned, both utilizing microdata and micro-econometric methods. In both articles, a difference-in-differences framework is applied to identify effects, using the same housing transaction data merged with household-level tax statistics. Paper 3, by contrast, stands apart as it employs aggregated macro data and a cointegrated VAR model, an empirical framework more closely associated with macroeconomic literature and time-series econometrics. Topically, Paper 1 and 2 are closely linked, as both analyze the Norwegian mortgage regulation. However, Paper 2 differs methodologically, as it is a descriptive paper that provides a literature review on the subject. Table 2 provides a summary of the papers.

In terms of results, there are similarities between the first and second papers, both

Table 2: A snapshot of the thesis.

Paper	Research question	Hypothesis	Theory	Data	Empirical methods	Key findings
I	How did the Norwegian DTI-limit affect household balance sheets?	A reduction in debt and house values for affected households	Life-cycle model with credit constraints	Housing transactions and tax statistics	Difference-in-differences	No effect on debt/house values, but higher interest rates for exposed households.
II	What is the effect of mortgage regulations?	Lower debt and house prices	Literature review		Descriptive	Some intended effects, but also severe costs
III	What drive regional house price booms and busts?	The user cost, disposable income and housing supply	Inverted demand approach	Macro data	Cointegrated VAR	Heterogeneity in house price booms and bust and in house price response to the user cost at regional level
IV	How did the apartment size regulation affect house prices and demographics?	Higher prices on small apartments, lower on large apartments	General supply and demand theory	Housing transactions and tax statistics	Difference-in-differences	Higher prices for small apartments. Also higher prices for large apartments, possibly driven by increased demand from family households

Figure 2: Conceptual framework of the thesis.



of which focus on household-level outcomes. In contrast, the third and fourth papers examine macro-level effects, though they also explore heterogeneity across regions and different market segments. Overall, the first two chapters have a more national focus, while Chapters 3 and 4 place greater emphasis on regional aspects of housing markets.

In summary, all the chapters are connected to the central theme of regulations, government policies, and the housing market. However, each chapter distinguishes itself in various ways—whether through its topic, data sources, methodology, or primary focus of results.

0.4.2 Synthesis of papers

Chapter 1: Limiting debt to income: Or not? In the first paper, the impact of Norway's 2017 DTI limit on residential mortgages is examined. This regulation restricted banks from issuing mortgages where a household's total debt exceeded five times their gross income. However, banks were allowed to grant exceptions for up to ten percent of their total lending volume each quarter.

By combining tax register data with house transaction records, the study finds that DTI ratios remained high, with around 18 percent of home buyers exceeding the five-times income limit even after the regulation was introduced. This contrasts sharply with reports from the Norwegian Financial Supervisory Authority (FSA), which indicated that only a small fraction of new loans breached the limit. The discrepancy may be attributed to differences in data sources and definitions of income and debt. One possible explanation is the inclusion of co-borrowers outside the household, such as parents, in FSA reports—where their income counts toward the total—while tax data calculates income at the household level only. Another factor could be possibility of exclusion of unsecured debt in FSA reports, which is included in the total debt reported in tax statistics. Further analysis and data are necessary to fully understand these differences.

A difference-in-difference analysis confirms that the DTI limit had no significant effect on the house values, household debt, or financial wealth of affected buyers. This could be because the regulation did not strongly constrain most households purchasing homes. However, interest rates for those impacted by the regulation increased by 0.12 to 0.14 percentage points, with low-income households experiencing the largest hikes.

Chapter 2: How Does the Lending Regulation Work? A Summary of the Research Literature

The second paper is a literature review that examines the impact of residential mortgage and lending regulations. While it is written from a Norwegian perspective, it also incorporates relevant international research in the field.

The article concludes that the Norwegian lending regulation has achieved its intended purpose to some extent by helping reduce household debt and curb the growth in house prices. However, the aggregate effect on household debt growth is limited, and the impact on house price growth appears to have been short-lived. It is more uncertain whether the regulations have contributed to reducing household vulnerability to negative shocks. This is because, although affected households are less indebted, they also have fewer liquid assets available. When it comes to reducing the risk of negative effects from loan losses in banks, other tools, such as capital buffer requirements, are likely better suited.

The regulations also seem to impose significant costs on many households. Firstly, they limit households' ability to smooth consumption through borrowing. Additionally, the regulations may make it more difficult for certain groups to purchase a home, particularly young people and those with low incomes. The regulation may also reinforce the importance of wealthy parents for the ability to buy a home, which in turn could perpetuate wealth inequality across generations.

In sum, the findings presented in this paper suggest that the benefits of mortgage regulations are limited in size, while there are significant costs. This is likely exacerbated in recent times with rising inflation and interest rates. At the same time, it is challenging to accurately assess the achievement of goals, as the arguments for what the regulation is supposed to achieve seem to vary across different stakeholders and over time.

Chapter 3: From boom to bust: Probabilities of turning points in house prices. In this paper, historical booms and busts in house prices at the municipality level are examined. Further, the study tests whether deviations from fundamental prices—defined by factors such as disposable income, the housing stock, and the user cost of housing—affect the likelihood of turning points in house price cycles at the municipality level.

In the first part of the analysis, the Bry and Boschan (1971) algorithm is applied to identify booms and busts in house prices across 14 municipalities in Norway, utilizing quarterly data from 2003 to 2021. The findings reveal considerable variation in the timing, duration, and magnitude of house price cycles between municipalities, especially those that are located in separate regions of Norway. In most municipalities, the rise in real house prices during booms outweighs the decline during busts, leading to an overall increase in real house prices over the period analyzed. Overall, boom periods tend to last longer than busts.

A cointegrated vector autoregressive model is used to identify the long-term drivers of local house prices. The model incorporates key variables such as the real user cost of housing (which includes the tax-adjusted real mortgage rate, adjusted for depreciation and house price expectations), household disposable income, and the housing stock. The results indicate that house prices are highly sensitive to changes in the user cost. For Norway as a whole, a one percentage point increase in the user cost is associated with an 10 percent decrease in house prices. The elasticity of house prices with respect to household income is approximately 1.5 percent. There is significant variation in how local markets respond to long-term fundamentals, especially in their reaction to changes in the user cost.

Lastly, the model is applied to assess whether the real house price gap—the difference between actual prices and those implied by the model—influences the probability of turning points in house prices. The results suggest that an increase in the price gap from 0 to 15 percent raises the probability of a peak (and subsequent bust) by 7 percentage points.

Chapter 4: The price effect of size restrictions on residential building. This chapter focuses on the supply side of the housing market, specifically investigating the price effects of regulations governing the proportion of small and large apartments being built in Oslo's inner city. In 2008, a regulation was introduced requiring a minimum apartment size of 35 square meters. The goal was to ensure that new housing developments included a mix of both small and large units to support a diverse demographic structure.

The construction of new apartments below the 35 square meter threshold sharply declined after the regulation was implemented, both in the regulated inner city and the rest of Oslo. Using transaction data, a difference-in-difference framework is applied to assess the regulation's impact on apartment prices. The analysis finds that the prices of apartments smaller than 35 square meters rose by 3.3 percent compared to apartments in the control group (40-50 square meters) following the implementation of the size regulation. This price increase was consistent across both the regulated inner city and the rest of Oslo.

Price effects on larger apartments are also explored. Despite an increase in the supply of large newly built units in the regulated inner city, prices for these apartments rose compared to districts outside the regulated area. The study suggests that this rise in prices may be partly due to increased demand from families with children, aligning with the regulation's intended purpose.

0.5 Implications and limitations

The primary goal of this thesis is to explore how government policies and regulations in the housing market impact house prices and other household outcomes. The findings indicate that while these regulations sometimes achieve their intended goals, in other cases, they either fall short or lead to unintended side effects.

In the case of residential mortgage regulations, the thesis suggests that in Norway, these policies have modest effects in curbing the growth of house prices and household debt, particularly with respect to the Norwegian DTI limit, which is examined more thoroughly in Chapter 1. Moreover, the regulations may have negative side effects, such as limiting households' ability to smooth consumption. Additionally, they may produce unintended distributional consequences, increasing the reliance on wealthy parents for young households entering the housing market. These findings are valuable for policymakers as they consider future adjustments to these regulations.

While the regulations could yield positive welfare effects by reducing the likelihood and severity of future economic crises, quantifying these benefits is challenging, whereas the costs to households are more easily identifiable. The lack of strong evidence supporting significant welfare gains suggests that policymakers might consider making the regulations less stringent or, at the very least, adjusting them to minimize costs without substantially increasing household sector vulnerabilities. One possible modification could be raising the LTV limit for households with strong debt-servicing capabilities, which would reduce the dependence on wealthy parents for young people purchasing their first home.

Regarding the regulation on apartment size in Oslo's inner city, it appears that the policy achieved its primary goal of increasing the stock of larger apartments and encouraging more families with children to live in the area. However, the reduced availability of smaller apartments as a result of the regulation has significantly driven up their prices. This poses a challenge for housing affordability, particularly given the high price levels relative to income and the large proportion of single and young households—those most likely to prefer smaller apartments—seeking to live in the inner city.

These findings should be taken into account when policymakers consider revising the regulation. For example, they could lower the minimum apartment size or allow a certain percentage of apartments to be built below the current minimum of 35 square meters.

This thesis also reveals a clear pattern of regional variation in the effects of monetary policy, indicating that changes in the policy rate have a greater impact in some areas of the country than in others. Although monetary policymakers have only one national instrument and limited capacity to target specific regional markets, it remains crucial to recognize these differing effects. This awareness is particularly important when other economic shocks arise that monetary authorities aim to mitigate, as the severity of these shocks may also vary across regions.

All the findings in this thesis are based on Norwegian data and analysis of the Norwegian housing market. To establish the generalizability and external validity of these results, further evidence from similar regulations in other countries, as well as theoretical frameworks that explain the observed patterns and offer more long-term analysis, would be both necessary and valuable.

Additionally, future research could benefit from using even more detailed microdata, such as parent-child links in the housing market, to better understand the underlying mechanisms. This data could be combined with more granular loan-level information, provided that a comprehensive debt register—including both mortgage and other forms of debt—becomes available in Norway. Such data would enable a deeper examination of the effects of residential mortgage regulations, an area where further research is needed to assess the effectiveness and benefits of these policy tools.

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Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this work the author used Chat GPT in order to improve language. After using this tool, the author reviewed and edited the content as needed and take full responsibility for the content of the publication.

1 Limiting debt to income: Or not?

Limiting debt-to-income: Or not? *

Jeanette Fjære-Lindkjenn†

January 9, 2025

Abstract

Over recent decades, borrower-based macroprudential policies have become widely used tools to address the risks of financial imbalances. This paper examines the impact of the debt-to-income (DTI) limit for residential mortgages introduced in Norway in 2017. By combining tax register data with house transaction records, the analysis reveals that DTI ratios remained elevated, with approximately 18% of home buyers exceeding the regulatory limit of five even after the policy's implementation. Using a difference-in-differences approach, the findings indicate that the DTI limit had no significant effect on the house values, household debt, or financial wealth of affected households. However, interest rates rose for exposed home buyers, ranging from 0.12 to 0.14 percentage points, after the regulation was implemented. Notably, the rise in interest rates was particularly pronounced among low-income households.

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

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

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1 Introduction

Excessive growth in house prices and household credit during economic booms increases the likelihood, depth, and duration of subsequent economic crises (Anundsen et al., 2016; Jorda et al., 2013; Kiyotaki & Moore, 1997; Reinhart & Rogoff, 2009). In particular, high household debt growth prior to a downturn can significantly amplify the decline in household demand following a negative economic shock (Eggertsson & Krugman, 2012; Korinek & Simsek, 2016; Mian et al., 2013; Mian et al., 2017). In response to these risks, many countries have implemented macroprudential policies in the wake of the global financial crisis. These measures include capital requirements for banks and borrower-based tools, such as limits on residential mortgage leverage, aimed at curbing financial imbalances and strengthening bank resilience. Recent studies, however, highlight potential unintended consequences of residential mortgage regulations. These include reduced opportunities for first-time buyers and low-income households in the housing market, as well as a decrease in liquid assets, which could leave households more vulnerable to unforeseen income shocks (Aastveit et al., 2020; Acharya et al., 2022; Tracey & van Horen, 2022). These findings underscore the importance of examining whether borrower-based macroprudential regulations achieve their intended objectives and understanding their broader effects on household balance sheets.

This paper examines the impact of the debt-to-income (DTI) limit introduced in Norway in 2017 on house values, household debt, and financial wealth among Norwegian home buyers. The analysis utilizes tax-register data merged with housing transaction information from Eiendomsverdi. A difference-in-differences framework is employed, where the treatment group comprises households predicted to exceed the DTI limit in the absence of the regulation. These predictions are generated using a household model for DTI, estimated with data from home buyers in 2016, the year preceding the regulation.

The model predicts DTI by regressing it, using ordinary least squares, on household characteristics such as age, education, zip code, and balance sheet data. Households in 2017 and 2018 that are predicted—based on the pre-regulation model—to exceed the DTI threshold of five are classified as treated. A dynamic difference-in-differences approach is then applied to evaluate whether the introduction of the DTI limit led to reductions in house values, household debt, and financial wealth for treated households relative to non-treated households.

The findings indicate no significant impact of the DTI limit on house values or household debt among affected home buyers. Similarly, there is no observable effect on financial wealth, a proxy for liquid assets, which might otherwise have represented a potential adverse side effect of the regulation (Aastveit et al., 2020). The estimated effects on house values remain close to zero even for more vulnerable groups, such as low-income households and first-time buyers. While a small negative effect on household debt is observed for low-income households, it is statistically significant only at the 10 percent level.

The results are robust to various sensitivity tests within the difference-in-differences framework. These include adjustments for announcement effects, the use of placebo DTI-limit thresholds, and restricting the analysis to a narrower DTI range. Additionally, a regression discontinuity exercise corroborates the primary findings, demonstrating that the negligible or minimal effects persist when applying an alternative empirical framework.

Implicit interest rates—calculated as interest expenses relative to total debt—increased by 0.12 to 0.14 percentage points for house buyers with a DTI above five, compared to other households, after the implementation of the regulation. For low-income households and those owning secondary homes (referred to as investors), the increase was even larger, exceeding 0.20 percentage points. This suggests that borrowing costs have risen for exposed households.

One potential explanation is that banks imposed stricter terms, such as requiring fixed-rate mortgages or higher floating rates, for borrowers who exceeded the DTI limit. Alternatively, households might have increased their use of consumer loans, which were not subject to the regulation until mid-2019. If consumer loans were used to supplement or replace residential mortgages, this could explain the limited impact of the DTI limit on household debt and house values.

Further exploration of this hypothesis would require detailed data on consumer loans, which is not available in the dataset used for this study. Additionally, the dynamic difference-in-differences analysis shows a positive and significant effect also in 2013, raising concerns about the validity of the parallel pre-trends assumption. This complicates causal attribution of the observed changes to the regulation itself and suggests caution in drawing definitive conclusions.

The share of home buyers with DTI above the limit of five is stable at around 18% from 2016 to 2018. The lack of effect on house values and household debt might be a natural consequence of this finding; the limit does not bind for a large share of households. Banks have a flexibility quota of 10% of the lending volume which is allowed to deviate from the regulation each quarter. This quota might be large enough for the limit not to be binding. The banks might choose to use a larger share of the flexibility quota on new home buyers and a lower share on loans to existing home owners. Another possible explanation for the high share of home buyers in violation with the limit could be increased use of co-borrowing with parents outside the household. Young households desiring a high DTI to buy a more expensive home can share some of the risk with a parent, and use the parent's income as part of the denominator in the DTI-calculation. More data is needed to conclude on this.

The primary contribution of this paper is twofold. First, it is among the few studies to empirically analyze the effects of a DTI limit, a policy measure that has increasingly been used to complement the more extensively studied loan-to-value (LTV) limits. Second, to the best of my knowledge, this is the first study to report that the DTI limit in Norway does not appear to be binding for a significant share of home buyers and to propose potential explanations for this outcome.

Although further research is needed to fully understand the underlying mechanisms and to assess the external validity of these findings beyond Norway, this study provides preliminary insights that can inform policymakers tasked with designing similar regulations. The evidence suggests that the Norwegian DTI limit has not effectively curbed house prices or household debt, nor has it reduced vulnerabilities within the household sector. These findings highlight the need for future regulatory designs to either explore alternative measures or modify the implementation of DTI limits to ensure they are more binding and impactful.

Literature review

This paper contributes to the growing body of empirical literature on the effects of borrower-based macroprudential regulations. Numerous studies have examined the effectiveness of loan-to-value (LTV) limits in curbing household credit growth and house price inflation in various countries. For instance, Armstrong et al. (2019) investigate LTV limits in New Zealand, while Laufer and Tzur-Ilan (2021) and Tzur-Ilan (2023) analyze similar policies in Israel. Using a methodological framework comparable to this paper, studies such as Aastveit et al. (2020), Van Bekkum et al. (2019), and DeFusco et al. (2020) report that LTV limits generally succeed in dampening credit and house prices. However, some of these studies also document unintended consequences, such as reductions in household liquid assets following the implementation of the regulations. Related theoretical work, including Balke et al. (2023) and Fagereng et al. (2017), demonstrates how stricter LTV requirements can delay housing-related savings and postpone home ownership. This paper builds on this literature by focusing on DTI limits, a measure that has gained traction in recent years as a supplement to LTV policies.

The literature on DTI limits is relatively sparse but growing. For example, Acharya et al. (2022) examine the impacts of DTI and LTV limits across income quintiles in Ireland, finding that low-income households are disproportionately affected. Similarly, Peydró et al. (2020) analyze the effects of loan-to-income (LTI) limits in the UK, reporting significant adverse effects on low-income households. Closer to the Norwegian context, Dørum (2022) find no significant impact of Norway's DTI limit on credit or house prices but do identify a notable increase in credit costs for affected households, aligning with this paper's findings.

This analysis complements these studies by offering evidence from Norway where the DTI limit does not appear to bind for many households. It also explores potential reasons for this outcome, contributing valuable insights for policymakers in Norway and other countries considering or implementing similar regulations. Understanding why the DTI limit failed to produce the expected effects can help refine future policy designs to achieve desired outcomes more effectively.

The paper proceeds as follows. In Section 2 the institutional background and empirical approach are presented, while the data and summary statistics are shown in Section 3. In Section 4 the main results are introduced and discussed. Robustness checks are shown in Section 5 and the final section concludes.

2 Institutional background and empirical approach

2.1 Residential mortgage regulations in Norway

Table 1 Residential mortgage loan regulations in Norway.

	Guidelines		Regulation				
Year	2010	2011	2015	2017	2020	2020	2023
Maximum loan-to-value	0.90	0.85	0.85	0.85	0.85	0.85	0.85
Maximum debt-to-income	3			5	5	5	5
Maximum loan-to-value to avoid amortization		0.70	0.70	0.60	0.60	0.60	0.60
Interest rate stress		5pp	5pp	5pp	5pp	5pp	3pp*
Maximum loan-to-value secondary home	0.90	0.85	0.85	0.85	0.85	0.85	0.85
Maximum loan-to-value secondary home Oslo	0.90	0.85	0.85	0.60	0.60	0.60	0.85
Flexibility quota			0.10	0.10	0.20	0.10	0.10
Flexibility quota Oslo			0.10	0.08	0.20	0.08	0.08

Note: This table provides an overview of residential mortgage guidelines and regulations in Norway. Loan-to-value (LTV) is defines as the value of the mortgage relative to the value of the residence, while debt-to-income (DTI) is defined as total debt relative to income before taxes. *A required minimum stress interest rate of 7 per cent.

Macroprudential policies directed towards residential mortgages, for example upper limits on loan-to-value (LTV), debt-to-income (DTI) and amortization requirements, had been implemented in 26 European countries by 2022 according to the Financial Supervisory Authority of Norway, The Financial Supervisory Authority (2022b).

In Norway, residential mortgage loan guidelines, including a LTV-limit of 90% and a DTI-limit of 3, were implemented in March 2010. The LTV-limit was tightened to 85%

in December 2011, while the DTI-limit was removed. In July 2015, the guidelines were formalized as regulation. A flexibility quota of 10%t of the lending volume could deviate from the requirements each quarter. In January 2017, the regulation was tightened along several dimensions and the DTI-limit of 5, which is investigated in this paper, was introduced. In 2020, the flexibility quota was temporarily increased (due to the covid-19 crisis) from 8% in Oslo, and 10% in the rest of the country, to 20% in the whole country. In 2021, the regulation was extended to include unsecured loans like consumer loans. In 2023, the interest rate stress test was eased to 3 percentage points, but with a requirement that the total stress rate is 7% or higher.

There are also some requirements only applicable to the capital, Oslo. In 2017, a maximum LTV limit of 60% was implemented for purchasers of secondary homes. In addition, the flexibility quota was set to 8% in Oslo, compared to 10% in the rest of the country. The requirement for LTV on secondary homes was removed in 2023. Table 1 summarizes the guidelines and regulations.

2.2 Empirical approach

To estimate the causal effect of the DTI limit to house values, household debt and financial wealth for Norwegian households, I use an empirical approach which is similar to that of Van Bekkum et al. (2019) and Aastveit et al. (2020). The framework is a difference-in-difference approach, in which the treated group are households who in absence of regulation are predicted to have DTI above the limit of five after the limit is implemented. A central assumption is common time trends among those below and above the limit in absence of the limit. The identification is made in two steps.

$$DTI_i = \alpha_k + \beta_1 age_i + \beta_2 highedu_i + \eta_1 I_i + \eta_2 S_i + \sum_{j=2}^{5} \eta_{3,j} qi_{i,j} + \eta_4 U_i + \mathbf{b}'\mathbf{X_i} + \epsilon_i \tag{1}$$

First, DTI is regressed on a set of household characteristics in the year prior to implementation of the DTI-limit (2016). The left-hand side variable in equation (1) DTI with subscript i indicating household i. On the right hand side, α_k represents zip-code fixed effects, while β_1 and β_2 are coefficients on household age and education, respectively. η_1 is the coefficient on a dummy-variable equal to one if the household owns a secondary home, while η_2 and $\eta_{3,j}$ are coefficients on dummy variables for one-member households and each income quintile, j. U_i is equal to one if one or more household members are unemployed. **b'** is a coefficient vector on household balance sheet variables $\mathbf{X_i}$. These variables are current and lagged values of interest expenses, the value of primary home and financial wealth, in addition to lagged values of income and debt.

Based on the results from the estimation of Equation 1, DTI is predicted; $\widehat{DTI_{i,t}}$. A dummy variable equal to one if $\widehat{DTI_{i,t}}$ is above the DTI-limit of five is defined as the treatment variable. This dummy-variable is called $\widehat{DTI}^{treated}_{i,t}$. The actual and predicted distribution of DTI from the model are shown in Appendix A.

$$y_{i,t} = \alpha_k + \sum_{t=2013}^{2015} \beta_t \widehat{DTI}^{treated}_{i,t} + \sum_{t=2017}^{2018} \beta_t \widehat{DTI}^{treated}_{i,t} + \mathbf{b}'\mathbf{X_{i,t}} + \delta_t + \widehat{DTI}^{treated}_{i,t} + \epsilon_{i,t} \qquad (2)$$

The second step is showed in Equation 2. Here $\widehat{DTI}^{treated}_{i,t}$ is the treatment group in a dynamic difference-in-difference framework. Also here, zip-code fixed effects are included in addition to current and lagged house price growth at the municipality level. The β_t's show the effect of the interaction between treatment and years. For the results to indicate an effect of the DTI-limit, we should see statistically significant coefficients for the β_t's after implementation of the limit (2017 and 2018), while they should be insignificant in the years prior to implementation. Household controls are all included in $\mathbf{X_{i,t}}$; age, education and the dummy variables for unemployed, secondary home owners and single households. In addition, year-fixed effects, δ_t, and the treatment variable are included. In the main estimation, all standard errors are clustered by zip-code. I have also experimented with using bootstrap

Table 2 Restrictions on the data set.

Restrictions	Obs. 2016	Obs. 2018
No restrictions	23,096	24,083
Exclude self-employed	20,553	21,613
Include only positive debt	20,115	21,148
Exclude incomes below poverty line	18,154	19,340
Trim 1st and 99th percentile	16,412	17,497

Notes: This table shows the restrictions made on the data for housing transactions in 2016 and 2018 and how each restriction affects the number of observations. Trimming on the 1^{st} and 99^{th} percentile is done for the variables income, debt, wealth, DTI, house prices and living area.

wild standard errors and these are very similar to the normal cluster robust standard errors.

The dependent variables, $y_{i,t}$, in Equation 2 are house values, household debt and financial wealth (as a proxy for liquid assets). The logarithm of these variables are considered in all estimations. I also perform a difference-in-difference estimation with implicit interest rates as the dependent variable. In this estimation, I use the same set-up and include the same control variables as in Equation 2, but in this estimation the treatment group is households with actual DTI above five. This means that the first step (Equation 1) where DTI is predicted is redundant.

3 Data and summary statistics

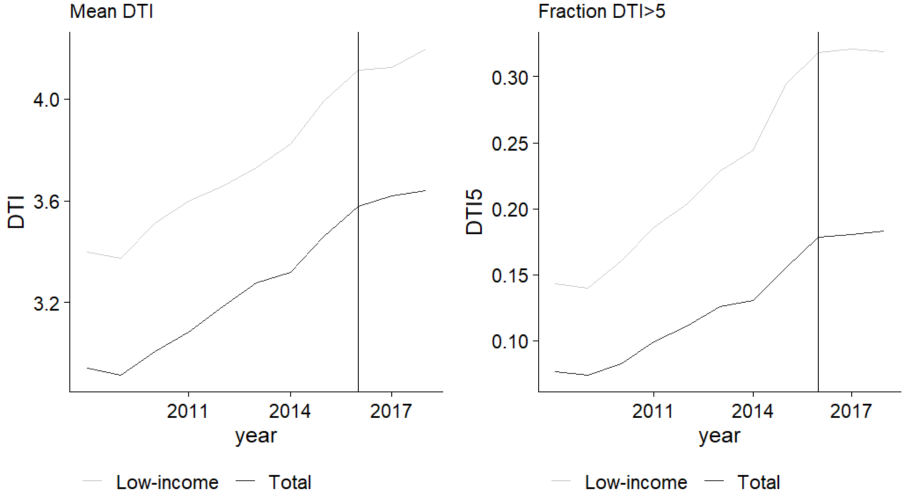
The dataset is derived from the Norwegian tax registry and includes annual observations on household balance sheets for the 20 most populated municipalities in Norway over the period 2004–2019. It provides detailed information on income, debt, wealth, interest expenses, and the market value of primary and secondary homes, along with demographic data such as education, profession, age, sex, and household type. These balance sheet data are linked, via personal identification numbers, to a dataset from Eiendomsverdi, which contains information on Norwegian housing transactions from 2000–2020. This dataset includes details on sales dates, asking and selling prices, as well as house and location characteristics.

The dataset is trimmed by excluding observations with negative incomes, while negative capital incomes are set to zero. This adjustment implicitly assumes that banks disregard negative incomes when calculating DTI ratios. The rationale for this assumption is that banks focus on expected income, and negative incomes are unlikely to recur in subsequent years. For households where one or more members experience income growth exceeding 10%, it is assumed that the individual has changed their employment status—for example, transitioning from student to employed or from unemployed to employed. Since banks are presumed to consider both current and expected future income when granting loans, the income for the succeeding year is used for these individuals.

Self-employed individuals are excluded from the analysis to avoid incorporating corporate debt and because their volatile incomes could distort the DTI measure. Only households with positive debt are included, as calculating the DTI ratio requires a non-zero debt level. Households with incomes below the Norwegian poverty line are also excluded, as a significant portion of these households are likely to have obtained mortgages through the Norwegian State Housing Bank—the primary agency for implementing national social housing policy—which is not subject to the regulation. Additionally, income, debt, wealth, DTI ratios, house values, and living area are trimmed at the 1st and 99th percentiles to mitigate the influence of outliers. The data restrictions and their impact on the number of observations for home buyers in 2016 and 2018 are summarized in Table 2.

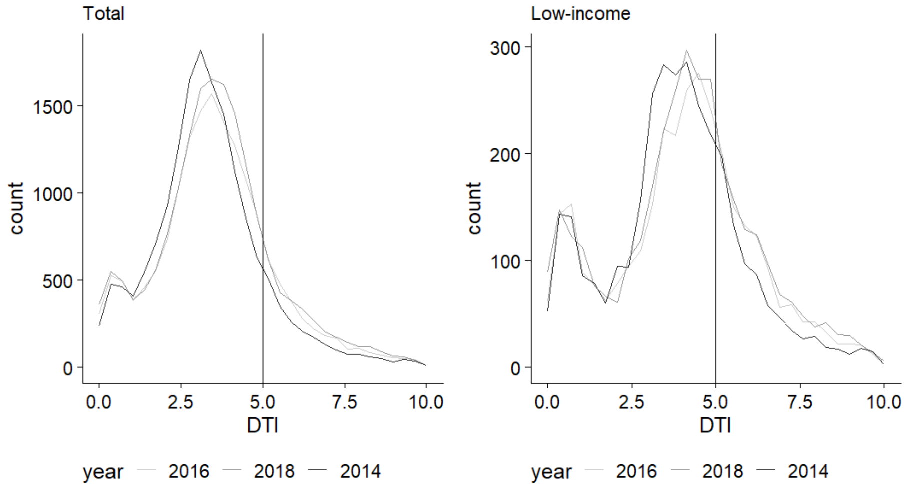
Figure 1 illustrates the trends in average DTI ratios and the proportion of households with a DTI above 5 among house buyers from 2009 to 2018. An upward trend is evident in both variables over the period, although the trend appears to level off somewhat in 2017, coinciding with the implementation of the regulation. This flattening suggests that the regulation has had some binding effect. However, the absence of a significant decline in the trend post-implementation indicates that the regulation's overall impact has been modest. The figure also highlights the same variables for the first income quintile (low-income households), defined as the 20% of households with the lowest incomes. While the trends for this group follow a similar pattern, the levels are notably higher compared to the broader population, underscoring the greater prevalence of high DTI ratios among low-income households.

Figure 1 Average DTI and fraction above limit for house buyers, 2008–2018.



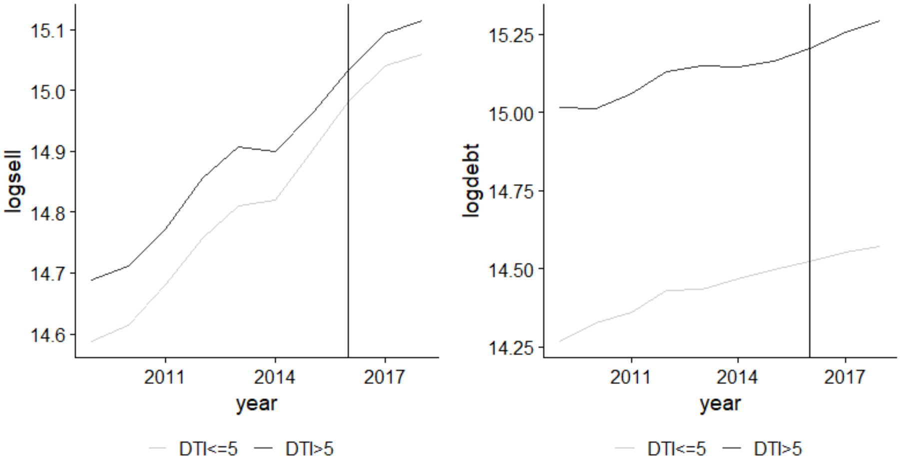
Notes: This figure illustrates average DTI ratios and the fraction with DTI above the limit of five over time for house buyers in Norway. DTI is defined as total debt divided by total income. $DTI > 5$ is the fraction of households with a DTI ratio above 5. Low-income is defined as the 20% of households with the lowest income (between NOK 300K and NOK 500K).

Figure 2 Distribution of DTI for house buyers, 2014, 2016 and 2018.



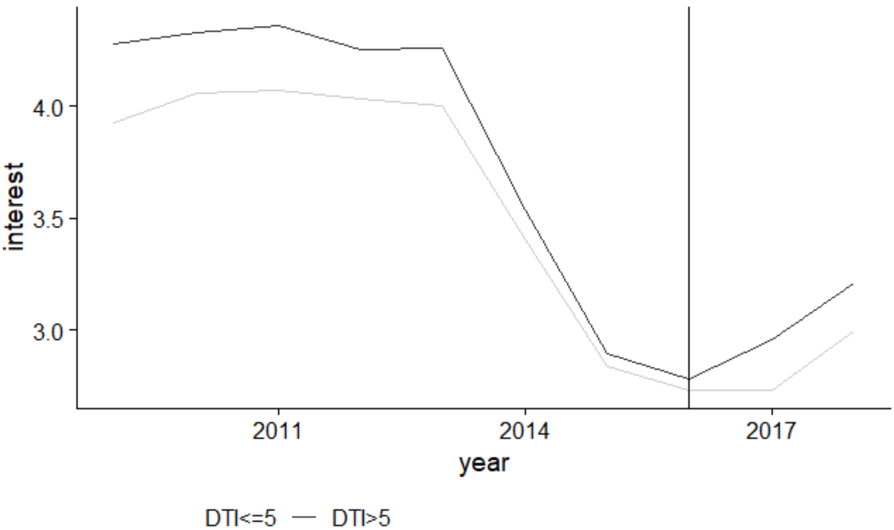
Notes: This figure illustrates the distribution of DTI before and after the introduction of the DTI-limit of five. DTI is defined as total debt divided by total income. The vertical line shows the DTI-limit of five. Low-income is defined as the 20% of households with the lowest income (between NOK 300K and NOK 500K).

Figure 3 House values and household debt for house buyers, 2009–2018.



Notes: The right panel depicts the trends in household debt, while the left panel depicts trends in house values for households with DTI above (black line) and below (grey line) the limit of five, respectively. All variables are in log of 1000 NOK.

Figure 4 Implicit interest rates for house buyers, 2009–2018.



Notes: This figure illustrates implicit interest rates, calculated as interest expenses as a share of debt. A one year lead of this variable is used in the calculations to make sure that interest expenses over the total year after a home purchase is made is included.

Figure 2 presents the full distribution of DTI ratios for house buyers in 2016, the year prior to the introduction of the DTI limit, and 2018, the year following its implementation. For comparison, the distribution in 2014 is also included to demonstrate how the DTI distribution had shifted to the right over time. The distributions for 2016 and 2018 are strikingly similar, which again suggests that the regulation had only a modest effect. However, the increasing trend observed between 2014 and 2016 does not continue from 2016 to 2018, implying that the regulation may have had some influence. Despite this, the persistent "fat tail" on the right side of the 2018 distribution indicates that a significant number of households still exceed the DTI limit. As before, the figure highlights that low-income households tend to have higher DTI ratios than other groups.

Figure 3 illustrates the trends in average house values and average household debt (in logs) for house buyers from 2009 to 2018, comparing those with a DTI above five to those with a DTI of five or below. This allows us to assess whether the pre-regulation trends in these variables differ between the two groups. The primary takeaway from Figure 3 is that the trends in house values and household debt appear relatively similar for households below and above the limit prior to the implementation of the regulation. In Figure 4, the trends in implicit interest rates are shown. Here, the DTI limit's effect is more apparent, as interest rates seem to increase noticeably more for home buyers with a DTI ratio above five. However, caution is warranted in interpreting these results, as the pre-regulation trends for the treatment and control groups are not clearly parallel, which could affect the validity of the comparison.

Table 3 provides summary statistics for key variables among house buyers in 2016 and 2018. Household income, debt, and house values all increased over this period. As living area remained relatively constant, the price per square meter of housing also rose slightly. Other variables, such as interest expenses, the value of primary homes (as recorded in the tax registry), and financial wealth, showed an upward trend, while the age distribution of house buyers remained stable across the two years. The household debt-to-house-value (DTV) ratio decreased slightly between 2016 and 2018. However, the DTI ratio increased from an average

Table 3 Summary statistics households purchasing a home in 2016 and 2018.

	2016				2018			
	1. qu.	Med.	Mean	3. qu.	1. qu.	Med.	Mean	3. qu.
Income (ths.)	534	785	905	1120	563	812	946	1178
Debt(ths.)	1846	2672	3005	3818	1961	2816	3217	4063
House value(ths.)	2415	3162	3583	4300	2570	3350	3813	4550
Liv.area (sqm)	62	83	102	128	62	83	102	129
H.value/sqm(ths.)	26	36	41	53	28	39	44	58
Interest exp.(ths.)	26	48	58	77	28	50	61	82
Value.pr.home(ths.)	2171	3078	3171	4054	2353	3282	3415	4347
Fin.cap.(ths.)	80	197	409	452	87	211	426	473
Age	28	34	38	45	28	34	38	46
DTV	0.619	0.894	0.912	1.111	0.605	0.889	0.895	1.098
DTI	2.47	3.48	3.58	4.55	2.52	3.53	3.64	4.55
$DTI > 5$			17.9				18.3	
Observations	16,412				17,497			

Notes: This table provides summary statistics for house buyers in 2016 and 2018. DTV is defined as total debt divided by the price of the house. DTI is defined as total debt divided by total income. $DTI > 5$ is the fraction of households with a DTI ratio above five. Age is measured in years. The remaining variables are measured in 1000 NOK.

of 3.58 in 2016 to 3.64 in 2018, despite the implementation of the DTI limit of five in 2017. The proportion of households with a DTI above five also rose slightly, from 17.9% to 18.3%, reflecting a trend similar to that shown in Figure 1. This contrasts sharply with figures reported by banks to The Financial Supervisory Authority (2022a), which show a significant drop in mortgage volumes with a DTI above five after the regulation. Although the data are not directly comparable, it is noteworthy that there are no signs of loans bunching near or just below the DTI limit of five in the tax data, whereas The Financial Supervisory Authority (2022a) clearly indicates such bunching.

Table 4 Baseline results from the dynamic difference-in-differences estimation.

	log(House values)	log(Household debt)	log(Fin. assets)
Treat x I(year=2018)	0.007	-0.003	-0.016
	(0.016)	(0.026)	(0.051)
$Education_1$	0.239***	0.457***	0.495***
	(0.005)	(0.007)	(0.012)
$Education_2$	0.0356***	0.605***	0.763***
	(0.015)	(0.008)	(0.014)
Age	0.005***	-0.012***	0.022***
	(0.000)	(0.000)	(0.000)
HPgrowth	0.006***	0.002***	0.007***
	(0.001)	(0.001)	(0.001)
LagHPgrowth	0.013***	0.005***	0.007***
	(0.001)	(0.001)	(0.001)
Investor	-0.044***	0.283***	0.340***
	(0.004)	(0.008)	(0.011)
Single	-0.062***	-0.153***	-0.089***
	(0.004)	(0.007)	(0.011)
Unemployed	-0.022***	-0.016***	-0.084***
	(0.006)	(0.011)	(0.021)
Zip-code fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Observations	96,100	96,100	96,100

Notes: This table provides the baseline results from the main difference-in-differences estimation of Equation 2: $y_{i,t} = \alpha_k + \sum_{t=2013}^{2015} \beta_t \widehat{DTI}_{i,t}^{treated} + \sum_{t=2017}^{2018} \beta_t \widehat{DTI}_{i,t}^{treated} + \mathbf{b'X_{i,t}} + \delta_t + \widehat{DTI}_{i,t}^{treated} + \epsilon_{i,t}$. $\widehat{DTI}_{i,t}^{treated}$ are households predicted to have DTI above five in absence of the DTI-limit. Household controls are included in $\mathbf{X_{i,t}}$; age, education (Edu_1 and Edu_2 are variables representing higher education) and the dummy variables for unemployed, secondary home owners and single households. In addition, I control for current and lagged house price growth at the municipality level. Separate terms for year-fixed effects, δ_t, and the treatment variable are also included. *p<0.1; **p<0.05; ***p<0.01. All standard errors are clustered at the zip-code level.

4 Results

4.1 Baseline results

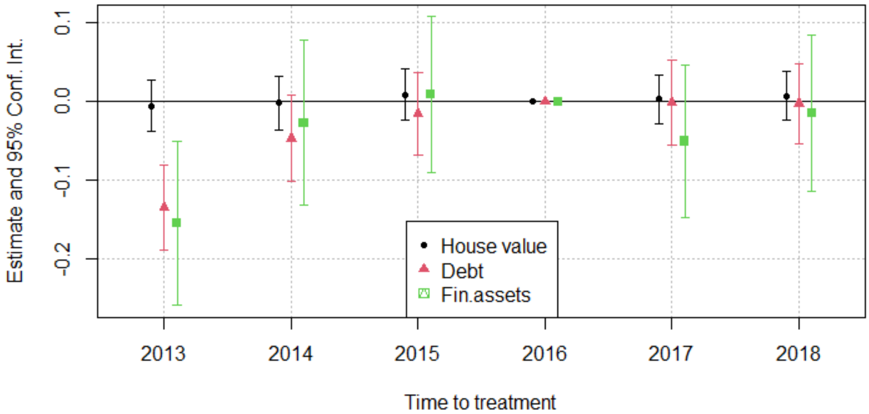
Table 4 presents the results of the baseline estimation, where all dependent variables are in logarithmic form. The first column shows the effect on the log of house values. The interaction term between the treatment variable (households predicted to have a DTI ratio above five) and the post variable (year 2018) is not statistically significant, with a positive sign contrary to the ex ante expectation that tighter mortgage regulations would negatively impact house values. Among the control variables, higher education and household age are positively associated with house values. Municipality-level house price growth in both the current and previous periods also correlates positively with house values. Conversely, dummy variables for households owning secondary homes, one-person households, and those with at least one unemployed adult show negative associations with house values.

The second column examines the effect on the log of household debt. Here, the interaction term between treatment and post is negative, aligning with expectations, but it is close to zero and remains statistically insignificant. The third column presents the effect on financial wealth, used as a proxy for liquid assets. Similarly, the interaction term is negative but statistically insignificant.

In summary, the difference-in-differences analysis provides no evidence of a significant negative effect of the DTI limit on house values, household debt, or financial wealth.

Figure 5 displays the coefficients for all interaction terms between year and the treatment group from 2013 to 2018. The coefficients for 2018 correspond to those reported in Table 4. Notably, there is no evidence of a short-term effect in 2017, the year the regulation was introduced. The inclusion of pre-2016 data serves to support the common trend assumption, ensuring that any negative, significant effects observed for the treatment group post-regulation would not already be present prior to the regulation.

Figure 5 Dynamic difference-in-difference estimation, baseline.



Notes: This figure depicts estimates of β_t's in Equation 2 with lines illustrating 95% confidence intervals. Household controls are included in $\mathbf{X_{i,t}}$; age, education and the dummy variables for unemployed, secondary home owners, and single households. In addition, I control for current and lagged house price growth at the municipality level. Year-fixed effects, δ_t, and the treatment variable are also included in the estimation. All standard errors are clustered at the zip-code level.

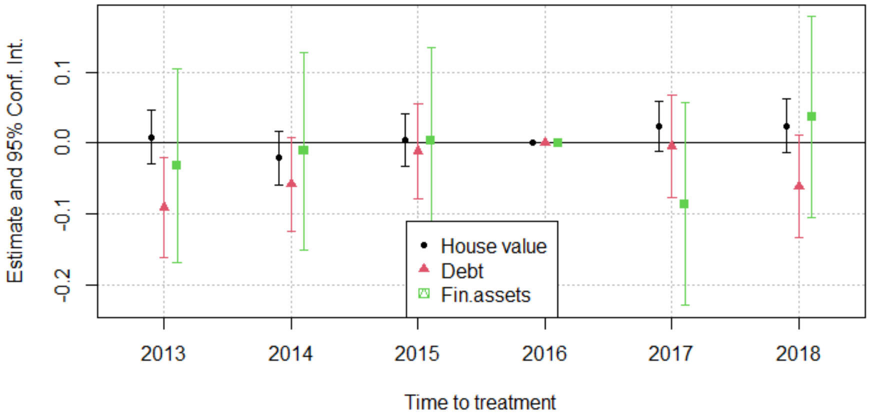
4.2 Low income households

Although the estimation using the total sample of households reveals no statistically significant effect, certain groups may be more impacted by the regulation than others. Low-income households and first-time buyers, for instance, are often cited as being disproportionately affected by mortgage regulations (see, for example, Acharya et al. (2022), Peydró et al. (2020)). To explore this possibility, I tested whether the DTI limit might have distinct effects on these groups that are not captured by the average effects reported in the previous subsection.

Low-income households are defined here as the 20% of households with the lowest incomes, earning approximately NOK 300,000–500,000 annually. Figure 6 replicates Figure 5, but focuses exclusively on low-income households. The results indicate no clear evidence of a negative effect of the regulation on house values for this group.

For household debt, the effect is negative with a p-value of 0.10, though it does not reach statistical significance at the 5% level. While this suggests a potential effect on household

Figure 6 Dynamic difference-in-difference estimation, low-income households.



Notes: This figure depicts estimates of β_t's in Equation 2 with lines illustrating 95% confidence intervals. Low-income households are defined as the 20% of home buyers with the lowest income in a given year. Household controls are included in $\mathbf{X_{i,t}}$; age, education and the dummy variables for unemployed, secondary home owners, and single households. In addition, I control for current and lagged house price growth at the municipality level. Year-fixed effects, δ_t, and the treatment variable are also included in the estimation. All standard errors are clustered at the zip-code level.

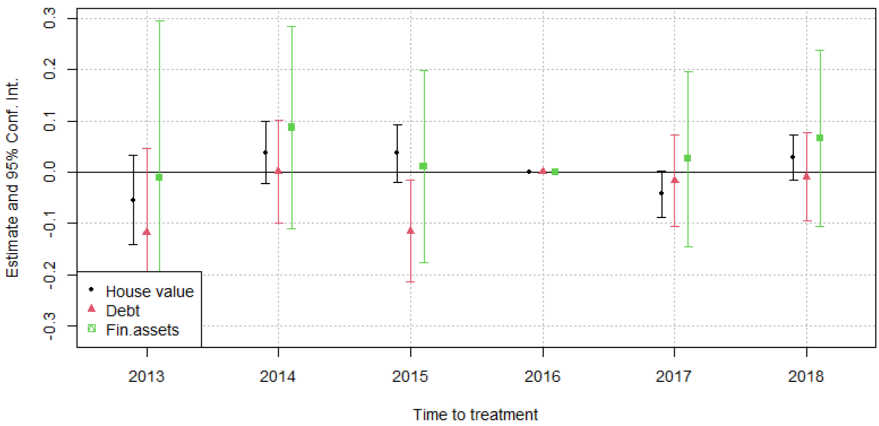
debt, caution is warranted in drawing causal conclusions, as a similar effect is observed in 2013 and 2014, well before the regulation was implemented.

4.3 First time buyers

The DTI limit may have had a more significant impact on first-time buyers compared to existing homeowners. First-time buyers are defined here as households without housing wealth in the year prior to purchasing a home, and who are between the ages of 20 and 35. Households owning secondary homes are excluded from this definition. While some individuals categorized as first-time buyers may have previously been in the housing market (for example, due to temporary moves abroad or divorce), the majority of households within this definition are likely to be true first-time buyers.

For first-time buyers, as with the complete sample, there is no evidence of a negative effect on house values, household debt, or financial assets following the introduction of the

Figure 7 Dynamic difference-in-difference estimation, first time buyers.



Notes: This figure depicts estimates of β_t's in Equation 2 with lines illustrating 95% confidence intervals. First time buyers are defined as households with the oldest household member between 20 and 35 years of age and with zero housing wealth the year prior to purchasing a home. Household controls are included in $\mathbf{X_{i,t}}$; age, education and the dummy variables for unemployed, secondary home owners, and single households. In addition, I control for current and lagged house price growth at the municipality level. Year-fixed effects, δ_t, and the treatment variable are also included in the estimation. All standard errors are clustered at the zip-code level.

DTI limit, as shown in Figure 7.

4.4 Effect on mortgage rates

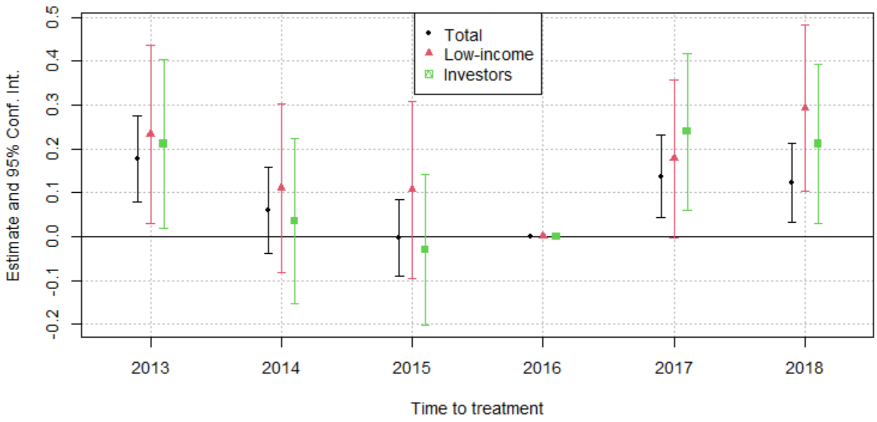
Although a significant share of homebuyers remained non-compliant with the DTI limit after its implementation, it is possible that banks have adapted to the regulation in other ways, such as by charging higher interest rates on mortgages for borrowers exceeding the limit. Table 5 and Figure 8 present the results of a dynamic difference-in-differences estimation, where the treated group consists of households with a DTI above five, and the dependent variable is the implicit interest rate one year ahead. For all households, we observe an increase in interest rates of approximately 0.12 percentage points. This increase is more pronounced for low-income households (0.29 percentage points) and households owning secondary homes (0.21 percentage points). The effect is statistically significant for all groups.

Table 5 Baseline results difference-in-difference estimation, implicit interest rates.

	All households	Low-income	Investors
Treat x I(year=2018)	0.1220***	0.2929***	0.2110**
	(0.0460)	(0.0961)	(0.0927)
$Education_1$	-0.1824***	-0.3991***	-0.1889***
	(0.0128)	(0.1201)	(0.0328)
$Education_2$	-0.2955***	-0.4533***	-0.3164***
	(0.0144)	(0.1252)	(0.0366)
Age	0.0209***	0.0197***	0.0270***
	(0.0006)	(0.0012)	(0.0012)
HPgrowth	-0.0017	0.0035	-0.0022
	(0.0014)	(0.0036)	(0.0034)
LagHPgrowth	0.0006	0.0020	-0.0020
	(0.0015)	(0.0039)	(0.0036)
Investor	0.1790***	0.3011***	
	(0.0149)	(0.0413)	
Single	0.0748***	0.0331	0.1741***
	(0.0148)	(0.0328)	(0.0379)
Unemployed	0.1214***	0.2379***	0.1695**
	(0.0259)	(0.0823)	(0.0688)
Zip-code fixed effects	✓	✓	✓
Year fixed effects	✓	✓	✓
Observations	96,100	19,448	22,414

Notes: This table shows the baseline results from the main difference-in-difference estimation. The treated group is households with DTI above 5. Household controls are included in $\mathbf{X_{i,t}}$; age, education (Edu_1 and Edu_2 are variables representing higher education) and the dummy variables for unemployed, secondary home owners and single households. In addition, I control for current and lagged house price growth at the municipality level. Separate terms for year-fixed effects, δ_t, and the treatment variable are also included. *p<0.1; **p<0.05; ***p<0.01. All standard errors are clustered at the zip-code level.

Figure 8 Dynamic difference-in-difference estimation, implicit interest rate



Notes: This figure depicts estimates of β_t's in Equation 2 with lines illustrating 95% confidence intervals. Household controls are included in $\mathbf{X_{i,t}}$; age, education and the dummy variables for unemployed, secondary home owners, and single households. In addition, I control for current and lagged house price growth at the municipality level. Year-fixed effects, δ_t, and the treatment variable are also included in the estimation. All standard errors are clustered at the zip-code level.

This could suggest that banks are requiring higher interest rates for mortgages with a DTI above five after the regulation, either by raising the floating rate or by mandating that these households take out fixed-rate mortgages, which at the time had higher rates than floating-rate loans. Additionally, the rise in consumer loan mortgages—on which interest rates are higher than those for mortgages and which were not covered by the regulation at the time—may partly explain the overall increase in average implicit interest rates.

However, as shown in Figure 8, the coefficients are also positive and significant for 2013, which warrants caution in drawing causal conclusions. This suggests that the assumption of parallel pre-trends may be violated. Moreover, a simple Wald test for parallel pre-trends is rejected, with a p-value of 3%. The key takeaway is that, while we observe an increase in interest rates for households with DTI above five following the implementation of the DTI limit, it is difficult to conclusively attribute this change to the regulation itself.

Table 6 Number of house buyers below and above the limit.

	2016		2017		2018	
	$DTI < 5$	$DTI > 5$	$DTI < 5$	$DTI > 5$	$DTI < 5$	$DTI > 5$
Total sample, households	13,480	2,932	13,592	2,994	14,297	3,200
Total sample, %	82.1	17.9	81.9	18.1	81.7	18.3
Low-income, households	2,238	1,044	2,252	1,065	2,383	1,117
Low-income, %	68.2	31.8	67.9	32.1	68.1	31.9
First-time-buyers, households	4,300	1,020	4,306	949	4,648	1,037
First-time-buyers, %	80.8	19.2	81.9	18.1	81.8	18.2

Notes: This table provides an overview of the number of house buyers before the implementation of the DTI-limit (2016) and after (2017 and 2018).

4.5 Extensive margin

In addition to examining the effect on outcome variables for house buyers—the intensive margin—there may also be effects on the extensive margin. Specifically, some households who would have purchased a home without the DTI limit may choose not to buy or may be unable to.

Since this dataset only includes house buyers, drawing causal conclusions about extensive margin effects is challenging. However, a simple comparison of the number of house buyers before and after the regulation, as well as those with DTI below and above the limit of five, does not suggest any strong extensive margin effects. The total number of house buyers has increased, as has the number of buyers with DTI above the limit, as shown in Table 6. For first-time buyers, there is a slight decrease in the number of house buyers with DTI above the limit in 2017. This could indicate an extensive margin effect, but it could also reflect an intensive margin effect, where some buyers have reduced their DTI. Given the overall increase in the total number of house buyers, the extensive margin effect is likely to be modest. While it is not possible to definitively rule out an extensive margin effect based on this analysis, the data suggest that any such effect is likely to be relatively small.

4.6 Discussion: Adaptation and avoidance

The most notable finding so far is that the DTI limit in Norway does not appear to be binding, which may help explain the lack of effect on house values and household debt. Several factors make this result surprising. First, studies from other countries, such as Acharya et al. (2022) and Peydró et al. (2020), have found clearer effects of DTI limits on house prices and household debt, particularly for low-income households. Second, data from banks reported to The Financial Supervisory Authority (2022a) (FSA) show clear bunching of DTI ratios around five, whereas the tax statistics used in this study reveal a fat tail on the right side of the DTI distribution, even after the limit was implemented (see Figure 2). These mentioned studies all rely on loan-level data reported by banks, while this study uses tax statistics at the household level, providing a more comprehensive view of household balance sheets.

The findings in this paper do not necessarily imply that banks are not complying with the DTI limit. The data used in this analysis differs in several ways from the data reported by banks to the FSA. For instance, the data includes intermediate financing (temporary mortgages for movers who buy a new home before selling their old one), which banks exclude from their DTI calculations. Additionally, households may have some tax-free income, such as income from renting out part of their home (up to 40%), which could influence their DTI. Moreover, the dataset in this study covers only 20 of the largest municipalities in Norway, rather than the entire country. In contrast, the FSA survey covers only a few weeks, while the tax statistics are reported at year-end and reflect the full year for flow variables such as income and interest expenses. Despite these data differences, it is puzzling that the DTI distribution remains largely unchanged before and after the DTI limit, according to the tax statistics, which capture the complete household balance sheet.

Banks are also allowed a flexibility quota of 10% (8% in Oslo) of the total mortgage volume, which can deviate from the DTI requirements each quarter. This flexibility might be large enough to prevent the DTI limit from being binding. Since refinancing of existing mortgages is included in the total volume, banks may allocate more of this flexibility to new mortgages. However, the results from The Financial Supervisory Authority (2022a) suggest that a significant portion of the flexibility quota is used for refinancing existing loans.

Another potential explanation for the modest change in the DTI distribution following the introduction of the DTI limit is that households with high DTIs may have co-borrowers outside the household, such as parents. A co-borrower shares responsibility for repaying the mortgage, and their income can be included in the DTI calculation. However, this would not be reflected in the tax statistics, as the debt would not be registered under the co-borrower's name, even though their income is used to calculate DTI. According to The Financial Supervisory Authority (2021), only 4% of house buyers had a co-borrower in the residential mortgage survey for fall 2021, though more than 18% of first-time buyers did. Several recent studies have documented substantial parental support in the housing market, including in Norway (Brandsaas, 2021; Wold et al., 2023).

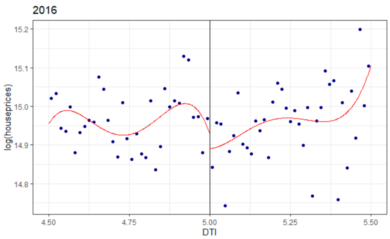
5 Robustness Checks

The difference-in-differences framework employed in Section 4 is based on the household model, as detailed in Appendix A, which is used to define the treatment group. In the first subsection, I present a regression discontinuity design as an alternative empirical approach. In the second subsection, I briefly discuss sensitivity checks within the difference-in-differences framework.

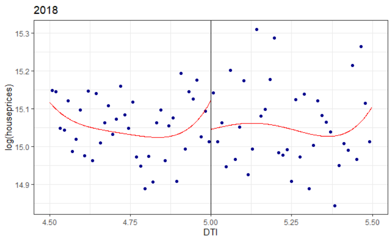
5.1 Regression discontinuity design

To examine how households with a DTI very close to the limit have responded to the regulation, regression discontinuity plots are presented in this section. The running variable is DTI, and the dependent variables are the log of house values and household debt. The cut-off is set at 5, and the sample is restricted to households with DTI between 4.5 and 5.5. The same analysis has been performed using the full sample and for wider DTI windows, such as 3-7 and 4-6, for robustness; however, these broader windows do not alter the main

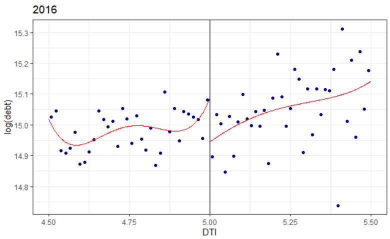
Figure 9 Regression discontinuity plots with DTI as running variable.



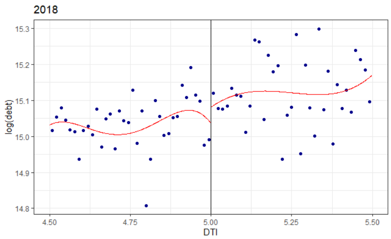
(a) House values 2016



(b) House values 2018



(c) Household debt 2016



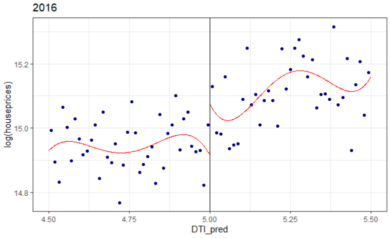
(d) Household debt 2018

findings.

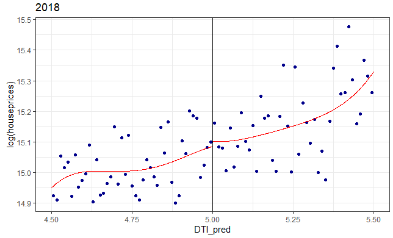
First, actual DTI is chosen as the running variable. The advantage of using actual DTI is that it does not rely on the household model, but the limitation is that households may adjust their DTI as a result of the regulation. Therefore, caution is warranted when making causal conclusions based on this exercise. Rather, this analysis aims to investigate whether behavioral changes occur near the limit, despite the lack of statistically significant effects in the difference-in-differences setup. It is also useful to examine the differences-in-discontinuities, as discussed by Grembi et al. (2016), which allows us to compare the discontinuity at DTI equal to 5 before and after the reform. This is illustrated in Figure 9.

The same exercise is repeated using predicted DTI as the running variable, as shown in Figure 10. This approach aligns more closely with the difference-in-differences framework, but focuses solely on households with DTI near the limit. The regression discontinuity plots confirm the previous findings, namely that there is no significant decline in house values or household debt around the DTI cut-off of five in 2018, after the regulation, compared to 2016, before the regulation. In Panel a and Panel b of Figure 10, we observe a positive

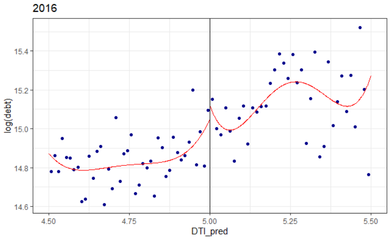
Figure 10 Regression discontinuity plots with predicted DTI as running variable.



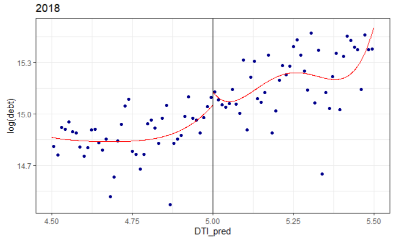
(a) House values 2016



(b) House values 2018



(c) Household debt 2016



(d) Household debt 2018

break around the cut-off in 2016, which is not present in 2018. While this might suggest a negative effect on house values from the regulation, the variation is quite large, and the result is sensitive to whether a slightly wider window of predicted DTI is included. Overall, the conclusion remains that the DTI-limit appears to have had a limited effect on house values and household debt in Norway.

5.2 Other sensitivity analysis

This section explores alternative assumptions within the difference-in-differences framework. The first test examines whether the lack of significant results is influenced by the tails of the DTI distribution. To address this, the baseline estimation is repeated with the sample restricted to households with a DTI between 3 and 7. The results, presented in Appendix B, align with the findings of the main analysis.

The second test investigates whether the results change when an alternative treatment group is used. Specifically, treatment is redefined to include households with a predicted DTI between 4 and 5. The results, shown in Appendix C, reveal that the significant positive effect on interest rates disappears with the revised treatment definition, while the other effects remain statistically insignificant.

The regulation was announced prior to its implementation, which may have influenced household behavior before the limit took effect. To rule out the possibility of announcement effects, the sample is adjusted to exclude data from the last quarter of each year. These results, detailed in Appendix D, closely mirror the baseline estimation, suggesting that announcement effects do not drive the conclusions.

6 Conclusion

Residential mortgage regulations are designed to mitigate financial imbalances that can arise from unsustainably high growth in house prices and household debt, thereby reducing the risk of severe economic crises. This paper analyzes the debt-to-income (DTI) limit introduced in Norway in 2017 using a difference-in-differences framework. The findings indicate that the limit had no significant effect on house values, household debt, or financial assets. Notably, the DTI-limit does not appear to be binding for many households, as 18% of house buyers continued to have a DTI above five even after the regulation was implemented.

However, implicit interest rates increased for affected house buyers following the regulation—by approximately 0.12–0.14 percentage points on average, and by over 0.20 percentage points for low-income households and investors.

Norway was one of 26 European countries to implement residential mortgage regulations by 2022 (The Financial Supervisory Authority, 2022b). For policymakers, understanding how such tools operate—even when they do not achieve their intended effects—is essential. The results of this study suggest that the Norwegian DTI-limit was not effective in curbing vulnerabilities in the household sector, as it failed to slow the growth of house values and household debt among house buyers. Further research is needed to examine the applicability of these findings to other countries and contexts. If these results hold elsewhere, policymakers may need to consider alternatives to DTI-limits or ensure that such limits are designed to be more binding.

Future research could benefit from expanded datasets, including information on mortgage refinancing, intergenerational links in the housing market, and consumer loans. Such data could enhance our understanding of why the DTI-limit fails to constrain a significant share of house buyers and inform more effective policy design in the future.

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Declaration of generative AI and AI-assisted technologies in the writing process

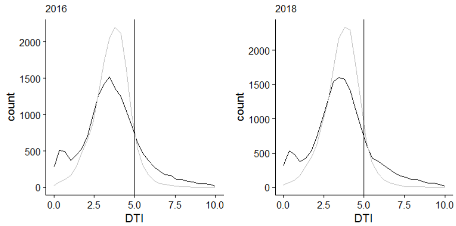
During the preparation of this work the author used Chat GPT in order to improve language. After using this tool/service, the author reviewed and edited the content as needed and takes full responsibility for the content of the publication.

Appendix A: Household OLS-model

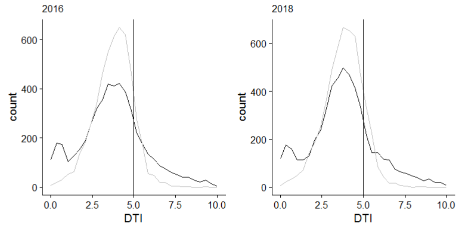
Table A.1. Goodness of fit household model.

	Baseline	Extended	Oslo	Low-income	FTB	Investors
Adj. R^2	0.361	0.363	0.400	0.352	0.249	0.373
RMSE	1.45	1.45	1.97	1.76	1.56	1.85
+/- 10 percent	26	26	27	24	24	19
+/- 20 percent	47	47	47	44	45	36
+/- 30 percent	63	63	62	56	61	51

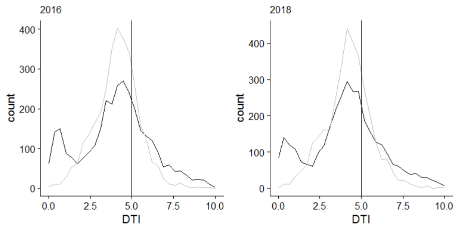
Figure A.1. Predictions from household model.



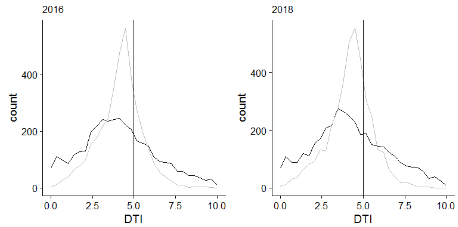
(a) Total



(b) Oslo



(c) Low-income



(d) Investors

To create a treatment group for the difference-in-difference analysis, a household model is applied, in which DTI is regressed on a set of household characteristics in the year prior to implementation of the DTI-limit (2016).

The model is shown in Equation 1. DTI at the household level is regressed on household age and education, zip-code, dummy variables for households with a secondary home, single households, each income quintile and households with one or more unemployed members.

Also, several household balance sheet variables are included: current and lagged values of interest expenses, the value of primary home and financial wealth in addition to lagged values of income and debt.

In the table above, a few measures of goodness of fit of the household model are presented. Half of the data in 2016 is randomly chosen to make the model and the other half to test it. We can see from the adjusted R^2 that the model can not explain all variation in DTI and that the root mean squared error is relatively high for all models. For the baseline model a little less than 50% of the observations have predicted DTI's that deviate no more than 20% from the actual value. For the model for Oslo, low-income and investors, the goodness of fit is a little less accurate than for the baseline model.

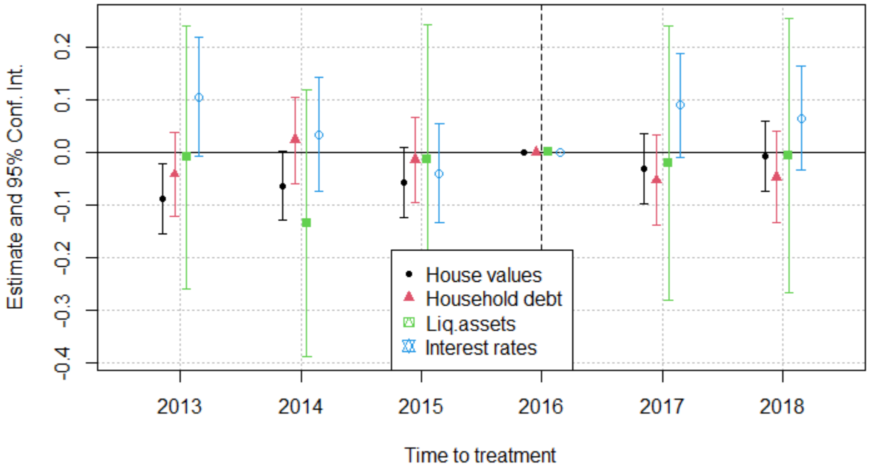
Machine learning methods such as Lasso, Random Forest and Boosting, have similar or poorer goodness of fit than the OLS-model. Neither does including more variables in the model or including squared terms of the X-variables and interaction terms of age and lagged income, debt and financial wealth (see column two) improve goodness of fit.

In Figure A.1., the actual distribution of DTI is shown in black and the predicted distribution of DTI from the OLS-model in grey in 2016 (in-sample) to the left and in 2018 (out-of-sample) to the right to give a visual image of how well the model fits. The main limitation of the model is that it is incapable of predicting the fat tails in the actual distribution. This is a limitation of the framework because the treatment group is in the right tail and the model underestimates the fraction of households in this group.

Appendix B: DTI between 3 and 7

In Figure B.1. I present the results for all households with DTI between 3 and 7. The results for house values, household debt and financial assets are in line with the findings in the baseline estimation, while the effect on interest rates is no longer significant.

Figure B.1. Results dynamic diff-in-diff for households with DTI between 3 and 7.

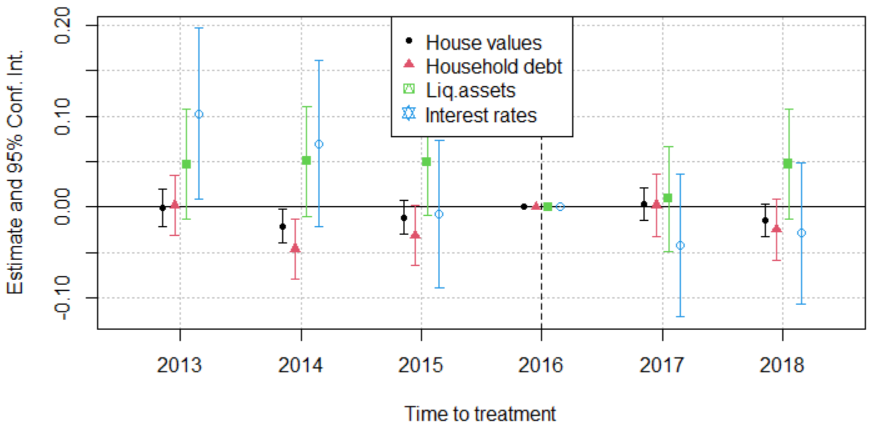


Notes: This figure depicts estimates of β_t's in Equation 2 with lines illustrating 95% confidence intervals. Household controls are included in $\mathbf{X_{i,t}}$; age, education and the dummy variables for unemployed, secondary home owners, and single households. In addition, I control for current and lagged house price growth at the municipality level. Year-fixed effects, δ_t, and the treatment variable are also included in the estimation. All standard errors are clustered at the zip-code level.

Appendix C: Alternative treatment

Figure C.1. illustrates the results with the treatment group defined as households with predicted DTI between 4 and 5. The effects on house values, household debt and financial assets remain insignificant and the effect on interest rates vanishes.

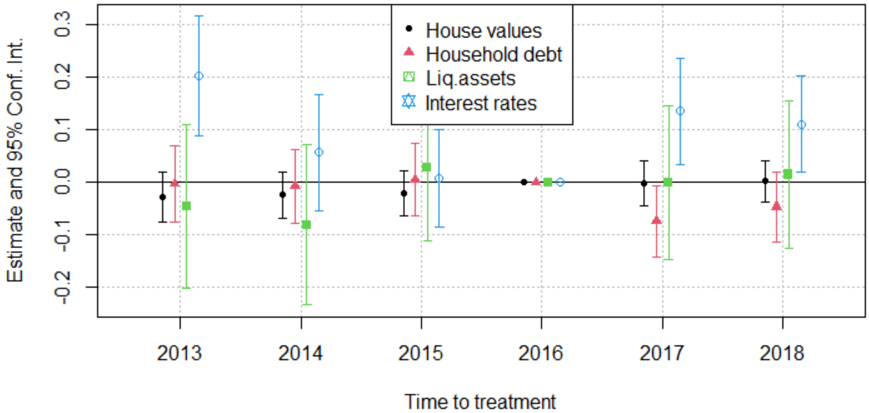
Figure C.1. Results dynamic diff-in-diff for treatment group predicted DTI 4–5.



Notes: This figure depicts estimates of β_t's in Equation 2 with lines illustrating 95% confidence intervals. Household controls are included in $\mathbf{X_{i,t}}$; age, education and the dummy variables for unemployed, secondary home owners, and single households. In addition, I control for current and lagged house price growth at the municipality level. Year-fixed effects, δ_t, and the treatment variable are also included in the estimation. All standard errors are clustered at the zip-code level.

Appendix D: Announcement effects

Figure D.1. Results dynamic diff-in-diff accounting for announcement effects.



Notes: This figure depicts estimates of β_t's in Equation 2 with lines illustrating 95% confidence intervals. Household controls are included in $\mathbf{X_{i,t}}$; age, education and the dummy variables for unemployed, secondary home owners, and single households. In addition, I control for current and lagged house price growth at the municipality level. Year-fixed effects, δ_t, and the treatment variable are also included in the estimation. All standard errors are clustered at the zip-code level.

The DTI-limit of five was announced by the Ministry of Finance 14 December 2016 and implemented 1 January 2017. Hence, the period from announcement to implementation was very short and since housing transactions involve some planning, this is likely too short for households to be able to adapt prior to the implementation. However, the FSA published an advise to implement such a limit 8 September 2016 and this suggestion was debated during the fall of 2016. Therefore, one could imagine that some households aiming for a high DTI responded to this by for example buying a house before year end to be able to borrow more. To investigate this, I have left out the last quarter of the year in each year and done a separate estimation. All results are similar to the main estimation, suggesting that announcement effects are not driving the results.

2 How Does the Lending Regulation Work? A Summary of the Research Literature.

How Does the Lending Regulation Work? A Summary of the Research Literature [*]

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January 9, 2025

In this article, we aim to address whether the Norwegian lending regulation has achieved its intended purpose and what costs it imposes on households. The research literature suggests that mortgage regulations contribute to somewhat lower growth in debt and house prices, but it is less certain whether they effectively reduce households' vulnerability to unforeseen events such as interest rate hikes and unemployment. At the same time, the lending regulation imposes costs on many households by limiting their ability to smooth consumption and potentially making it harder for young households to purchase their first home. The regulation may also amplify the importance of having wealthy parents for entering the housing market. High inflation and interest rates may reduce the need for the regulation while increasing its costs.

[*]A Norwegian version of this article was published in *Samfunnsøkonomen 2-24* (link: https://www.samfunnsokonomen.no/aktuell-analyse/hvordan-virker-utlansforskriften-en-oppsummering-av-forskningslitteraturen/). This article should not be used to represent the views of Norges Bank. The opinions expressed here reflect those of the authors and cannot necessarily be attributed to Norges Bank. We extend our thanks to Gisle Natvik, Henrik Borchgrevink, Kjersti-Gro Lindquist, the editor of Samfunnsøkonomen, Rune Jansen Hagen, and an anonymous referee for their valuable comments.

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1 Introduction and background

In the aftermath of the 2008–2009 financial crisis, numerous studies highlighted how rapid growth in household debt and house prices can amplify economic downturns. Specifically, several studies found that high debt growth prior to a negative shock can exacerbate the decline in household demand when the shock occurs.[1] Against this backdrop, macroprudential policies have emerged over the past decade as a popular complement to monetary and fiscal policy. Tools such as capital buffer requirements for banks and regulations on household mortgages have become common. According to The Financial Supervisory Authority (2022), 26 European countries had implemented some form of mortgage regulation by 2022.

In recent years, studies have provided a more nuanced understanding of debt as the primary driver of household vulnerability. Liquidity constraints have been increasingly emphasized as critical to understanding household vulnerability to interest rate increases and income shocks. Relevant studies include Kaplan and Violante (2014), Holm et al. (2021), Fagereng et al. (2021), Gulbrandsen (2023), Almgren et al. (2022).

Table 1 The Norwegian lending regulation

	Guidelines		Regulation				
Year	2010	2011	2015	2017	2020	2020	2023
Maximum loan-to-value	0.90	0.85	0.85	0.85	0.85	0.85	0.85
Maximum debt-to-income	3			5	5	5	5
Maximum loan-to-value to avoid amortization		0.70	0.70	0.60	0.60	0.60	0.60
Interest rate stress		5pp	5pp	5pp	5pp	5pp	3pp*
Maximum loan-to-value secondary home	0.90	0.85	0.85	0.85	0.85	0.85	0.85
Maximum loan-to-value secondary home Oslo	0.90	0.85	0.85	0.60	0.60	0.60	0.85
Flexibility quota			0.10	0.10	0.20	0.10	0.10
Flexibility quota Oslo			0.10	0.08	0.20	0.08	0.08

Note: This table gives an overview of residential mortgage guidelines and regulations in Norway. Loan-to-value is defined as the value of the mortgage relative to the value of the residence, while debt-to-income is defined as total debt relative to income before taxes. *A required minimum stress interest rate of 7%.

[1] See for example Anundsen et al. (2016), Eggertsson and Krugman (2012), Jorda et al. (2013), Korinek and Simsek (2016), Mian et al. (2013), Mian and Sufi (2018), Mian et al. (2017), Reinhart and Rogoff (2009).

In Norway, the first guidelines for mortgages were introduced in 2010, and these guidelines were formalized into regulation in 2015. The requirements have varied over time, as summarized in Table 1. The first column outlines the original 2010 guidelines, which included a (voluntary) maximum debt-to-income ratio of 3 (total debt relative to income), a maximum loan-to-value ratio of 90% (the mortgage amount relative to the value of the property), and an amortization requirement for loans with "high loan-to-value ratios." At the end of 2011, both the loan-to-value and amortization requirements were tightened, while the debt-to-income ratio requirement was removed. A stress test was also introduced, requiring households to be able to service their mortgages if interest rates increased by 5 percentage points. These changes are reflected in the second column of Table 1.

In 2015, the guidelines were formalized into regulation, and banks were allowed a flexibility quota of 10% of their lending volume to deviate from the requirements each quarter (see column 3). In 2017, the regulation was tightened across multiple dimensions. The amortization requirement became stricter, and a maximum debt-to-income ratio was reintroduced—this time set at 5 times income. A separate rule was introduced for Oslo, requiring a maximum loan-to-value ratio of 60% for secondary homes. Additionally, Oslo's flexibility quota was reduced to 8%. These changes are reflected in column 4.

During the economic downturn caused by the COVID-19 pandemic, the flexibility quota was temporarily increased to 20% in the second and third quarters of 2020, both in Oslo and the rest of the country (columns 5 and 6). The most recent changes to the regulation, shown in the final column of Table 1, took effect in January 2023. The interest rate stress test was reduced to 3 percentage points, but with the condition that the total stress-tested interest rate must be at least 7%. At the same time, the loan-to-value requirement for secondary homes in Oslo was removed.

The current lending regulation is set to expire at the end of 2024, making this a natural time to evaluate the experiences gained so far. In this article, we summarize recent research on the effects of various aspects of the lending regulation. We aim to address two main questions:

1. Do mortgage regulations achieve its intended purpose?
2. What are the costs of mortgage regulations?

We place particular emphasis on empirical studies of the Norwegian regulation, as these studies provide clear insights into how the measures have worked in Norway. However, the empirical analyses primarily capture the effects of individual measures over relatively short time frames, as the regulation has only been in place for just over a decade. Therefore, we also incorporate more theoretical studies to provide insights into the mechanisms of mortgage regulations over the long term. In addition, we include empirical studies from other countries where Norwegian studies are lacking or where international research can help support or refine the Norwegian findings. Differences between Norway and other countries—such as institutional structures and economic conditions—mean that Norwegian studies are the most relevant, and they carry the most weight in our conclusions.

2 Do mortgage regulations achieve its intended purpose?

2.1 What is the intended purpose?

The arguments for what the Norwegian lending regulation is meant to achieve varies across stakeholders and over time. Here, we aim to highlight some key considerations, based on the authorities' stated reasons for introducing the mortgage guidelines in 2010, as well as the rationale for the current lending regulation. However, we cannot rule out that other considerations were also weighed by the authorities.

According to The Financial Supervisory Authority (2010, 2020) and The Ministry of Finance (2024), the regulation was introduced against the backdrop of strong growth in house prices and household debt. We interpret this as indicating that a primary objective of the regulation is to curb the growth of debt and house prices. Additionally, The Financial Supervisory Authority (2020) emphasizes the aim of limiting debt for particularly vulnerable households, and The Financial Supervisory Authority (2010) highlights that the mortgage guidelines could help reduce the prevalence of particularly high loans, thereby making households and banks more resilient to economic downturns.

We interpret this as an intention to reduce household vulnerability to income loss, rising interest rates, and house price declines, as well as to limit loan losses for banks.

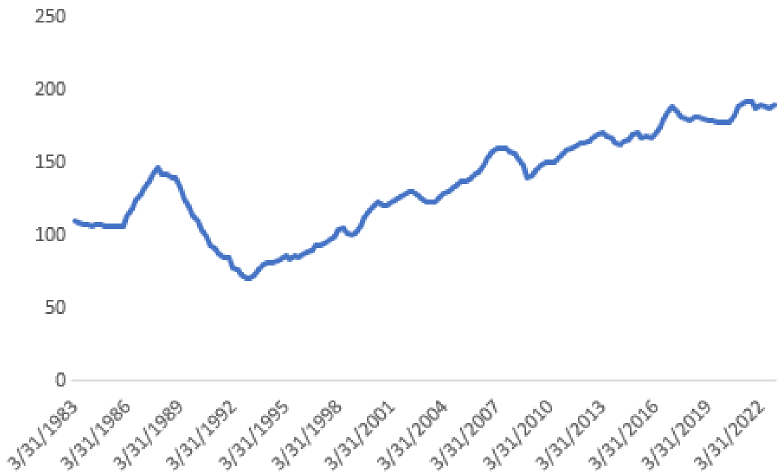

Figure 1 House prices/disposable income per capita, index 1998=100.

Notes: This figure depicts house prices relative to disposable income per capita in Norway from 1983 to 2022.
Sources: Real Estate Norway, Statistics Norway and Norges Bank

In Norway, growth in house prices and household debt has been high over several decades. In Figure 1, we can see that house prices have increased significantly more than per capita income over the past 40 years. Norwegian households' debt has also risen as a share of disposable income over the same period. In Figure 2, the blue line depicts the average debt-to-income ratio for Norwegian households (debt as a share of disposable income), which has increased from below 100% in the 1990s to nearly 240% in 2023. At the same time, low and declining interest rates have kept the interest rate burden (interest expenses as a share of disposable income) low, despite the rising debt-to-income ratio. This is reflected in the dark gray line in Figure 2. Over the last couple of years, however, the nominal interest rate burden has increased significantly, in line with higher policy rates.

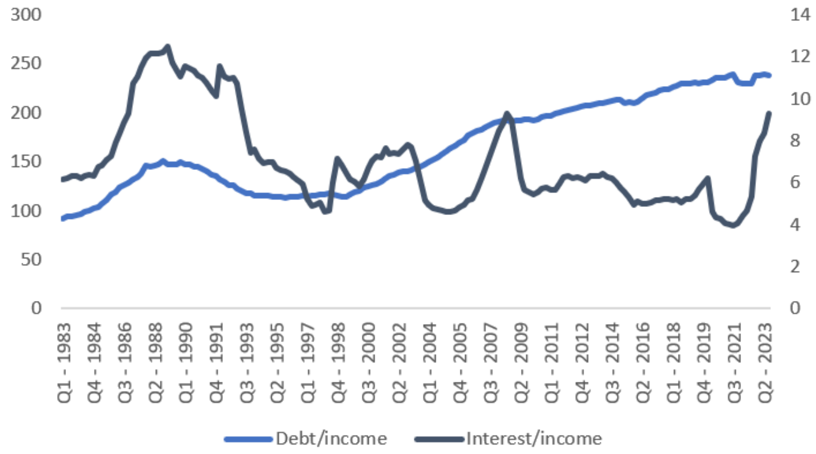

Figure 2 Debt to income and interest rate burden, %.

Notes: This figure depicts average debt relative to disposable income and interest rate expenses as share of disposable income for all Norwegian households from 1983 to 2023. **Sources**:Statistics Norway and Norges Bank

Mortgage regulations, in the form of requirements for maximum loan-to-value and debt-to-income ratios, as well as amortization requirements, reduce the debt of households that are subject to these requirements. This can reduce the purchasing power of these households in the housing market, leading them to either forgo purchasing a home or buy a less expensive one. This reduction in demand can, in turn, dampen growth in house prices. How significant these effects are for the affected households, and what impact does the regulation have on debt growth and house price growth at the macro level? To answer this, we begin with a review of empirical studies of the Norwegian measures, as these are particularly relevant in evaluating the Norwegian lending regulation.

Aastveit et al. (2020) analyze the impact of the loan-to-value ratio requirement on Norwegian households using microdata from housing transactions and tax statistics at the household level. To identify the effect of the requirement, households exposed to the requirement are compared with those not exposed, both before and after the guidelines were introduced in 2010 and tightened in 2012. The study shows that homebuyers reduced their debt somewhat, both after the introduction of the guidelines and after the tightening. However, most of the reduction in debt uptake came from postponed or canceled home purchases. At the macro level, the study finds a negative but modest effect on household debt growth.

Dørum (2022) and Fjære-Lindkjenn (2023) examine the effect of the Norwegian debt-to-income ratio requirement introduced in 2017, using the same household data and a similar method as Aastveit et al. (2020). Both studies find that the requirement led to higher interest expenses without any significant effect on the debt of affected households. The increase in interest expenses may be caused by banks requiring a higher floating or fixed interest rate for households in violation of the requirement. Another possible explanation could be that some of the mortgage debt for households in violation of the requirement was replaced with unsecured debt. The share of homebuyers in violation of the debt-to-income limit of five remained relatively high even after the requirement was introduced. In contrary, the Financial Supervisory Authority's (The Financial Supervisory Authority, 2023) mortgage survey shows that banks report very few borrowers exceeding the debt-to-income ratio of five after the requirement was implemented in 2017. The reason for the difference between the mortgage survey and the household-level tax statistics is not clear, but one possible explanation could be that households have a co-borrower outside the household, such as a parent, who is included in the income calculation by banks but does not appear in the tax records. However, further analysis is needed to understand the apparent discrepancy between what the mortgage survey shows and what the tax data indicates.

Borchgrevink and Thorstensen (2018) also study the effect of the debt-to-income ratio requirement. The authors compare house price developments in geographic areas exposed to the requirement before its introduction with other areas. They find that the requirement had a negative but short-lived effect on house price growth. They also find some evidence of reduced debt in exposed areas after the introduction of the requirement.

In an ongoing study of the impact of amortization requirements on mortgages in Norway, both theoretically and empirically, Ellingsrud et al. (2023) find that a stricter amortization requirement leads to slightly lower loan-to-value ratios and house prices. Studies from Denmark and Sweden also find a negative relationship between amortization requirements and debt (Bäckman et al., 2020, 2023).

The empirical findings from Norway align well with the results from Balke et al. (2023), who analyze household adjustments to the loan-to-value ratio requirement in the long run using a theoretical model with heterogeneous agents. The analysis shows that a stricter loan-to-value ratio requirement reduces household debt in the long term, both by lowering the number of homeowners and by reducing debt among homeowners. They also find a marginal negative effect on house prices in the long term.

There are also several empirical studies from other countries that largely support the findings from Norway. One example is Van Bekkum et al. (2019), one of the few international studies, which, like several of the Norwegian studies, uses household-level tax statistics to analyze the effects of the loan-to-value ratio requirement. The study finds that first-time homebuyers in the Netherlands reduce their borrowing as a result of the requirement. Other studies use aggregated data or loan data reported by banks to analyze the effect of mortgage regulations on household debt and house prices. Here are some examples from countries that share certain characteristics with Norway. Acharya et al. (2022) use loan data and find that the loan-to-value and loan-to-income requirements in Ireland had little effect on total lending to households, but that a reallocation of lending from areas in violation of the requirements to areas that were less in violation dampened house price growth in areas that had high house price growth before the regulation was introduced. Peydró et al. (2020) analyze loan-to-income requirements in the UK, also using loan data. They find a modest negative effect on debt and house prices. DeFusco et al. (2020) find that debt-to-income requirement in the US resulted in a significant reduction in lending, while Armstrong et al. (2019) find that loan-to-value ratio requirements in New Zealand had a short-lived negative effect on house prices. They further find that the effect on house prices was more short-lived in areas that had high house price growth prior to the regulation.

In summary, most studies conclude that regulations on loan-to-value ratios, debt-to-income ratios, and amortization requirements have played a role in curbing the growth of house prices and household debt. However, their impact on debt at the macroeconomic level appears relatively modest, while the effect on house price growth tends to be short-lived.

2.2 Make households less vulnerable to negative shocks

High household debt is often highlighted as a central vulnerability in the Norwegian economy (see Figure 2 and Norges Bank (2023)). Lower debt and slower house price growth are unlikely to be goals in and of themselves. It is natural for house prices to increase when there is greater scarcity of housing and if the real interest rate declines. The ability to borrow increases households' welfare by enabling smoother consumption over the life cycle. However, authorities may want to limit the growth of house prices and debt, because high growth in these factors during an economic upswing can contribute to amplifying economic downturns. Several articles using aggregated macro data have documented that high growth in house prices and debt prior to a downturn increases the likelihood of a crisis and makes the crisis deeper and more prolonged. See, for example, Aastveit et al. (2023), Anundsen et al. (2016), Jorda et al. (2013), Reinhart and Rogoff (2009). Contributions from Mian and Sufi in the aftermath of the financial crisis, such as Mian et al. (2013), Mian and Sufi (2018), Mian et al. (2017), have also been central. They use regional variations in factors such as debt, wealth, income, and consumption across different regions in the US to show that high growth in household debt contributed to exacerbating the downturn through a larger decline in household demand. Therefore, lower debt growth may make households less vulnerable to unexpected events such as rising interest rates, income loss, or declining house prices (The Financial Supervisory Authority, 2010, 2020).

In the aftermath of the financial crisis, there has been much research focusing on the underlying mechanisms of this demand channel. Most studies assume that households act rationally to maximize their own welfare, borrowing at levels optimal for their individual circumstances. However, households may borrow more than what is socially optimal, as they often overlook the fact that high levels of debt can lead to greater reductions in consumption following a negative economic shock. This appears to create a negative externality on other households, which experience a more significant income reduction than they otherwise would have, see for example Eggertsson and Krugman (2012), Farhi and Werning (2016), Korinek and Simsek (2016). Such a demand externality can be amplified through house prices, as highly indebted households may sell their homes in response to a negative shock. This drives down house prices and thus reduces household equity, making them more credit-constrained. As a result, the negative externality on aggregate demand is further exacerbated, see for example Kiyotaki and Moore (1997) and Bianchi (2011).

The focus on debt as the main driver of household's consumption responses has, however, been nuanced by several studies in recent years. For example, liquidity has been highlighted as another important factor in understanding how households—and thus the macroeconomy—respond to shocks, see for example Kaplan and Violante (2014). Gulbrandsen (2023) summarizes much of the empirical literature investigating the relationship between debt and consumption. Several international studies find that it is households' access to liquid assets, rather than the level of debt itself, that is important for their consumption response to income shocks. This is supported by findings in a Norwegian study by Holm et al. (2021). The authors study the effect of lottery winnings on consumption and conclude that the consumption response to positive income shocks depends on the household's liquid assets, not its debt level. In ongoing work, which also uses Norwegian microdata, Fagereng et al. (2024) find that debt is the most important factor for consumption responses to income drops due to unemployment. The findings from these Norwegian studies suggest that high debt amplifies consumption responses to large, negative income shocks, but that liquidity is more important in the case of positive income shocks.

In the case of an interest rate hike, both Norwegian and international studies agree that both liquid assets and debt are crucial for the consumption response. Debt plays a larger role during interest rate hikes than other income shocks because it determines how much household disposable income is reduced by an interest rate increase. Holm et al. (2021), using Norwegian microdata, find that households with high net debt reduce consumption more than others when there is an unexpected interest rate hike. This is particularly true for households with few liquid assets, but also to some extent for households with liquid buffers. International studies also find a strong cash flow channel from monetary policy, see for example Almgren et al. (2022), Cloyne et al. (2019), Di Maggio et al. (2017), Flodén et al. (2020), Kinnerud (2024).

There are relatively few empirical studies that evaluate the effect of mortgage regulations on households' vulnerability to negative shocks. This may be due to a lack of access to household-level data, making it challenging to analyze households' full adaptation to regulations. One exception is Aastveit et al. (2020), who use Norwegian microdata to analyze the effect of loan-to-value (LTV) requirements on households' vulnerability in the short and medium term. They find that households reduce their liquid savings to meet the LTV requirements, making them both less indebted and less liquid. The total effect on households' vulnerability is not straightforward. The authors find that, overall, homeowners become more vulnerable to income loss, such as unemployment, due to the reduction in liquid assets. Aggregated, they find that the effect of Norway's LTV requirement on households' vulnerability is limited.

The main conclusion from Aastveit et al. (2020) is in line with theoretical studies, calibrated to both Norwegian and US data. Fagereng et al. (2017) use a model estimated on Norwegian data to show that a stricter LTV requirement has little effect on the aggregate consumption response to income changes in the long term. Balke et al. (2023), using a similar model estimated on US data, find that a stricter LTV requirement makes homeowners less vulnerable in the long run, while making renters more vulnerable. The overall effect on households' vulnerability is again small, while the study finds reduced sensitivity to higher interest rates.

Most international empirical studies on mortgage regulations use loan data or aggregated data and are less able to study the total effect on households' vulnerability. An exception is Van Bekkum et al. (2019), who, like Aastveit et al. (2020), find that LTV requirements both reduced household debt and liquidity. However, Van Bekkum et al. (2019) conclude that the overall effect on households is reduced vulnerability to income loss, while households experience less fluctuation in asset values after the reform in the Netherlands. An explanation for why the conclusion differs from the Norwegian study might be that the liquidity decline in this study is short-lived, whereas the liquidity decline appears to be somewhat more long-lasting in Norway.

In summary, most studies show that mortgage regulations have a modest effect on households' vulnerability to unexpected events. Moreover, the effect on households' liquid assets is important to consider when assessing how mortgage regulations impact households' vulnerability to negative shocks.

2.3 Reduce the risk of losses in the banking sector

Limiting loan losses in banks appears to be a somewhat secondary goal of the Norwegian lending regulation, but it is mentioned by the Financial Supervisory Authority (The Financial Supervisory Authority, 2010) in the circular regarding the initial guidelines for mortgage loans. During the Norwegian banking crisis in the early 1990s, significant losses in the banking sector led to banks tightening their lending practices, which in turn exacerbated the downturn. The experiences from the banking crisis are likely an important motivation for the goal of limiting loan losses in banks. Capital buffer requirements have likely been a more central tool for mitigating the negative effects of loan losses in banks on the economy than mortgage regulations. Norwegian banks' capital buffers have also increased significantly since the banking crisis, in line with stricter capital buffer requirements (Norges Bank, 2023).

Historically, loan losses on mortgages have been very small in Norway, even during the banking crisis (Andersen & Juelsrud, 2022). Kragh-Sørensen and Solheim (2014) assess the risk of direct losses on household loans as small. However, losses may still occur indirectly, through losses on loans to consumption-sensitive industries due to a fall in consumption or through losses on loans to commercial real estate and construction sectors if households demand for housing i reduced. They conclude that the potential for losses in the real estate sector has likely grown relative to consumption-sensitive industries in recent decades because banks have reduced their lending to consumption-sensitive sectors, while lending to real estate constitutes a large portion of the loan portfolio.

To the best of our knowledge, there are no Norwegian studies on mortgage regulations that directly address the risk of loan losses in banks. In a study examining the US debt-to-income requirement, DeFusco et al. (2020) find that the requirement had a very limited effect on total loan losses, even though the regulation reduced loans to households with high debt-to-income ratios. Acharya et al. (2022) investigate the effect of loan-to-value and debt-to-income requirements on bank risk-taking in Ireland. They find that banks responded to the requirements by reducing the risk in their mortgage loan portfolios. At the same time, banks increased the risk in their corporate lending and bond portfolios to maintain their overall level of risk-taking. Therefore, there is no evidence that mortgage regulations reduced the risk of losses in banks in Ireland. On the contrary, risk-taking in corporate loans increased, and as mentioned, corporate loans are where banks have historically experienced significant losses in Norway.

3 What are the costs of mortgage regulations?

Mortgage regulations are restrictive measures for both borrowers and lenders (The Financial Supervisory Authority, 2022). In addition to assessing if the regulations work as intended, it is therefore important to evaluate the costs of these regulations. A central cost is that households' ability to smooth consumption over their lifetime is restricted by the regulation, as it limits their ability to borrow. The regulation may result in households having to purchase a cheaper home than what would have been optimal without the mortgage regulations or lead to households who prefer to own their own home opting to rent instead. Furthermore, the regulations may conflict with other policy goals, particularly concerning issues of distribution. Evasion of the regulation may also occur.

3.1 Reduced consumption smoothing

Most households have low income early in life, experience a rising income profile from entering the workforce until retirement, and then see their income decline again toward the end of life. With standard assumptions about households' utility functions, they will seek to smooth consumption of housing and other goods over time, and the ability to borrow contributes to increasing their welfare. Debt allows households to decouple their consumption profile over their lifetime from their income profile. In particular, households can purchase housing when they are young and gradually pay down the loan over time.

Upper limits on loan-to-value ratios and debt-to-income ratios, as well as amortization requirements, can restrict this ability to smooth consumption. Balke et al. (2023) demonstrate this in a dynamic model featuring heterogeneous agents with rising income profiles over their lifetimes. Loan-to-value ratio requirements significantly reduce welfare by limiting consumption smoothing and delaying home purchases, particularly for households with a steep income profile. A similar type of model, estimated using Norwegian microdata, leads to comparable results in Fagereng et al. (2017). The same mechanisms and welfare effects occur with stricter amortization requirements (Ellingsrud et al., 2023). Young households with debt and limited liquid assets reduce their consumption under stricter amortization rules, as these rules constrain their ability to smooth consumption. Furthermore, households delay their first home purchase to some extent.

3.2 Distributional Effects and Exclusion

Mortgage regulations do not affect all households equally but impact those who, in the absence of the regulation, would have had high loan-to-value (LTV) and debt-to-income (DTI) ratios. Young households, on average, have lower incomes and fewer savings than older households and therefore tend to have higher LTV and DTI ratios (The Financial Supervisory Authority, 2023). An upper limit on LTV and DTI ratios will thus typically be binding for young households, especially first-time buyers without affluent parents. While this aligns with the regulation's intention, it may conflict with other housing policy objectives, such as ensuring a high homeownership rate and reducing social and geographic differences in the housing market (*Hurdalsplattformen*, 2021).

Several studies on mortgage regulations find that young individuals are indeed hindered from buying homes as a result of the regulation. Aastveit et al. (2020) find that only prospective first-time buyers without liquid savings delayed or canceled home purchases when Norway tightened the LTV-requirement.[2] Balke et al. (2023) show that stricter LTV requirements also prevent young households from buying homes over the long term, particularly those with low income and wealth.

International studies on mortgage regulations increasingly emphasize that such regulations can prevent certain groups from purchasing homes. Tracey and van Horen (2022) find that easing LTV requirements in the UK increased homeownership among young households. Similarly, low-income households can also be disproportionately affected by mortgage regulations. Acharya et al. (2022) and Peydró et al. (2020) find that LTV and DTI requirements in Ireland and the UK primarily impacted low-income households.

3.3 The "Parental Bank" and Wealth Inheritance

House prices have risen significantly over time in Norway, as in many other countries. On one hand, this can help reduce wealth inequality, as housing wealth is typically less concentrated than other forms of wealth (Martínez-Toledano, 2022). On the other hand, rising house prices can create and reinforce wealth inequality by making homeowners wealthier than those who do not own property (Eggum & Røed Larsen, 2024). It is also well-documented that wealth is passed from parents to children; see, for instance, Fagereng et al. (2021). The housing market and mortgage regulations play a central role, as parental income and wealth can influence whether children enter the housing market and the type of housing they can afford.

Using Norwegian microdata, Wold et al. (2023) find that households with wealthier parents enter the housing market earlier, purchase more expensive homes, and have higher loan-to-value ratios. After controlling for a range of factors, children of parents with above-median wealth are still 15% more likely to own a home by age 30, buy homes that are 15% more expensive, and take on nearly 10% more debt than children of parents with below-median wealth. Key channels include direct parental transfers, parents co-purchasing homes

[2] Vatne and Juelsrud (forthcoming) also find that potential homebuyers with low wealth delay purchasing homes due to the requirements.

with their children, and parents selling homes to their children at discounted prices.

The study further shows that buying a home at a young age is important for wealth accumulation later in life. Thus, differences in early housing purchases and borrowing, contribute to higher wealth for children of wealthier parents. The significance of parental wealth has increased over time, coinciding with the introduction of mortgage regulations. This is especially true regarding the influence of parental wealth on housing value and loan-to-value ratios. This may be explained by the parental bank becoming a more important source of financing, as banks are subject to mortgage regulations, while parental bank is not.

3.4 Evasion and Banking Practices

The introduction of any kind of regulation carries a risk of evasion. The mortgage regulations have been extended to include car loans and consumer loans, limiting households' ability to circumvent the rules by replacing mortgage debt with other forms of borrowing. The establishment of a debt registry[3] provides an overview of unsecured debt, enabling banks and authorities to better monitor households' total debt levels. A debt registry that also includes mortgages and other types of debt would further enhance the ability to ensure compliance with the regulation.

However, banks can still adapt to the regulation in ways that may not align with its intent. For instance, banks in Ireland adjusted their risk profiles by compensating for the lower risk in their mortgage portfolios with increased risk in corporate loans and bond portfolios (Acharya et al., 2022). It is also conceivable that non-regulated actors may offer products that banks would otherwise provide.

Banks are highly specialized in conducting thorough risk assessments of their borrowers. If other actors with less expertise take over parts of the mortgage market due to regulations, this would likely contradict the intended goals of these regulations.

[3] See www.norskgjeld.no and www.gjeldsregisteret.com.

4 Other relevant literature

The Norwegian housing market consists of several submarkets. This applies particularly to geographic submarkets but also to other dimensions, such as the distinction between the ownership market and the rental market, which in turn relates to the division between secondary and primary housing.

In this section, we will discuss literature that sheds light on how the mortgage regulations impact these different submarkets. Additionally, we will review other studies relevant to understanding the regulation's effects.

We begin by examining how the impact of mortgage regulations is influenced by higher inflation and interest rates, followed by an analysis of whether the regulation affects certain geographic areas more strongly than others. We then discuss how the regulation influence the interaction between the ownership and rental markets, as well as between primary and secondary home buyers. Finally, we address other individual studies related to our research question.

4.1 Inflation and interest rates

Mortgage regulations were introduced during a period of low interest rates and low inflation. The interaction between inflation, interest rates, and mortgage regulations has received little attention in the literature.[4] It is likely that the need for these regulations decreases with higher interest rates, as higher rates naturally dampen growth in house prices and household debt. In addition, the costs of the regulations may increase with rising inflation and interest rates, particularly because the requirements in the mortgage regulations are based on nominal debt rather than real debt.

In ongoing research, Ellingsrud et al. (2023) investigate both theoretically and empirically the effects of increased inflation on indebted households. Inflation reduces the real value of

[4]The impact of inflation on the housing market may depend on which prices are increasing. In this section, we assess general inflation (consumer price (CPI) growth) and do not consider the effects of different growth rates for wages, house prices, construction costs, etc.

debt, thereby increasing the net wealth of indebted households. Their study examines this well-known wealth effect of inflation but also considers the impact of rising nominal interest rates as a response to inflation.

In Norway, interest rates are set with the aim of achieving an inflation target, and most households have floating-rate mortgages. As such, it is important to consider the effect of higher interest rates when assessing inflation's impact on indebted households. When interest rates rise due to higher inflation and repayment plans are nominal (as with standard annuity loans), borrowers effectively pay down their loans faster in real terms. This acts similarly to a stricter amortization requirement unless borrowers can renegotiate their loans cost-free.

The analysis shows that, for a given real interest rate, most households lose out under higher inflation—especially young households with high debt and low liquid assets—due to nominal mortgage contracts and amortization requirements. The mortgage regulations further limit the ability of households constrained by debt-to-income and loan-to-value requirements to renegotiate their mortgages, exacerbating costs in the form of reduced consumption smoothing during periods of higher inflation.

4.2 Geographical heterogeneity

The Norwegian housing market is segmented into geographic submarkets, which vary significantly in their exposure to the mortgage regulations. Anundsen and Mæhlum (2017) link housing transaction data to household-level tax statistics, showing that household debt-to-income (DTI) ratios are higher in areas with relatively high house prices. The DTI ratio is highest in the five largest cities and the surrounding Oslo area, whereas the loan-to-value (LTV) ratio is relatively lower in these regions. This is likely due to sustained house price growth in these areas, allowing existing homeowners to build equity and thus be less constrained by LTV requirements. However, high house price growth makes it more challenging for first-time buyers to finance their purchases under the LTV limit.

Borchgrevink and Thorstensen (2018) examine the geographic differences in exposure to the regulation to assess the impact of the maximum DTI ratio of five times income, introduced in 2017. They find that areas highly exposed to the DTI requirement before the regulation—the major cities and particularly Oslo—experienced reduced house price and debt growth following the implementation of the DTI limit.

4.3 The rental market and secondary homes

A natural consequence of mortgage regulations preventing young people and first-time buyers from purchasing homes, is an increased demand for rental properties. This drives up rental prices, making it more attractive for investors to purchase secondary homes for rental purposes. Furthermore, increased demand for secondary homes can push house prices higher, counteracting the regulation's intent to curb price growth. This mechanism is discussed in Greenwald and Guren (2021) and may explain why stricter mortgage regulations do not always result in significant declines in housing prices.

Mehlum and Tomter (2023) highlight how the interaction between primary and secondary home purchases can lead to unintended effects of changes in mortgage regulations. For example, they show that easing financing for primary home buyers might paradoxically weaken their purchasing power, as secondary home buyers—who gain more equity as housing prices rise—drive prices up further.

The link between primary and secondary home purchases also relates to findings on the importance of wealthy parents in securing their children's position in the housing market (Wold et al., 2023). Wealthy parents not only enhance their children's opportunities for homeownership but may also facilitate access to the rental market by purchasing secondary homes that can be rented out to their children at discounted rates.

4.4 Other side effects

The mortgage regulations may also have other indirect effects. For instance, Kabas and Roszbach (2021), using Norwegian microdata, demonstrate that reduced borrowing due to the loan-to-value (LTV) limit can have positive effects on the labor market. They find that less indebted workers who lose their jobs take more time to find new employment, which ultimately results in higher wages.

Additionally, mortgage regulations could influence the decision of whether to sell first or buy first in the housing market. Moen et al. (2021) and Grindaker et al. (2021) show that if a higher proportion of buyers purchase a new home before selling their existing one, house price growth will increase. If the mortgage regulations affect this proportion, that may amplify its short-term impact on prices. A decline in first-time buyers, for instance, could trigger this mechanism by reducing the number of buyers in the market. This, in turn, might lead to fewer homeowners choosing to buy before selling, potentially reinforcing a slowdown in house price growth.

5 Conclusion

The Norwegian lending regulation appears to have functioned as intended, contributing modestly to a reduction in household debt and a slowdown in house price growth. However, the aggregate effect on household debt growth is limited, and the impact on house price growth appears to have been short-lived. It is more uncertain whether the regulation has reduced households' vulnerability to negative shocks. The reason is that, although affected households are less indebted, they also have less liquid assets available. A direct consequence of the regulation is that housing wealth is less likely to be used as a buffer against shocks, as it cannot be as easily leveraged. When it comes to reducing the negative macro effects of loan losses in banks, other measures, such as capital buffer requirements, are likely better suited.

The regulation also seems to impose significant costs on many households. First, it limits households' ability to smooth consumption through borrowing. Furthermore, it can make it more difficult for certain groups, especially young and low income households, to purchase homes. The regulation may also reinforce the importance of wealthy parents in enabling homeownership, which in turn could contribute to wealth inequality passed down from one generation to the next.

The findings presented in this article suggest that the benefits of mortgage regulations are limited in size, while significant costs exist. This has likely been amplified recently, with higher inflation and interest rates. At the same time, it is difficult to precisely assess the regulation's effectiveness, as the arguments for what the regulation is intended to achieve seem to vary across different stakeholders and over time. Given that the current Norwegian lending regulation is set to expire in 2024, we primarily encourage policymakers to clearly define the objectives of the regulation. This will make it easier to assess its effectiveness and make necessary adjustments as new information and experiences arise. Furthermore, we hope this article provides relevant and useful information as authorities now consider the benefits of the regulation against the costs, and evaluate it in light of the goals of other housing policies.

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Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this work the author used Chat GPT in order to translate the article from Norwegian to English. After using this tool, the author reviewed and edited the content as needed and take full responsibility for the content of the publication.

3 From boom to bust: Probabilities of turning points in house prices.

From boom to bust:
Probabilities of turning points in house prices

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January 9, 2025

Abstract

This paper empirically tests whether deviations from fundamental prices— defined as the price that can be explained by disposable income, the housing stock, and the user cost of housing— affect the probability of turning points in house price cycles on a regional level. We apply Bry and Boschan (1971)'s algorithm to identify booms and busts in house prices for 14 municipalities in 4 different regions in Norway, using quarterly data over the period 2003 – 2021. A cointegrated vector autoregressive model is used to identify long-run drivers in local house prices. We find that while house price cycles, in many cases, are similar for municipalities within the same region, there is considerable heterogeneity in the timing, duration, and amplitude of booms and busts in real house prices across the regions. We also find substantial heterogeneity across regions in the response to long-run fundamentals, particularly in the response to a change in the user cost. The model is utilized to investigate if the real house price gap, the difference between actual and model-implied prices, affect the probability of turning points in house prices. We find that an increase in the price gap from 0% – 15% increases the probability of a peak (and an ensuing downturn) in house prices by 7 percentage points.

JEL-codes: *E32; R31*
Keywords: *Boom-bust cycle; House prices; Regional Housing Markets*

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1 Introduction

Excessive growth in house prices and household credit when the economy is experiencing a boom has been shown to amplify economic downturns (Anundsen et al., 2016; Jorda et al., 2013; Mian et al., 2017; Reinhart & Rogoff, 2009). Furthermore, busts in housing investments have proven to be a reliable predictor of downturns in the real economy (Leamer, 2015; Leamer, 2007). A broad group of countries have experienced a house price boom in the post financial crisis era (Wetzstein, 2017). However, price growth has not been evenly distributed across regional markets, and there has been growing focus on regional heterogeneity. In particular, house price growth in metropolitan areas has received attention (Glaeser & Gyourko, 2018; Gyourko et al., 2013). Regional house price cycles have welfare implications, which motivates the need to better understand how house prices in different municipalities and regions evolve and how they respond to changes in fundamentals, such as disposable income, the user cost of housing, and housing supply.

We use Norwegian data at the municipality level to investigate the drivers of local housing cycles using a cointegrated vector autoregressive model (CVAR) (Johansen, 1988). We empirically test how the gap between the fundamental and actual prices affect the probability of peaks and troughs in house prices. We begin by applying the algorithm given by Bry and Boschan (1971) and Harding and Pagan (2002) to identify the observed booms and busts in house prices for Norway as a whole and at the municipality level for 14 of the largest municipalities between 2003 and 2021. We categorize the municipalities into four regions (capital extended, east, west, and mid/north) to compare house price cycles both within regions (municipalities that are located close to each other) and across regions (municipalities that are located further away from each other). Second, we use a cointegrated vector autoregressive model (Johansen, 1988) to estimate the link between real house prices and fundamentals— which include the real user cost, real disposable income, and the housing stock— in each municipality. Lastly, we perform a panel regression to investigate whether the real house price gap, defined as the difference between actual and fundamental prices, affect the probability of turning points in house price cycles. Fundamental house prices are given by the house price path, which is implied by the evolution of the underlying fundamentals and the long-run coefficients estimated by the CVAR model.

Our findings reveal that there is considerable heterogeneity in house price cycles when analyzing Norway as a whole and the 14 municipalities over the period 2003 – 2021. Although house prices, in many cases, follow a similar pattern across municipalities within the same region over time, there is variation in both the timing and duration as well as the amplitude of booms and busts across the regions. There is also heterogeneity in the long-run drivers of house prices, particularly in the response to a change in the real user cost, defined as the tax-adjusted real mortgage rate. However, within each region, the long-run user cost coefficients are more homogeneous, which is in line with findings in, for example, Meen (1999) regarding the importance of spacial dependence or spacial spillovers for housing markets that are located close to each other in space. For Norway as a whole, the results indicate that an increase in one percentage point in the real user cost is associated with a decline in real house prices of 9.9% in the long run. For the capital, Oslo, and the surrounding municipalities, the response to a change in the user cost is larger than for the country as a whole, while it is lower for the remaining eastern municipalities.

Further, the results reveal that the long-run effect of a change in disposable income[1] on real house prices is of a more similar size, across the regions, with an elasticity of between 1 and 2, thereby implying that an increase of 1% in income is associated with an increase of 1%–2% in real house prices in the long run. Fluctuations in disposable income vary between the municipalities, which means that differences in disposable income remain important for heterogeneity in house price cycles across municipalities.

We apply a panel regression to show that an increase in the real house price gap increases the probability of a house price peak. We find that an increase in the real house price gap from 0% to 15% is associated with an increase in the probability of a peak in house prices of approximately 7 percentage points to 9%.

[1]The income measure is the disposable income per housing unit, defined as total disposable income relative to the total stock of housing in each municipality.

To test the robustness of our results, we perform an out-of-sample exercise in which we estimate the model from 2003q1 – 2013q4 and perform a panel regression on the out-of-sample period from 2014 to 2021. This exercise yields results in line with the baseline in which a price gap of 15% is associated with a probability of a peak of approximately 10%.

This paper contributes to three strands of the literature. First, it relates to the literature that analyzes long-run drivers of house prices and house price bubbles and booms and busts in housing markets (Aastveit et al., 2023; Agnello & Schuknecht, 2011; Anundsen, 2019; Bourassa et al., 2019; Clapp et al., 2020; Croce & Haurin, 2009; Fabozzi & Xiao, 2019; Phillips et al., 2015; Plakandras et al., 2024). Our paper contributes to this literature by connecting a regional version of the model given by Anundsen (2019) with Bry and Boschan (1971)'s algorithm to define house price turning points. While most of these studies aim to detect bubble periods, we focus on turning points in house prices, regardless of whether prices are over- or undervalued.

Second, the findings are related to the literature on regional house-price models (Meen, 1990, 1999; Oikarinen et al., 2018). While Meen (1999) documents regional heterogeneity in the adjustment parameters, resulting in heterogeneity in the return to fundamental prices, we also relate to the literature that documents regional heterogeneity in the long-run relationships between real house prices and fundamentals, (Cuhna & Lobão, 2022; Hwang & Quigley, 2006; Oikarinen et al., 2018; Plakandras et al., 2024). We contribute to this literature by documenting heterogeneity in house price cycles and house price response to fundamentals at the municipality level, particularly across regions that are geographically distant from one another.

Third, the paper is related to the literature on predicting house price cycles (Bauer, 2017; Chen et al., 2014; Duca et al., 2021). We contribute to this literature by exploiting regional variation in house price cycles and their fundamental drivers to assess the probability of turning points in house prices. In addition to a more standard CVAR model at the municipality level, we use Bry and Boschan (1971)'s algorithm to detect turning points, which allows us to perform a panel regression to assess the probability of a turning point in house prices.

The paper is also relevant for central banks' work with financial stability. Here, we relate to literature concerned with credit cycles (Borio, 2012; Jorda et al., 2013; Kiyotaki & Moore, 1997). This paper is also related to the literature on *leaning against the wind* (Schularick et al., 2021; Svensson, 2017).

The remainder of the paper is structured in the following manner. Section 2 describes the data and institutional setting in Norway. Section 3 describes house price cycles and historical booms and busts. Section 4 presents the results from the cointegration model, and we estimate fundamental house price paths at the municipality level. Section 5 combines turning points in observed house prices and the fundamental house price paths to assess the probability of turning points in real house prices. Section 6 concludes.

2 Data and institutional setting

2.1 The residential real estate sales process

It is customary in Norway to sell residential real estate through ascending-bid auctions. The auction takes place after a showing (open house) of the residence and is led by a realtor, who is hired by the seller but obligated by law to protect the interests of both the seller and the buyer. Bids are legally binding, as are the acceptance of bids. The bids are usually placed on digital platforms, but it is also possible to bid over telephone or telefax. The realtor informs the participants of developments in the auctions. After the auction is completed, either by the seller accepting a bid or not accepting any bids, participants in the auction re permitted to view the bidding log, which provides an overview of all bids placed during the auction.

2.2 Mortgage lending regulations

Mortgage lending regulations and changes in credit supply are important determinants of house prices (Favara & Imbs, 2015; Greenwald & Guren, 2021; Mian & Sufi, 2022). Mortgage lending regulations in Norway, like in most Western countries, changed over the sample period.

After the Great Financial Crisis of 2007 – 2009, the need for stricter regulations of banks was an important topic for policy makers both in Europe and in the US. In 2013, the European bank regulation, Basel III, came into effect with stricter regulations of bank's capital buffers and requirements for liquidity and stable refinancing (Jacobsen et al., 2011). Prior to the implementation of these regulations, banks reported a tightening in the credit practice for households. The same year, the counter-cyclical capital buffer was introduced, while a systemic capital buffer was implemented in 2015.

In addition, borrower based regulations was implemented as guidelines in 2010 (see, e.g., Fjære-Lindkjenn et al. (2024) for an overview). The guidelines were changed to regulations in 2015, and the regulations were tightened along several dimensions in 2017. These regulations set upper limits on loan-to-value (LTV) ratios, debt-to-income (DTI) ratios, and requirements for amortization for mortgages with high LTV-ratios. In addition, the regulations include an interest rate stress test for all mortgages.

2.3 Data

We use data on house prices, disposable income, and number of dwellings at the municipality level, mortgage rates, tax deduction rates for mortgages, and the consumer price index (CPI) are on a national level. Table 1 provides an overview of the data.

The house price indices are collected from Eiendom Norge. The price indices are produced in collaboration among Eiendom Norge, Eiendomsverdi, and Finn.no. They are based on all transactions that passed through the online advertisement platform Finn.no, which accounts for approximately 70% of all transactions. They produce price indices for 81 geographical areas in Norway, spanning from municipalities to more remote areas. The index for Norway is computed based on seven main regions that encompass the entire country, in which the total index for the country is computed using transaction-based weights from the last 24

Table 1 Summary of data and sources, 2003 – 2021.

Variable	Geographic level	Frequency	Source
House prices	Municipality	Quarterly	Eiendom Norge
Disposable income	Municipality	Annual	microdata.no
Disposable income	National	Quarterly	Statistics Norway
Dwellings	Municipality	Annual	Ambita
Building works completed	Municipality	Quarterly	Statistics Norway
Mortgage rates	National	Quarterly	Statistics Norway
Tax deduction	National	Annual	TNTA
Consumer price index	National	Quarterly	Statistics Norway

Notes: This table presents an overview of the data used in the analysis. TNTA stands for The Norwegian Tax Administration.

months.

The indexes are computed using a version of the the sales price appraisal ratio (SPAR) method.[2] The appraisal value applied is based on a hedonic model and a ratio between the predicted price from the hedonic model and observed sales price is computed. The median ratio is used to assess the price change since the last period. Lastly, to uncover the underlying price growth, a smoothing process in two steps is performed. First, a moving average for a smaller area is computed. Second, a non-adjusted series for a more aggregate area is applied and a ratio between the moving average in the smaller area and the un-smoothed index for the aggregate area is calculated. This ratio is used to adjust the moving average for potential oversmoothing in the fist step.

Data on disposable income are downloaded from Statistics Norway's platform microdata.no[3] and based on annual tax records at the individual and household levels. The tax records are combined with residential information and aggregated at the municipality level to construct a series for total disposable income in each municipality. Quarterly growth rates for disposable income at the national level from National Accounts are used to interpolate

[2] An explanation of the methodological approach that is used to compute the indices can also be found at www.eiendomnorge.no/about/category967.html.

[3] Microdata.no is an online platform serviced by Statistics Norway that provides instant online access to large amounts of detailed and mergeable Norwegian microdata. The service is open to employees and students at universities and colleges, approved research institutions, ministries, and directorates. Link: https://www.microdata.no/en/

the annual data to a quarterly frequency at the municipality level.

Data on the total number of dwellings in each municipality per year is obtained from Ambita.[4] A quarterly series for dwellings is constructed by adding the quarterly share of new housing completions through the year. For this, the quarterly statistics on building works completed from Statistics Norway is used.

We construct a variable for real disposable income per housing unit. This variable is constructed by dividing the total real disposable income per municipality by the total number of dwellings. Real disposable income per housing unit is used in the fundamental value model for real house prices and is illustrated in Figure 1. It is evident that this variable, in general, increases over time, but that the upward trend has abated somewhat and that there is some heterogeneity across municipalities.

Data on mortgage rates and the consumer price index (CPI) are downloaded from Statistics Norway, while the tax deduction data is from The Norwegian Tax Administration (TNTA).

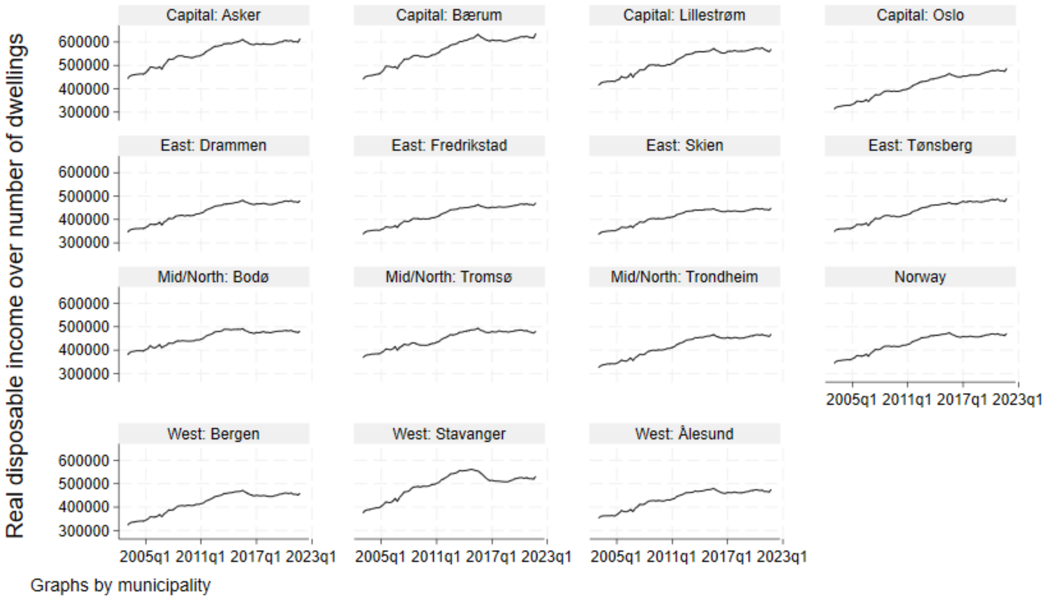
3 Historical booms and busts

We begin by dating historical booms and busts at the national level and for the 14 municipalities[5] over the period 2003q1 – 2021q4. We apply the approach given by Harding and Pagan (2002) for identifying turning points, which is a quarterly version of Bry and Boschan (1971)'s algorithm. The aim of their algorithm is to identify turning points between phases of growth (booms) and decline (busts) in real economic variables based on predefined censoring rules. The censoring rules include (*i*) the window— the time span in which the algorithm searches for local minima and maxima, (*ii*) the phase— the minimum time of growth or contraction to be considered a boom or bust, and (*iii*) the cycle length— a minimum time of

[4] Ambita is a technology firm that services a database from The Land Registry, which has information on all properties and housing cooperatives in Norway. Link: https://www.ambita.com/

[5] We provide an overview of the 14 municipalities, the four regions, and changes in municipality boarders over the time period in Appendix A.

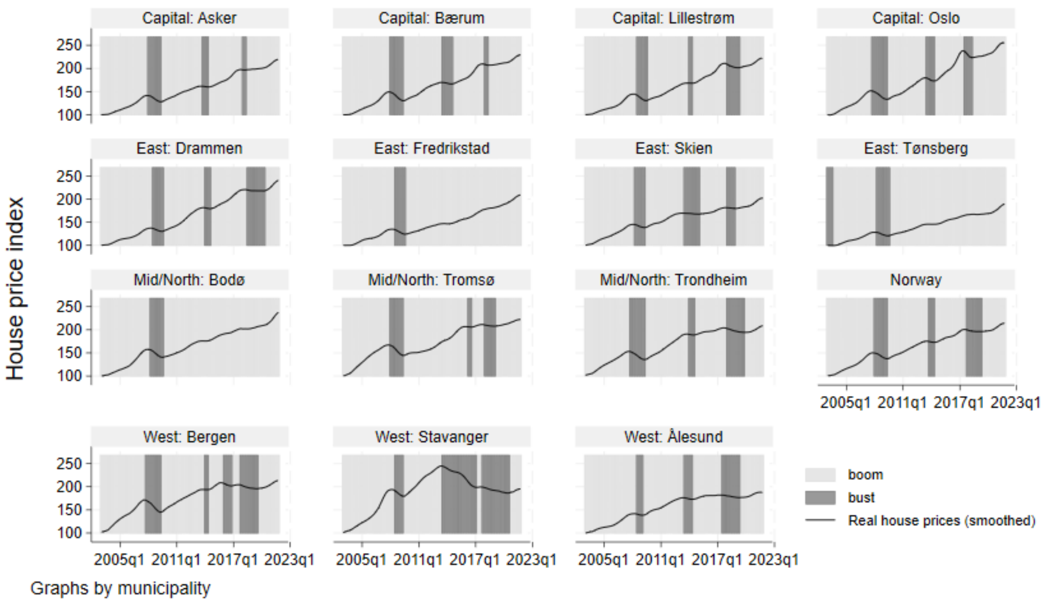
Figure 1 Real disposable income over number of dwellings, 2003 – 2021.



Notes: The figure displays a quarterly series for total real disposable income in Norwegian kroner (NOK) relative to the housing stock in each municipality from 2003q1 – 2021q4. The municipalities are categorized into four regions: Capital, East, West and Mid/North, based on location; see Appendix A for an overview of the municipalities and regions. **Data sources**: Statistics Norway and Ambita.

a complete cycle, from peak-to-peak or trough-to-trough. Originally, the window was set to 6 months, the phase to 6 months, and the cycle to 15 months by Bry and Boschan (1971). We settle on a window of 2 quarters, phase of 2 quarters, and cycle of 6 quarters, which provides a reasonable number of booms and busts and is in line with Bry and Boschan (1971).

Figure 2 Real house prices, 2003 – 2021.



Notes: This figure shows a quarterly index for real house prices in each municipality from 2003q1 — 2021q4. The nominal quarterly house price indices from Eiendomsverdi/Eiendom Norge are deflated by the consumer price index (CPI) to obtain a series for real house prices. The series are smoothed with a four-quarter moving average, as the original index is not adjusted for seasonal variation. To assess turning points, the following censoring rules are applied: window = 2, phase = 2, cycle = 6 (quarters). The municipalities are categorized into four regions— Capital, East, West and Mid/North— based on location; see Appendix A for an overview of the municipalities and regions. **Data sources:** Eiendomsverdi/Eiendom Norge, Statistics Norway.

Figure 2 depicts real house prices with shaded areas for housing booms and busts for Norway and the 14 municipalities over the sample period.[6] The municipalities are categorized into four regions— Capital, East, West and Mid/North, based on location (see Appendix

[6]The series are smoothed with a four-quarter moving average, as the original index is not adjusted for seasonal variation.

A for an overview of the municipalities and regions). The municipalities have experienced between one and four busts, and for all areas— except Stavanger— busts are considerably shorter than booms. In addition, we see that house prices typically decline less in busts than they increase in booms, hence, real house prices increase over the cycle. Real house prices follow a similar pattern across most municipalities, particularly within the same region. However, there is heterogeneity both in the timing and the duration of the phases across regions. Stavanger, Bergen, and, to a certain extent, Ålesund— municipalities along the west coast of Norway (bottom panel)— are highly exposed to the petroleum sector and experienced a weaker trend and a longer bust period after 2014, when oil prices plummeted. We can also see that the capital, Oslo, and the surrounding municipalities (top panel) experienced higher growth in real house prices over the period than the remaining municipalities.

For national house prices, the first peak is detected in the fourth quarter of 2007. The ensuing decline in house prices, related to the global financial crisis, lasted until the second quarter of 2009. All municipalities experienced a peak in real house prices at the end of 2007, or beginning of 2008, and an ensuing downturn, although with slightly different timing, duration, and extent of price drop. The country as a whole, and approximately half of the municipalities, experienced a new bust in 2013. This decline may have been a response to the capital buffer requirements that were introduced in Norway and most European countries in 2013, as described in Section 2.2. The bust in 2013 lasted for between two and three quarters, except in Stavanger, in which the decline in real house prices lasted until 2017, followed by a short boom and a new bust lasting until the third quarter of 2020. Again, house price developments in Stavanger must be seen in the context of the oil price drop in 2014. The most recent bust at the national level was in 2017, after a period of rapid price growth. This bust in prices was particularly sharp in the capital, Oslo, but was also apparent in several other municipalities.

Table 2 summarizes the number of peaks and troughs in each municipality as well as the duration of the boom and bust phases and the complete cycle between 2003 and 2021. Price

Table 2 Summary statistics: Peaks and troughs, 2003-2021.

Average	Capital: Asker	Capital: Bærum	Capital: Lillestrøm	Capital: Oslo
No. peaks	3	3	3	3
No. troughs	3	3	3	3
Bust (qtrs.)	3.7	4.3	4.3	5
Boom (qtrs.)	15.5	14.5	15.5	14
P-to-P (qtrs.)	20	20	19	19.5
T-to-T (qtrs.)	18	18	19.5	18
Bust (% growth)	-0.8	-1.1	-1.0	-1.5
Boom (% growth)	4.5	5.0	4.8	6.2
P-to-P (% growth)	3.3	3.4	3.7	4.4
T-to-T (% growth)	4.3	4.6	4.4	5.2

Average	East: Drammen	East: Fredrikstad	East: Skien	East: Tønsberg
No. peaks	3	1	3	1
No. troughs	3	1	3	2
Bust (qtrs.)	5.3	5	5.3	6
Boom (qtrs.)	16		13.5	
P-to-P (qtrs.)	20.0		19.5	
T-to-T (qtrs.)	21.5		19.0	24.0
Bust (% growth)	-0.5	-1.7	-0.5	-1.3
Boom (% growth)	5.4		2.9	
P-to-P (% growth)	4.7		2.2	
T-to-T (% growth)	5.2		2.6	4.1

Average	Mid/North: Bodø	Mid/North: Tromsø	Mid/North: Trondheim	Norway
No. peaks	1	3	3	3
No. troughs	1	3	3	3
Bust (qtrs.)	6	4.3	6	5.3
Boom (qtrs.)		16	15.5	15
P-to-P (qtrs.)		20	20.5	19.5
T-to-T (qtrs.)		19.5	21	20
Bust (% growth)	-2.3	-1.1	-1.2	-0.8
Boom (% growth)		3.8	4.2	3.9
P-to-P (% growth)		2.3	2.8	2.9
T-to-T (% growth)		3.6	3.7	3.6

Average	West: Bergen	West: Stavanger	West: Ålesund
No. peaks	4	3	3
No. troughs	4	3	3
Bust (qtrs.)	5.2	10.3	5
Boom (qtrs.)	9	9	14.5
P-to-P (qtrs.)	13.3	18.5	18
T-to-T (qtrs.)	13.7	22.5	20.5
Bust (% growth)	-1.2	-2.2	-0.5
Boom (% growth)	2.5	3.0	2.9
P-to-P (% growth)	1.1	0.4	2.5
T-to-T (% growth)	2.0	0.4	2.5

Notes: This table presents the summary statistics for the real house price cycles illustrated in Figure 2 for Norway and 14 Norwegian municipalities. To assess the turning points, the following censoring rules are applied: window = 2, phase = 2, cycle = 6. N = 15. All growth rates are annual percentage growth rates. The municipalities are categorized into four regions— Capital, East, West and Mid/North— based on location; see Appendix A for an overview of the municipalities and regions. **Data sources:** Eiendomsverdi/Eiendom Norge, Statistics Norway.

developments in each phase, and over the cycle, are also presented.[7] We see that the average length of a bust varied between 3.7 quarters for Asker and 10.3 quarters for Stavanger. Booms varied between an average duration of 9 quarters in Bergen and Oslo and 16 quarters in Tromsø and Drammen. Price developments also varied among the municipalities, and we find that the average annual growth in real prices during a boom was 2.5% in Bergen, while it was 6.2% in Oslo. For busts, the decline varied between 1% in Lillestrøm and 2.3% in Bodø.

4 A long-run model for regional prices

4.1 Methodological framework

A theoretical framework for modeling house prices that is commonly used in the literature is based on the life-cycle model of housing and is often referred to as the inverted demand approach (see, e.g., Meen (1990) and Meen (1999)). In this framework, a representative agent maximizes life-time utility with respect to a housing good and other consumption goods. There also exists a rental sector, and— in an efficient market— the rent equals the user cost of housing. Since the imputed rent is unobserved, we assume that it develops proportionally with income and the housing stock and use them as proxies for rental prices. In equilibrium, house prices depend on the user cost of housing, household disposable income, and housing supply.

As a point of departure for an empirical analysis of regional house prices in Norway, we follow Anundsen (2019) and use a semi-logarithmic specification.

$$ph = \beta_y y + \beta_h h + \beta_{UC} UC \tag{1}$$

In Equation 1, ph is the logarithm of real house prices, y is the logarithm of real disposable

[7] In Appendix B, summary statistics of peaks and troughs in 81 geographical areas in Norway is presented, thereby illustrating the heterogeneity across an even larger set of areas.

income, h is the logarithm of the housing stock,[8] and UC is the user cost, which is defined as $UC = (1-\theta)i - \pi + \delta - \Delta ph$, where θ is the tax rate for which interest rate expenses are deductible, i denotes the nominal mortgage interest rate, and π is the inflation rate. Thus, we assume that it is the real interest rate that affects house prices in the long run.[9] The user cost also includes a term δ, which is the depreciation rate, and Δph is expected capital gains (expressed as expected house price growth). In the econometric analysis, the depreciation rate (which is assumed to be constant over time) is discarded. There are multiple ways of handling expected capital gains in the calculation for the user cost (see Section 4.3 in Duca et al. (2021) for a discussion). We again follow Anundsen (2019) and assume that expected capital gains are captured by lagged house price growth, which are included as separate terms with five lags (quarterly) in the econometric model.[10] According to theory, we would, *ex ante*, expect that $\beta_y > 0$ and $\beta_h, \beta_{UC} < 0$.

Further, in the econometric specification, the total disposable income is measured relative to the housing stock, thereby providing a variable of disposable income per housing unit, thus Equation 1 is re-written as Equation 2, in which $\tilde{Y} = Y/H$ or $\tilde{y} = y - h$ in log form. The reason for this choice is that we estimate the model over a relatively short time period, which makes it challenging to obtain precise estimates of a large number of coefficients. By combining the measure for total disposable income and housing stock, we reduce the number of coefficients that need to be estimated. An implication of this choice is that we implicitly assume that the coefficients on income and the housing stock are the same, but with opposite signs, which implies that we assume an income elasticity of demand equal to one. This assumption is supported for US data in several studies (Anundsen, 2015; Duca

[8]The housing stock and disposable income are usually measured relative to the population or the number of households. Since we use a measure for disposable income relative to the housing stock in the econometric analysis, the population variable cancels out.

[9]Nominal interest rates could also be important for house prices due to cash-flow effects (Almgren et al., 2022; Cloyne et al., 2020; Holm et al., 2021), at least in the short-run. To test if nominal interest rates are also significant for house prices in the long-run, we estimated the model with a separate term for the nominal mortgage rate. We find that the nominal mortgage rate has a significant effect on house prices for 3 out of the 14 municipalities investigated as well as for Norway as a whole. Detailed results in this regard are presented in Table E.2 in Appendix E.

[10]A figure depicting the real user cost, as well as the real and nominal mortgage rate is included in Appendix F.

et al., 2011; Meen, 2001). For Norwegian data, a similar restriction has been used and tested for in Anundsen (2019) and Anundsen (2021).

$$ph = \tilde{\beta}_{\tilde{y}}\tilde{y} + \tilde{\beta}_{UC}UC \tag{2}$$

In a well-specified fundamental value model of house prices, one should expect house prices to revert to equilibrium over time. An assumption for this to hold is that the linear combination of $ph - \tilde{\beta}_{\tilde{y}}\tilde{y} - \tilde{\beta}_{UC}UC$ is stationary[11]— that is, that house prices cointegrate with the user cost and disposable income relative to the housing stock. We follow Anundsen (2019) and use the system-based test for cointegration developed by Johansen (1988), which implies that we test whether there exists an equilibrium relationship between the variables in Equation 2. Consider the following VAR(p) model:

$$\Delta \mathbf{x}_t = \Pi \mathbf{x}_{t-1} + \sum_{i=1}^{p-1} \Gamma_{\mathbf{x},i} \Delta \mathbf{x}_{t-i} + \Phi \mathbf{D}_t + \epsilon_t \tag{3}$$

The vector $\mathbf{x}_t$ consists of the endogenous variables ph, UC, and $\tilde{y}$. In $\mathbf{D}$, three centered seasonal dummies, a deterministic trend, and a constant term is included. When testing for cointegration, we include a trend term to increase the power of the test, which is in line with the recommendations of Johansen (1995) and Harbo et al. (1998). A lag length of $p = 5$ is used for all regions, a selection that is based on the Akaike information criterion (AIC). By assuming that there exists one cointegrating relationship between the variables in $\mathbf{x}_t$, an assumption that we test in the econometric analysis, the matrix $\mathbf{\Pi}$ will have rank one and has the following Vector Error Correction Model (VECM) representation:

$$\begin{pmatrix} \Delta ph_t \\ \Delta \tilde{y}_t \\ \Delta UC_t \end{pmatrix} = \begin{pmatrix} \alpha_{ph} \\ \alpha_{\tilde{y}} \\ \alpha_{UC} \end{pmatrix} (ph - \beta_{\tilde{y}}\tilde{y} - \beta_{UC}UC - \beta_T T)_{t-1} + \sum_{i=1}^{p-1} \Gamma_{\mathbf{x},i} \Delta \mathbf{x}_{t-i} + \tilde{\mathbf{\Phi}}\tilde{\mathbf{D}}_t + \epsilon_t \tag{4}$$

[11] In addition, we need the adjustment parameters to be negative.

The αs are the adjustment parameters, indicating the speed at which the endogenous variables revert to equilibrium, while the βs express the long-run relationships between the cointegrating variables. The cointegration vector is normalized with respect to house prices so that $\beta_{ph} = 1$ and the user cost is assumed to be weakly exogenous, so that $\alpha_{UC} = 0$ (see Johansen (1992) for details).[12] $\tilde{\mathbf{D}}_t$ only contains the constant and the seasonal dummies, since the deterministic trend term is restricted to enter the cointegration space.

After estimating the long-run coefficients $\hat{\beta}_{\tilde{y}}$ and $\hat{\beta}_{UC}$ using the fundamental model in Equation 4, we construct an implied future fundamental path for house prices, conditional on our estimated long-run coefficients from the fundamental model (Equation 4). This approach is similar to that of Anundsen (2019, 2021). An underlying assumption is that house prices were in equilibrium in 2014q1.[13] Thus, any changes to the fundamental value (relative to the value it assumed in 2014q1) are driven by changes in income, the user cost, or the housing stock. This accounting relationship is shown in Equation 5.

$$ph_t^* = ph_{t-1}^* + \hat{\beta}_{\tilde{y}}\Delta\tilde{y} + \hat{\beta}_{UC}\Delta UC \tag{5}$$

The implied fundamental path ,ph_t^*, can be compared to actual house prices, ph_t, to investigate whether there exists a gap between the two— that is, whether house prices are overvalued or undervalued. Given the existence of a cointegrating relationship and a well-specified econometric model, actual prices are expected to revert to the model-implied path over time. A large and persistent gap signals imbalances or a structural break in the underlying econometric model.

Ideally, we would estimate the model shown in Equation 4 in an in-sample time-period and calculate the implied fundamental path out-of-sample. However, due to a relatively short time-period with quality data at the municipality level, we chose to estimate the model for the period 2003q1 – 2019q4 and construct the fundamental path quasi out-of-sample, for

[12]The exogeneiety of the user cost ensures that we find one cointegrating relationship for house prices. We test this assumption in the econometric analysis.

[13]The starting point for the fundamental path is determined on the basis of the assumption that $ph^*_{2014q1} = ph_{2014q1}$. This assumption is not possible to test, but an assessed neutral business cycle at this time (Norges Bank, 2015) supports that this is a reasonable starting point for the fundamental price path.

the period 2014q1 – 2021q4. The implied fundamental price is hence calculated on parts of this period, 2003q1 – 2019q4, which implies that there is an overlap for the period 2014q1 – 2019q4.

4.2 Results of the cointegrated VAR model

The cointegrated VAR-model, as presented in Equation 4, is estimated using both national aggregate data and data for the 14 municipalities. The main estimation period is 2003q1 – 2019q4. The Jarque-Bera test indicates that residuals are normally distributed in all municipalities, and we find little evidence of autocorrelation. The AIC indicates an optimal lag-length of five quarters for Norway as a whole and all the municipalities— except Oslo, in which it indicates two. We use five lags for all areas in the main estimation. The trace test for cointegration (Johansen, 1988) indicates one cointegrating relationship in Norway as a whole and in eight municipalities, while it indicates two cointegrating relationships in six municipalities. The results from the trace test are reported in Appendix C. We continue under the modelling assumption of one cointegrating relationship in all municipalities in the main estimation to be able to construct one equilibrium path for real house prices. This is also consistent with the underlying theoretical model. In addition, the restriction of weak exogeneity of the user cost ($\alpha_{UC} = 0$) is imposed. This assumption is tested and reported with a p-value for the test of overidentifying restrictions in Table 3.

Table 3 presents the long-run coefficients for the real user cost and real disposable income per housing unit from the CVAR analysis for Norway as a whole and for the 14 municipalities separately. Moreover, the estimated adjustment parameter, $\hat{\alpha}_{ph}$, of real house prices, the sum of the coefficients on the house price lags, and the p-value from the likelihood ratio test for over identifying restrictions is displayed in the table. The test does not reject the null hypothesis that the restrictions imposed.[14] are valid for 11 out of the 14 municipalities as well as for the national model

It is evident that the coefficients on the user cost are all negative. For Norway as a whole

[14] In our case, the only restriction is that of weak exogeneity of the user cost: $\alpha_{UC} = 0$.

Table 3 Results for the long-run coefficients from the cointegrated VAR analysis of house prices.

Variable	Capital: Asker	Capital: Bærum	Capital: Lillestrøm	Capital: Oslo
User Cost	-14.068***	-14.141*	-12.136***	-15.833***
Disp.income/H.stock	1.592***	1.469***	1.712***	1.469***
Adj. parameter	-0.067**	-0.067***	-0.083***	-0.098***
Sum house price lags	0.544	0.667	0.752	0.648
P-value restr.	0.427	0.428	0.073	0.231
Variable	**East: Drammen**	**East: Fredrikstad**	**East: Skien**	**East: Tønsberg**
User Cost	-6.225*	-17.858***	-6.844***	-6.139***
Disp.income/H.stock	1.916***	1.139***	1.842***	1.272***
Adj. parameter	-0.111***	-0.047**	-0.088***	-0.116***
Sum house price lags	0.780	0.449	0.771	0.597
P-value restr.	0.014	0.044	0.888	0.393
Variable	**Mid/North: Bodø**	**Mid/North: Tromsø**	**Mid/North: Trondheim**	**Norway**
User Cost	-9.970*	-5.338*	-11.073***	-9.895***
Disp.income/H.stock	1.910	1.494***	1.484***	1.618***
Adj. parameter	-0.134***	-0.137***	-0.097**	-0.106***
Sum house price lags	0.804	0.833	0.968	0.802
P-value restr.	0.457	0.268	0.070	0.489
Variable	**West: Bergen**	**West: Stavanger[1]**	**West: Ålesund**	
User Cost	-10.014***	-10.581*	-2.854***	
Disp.income/H.stock	1.378***	2.059	1.530***	
Adj. parameter	-0.154***	-0.009	-0.207***	
Sum house price lags	0.934	1.102	0.935	
P-value restr.	0.104	0.000	0.140	

Notes: This table reports the long-run coefficients (β) in Equation 4 and the p-value for the test for overidentifying restrictions. The estimation period is 2003q1 – 2019q4. The dependent variable is real house prices (nominal house prices deflated by cpi) and real disposable income is disposable income deflated by CPI and measured relative to housing supply. The real user cost is measured as the nominal, tax-adjusted, mortgage rate subtracted cpi. In this definition of the user cost, we discard depreciation and assume that expected capital gains are captured by the coefficients on the house price lags. [1]In the estimation for Stavanger, a quarterly series for oil investments is included to establish a cointegrating relationship, which implies that Equation 2 is substituted with $ph = \beta_{\tilde{y}}\tilde{y} + \beta_{UC}UC + \beta_o oil$ for Stavanger. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

(row 3, column 4 in Table 3), the results indicate that an increase of 1 percentage point in the real user cost is associated with a long-run decline in real house prices by approximately 10%. This result corroborates the findings of Anundsen (2019), which found that a 1 percentage point increase in the real user cost is associated with a long-run decline in real house prices of 13.8%. As indicated by Anundsen (2019), house prices in Norway are more sensitive to changes in the user cost than in several otherwise comparable countries (he shows estimates for the US and Finland). This can be explained by the high share of floating-rate mortgages (over 90 percent) in Norway. The high level of debt relative to income in Norway might also contribute to the high responsiveness of house prices to the user cost.

For the capital, Oslo (row 1, column 4 in Table 3), the user cost coefficient is larger than for the country as a whole. An increase in the real user cost by 1 percentage point indicates a decline in real house prices by approximately 16%. Moreover, for the surrounding municipalities (row 1)— Asker, Bærum, and Lillestrøm— the user cost coefficient is higher than that for the country as a whole. For the remaining eastern municipalities, the user cost coefficient is substantially lower (approximately 6%).[15] For the municipalities in the mid/north and in the west, the user cost responses lie somewhere in between the capital municipalities and the eastern municipalities. There is also some variation within these regions, which is not surprising given that for these regions the municipalities within the same region are also located rather far from each other.[16]

The results presented in this section indicate that there is not only heterogeneity in the short-run parameters and the return to equilibrium, as in Meen (1990, 1999), but also in the long-run coefficients, which is in line with findings in, for example, Hwang and Quigley (2006), Oikarinen et al. (2018), and Plakandras et al. (2024). This implies that relative house prices among regions could diverge over time. In theory, divergence in house prices between municipalities and regions should lead to migration to the regions with relatively lower house

[15] The exception is the estimates in Fredrikstad. In this municipality, the trace test indicates two cointegrating relationships, and we are uncertain whether the high user cost coefficient is a result of an actual higher long-run response to the user cost in Fredrikstad or merely uncertainty due to the relatively short estimation period.

[16] We show a map of Norway, that illustrates the locations of the municipalities and the regions in Figure A.1 in Appendix A.

prices, which again increase demand in these regions relative to the more expensive regions. In the long-run, this mechanism should prevent divergence in relative house prices and might partially explain why we see similar long-run coefficients for municipalities that are located close to each other in space. However, in Norway, distances between high-density regions are large and mobility among these regions is in general much lower than is the case for, for example, different states in the US. This could explain why we do find rather large differences among a few of the municipalities. For example, the absolute value of the user cost coefficient is almost 14 percentage points larger for Oslo than for Ålesund. As is evident from Figure F.2 in Appendix F, real house prices have, to a certain extent, diverged among these municipalities over the last few decades, particularly in the period when the user cost declined markedly (see Figure F.1 in Appendix F).

The variation across municipalities and regions in the long-run relationship between house prices and the user cost can be related to fundamental differences among the municipalities in, for example, restrictions on housing supply and household debt. However, it is worth noting that some of this heterogeneity might also be explained by the weak trend in the user cost (see Figure F.1), which makes the long-run relationships between house prices and the user cost more uncertain than that for disposable income, particularly since the long-run relationships are estimated over a relatively short time-period.

An increase in disposable income per housing unit by 1% implies an increase in real house prices of approximately 1.6% in Norway as a whole. The heterogeneity across municipalities is less visible than that for the real user cost. At the municipality level, the coefficient for disposable income over housing stock varies between 1.1% in Fredrikstad and 2.1% in Stavanger. However, there is also heterogeneity in the disposable income per housing unit across the municipalities over time, as illustrated in Figure 1, which implies that it is still important for heterogeneity in the house price cycles across regions.

The adjustment parameters for real house prices are negative in all municipalities, thereby indicating that when real house prices are not in equilibrium as defined by this model, they will revert to equilibrium over time. This is additional evidence of cointegration, as the

Engle and Granger (1987) representation theorem says that cointegration implies equilibrium correction, and vice versa. The adjustments parameters vary between - 0.21 in Ålesund to -0.05 in Fredrikstad.[17] This implies that actual prices will revert back to equilibrium in 5 quarters in Ålesund and 5 years (20 quarters) in Fredrikstad.

4.3 Fundamental paths for real house prices

Based on the results from the cointegration analysis and Equation 5, an implied fundamental path for real house prices can be constructed for each municipality. This path is illustrated in Figure 3 for Norway and the 14 municipalities. The fundamental path is constructed for the period 2014q1 – 2021q4, which is quasi out-of-sample, and with the assumption that real house prices were in line with fundamentals in 2014q1.

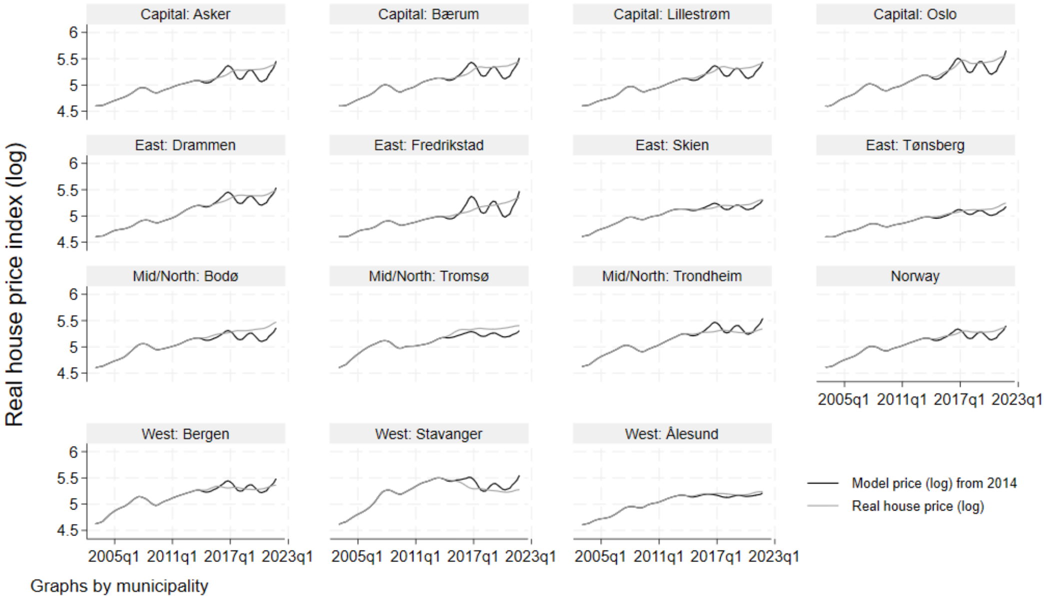
Figure 3 illustrates actual real house prices for the entire sample period and the fundamental price path constructed from 2014q1 – 2021q4.[18] The fundamental path fluctuates around actual prices in most areas, which indicates that actual prices have been well in line with fundamentals over this period, although occasionally above (overvalued) and occasionally below (undervalued). Since the fundamental price paths are volatile, in line with what Anundsen (2019) finds for Norway overall, one should be careful interpreting short-term deviations of real house prices from fundamentals as over- or undervaluation of prices. However, a persistent gap between fundamental and actual house prices suggests that actual house prices are not in line with the fundamentals.

At the end of the period (2021q4), house prices are in line with fundamentals in most of the municipalities and the country as a whole. In certain areas, such as Stavanger and Trondheim, fundamental prices are mostly above the actual prices, which is an indication of undervalued house prices, while in others— such as Bodø, Tromsø, and Tønsberg— fundamental prices are mostly below actual prices, which is a sign of overvalued prices according to this model

[17]The adjustment parameter is reported to be -0.01 in Stavanger; however, this is not statistically significant.

[18]Both series are smoothed with a four-quarter moving average, as the original house price index is not adjusted for seasonal variation.

Figure 3 Fundamental paths for real house prices – (2014 – 2021) vs. actual real house prices (2003 – 2021).



Notes: This figure depicts the fundamental paths for real house prices (black line) from 2014q1 – 2021q4, when applying the long-run coefficients from the VEC model, presented in Table 3, and actual real house prices (grey line) for the full sample period, 2003q1 – 2021q4. Both series are smoothed with four-quarter moving averages, as the original house price index is not adjusted for seasonal variation. House prices are assumed to be in line with fundamentals in 2014q1, and after that the fundamental path for real house price is constructed using Equation 5: $ph^*_t = ph^*_{t-1} + \hat{\beta y}\Delta\tilde{y} + \hat{\beta_{UC}}\Delta UC$. For Stavanger, the fundamental path is constructed with the parameter for oil investments: $ph^*_t = ph^*_{t-1} + \hat{\beta}_{\tilde{y}}\Delta\tilde{y} + \hat{\beta}_{UC}\Delta UC + \hat{\beta}_o\Delta oil$.
Data sources: Eiendomsverdi/Eiendom Norge, Statistics Norway.

and the development in the fundamentals.

There is a sharp increase in the model-implied price toward the end of the period in most areas. This is related to an increase in disposable income in 2021 as well as a sharp decrease in the mortgage rate in 2020. Both drivers are related to the COVID-19 pandemic. One could argue that this period was special in a historical context and that this might have led to a break in the cointegrating relationship at this point, thus making the end point of the figures challenging to interpret. Nevertheless, it is interesting to see that over the two periods, 2016 and 2020, when actual real house prices increased rapidly, the model implied even higher fundamental real house prices due to the strong effect that the mortgage rate had on fundamental house prices.

Table 4 presents the summary statistics for the real house price gap; the difference between actual house prices and the predicted fundamental house prices from the VEC model, at the national level and for each municipality, calculated from 2014q1 − 2021q4. At the national level, the mean value of this gap is 4%, which implies that, on average, actual real house prices have been above the fundamental prices; hence, house prices have been overvalued on average. However, the house price gap has varied between almost 16% overvaluation and 8% undervaluation over this period. The largest overvaluation among the municipalities was in Fredrikstad (27%) in the second quarter of 2020, while the largest undervaluation was in Stavanger (-28%) in the fourth quarter of 2021.

5 From boom to bust: Probabilities of turning points

5.1 Methodological framework

Using a linear probability model, we test if the gap between actual prices and fundamental prices affect the probability of turning points in house price cycles on a regional level. We pool the data from the 14 municipalities in our sample into a single panel. The specification is shown in Equation 6, in which the variables vary over time, t, and with municipality, m.

Table 4 Summary of the real house price gap in percentage, 2014 – 2021.

Variable	Capital: Asker	Capital: Bærum	Capital: Lillestrøm	Capital: Oslo
Minimum	-16.00	-17.53	-9.95	-11.69
Mean	4.96	3.83	5.51	5.94
Maximum	24.19	24.04	20.05	24.42
SD	10.92	11.53	8.49	10.21
Variable	East: Drammen	East: Fredrikstad	East: Skien	East: Tønsberg
Minimum	-14.04	-27.19	-8.20	-4.79
Mean	3.21	1.63	1.81	4.57
Maximum	17.77	27.21	9.93	12.54
SD	9.19	14.08	5.31	4.94
Variable	Mid/North: Bodø	Mid/North: Tromsø	Mid/North: Trondheim	Norway
Minimum	-4.48	0.83	-20.27	-8.22
Mean	9.78	10.05	-4.74	4.27
Maximum	24.30	17.36	4.32	15.57
SD	8.20	4.51	7.26	6.73
Variable	West: Bergen	West: Stavanger[1]	West: Ålesund	
Minimum	-13.70	-27.70	0.41	
Mean	-0.87	-7.82	3.10	
Maximum	7.05	4.22	6.92	
SD	6.40	8.05	1.99	

Notes: This table presents the minimum, mean, maximum and standard deviation of the real house price gap— the difference between the actual house prices and the predicted fundamental house prices from the VEC-model, in %for each municipality. [1] In the estimation for Stavanger, a quarterly series for oil investments is included for establishing a cointegrating relationship, which implies that Equation 2 is substituted with $ph = \beta_{\tilde{y}}\tilde{y} + \beta_{UC}UC + \beta_{o}oil$ for Stavanger.

$$peak_{m,t} = \alpha^p_m + \beta^p(ph_{m,t} - ph^*_{m,t}) + \mu^p \mathbf{X}_{m,t} + \epsilon^p_{m,t} \tag{6}$$

The dependent variable, $peak_{mt}$, is a dummy-variable equal to 1 when there is a peak in real house prices, and 0 otherwise. Note that $peak_{mt}$ does not indicate a bubble or overvaluation of prices, but is simply an indicator for the top of the cycle, regardless of whether this is a bubble cycle or a cycle that can be explained by fundamentals. The explanatory variable $ph_{m,t} - ph^*_{m,t}$ is the real house price gap— approximately the percentage difference[19] between actual house prices and the fundamental house price path from the VEC model, as specified in Equation 5. We first test a specification without municipality fixed effects and subsequently include municipality fixed effects, α^p_m, in the estimation. $\mathbf{X}_{m,t}$ is a vector of municipality-specific control variables that vary over time. These control variables are the duration of the house price gap (the number of consecutive periods with a positive price gap) and the registered unemployment rate, which serves as a proxy for the local business cycle, as well as polynomials of the price gap to capture potential non-linearities.

The coefficient of main interest is β^p, which measures the increase in the probability of a peak when the real house price gap increases by 1 percentage point. The expected sign of the coefficient β^p is positive, as one would expect that an increase in the real house price gap increases the probability of turning from a boom to a bust. Note that in this framework, a boom period is simply a period with increasing prices and a bust period is a period with declining prices. This implies that all quarters are either boom or bust periods, and a boom period does not necessarily mean that prices are increasing more than implied by fundamentals, it merely implies that they are increasing.

5.2 Results panel regression: Quasi out-of-sample

We begin with the quasi out-of-sample estimation based on the VEC model from Section 4, which we estimated for the period 2003q1 – 2019q4. The fundamental house price path was

[19] Log point difference.

Table 5 Results of the panel regression: Peak, quasi out-of-sample.

Model	(1) Baseline	(2) w/FE	(3) controls 1	(4) controls 2	(5) Polynomials
Price gap	0.002*	0.002***	0.004**	0.005***	0.007***
Duration gap			-0.007***	-0.006**	-0.007**
Unemployment rate				-0.025***	-0.021**
$Pricegap^2$					0.000
$Pricegap^3$					-0.000***
$Pricegap^4$					-0.000
Municipality fixed effects		✓	✓	✓	✓
Observations	448	448	301	277	277
Municipalities	14	14	12	11	11

Notes: The table reports estimates of β; for how the probability of a peak is affected by an increase in the house price gap, see Equation 6: $peak_{m,t} = \alpha_m + \beta(ph_{m,t} - ph^*_{m,t}) + \mu\mathbf{X}_{m,t} + \epsilon_{m,t}$. β is estimated by OLS. The estimation period is 2014q1 – 2021q4 and 14 municipalities are included. *p<0.1; **p<0.05; ***p<0.01.

constructed for the period 2014q1 – 2021q4, which overlaps parts of the estimation period. The quasi out-of-sample panel regression is estimated on quarterly data for the period 2014q1 – 2021q4 for 14 municipalities, thus leaving us with 448 observations.

In Table 5, results from the estimation of β^p in Equation 6 are reported. Since we use a specification with municipality fixed effects, we choose to use a linear probability model due to the strict assumptions and inability to report partial effects in the logit model with fixed effects (Wooldridge, 2010).[20]

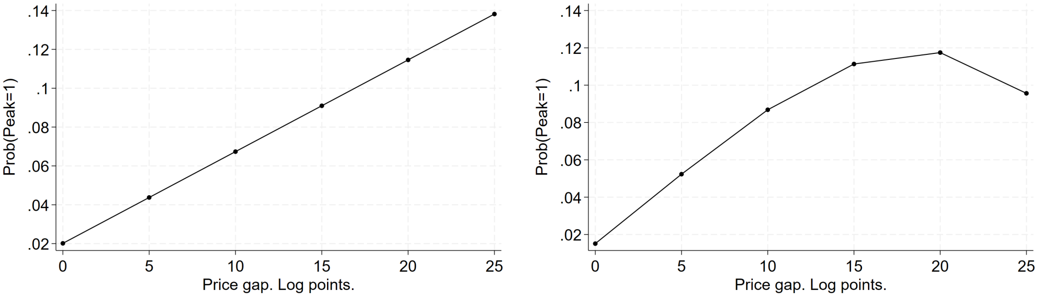
The first column presents the results of the probability of a peak dependent on the price gap without municipality fixed effects or control variables. We find that an increase in the price gap by 1 percentage point indicates an increase in the probability by 0.16 percentage points. When we include municipality fixed effects (column 2), the effect increases slightly

[20] However, log odds ratios from a logit estimation with fixed effects are included in Appendix D, thereby providing coefficients with the same sign and significance as those in the linear model reported here.

to 0.22 percentage points and the precision of the estimate increases. We also include the duration of the price gap as a control variable (column 3) and see, somewhat surprisingly, that the duration has negative and significant effect on the price gap. In addition, the effect of the price gap doubles in size. This result indicates that it is the gap between actual and fundamental prices that has a positive effect on the probability of a peak, not the duration of the boom period. It is evident from Figure 3 that the price gap usually does increase with duration, but the duration alone does not add to the probability of a peak; in contrast, it has a negative effect. The negative sign of the duration coefficient could be explained by several municipalities with a positive price gap at the end of the estimation period (2021q4), in which we do not reach the peak within our estimation horizon. We also include registered unemployment at the municipality level as a proxy for the local business cycle (column 4). An increase in the local unemployment rate also appears to have a negative effect on the probability of a peak. The effect of the price gap increases slightly in size, and statistical significance is maintained when unemployment is added as a control variable. In the linear model, the effect increases to 0.47 percentage points when all control variables and municipality fixed effects are included in the regression. In the fifth column, we add a fourth-degree polynomial of the price gap to the model to be able to assess non-linearity within the linear probability model framework. The polynomials appear to have small effects on the probability of the peak, although the third-degree polynomial is statistically significant.

Figure 4 illustrates the predicted probability of a peak from the linear probability model for different levels of the real house price gap. In panel (a), the linear model with control variables, as depicted in Column 4 in Table 5 illustrates that when the price gap increases from 0% to 15%, the probability of a peak increases by 7 percentage points to 9%. Panel (b) shows the results reported in Column 5, with a fourth-degree polynomial of the house price gap included in the estimation. The results are similar to the ones from the linear model, and the probability of a peak increases by 9 percentage points to 11% when the price gap increases from 0% to 15%. In the latter model, the effect is largest for small values of the price gap and diminishes when the price gap becomes very large. We could, instead,

Figure 4 The predicted probability of a peak and the real price gap: Quasi out-of-sample.



(a) Linear **(b)** Polynomial

Notes: This figure illustrates the predicted probability of a peak from the linear probability estimation of Equation 6: $peak_{m,t} = \alpha^p_m + \beta^p(ph_{m,t} - ph^*_{m,t}) + \mu^p \mathbf{X}_{m,t} + \epsilon^p_{m,t}$ with predictions made quasi out-of-sample. The left panel illustrates the linear model with control variables, as shown in Column 4 of Table 5, while the right panel illustrates the fourth degree polynomial, as shown in Column 5 of Table 5.

have expected that the effect of the house price gap was increasing in the size of the gap, as highly overvalued prices could be associated with a high probability of a turning point and a downturn in prices. However, the diminishing effect might indicate that a rather large price gap is caused by factors outside the model or a structural break.

One can also investigate the effect of an increase in the house price gap on the probability of a trough— see Equation 7. In this case, the expected sign of β^t is negative.

$$trough_{m,t} = \alpha^t_m + \beta^t_(ph_{m,t} - ph^*_{m,t}) + \mu^t \mathbf{X}_{m,t} + \epsilon^t_{m,t} \tag{7}$$

The results from this estimation are provided in Table 6. We see that the house price gap does not have a significant impact on the probability of a trough. The reason we are not able to measure any negative and significant effect on the probability of a trough might be that we have only a few observations of a trough in our estimation period.

Our results indicate that a higher house price gap (more overvalued prices) increases the probability of a peak (and an ensuing downturn), while we cannot say that it decreases the probability of a trough (and an ensuing upturn).

Table 6 Results of the panel regression: Trough, quasi out-of-sample.

Model	(1) Baseline	(2) w/FE	(3) controls 1	(4) controls 2	(5) Polynomials
Price gap	-0.000	0.000	0.000	0.001	0.002
Duration gap			0.002	0.003	0.005
Unemployment rate				-0.006	-0.004
$Pricegap^2$					-0.000
$Pricegap^3$					-0.000
$Pricegap^4$					0.000
Municipality fixed effects		✓	✓	✓	✓
Observations	448	448	448	416	416
Municipalities	14	14	14	13	13

Notes: The table reports estimates of β; for how the probability of a peak is affected by an increase in the house price gap, see Equation 7: $trough_{m,t} = \alpha^t_m + \beta^t_(ph_{m,t} - ph^*_{m,t}) + \mu^t \mathbf{X}_{m,t} + \epsilon^t_{m,t}$. β is estimated by OLS. The estimation period is 2014q1 – 2021q4 and 14 municipalities are included. *p<0.1; **p<0.05; ***p<0.01.

Table 7 Results of the panel regression: Out-of-sample.

	Peak				Trough			
Model	LPM	LPM 2	LPM 3	4.Poly.	LPM	LPM 2	LPM 3	4.Poly.
Price gap	0.003***	0.007***	0.007**	0.006**	-0.002**	-0.002**	-0.002	-0.004
Duration gap		-0.006***	-0.005***	-0.006**		0.001	0.001	0.000
Unemployment rate			-0.015**	-0.017***			-0.000	-0.002
$Pricegap^2$				0.000				0.000
$Pricegap^3$				-0.000				0.000
$Pricegap^4$				-0.000				-0.000
Municipality fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	448	448	416	416	448	448	416	416
Municipalities	14	14	13	13	14	14	13	13

Notes: The table reports estimates of β; for how the probability of a turning point is affected by an increase in the house price gap, see Equations 6 and 7: $peak_{m,t} = \alpha_m + \beta(ph_{m,t} - ph^*_{m,t}) + \mu \mathbf{X}_{m,t} + \epsilon_{m,t}$. β is estimated by OLS with municipality-fixed effects. The estimation period is 2014q1 – 2021q4 and 14 municipalities are included. *p<0.1; **p<0.05; ***p<0.01.

5.3 Results of the panel regression: Out-of-sample

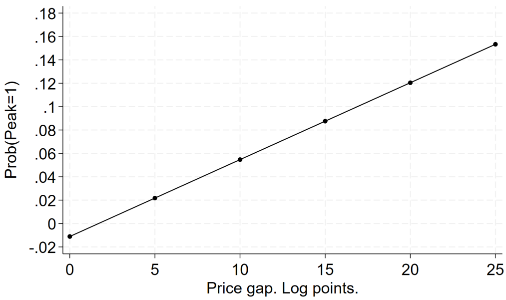
The effect of the price gap on the probability of a peak in house prices should also be present when the price gap is calculated out-of-sample. Due to the limited time period with quality data at the municipality level, it is challenging to establish a cointegrating relationship using a shorter time period than 2003q1 – 2019q4. As a sensitivity check, we perform an out-of-sample exercise, in which we estimate fundamental prices between 2014q1 and 2021q4 based on a VEC model estimated from 2003q1 – 2013q4. The VEC estimation from 2003q1 – 2013q4 provides long-run coefficients with the same sign as in the baseline estimation, but with slightly lower coefficients for the real user cost. The adjustment parameters are all negative and significant, which is reassuring for the assumption of cointegration. The long-run coefficients and adjustment parameters are reported in Appendix E.

The panel regression is performed on quarterly data from 2014q1 – 2021q4 for the same 14 municipalities, again leaving us with 448 observations. The results are provided in Table 7. The results indicate a slightly larger effect on the probability of a peak (see Columns

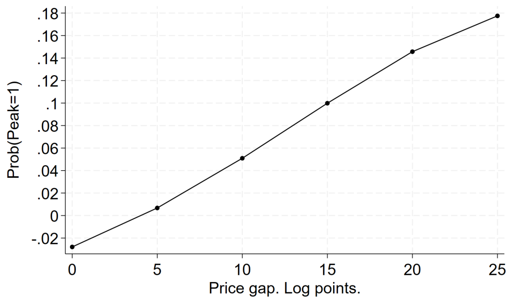
1-3) when the price gap is calculated out-of-sample. Without control variables, the effect of an increase in the price gap of 1 percentage point is estimated to be 0.3 percentage points, while the effect is estimated to be 0.7 percentage points when control variables are included. Therefore, the out-of-sample exercise increases our confidence in the model as a useful tool for assessing the probability of house price peaks.

In the out-of-sample exercise, the house price gap also has a statistically significant effect on the probability of a trough, which is in contrast to the quasi-out-of-sample results. However, the effect is no longer significant when control variables are added to the model.

Figure 5 The predicted probability of a peak and the real price gap: Out-of-sample.



(a) Linear



(b) Polynomial

Notes: This figure illustrates the predicted probability of a peak from the linear probability estimation of Equation 6: $peak_{m,t} = \alpha^p_m + \beta^p(ph_{m,t} - ph^*_{m,t}) + \mu^p \mathbf{X}_{m,t} + \epsilon^p_{m,t}$ with predictions made out-of-sample. The left panel illustrates the linear model with control variables, as shown in Column 3 of Table 7, while the right panel illustrates the fourth degree polynomial, as shown in Column 4 of Table 7.

Once again, we illustrate the magnitude of the effect for different magnitudes of the house price gap (see Figure 5). We recognize that the size of the effect is slightly larger than that in the previous subsection, and we see that the polynomials of the price gap are slightly less important. In summary, it is evident that when the price gap is around 15%, the predicted probability of a peak, and an ensuing downturn in prices, is approximately 10% in all estimations.

6 Conclusion

House price cycles are closely related to real business cycles. High and persistent growth in house prices and credit can amplify economic downturns. In addition, house prices are closely linked to housing investments, which has been shown to act as a leading indicator for economic downturns. Further, house price cycles are regional. In this paper, we sought to increase our understanding of house price cycles at the regional level in Norway by describing and explaining the fundamental drivers of these cycles. We also applied our model and the real house price gap as a tool to assess the probability of a turning point in house prices.

We documented the timing and duration of booms and busts in real house prices for Norway as a whole and for 14 of its largest municipalities located in 4 different regions in Norway for the period between 2003 and 2021. We found that although house price cycles in many cases are similar across municipalities within the same region, there is considerable heterogeneity in the timing and duration, as well as the amplitude, of booms and busts in real house prices across the regions.

Further, we used a CVAR model to establish a long-run relationship between real house prices and fundamental drivers, defined as real disposable income, the housing stock, and the real user cost of housing, at the municipality level. By this, we sought to explain the drivers of local house price cycles. We found that the effect of the user cost on real house prices also varies across the regions, with the strongest effect found in the capital, Oslo, and its surrounding municipalities. The effect of disposable income relative to the housing stock is more similar across regions with an elasticity of between 1 and 2.

Finally, the fundamental house price path from the CVAR model was applied to investigate whether the real house price gap— the difference between actual and fundamental prices, as implied by the model— affected the probability of turning points in house prices. We found that an increase in the house price gap from 0% to 15% increased the probability of a peak (a downturn) by approximately 7 percentage points to 9%. The effect was statistically significant and of a similar size both when fundamental prices were calculated quasi out-of-sample and completely out-of-sample.

This paper found that the housing cycle and its fundamental drivers vary across Norwegian municipalities. Thus, a framework for analyzing house price cycles at a more detailed geographical area can be a useful supplement to national house price models. Although monetary and macroprudential policymakers are mainly concerned with the national cycle, regional models increase the understanding of heterogeneous regional cycles.

The cointegration framework we used at the regional level has also shown to be valid for other countries than Norway at both the national and regional levels (Cuhna & Lobão, 2022; Hwang & Quigley, 2006; Meen, 1999; Oikarinen et al., 2018; Plakandras et al., 2024), which is promising in terms of the external validity of our results. Future research on this topic would benefit from longer time-series, which could enable a more in-depth out-of-sample analysis as well as a more fine-tuned estimation of long-run coefficients, such as separate coefficients for disposable income and housing stock.

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Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this work the authors used Chat GPT in order to improve language. After using this tool/service, the authors reviewed and edited the content as needed and take full responsibility for the content of the publication.

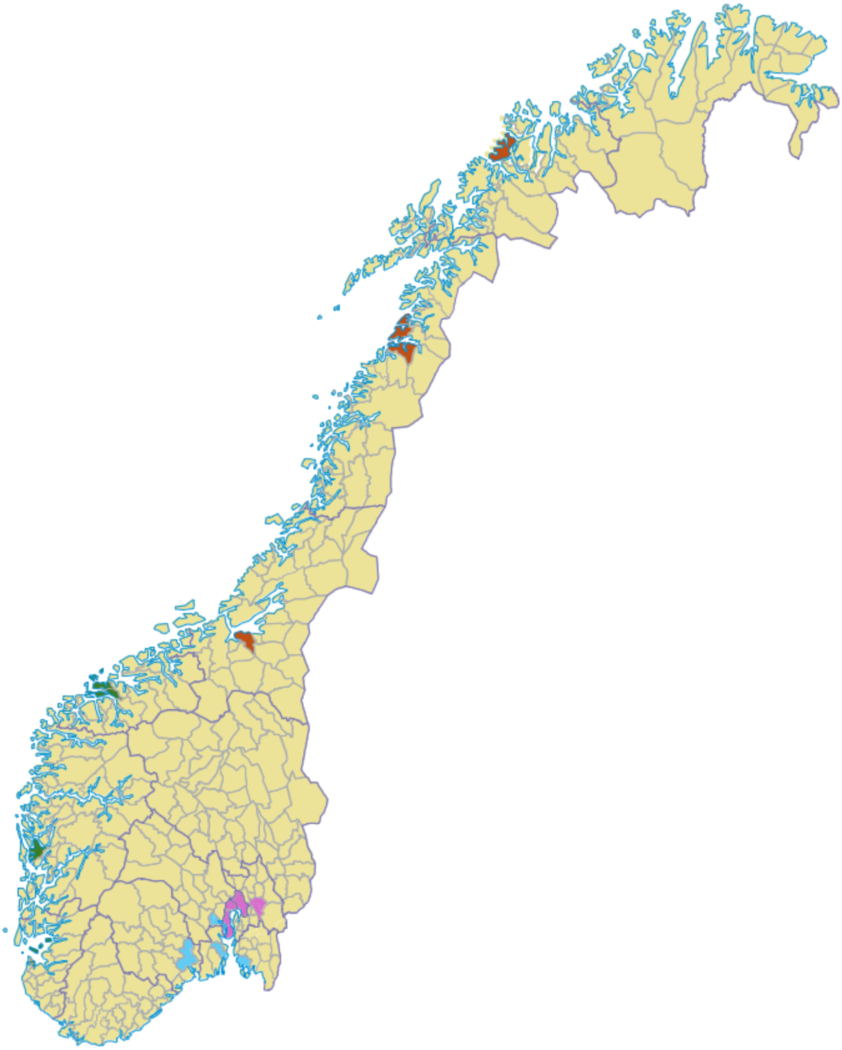
Appendix A: Overview of municipalities

Table A.1 Overview of changes in municipalities.

Municipality	Year	Merged with
Capital extended		
Oslo		
Bærum		
Asker	2020	Røyken, Hurum
Lillestrøm	2020 (established)	Sørum, Fet, Skedsmo
East		
Drammen	2020	Nedre Eiker, Svelvik
Tønsberg	2020	Re
Skien		
Fredrikstad		
Middle and North		
Trondheim	2020	Klæbu
Tromsø		
Bodø		
West		
Bergen		
Stavanger	2020	Finnøy, Rennesøy
Ålesund	2020	Ørskog, Skodje, Haram, Sandøy

The 14 municipalities include the most populated areas in Norway in which most of the housing transactions take place. A few of the municipalities have merged over the period analyzed. Data for these municipalities are merged over the complete sample to avoid any breaks in the series. See details in Table A.1. In Figure A.1 we present a map with the selected municipalities to provide an idea of where they are located relative to each other.

Figure A.1. Map of the municipalities.



Notes: This figure provides an overview of the location of the selected municipalities. The municipalities close to Oslo (Oslo, Bærum, Asker, and Lillestrøm) are indicated in purple, the remaining eastern municipalities (Drammen, Tønsberg, Fredrikstad, and Skien) are indicated in blue, while the western municipalities (Bergen, Stavanger, and Ålesund) are indicated in green and the middle and northern municipalities (Trondheim, Bodø, and Tromsø) are indicated in red. In 2003, these 14 municipalities represented 36.6% of Norway's total population, increasing to 39.7% by 2021. **Source:** The Norwegian Mapping Authority: https://www.kartverket.no/til-lands/kart/illustrasjonskart.

Appendix B: Summary statistics: Peaks and troughs

Table B.1. Summary statistics: Peaks and troughs— Norway.

	p10	p25	p50	p75	p90
No. peaks	2	3	3	3	4
No. troughs	2	3	3	3	4
Bust (qtrs.)	3.5	4.0	5.0	6.0	7.3
Boom (qtrs.)	7.5	9.0	14.5	16.0	18.0
P-to-P (qtrs.)	12.3	15.0	19.0	20.0	23.0
T-to-T (qtrs.)	13.0	14.7	19.5	21.5	24.0
Bust (% growth)	-1.4	-1.0	-0.8	-0.6	-0.5
Boom (% growth)	1.4	2.0	3.0	4.6	6.0
P-to-P (% growth)	0.3	1.1	2.2	3.7	4.7
T-to-T (% growth)	0.4	1.2	2.7	4.2	5.1

Notes: This table presents the summary statistics for real house price cycles for 81 Norwegian regions. To assess turning points, the following censoring rules are applied: window = 2, phase = 2, cycle = 6. N = 81. **Data sources**: Eiendomsverdi/Eiendom Norge, Statistics Norway.

Table B.1 presents the summary statistics for peaks and troughs in real house prices for all areas in Norway in which price indices are calculated by Eiendom Norge— a total of 81 areas. These areas include municipalities and larger geographical, but less densely populated, districts. It is evident that the number of peaks and troughs vary between 2 for the 10% of the areas with the lowest number of peaks and troughs, to 4 for the areas with 90% of the areas with the highest number of peaks and troughs. The duration of booms varies between 8 and 18 quarters, while the same figures for busts are 4 and 7. There is also heterogeneity in the price development over the phases, in which busts involve a decline in real prices by 0.5% to 1.4% for the first and last deciles, respectively. For booms, real prices increase between 1.4% and 6% for the first and tenth deciles, respectively.

Appendix C: Results of the trace test

Table C.1. Results of the trace test.

Trace stat.	Capital: Asker	Capital: Bærum	Capital: Lillestrøm	Capital: Oslo	Crit.value
0	49.57	49.43	43.05	39.32	34.55
1	18.31	22.46	12.57*	12.56*	18.17
2	0.35*	0.35*	0.95	0.94	3.74
Trace stat.	**East: Drammen**	**East: Fredrikstad**	**East: Skien**	**East: Tønsberg**	**Crit.value**
0	41.30	42.07	55.97	39.63	34.55
1	14.11*	19.32	21.57	13.13*	18.17
2	1.14	0.22*	0.96*	0.31	3.74
Trace stat.	**Mid/North: Bodø**	**Mid/North: Tromsø**	**Mid/North: Trondheim**	**Norway**	**Crit.value**
0	60.75	46.88	52.15	47.56	34.55
1	17.18*	23.38	17.72*	17.87*	18.17
2	1.13	2.64*	0.18	0.24	3.74
Trace stat.	**West: Bergen**	**West: Stavanger’**	**West: Ålesund**		**Crit.value**
0	53.94	80.75	50.69		34.55
1	18.44	33.90	15.89*		18.17
2	0.71*	15.23	0.20		3.74

Notes: This table reports the trace statistics from the cointegration test of Johansen (1988). The estimation period is 2003q1 – 2019q4. The null hypothesis is that there exist a maximum of zero, one and two cointegrating relationships. *Indicates the first test statistic below critical value and, therefore, the number of cointegrating relationships reported from the test. ’In the estimation for Stavanger, a series for oil investments is included in the model, and the critical values are 54.64, 34.55, and 18.17.

Appendix D: Panel regression: Logit estimation

Table D.1. Results of the quasi out-of-sample panel regression: Logit estimation.

	Peak			Trough		
Model	Logit	Logit 2	Logit 3	Logit	Logit 2	Logit 3
Price gap	0.108**	0.313***	0.315**	0.002	0.009	0.028
Duration gap		-1.154**	-1.013*		0.040	0.069
Unemployment rate			-0.970			-0.118
Municipality fixed effects	✓	✓	✓	✓	✓	✓
Observations	352	231	207	352	352	320
Municipalities	11	9	8	11	11	10

Notes: This table reports the estimates of β in Equations 6 and 7. The estimation period is 2014q1 – 2021q4 and 14 municipalities are included. *p<0.1; **p<0.05; ***p<0.01.

Appendix E: Robustness of the VECM model

Table E.1. Results of the long-run coefficients from cointegrated VAR analysis of house prices 2003q1 – 2013q4.

Variable	Capital: Asker	Capital: Bærum	Capital: Lillestrøm	Capital: Oslo
User Cost	-4.036***	-5.494**	-4.357**	-6.555***
Disp.income/H.stock	1.627***	1.613***	1.561***	1.362***
Adj. parameter	-0.213***	-0.149***	-0.202***	-0.288***
Variable	East: Drammen	East: Fredrikstad	East: Skien	East: Tønsberg
User Cost	-6.225*	-4.096**	-4.624***	-2.781**
Disp.income/H.stock	1.916***	1.047***	1.778***	1.223***
Adj. parameter	-0.111**	-0.166**	-0.181***	-0.273***
Variable	Mid/North: Bodø	Mid/North: Tromsø	Mid/North: Trondheim	Norway
User Cost	-13.199***	-2.476	-8.244***	-5.646***
Disp.income/H.stock	1.827***	1.232***	1.334***	1.618***
Adj. parameter	-0.120***	-0.116***	-0.131**	-0.185***
Variable	West: Bergen	West: Stavanger[1]	West: Ålesund	
User Cost	-10.682***	-8.802***	-3.567***	
Disp.income/H.stock	1.378***	2.139***	1.892***	
Adj. parameter	-0.164***	-0.152***	-0.200***	

Notes: This table reports the long-run coefficients (β) from Equation (4). The estimation period is 2003q1 – 2013q4. The dependent variable is real house prices (nominal house prices deflated by CPI), the real user cost is measured as the nominal, tax-adjusted, mortgage rate subtracted CPI, and the real disposable income is disposable income deflated by CPI and measured relative to housing supply. *p<0.1; **p<0.05; ***p<0.01.

Table E.2. Results of the long-run coefficients from cointegrated VAR analysis with nominal mortgage rates.

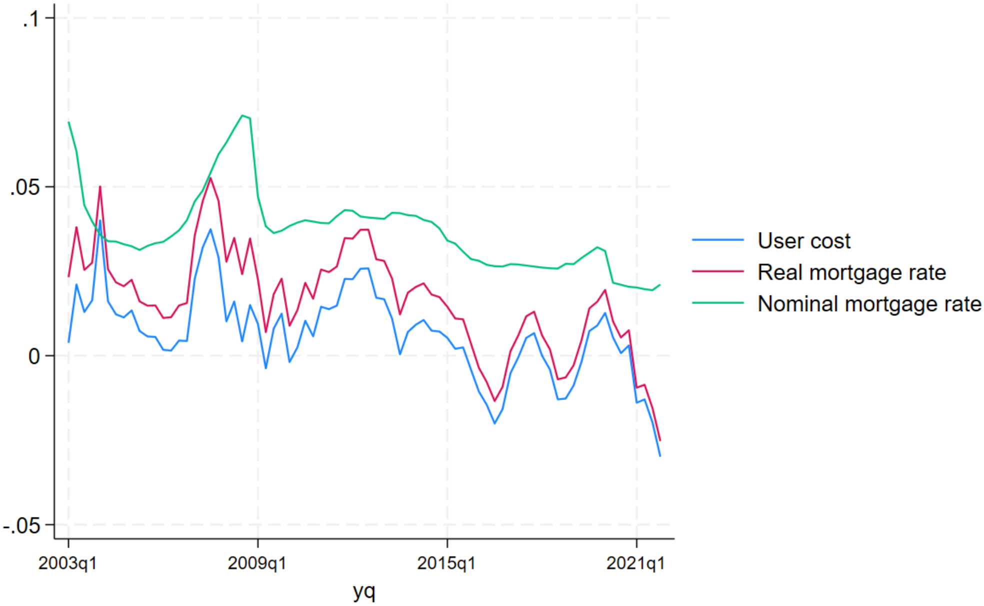
Variable	Capital: Oslo	Capital: Lillestrøm
User Cost	-3.6962	-3.0922
Log(nom.rates)	-0.2656***	-0.2107***
Disp.income/H.stock	1.3776***	1.5332***
Adj. parameter	-0.2420***	-0.1702***
Variable	**East: Drammen**	**Norway**
User Cost	-2.4774	-1.7564
Log(nom.rates)	-0.2560***	-0.1338***
Disp.income/H.stock	1.9754***	1.4894***
Adj. parameter	-0.1229***	-0.2526***

Notes: This table reports the long-run coefficients (β) from Equation 4, but including a separate variable for the logarithm of nominal mortgage rates in addition to the real user cost, and the p-value for the test for over-identifying restrictions. The estimation period is 2003q1 – 2019q4. The dependent variable is real house prices (nominal house prices deflated by CPI) and real disposable income is disposable income deflated by CPI and measured relative to housing supply. The real user cost is measured as the nominal, tax-adjusted, mortgage rate subtracted CPI. In this definition of the user cost, we exclude depreciation and assume that expected capital gains are captured by the coefficients on the house price lags. *p<0.1; **p<0.05; ***p<0.01.

In Table E.2, we present the results from the estimation of a long-run cointegration model for house prices, including a separate term for nominal mortgage rates. We include the logarithm of nominal mortgage rates in the estimation to also capture non-linearities in the house price sensitivity to nominal mortgage rates. If we assume that nominal mortgage rates are 5%, then an increase of 1 percentage point in the nominal rate to 6% is equivalent to an increase of 20%. For Norway, this increase in the nominal mortgage rate is associated with a decline in real house prices by 2.7%, which is in addition to the effect from the real user cost. For Oslo, the equivalent number is a decline by 5.3% in real house prices; for Lillestrøm, it is 4.2%; and for Drammen, it is 5.1%.

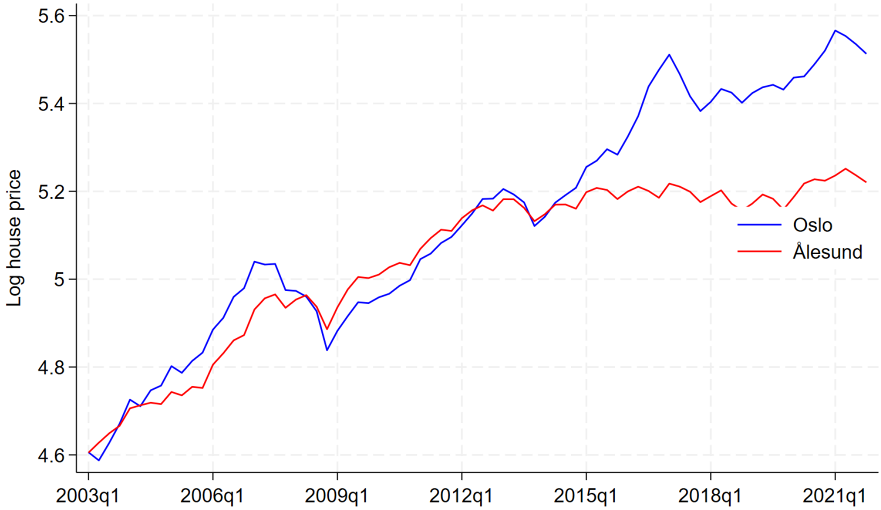
Appendix F: Additional figures and tables

Figure F.1. Real user cost and mortgage rates.

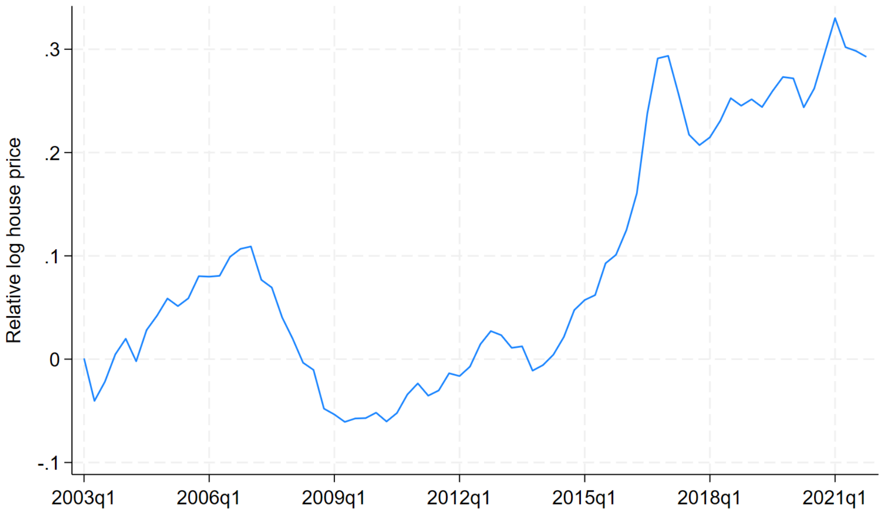


Notes: This figure depicts a quarterly time-series for the real user cost, real mortgage rates, and nominal mortgage rates from 2003q1 – 2021q4. The user cost is defined as $UC = (1-\theta)i - \pi + \delta - \Delta ph$, where θ is the tax rate for which interest rate expenses are deductible, i denotes the nominal mortgage interest rate, π denotes the inflation rate, δ denotes the depreciation rate, and Δph denotes the expected house price growth. In the econometric analysis, the depreciation rate (which is assumed to be constant over time) is discarded and expected house price growth is captured by lagged house prices, which are included as separate terms with five lags (quarterly) in the econometric model. **Data sources:** Statistics Norway, The Norwegian Tax Administration.

Figure F.2. Relative log real house price index for Oslo and Ålesund.



(a) Log real house price index



(b) Relative log real house price development

Notes: This figure depicts the logarithm of the real house price index, the variable used in the cointegration model for the municipalities Oslo and Ålesund (panel a), and the relative house prices between Oslo and Ålesund (panel b). **Data sources:** Eiendomsverdi/Eiendom Norge.

4 The price effect of size restrictions on residential building.

The price effect of size restrictions on residential building *

Nini Barth[†] and Jeanette Fjære-Lindkjenn[‡]

Abstract

We investigate the price effect of enforcing regulation on the share of small and large apartments being built in the inner city of Oslo. An apartment size regulation set a minimum of 35 square meters per apartment in 2008. The regulation was intended to ensure that the stock of new units would consist of both small and large apartments to secure a mixed demographic structure. Newly built apartments below the minimum size plummeted after the regulation was implemented in both the regulated inner city area and the rest of Oslo. We use transaction data for apartments to investigate the effect of the regulation on apartment prices within a difference-in-difference framework. We find that the price of apartments below the minimum size of 35 square meters increased by 3.3 percent compared to the control size segment of 40-50 square meters after the implementation of the apartment size regulation. We also examine the price effect on large apartments. Despite an increase in the number of large newly built apartments in the regulated inner city, we find that the prices of these apartments increased compared to the districts in Oslo that was not regulated. We suggest that this price increase can partially be explained by higher demand for large apartments from families with children, in line with the intention of the regulation.

Keywords: Housing supply and markets, Government policy
JEL-codes: R31; R38

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1 Introduction

High and increasing house prices in metropolitan areas, along with the role of restrictions on housing supply, have been extensively debated in recent years, see e.g., Glaeser and Gyourko (2018). Restrictions in and regulations of housing supply in metropolitan areas can have large welfare implications by reducing labour mobility and hindering potential agglomeration effects (Ciccone, 2002; Hsieh & Moretti, 2019; Saks, 2008). In addition, cities may increasingly cater to the wealthy and established as housing becomes more unaffordable (Baum-Snow & Hartley, 2020; Su, 2022). Government regulations on housing development, such as zoning, building heights limits and land use restrictions, vary across cities and between local housing markets within the same neighbourhoods. These regulations impact local house prices and demographics by shaping the mix of housing units available. Collectively, these regulations influence the overall housing supply within a city.

In this paper, we study effects of a 2008 policy change in the Norwegian housing market on square meter prices of apartments. The policy set restrictions on the shares of various size segments that would be allowed to be constructed in a building project in specified districts in Oslo, the capital of Norway. The regulation sought to improve housing quality, as well as the construction of a wider variety of apartments to accommodate diverse demographic and social groups. Specifically, it aimed to ensure the development of larger apartments suitable for families with children (Oslo Kommune, 2007). A lower limit on apartments allowed to be built was set to 40 square meters in 2008 and relaxed to 35 square meters after 2013. There were also minimum and maximum share restrictions on other size segments. A minimum of 50 percent of the apartments had to be above 80 square meters. This share was relaxed to a minimum of 40 percent in 2013. Using transaction data on residential apartments and a difference-in-difference approach, we explore whether the lower bound on apartment size led to increased prices of these apartments. We also investigate whether the increase in the supply of apartments above 80 square meters had an impact on square meter prices in this size segment in the inner city. Furthermore, we merge the data with tax registers providing information at the household level to examine whether there was an increase in families with children buying apartments in the inner city as was indented by the norm.

The data on newly built apartments show that the construction of apartments below the minimum size of 35 square meters dropped markedly after the implementation of the apartment size regulation in both in the regulated inner city area and the rest of Oslo. For apartments above 80 square meters, it seems that the regulation only affected new apartments in the inner city, where there was a substantial increase in their construction.

We find that the square meter price of apartments below the lower size limit of 35 square meters increased significantly after the apartment size regulation was implemented in 2008. Using a static difference-in-difference framework, we find an effect amounting to a 3.3 percent price increase for apartments below 35 square meters. The effect is estimated using apartments between 40 and 50 square meters as the control group. In these estimations, all districts in Oslo are included in the treatment group since newly built apartments below the lower limits decreased markedly after the regulation was introduced in the entire city, not only the regulated districts. When we investigate the effects on the regulated area, the inner city, and other districts in Oslo, separately, we find highly similar sizes of the effects. We continue with a dynamic difference-in-difference analysis to investigate the timing of effects. The effect of the apartment size regulation is statistically significant from 2016 to 2021. Since the building processes can take up to several years from the time when the general permission is given until the construction is finished, and new apartments comprise a small fraction of the stock of apartments, it seems reasonable that the price effect of the introduction of the regulation comes with a time lag.

We perform several sensitivity checks to test the robustness of our results and whether we can interpret them as causal effects of the apartment size regulation. Our control group consists of small apartments between 40 and 50 square meters. Although our control group also consists of small apartments, one hypothesis could be that the increase in prices of apartments below 35 square meters relative to the slightly larger apartments in the control group is caused by a general trend of rising prices in all size segments, not by the apartment size regulation (Landvoigt et al., 2015). Higher prices in the larger size segments might force more households to buy smaller apartments, which could lead to increased demand and relatively higher price growth for the smallest size segment. To test this hypothesis, we estimate the model with placebo size groups in the small apartment segment, with apartments below 25 square meters relative to apartments between 26 and 35 square meters, and apartments between 40 and 44 square meters relative to apartments between 45 and 50 square meters. There is no statistically significant difference between the price growth in the placebo treatment and the control groups. We also test and find no effects with the placebo size segment measuring 60-69 square meters relative to 70-79 square meters. Furthermore, we test for effects in placebo cities and find no positive, statistically significant effects on apartments below 35 square meters compared to 40-50 square meters in these cities.

We also explore the impact of the apartment size regulation on the square meter prices of apartments above 80 square meters, again using a difference-in-difference framework. Newly built apartments in this size segment increased in the inner city as a response to the regulation. The regulation did, however, not seem to alter the trend in new apartments above 80 square meters in the outer city. In this part of the analysis the control group therefore consists of apartments above 80 square meters in the outer city. We find that the prices of large apartments in the inner city increased by 4.4 percent after the implementation of the apartment size regulation, relative to large apartments in the outer city. One interpretation of the positive price effect on large apartments is that demand for large apartments has increased more than supply and that prices would have risen even more without the increase in supply caused by the apartment size regulation.

Lastly, we investigate if the increase in prices of large apartments in the inner city is caused by an increase in demand from families with children, in line with the intention of the regulation. Aggregate data on household types by district show an increase in the number of families living in the inner city. Transaction data merged with household data also show an increase in the number of families with children buying large apartments in the inner city. These findings suggest that the regulation may have successfully encouraged more families with children to move to the inner city, potentially driving up demand and contributing to the rise in prices of larger apartments. However, it is important to interpret these results cautiously, as there has been a general increase in households of all types within the inner city, and the proportion of families remains relatively low compared to the outer city. As such, the rise in large apartment prices could stem from broader demand trends, rather than being exclusively driven by families with children.

Our article contributes to two strands of literature documenting the costs of regulations on housing development. First, our findings add to the literature investigating price effects of stringent regulations. The strong link between strict housing development regulations and rising house prices is well established (Gyourko & Krimmel, 2021; Jackson, 2018; Quigley & Raphael, 2005; Zabel & Dalton, 2011), see Gyourko and Molloy (2015) and Molloy (2020) for a review. The wedge between actual house prices and counterfactual prices if the regulations were more lenient is referred to as a regulatory or zoning tax (Glaeser & Gyourko, 2003, 2018). This zoning tax can be substantial. Gyourko and Krimmel (2021) estimate the zoning tax as the gap between the value of land with the extensive and intensive margins and find that this gap amounts to USD 400,000 for a quarter acre in San Francisco. Gyourko et al. (2021) construct a regulatory index and document the degree and heterogeneity of regulatory restrictiveness across metropolitan areas in the US. Gyourko and Krimmel (2021) find that the index is strongly correlated with their measures of zoning taxes. Although a number of studies find a positive correlation between stringent regulations and house prices, establishing a causal relationship is more complicated, as these regulations often depend on various community-specific characteristics (Davidoff, 2015; Saiz, 2010). As Molloy (2020) points out, there is also great variation in regulations both across and within cities, and these regulations frequently overlap, complicating efforts to isolate the impact of any single policy. We overcome this problem by using other apartments within the same city as control group in the difference-in-difference framework. We contribute to this literature by identifying a causal relation between the construction halt for apartments below 35 square meters and their relative price increase. The second strand of literature to which we contribute comprises the changes in buyer composition and demography in cities over time (Baum-Snow & Hartley, 2020; Couture et al., 2020; Su, 2022). While the existing research primarily focuses on the increase in high-income and highly educated households in central cities, we focus on families with children and show that there was an increase in the number of families living in the inner city after the implementation of the apartment size regulation.

The rest of this paper is structured as follows. In Section 2 we describe the institutional setting and the implemented apartment size regulation. In Section 3, we present the empirical framework, and in Section 4, we describe the data. We discuss the results regarding the square meter prices of the small apartments and the large apartments in Sections 5 and 6, respectively. In Section 7, we draw our conclusions.

2 Institutional setting

2.1 Residential real estate sales process

Residential real estate is normally sold through ascending-bid auctions in Norway, where an open house showing takes place before the auction. Both the showing and the auction are led by a realtor. The realtor is hired by the seller but obligated by law to protect the interests of both the seller and the buyer. The bids are usually placed on digital platforms, and all bids are legally binding. The realtor informs the participants of developments in the auctions. The auction is completed, whether the seller accepts a bid or rejects all bids.

2.2 Apartment size regulation - Leilighetsnormen

In January 2008, the local government in Oslo implemented a size regulation on the construction of residential apartments, called Leilighetsnormen, in the inner city of Oslo.[1] Prior to this, there existed a regulation on the number of rooms in apartments, while the apartment size regulation set limits for the share of different size segments in a the construction project. The regulation aimed to promote high housing quality, as well as the construction of

[1] The regulation was announced in September 2007 and implemented in January 2008.

a larger variety of apartments in which a range of demographic and social groups could live. Particularly, the regulation was supposed to secure building of large apartments suitable for families with children (Oslo Kommune, 2007). The apartment size regulation was originally applicable to five districts in the inner city and set upper limits on the percent of small apartments that could be built. In the first column of Table 1, we summarize the regulation implemented in 2008, in which the minimum size of apartments allowed to be built was set at 40 square meters. In addition, a maximum share of 20 percent of apartments between 40 and 50 square meters was allowed in each building project, while the shares of apartments between 50 and 80 square meters and above this range were set at a minimum of 30 and 50 percent, respectively.

Table 1 Overview the apartment size regulation - Leilighetsnormen.

	January 2008	September 2013
Inner city:	Nydalen, Gamle Oslo, Sagene, Grünerløkka, St. Hanshaugen	Gamle Oslo, Sagene, Grünerløkka, St. Hanshaugen
Minimum size	40 square meters	35 square meters
Square meter segment 1	40-50: Maximum 20 percent	35-50: Maximum 35 percent
Square meter segment 2	50-80: Minimum 30 percent	80+ : Minimum 40 percent
Square meter segment 3	80+ : Minimum 50 percent	

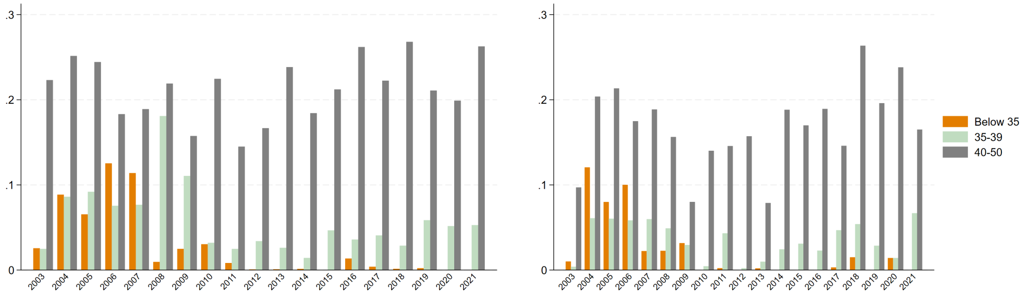
Notes: This table shows an overview of the districts and areas that were covered by size regulation. Oslo is divided into 15 districts. Here we name the regulated districts the inner city and the other districts the outer city.

The apartment size regulation in 2008 was criticized for not matching the demand for residential apartments in the inner city (Oslo Kommune, 2013). The inner city has a large percentage of single households and a high and increasing demand for small apartments. Six years after the original regulation was implemented, it was changed in September 2013 to better match the demand for apartments (see Table 1, second column). This liberalization of the regulation included decreasing the minimum size allowed to be built from 40 to 35 square meters and increasing the share of apartments allowed to be built between 35 and 50 square meters from 20 to 35 per cent. In addition, the local government argued that the apartment size regulation should be practiced in a flexible way, in which exceptions were allowed, for example, due to technical reasons or complications related to the existing housing stock.

2.3 Newly built apartments

The apartment size regulation applies to the construction of new apartments. Due to the slow nature of building processes, it takes time for the impact of this regulation to become visible on the housing stock and consequently on house prices. The building process can involve regulating the lots for residential housing,[2] applying for and receiving a building permit from the local government, preselling a certain required share of the apartments to receive external financing, and, finally, the building process itself. The total process can take several years.[3]

Figure 1 New apartments: Size shares small apartments (2003-2021).



(a) Inner city **(b)** Outer city

Notes: The figure shows the apartments size shares of newly built apartments at the time of sale in Oslo between 2003 and 2021. The inner city is defined as the four districts covered by the regulation (Gamle Oslo, Grünerløkka, St. Hanshaugen, and Sagene), while the outer city is defined as the remaining 11 districts in Oslo.

Figure 1 and Figure 2 display the size shares for new apartments at the time of sale, between 2003 and 2021[4]. In Figure 1 the shares of new apartments below 35 square meters and between 35 and 39 square meters (represented by orange and light green bars) in the

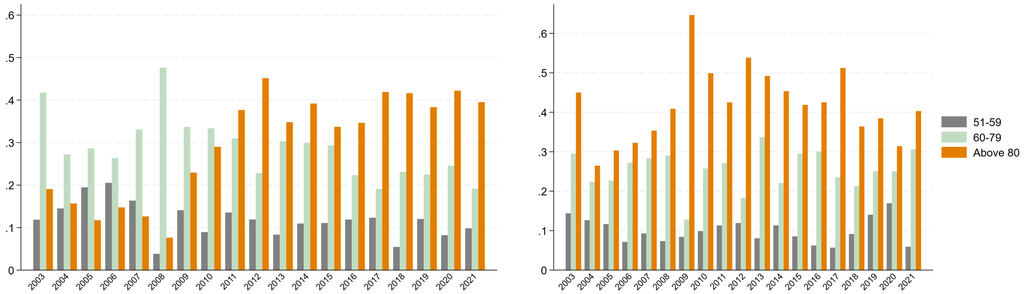
[2]However, several large construction firms have regulated lots available in their portfolios.

[3]Samfunnsøkonomisk Analyse collect data on new development projects at the time of sale and provide a data base over all new apartments in projects of 15 units or more. According to the cited source, it normally takes two to three years from the presale to the completion of apartment buildings in Oslo.

[4]The number of new apartments by the respective size groups are shown in Figure A.1 and Figure A.2 in Appendix A.

inner city ranges from 5 percent in 2003 to between 17 and 20 percent annually in the years leading up to the apartment size regulation introduced in 2008. As a consequence of the regulation, the number of new apartments these size categories plummeted in the inner city, and interestingly also dropped markedly in the outer city. It seems that the municipality has enforced the apartment size regulation outside the regulated districts (Kristiansen, 2023).[5] The average share of apartments that were built in the inner city between 40 and 50 square meters was 21.8 and 21.2 in the pre- and post period of the policy change respectively. Thus, the policy change does not appear to have affected the share of apartments built in this size segment.

Figure 2 New apartments: Size shares large apartments (2003-2021).



(a) Inner city

(b) Outer city

Notes: The figure shows the apartments size shares of newly built apartments at the time of sale in Oslo between 2003 and 2021. The inner city is defined as the four districts covered by the regulation (Gamle Oslo, Grünerløkka, St. Hanshaugen, and Sagene), while the outer city is defined as the remaining 11 districts in Oslo.

Figure 2 illustrates share of new apartments (at time of sale) by size segments above 50 square meters. The size regulation required that, starting in 2008, 50 percent of the apartments built in the inner city had to exceed 80 square meters. This requirement was

[5]In the article, the City Development Commissioner at the time states that the Planning and Building Agency considers 35 square meters to be a minimum requirement for ensuring good living quality. However, she also notes that "developers, can for example, factor in the proportion of high-quality shared space along with the private area per unit. If there are high quality projects or other compelling reasons, it is possible to build units below 35 square meters." We have reached out to the Planning and Building Agency and asked how they have enforced the minimum requirement of 35 square meters outside the regulated area, but have not received a response. Based on the former City Development Commissioner's statement, we interpret that an additional threshold exists for developers seeking approval to build units below 35 square meters also in the unregulated districts.

relaxed to 40 percent in 2013. As shown in Figure 2, this regulation had a strong impact on the number of new apartments above 80 square meters (orange bars) in the regulated inner city area. Unlike the regulation on the smallest apartments, this policy did not appear to affect the size distribution of newly built large apartments in the outer city.

Samfunnsøkonomisk Analyse (2022) investigated how the apartment size regulation affected the housing stocks in the districts in the inner city that were covered by the regulation compared to the other districts in Oslo. In line with the above-mentioned results, the share of the housing stock below 35 square meters declined, while the share of apartments between 40 and 50 square meters increased. This trend started slowly after the original regulation and accelerated after the update of the regulation in 2013.

3 Empirical framework

We examine two possible effects of the apartment size regulation: the impact on the prices of the smallest apartments and the impact on the prices of the largest apartments. First, we examine the effect of the apartment size regulation on prices per square meters on apartments below 35 square meters. The regulation set a construction halt on this size segment throughout the implementation period. The empirical framework is a difference-in-differences approach in which we compare the development in prices in the treated group (apartments below 35 square meters) to a control group before and after the regulations were implemented. As the construction of apartments below 35 square meters was halted in the outer city as well, we cannot use the outer city as our control group. Therefore, we use apartments between 40 and 50 square meters as the control group. We start with a simple difference-in-differences setup where we compare pretreatment to posttreatment as represented in Equation 1.

$$p_i = \alpha_i + \omega P_i^{2008} I_i^{35} + \mathbf{b'X_i} + Y_i^y + Q_i^q + D_i^d + Y_i^y D_i^d + \eta u_d + \rho u_m I_i^{35} + \varepsilon_i \tag{1}$$

The dependent variable p_i denotes the log price per square meter for transaction i. P_i^{2008} is a dummy variable equal to 1 posttreatment. I_i^{35} is a dummy variable equal to 1 if the transaction occurs in the treatment group, that is, below 35 square meters. $P_i^{2008} I_i^{35}$ represents an interaction between the posttreatment and the treatment groups; thus, ω is the coefficient identifying the effect of the treatment. We also include a vector, $\mathbf{X_i}$, consisting of apartment characteristics: apartment size, apartment size squared, and apartment age deciles.[6] The model is run with yearly, Y_t, quarterly, Q_τ and district, D_d, fixed effects, as well as district times yearly fixed effects. We also add the quarterly unemployment rate at the municipality level, u_m, and an interaction term between unemployment and the treated size segment to control for the local business cycle and its potential different effects on the treated size segment versus the control size segment.

We also expand on the analysis and use a dynamic framework to capture the timing and duration of the effects since building processes are slow and the effects naturally come with a time lag. The model is presented in Equation 2.

$$p_i = \alpha_i + \sum_{y=2003, y\neq 2007}^{y=2021} \beta_y I_i^{35} + \mathbf{b}'\mathbf{X_i} + Y_i^y + Q_i^q + D_i^d + Y_i^y D_i^d + \eta u_d + \rho u_d I_i^{35} + \epsilon_i \qquad (2)$$

The dynamic difference-in-differences includes the same control variables and fixed effects as in the static estimation in Equation 1. In the dynamic setup the β_y values show the effect of the interaction between treatment and years. The year prior to the implementation of the regulation, $y = 2007$, is left out. For the results to indicate an effect of the regulation, we should see statistically significant coefficients for the β_y values after the implementation of the regulation, while they should be insignificant in the years prior to the implementation. In all estimations, based on Equations 1 and 2, standard errors are clustered by a three-digit postal code. As the minimum size restriction of 40/35 square meters seems to have been implemented in both the regulated inner city and the non-regulated outer city, we treat the entire city of Oslo as regulated by this size restriction in the static and dynamic setups. We also present the results of the effects in the inner and outer city areas separately.

[6] The age deciles are calculated across all size segments combined, as the regulation may influence the age distribution within each segment.

Second, we investigate whether the increase in the supply of apartments above 80 square meters had an effect on the prices in this size segment. We apply a difference-in-difference framework as above. Apartments above 80 square meters in the regulated areas are set as the treated group. Since the trend in the construction of apartments above 80 square meters in the outer city was unaltered by the regulation, we use apartments above 80 square meters in the unregulated areas as the control group.[7] The model is presented in Equation 3:

$$p_i = \alpha_i + \psi P_i^{2008} I_i^{inner} + \mathbf{b}'\mathbf{X_i} + Y_i^y + Q_i^q + D_i^d + Y_i^y D_i^d + \vartheta_i \tag{3}$$

I^{inner} is a dummy variable equal to 1 if the district was covered by the regulation. The remaining variables are the same as those in Equation 1. Thus, we are interested in ψ, which captures the effect on the price per square meter in the inner city area after the implementation of the regulation.

4 Data and summary statistics

4.1 Transaction data and price trends

The dataset used for analyzing price effects stemming from the size regulation is obtained from Eiendomsverdi. The dataset contains information about residential real estate transactions for second-hand apartments in Oslo over the period 2003-2021 including the sales date, asking and selling prices, apartment size, and other apartment and location characteristics. Table 2 displays summary statistics for both the inner city and the outer city of Oslo.[8]

We can see that selling prices for apartments vary between a little less than NOK 700,000 and NOK 16.19 million in the inner city, while the apartment sizes vary between 18 and 176

[7]There may still be spillover effects of the regulation on the outer city, as these markets are closely interconnected. For instance, demand could shift from the outer city to the inner city. If the regulation leads to increased demand for large apartments in the inner city, it could result in reduced demand in the outer city. In such a scenario, we might overestimate the positive impact of the regulation on inner-city apartment prices.

[8]The data are trimmed by removing units with missing construction year and the oldest buildings (comprising 1 percent) are removed. We trim on the 0.1^{st} and 99.5^{th} percentile on living room area on all observations, and 0.5^{th} and 99.5^{th} percentile per year for price per square meter and price.

Table 2 Summary statistics (transaction data), Oslo (2003– 2021).

Area	Variable	Min.	pct10	pct50	Average	pct90	Max.
Inner city	Sales prices (NOK 1000)	693.4	1,750.0	3,206.2	3,477.7	5,568.2	16,189
	Interior square meter (sqm)	18	35	56	59	85	176
	Square meter price (NOK 1000)	14.5	34.3	58.1	61.0	91.2	156.7
	Number of observations			41,512			
	Share with *size* <= 35 sqm			0.11			
Outer city	Sales prices (NOK 1000)	670.0	1,692.0	3,050.0	3,522.0	6,024.0	17,202
	Interior square meter (sqm)	18	40	67	70	104	177
	Square meter price (NOK 1000)	12.7	27.3	47.5	51.4	82.0	156.3
	Number of observations			50,778			
	Share with *size* <= 35 sqm			0.06			

Notes: pct10 refers to the 10 percent of observations with the lowest value, while pct90 refers to the 10 percent of observations with the highest value. pct50 is the median.

square meters. The median selling price is NOK 3.21 million, while the median square meter price is NOK 58,100. There are 41,512 transactions, and the share of apartments below 35 square meters is 11 percent. In the outer city, there are 50,778 transactions and the share of apartments of below 35 square meters is 6 percent. The median selling and square meter prices are slightly lower than in the inner city, at NOK 3.05 million and NOK 47,500, respectively, while the minimum and maximum values for prices and sizes are quite similar.

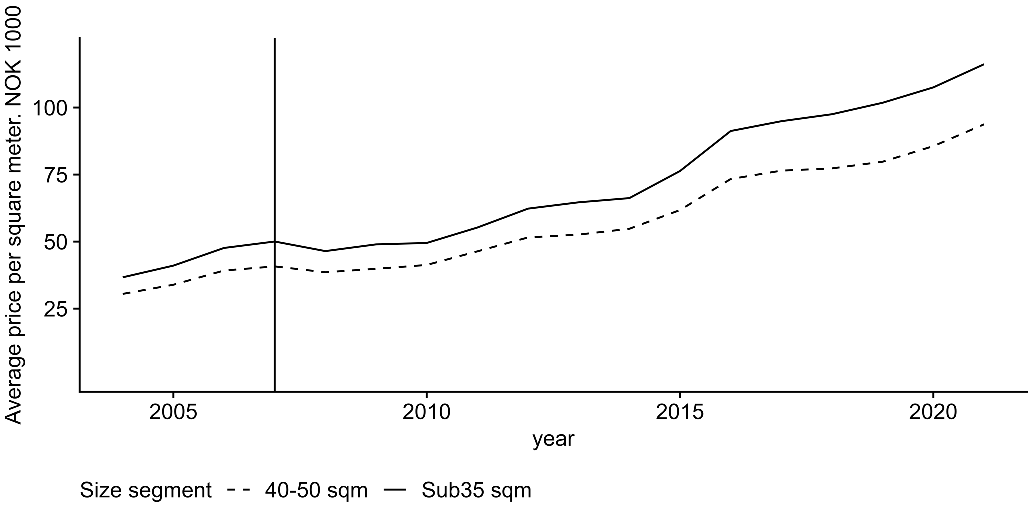
Figure 3 displays the annual average square meter price for apartments below 35 square meters (solid black line) and apartments between 40 and 50 square meters (dashed line) (NOK 1000) between 2004 and 2021 in Oslo. We see that the prices in the smallest size segment are higher than the slightly larger apartments. Furthermore, the prices have increased more for the smallest size segment (below 35 square meters) than for the slightly larger apartments (40-50 square meters) over this time period.

In Figure 4, we show average square meter prices for the inner city and the outer city separately. We can see that square meter prices are both higher and increasing more rapidly for the smallest size segment, in both the inner city and the outer city, in line with the trend for Oslo in total.[9]

Figure 5 illustrates the average prices for apartments between 80 and 100 square meters

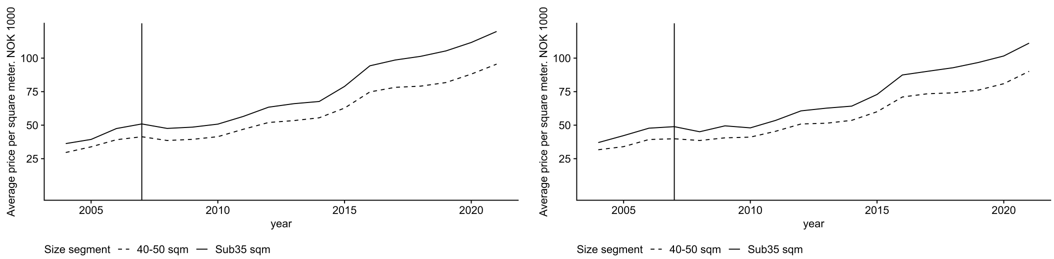
[9]Relative price trends between the treated and control size segments are included in Figure A.3 and Figure A.4 in Appendix A.

Figure 3 Average square meter prices of small apartments in Oslo (2004-2021).



Notes: This figure shows average annual square meter prices for apartments in Oslo (NOK 1000) from 2004 to 2021 for the treatment (< 35 square meters) and control (40-50 square meters) size segments.

Figure 4 Average square meter prices of small apartments in inner city and outer city (2004-2021).

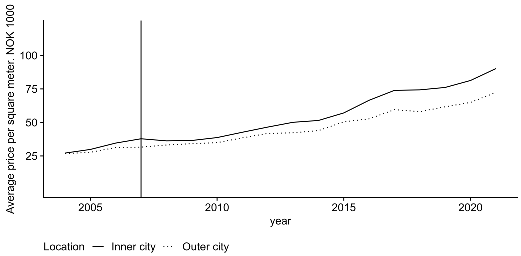


(a) Inner city. NOK 1000

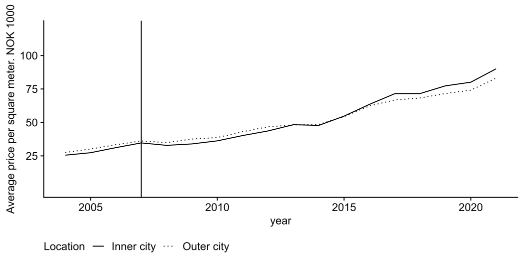
(b) Outer city NOK 1000

Notes: This figure shows average annual square meter prices for apartments in Oslo (NOK 1000) from 2004 to 2021 for the treatment (< 35 square meters) and control (40-50 square meters) size segments. The inner city is defined as the four districts covered by the regulation (Gamle Oslo, Grünerløkka, St. Hanshaugen, and Sagene), while the outer city is defined as the remaining 11 districts in Oslo.

Figure 5 Average square meter prices of large apartments in Oslo (2004-2021).



(a) Apartments 80-100 square meters



(b) Apartments above 100 square meters

Notes: This figure shows average annual square meter prices for large apartments in Oslo (NOK 1000) from 2004 to 2021. The inner city is defined as the four districts covered by the regulation (Gamle Oslo, Grünerløkka, St. Hanshaugen, and Sagene), while the outer city is defined as the remaining 11 districts in Oslo.

in panel a) and above 100 square meters in panel b), in the inner city and the outer city of Oslo. It is evident that square meter prices for apartments between 80 and 100 square meters have been growing at a faster pace in the inner city compared to the outer city, including in the period before the implementation of the apartment size regulation. For apartments above 100 square meters, the price trends look parallel in the preperiod, and the price per square meter has increased slightly more in the inner city compared to the outer city by the end of the period.

4.2 Household balance sheet data

The transaction data are linked to a dataset from the Norwegian tax registry, which includes annual observations on households' balance sheets for the period 2004-2019. The data consist of balance sheet information, including income, debt, and wealth, as well as education, profession, age, gender, and household type. In particular, the dataset shows the number of people, including the number of children, in the household. This allows us to analyze effects on different types of households buying apartments.[10]

Summary statistics for apartment buyers in Oslo from 2004 to 2019 are provided in Table

[10] We trim on the 0.1^{st} and 99.5^{th} percentiles on living room area on all observations and 0.5^{th} and 99.5^{th} percentiles per year for price per square meter and price.

3. We can see that there are 33,251 observations in the inner city (regulated) area and 40,972 observations in the outer city area.[11] The buyers in the inner city are younger, have higher education on average and have a slightly lower household income than their counterparts in the outer city. The shares of single buyers (one-person household) are 35.4 and 31.0 percent in the inner city and outer city respectively, while the shares of families (one or more adults living with children) buying apartments are 16.5 percent in the inner city and 27.2 percent in the outer city. Couples (two adults) buy a shares of 29 percent and 27.2 percent in the inner city and outer city respectively, while multiperson households (three or more adults) buy shares of 19.2 percent and 14.7 percent. Although we use the term *couples*, we do not know the actual status of these households; we only know how many people bought the apartments. In the *couple* category, there might be a single person buying an apartment together with a parent.[12]

Table 3 Summary statistics, apartment buyers in Oslo (2004-2019).

	Inner city		*Outer city*	
	Median	Mean	Median	Mean
Square meter price (NOK 1000)	54.3	57.4	45.6	48.9
Household income (NOK 1000)	640.1	807.9	640.8	842.4
Age (Year)	30	34	34	39
High education (Percent)		73.6		58.6
Single (Percent)		35.4		31.0
Family (Percent)		16.5		27.2
Couple (Percent)		29.0		27.2
Multiperson (Percent)		19.2		14.7
Observations	33,251		40,972	

Notes: This table shows summary statistics for apartment buyers in Oslo from 2004 to 2019. The inner city is defined as the four districts covered by the regulation (Gamle Oslo, Grünerløkka, St. Hanshaugen, and Sagene), while the outer city is defined as the remaining 11 districts in Oslo.

[11]This dataset contains slightly fewer observations than the transaction dataset, primarily due to the household dataset covering a time period that is three years shorter.

[12]We have checked whether co-buying with parents affects our results by excluding households where the age difference between individuals buying an apartment together is more than 20 years. According to our data, the share of households co-buying with parents is just 2 percent and does not affect the main patterns presented here. See Figure B.1 and Figure B.2 in Appendix B. We also show the development in other household characteristics, such as income, age, and education, in Figure B.3.

5 Empirical results: Effect of apartment size regulation of small apartments

In this section, we explore whether prices of apartments below 35 square meters were affected by the implementation of the apartment size regulation in 2008. As shown in Section 2.3, there was a drop in the number of newly built apartments below 35 square meters in the whole city. Therefore, we classify apartments below 35 square meters *in the whole city* as the treatment group and we use apartments between 40 and 50 square meters in the whole city as the control group.

5.1 Price effect on the smallest apartments

Table 4 displays the estimated results on prices of apartments below 35 square meters after the implementation of the regulation. The estimations are done in the difference-in-difference setup displayed in Equation 1. The treatment group comprises apartments below 35 square meters and interacted with postperiod from 2008.[13] Apartments between 40 and 50 square meters comprise the control group. We have conducted a Wald test for parallel pretrends between the treatment and the control groups across size segments. The Wald test results are presented in Table A.4 in Appendix A, and we cannot reject the null hypothesis of parallel pretrends.

The price effect on apartments below 35 square meters is captured by the coefficient on the interaction $P_{2008} * I^{35}$ in the first row of Table 4. The first column presents the results before including any control variables or fixed effects in the specification. The effect of the regulation is then measured to be 1.4 percent but not statistically significant. When we add apartment-specific control variables to the specification, the effect drops to below 1 percent

[13] From 2008 to 2013, the minimum size for apartments was 40 square meters. We have tested whether the period of restrictions on building apartments between 35 and 39 square meters had an effect on these apartments. The estimation results are presented in Table A.3 in Appendix A. The interaction of 35-39 square meters with the time between 2008 and 2013 is statistically insignificant. Hence, we continue with the specification of below 35 square meters throughout the period.

Table 4 Estimation results price effect on apartments below 35 square meters in Oslo.

Dependent Variable:	log(sqmprice)				
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
$P^{2008} * I^{35}$	0.014	0.009	0.033***	0.033***	0.040***
	(0.013)	(0.010)	(0.006)	(0.006)	(0.006)
I^{35}	0.197***	-0.019	-0.019*	-0.027***	-0.033***
	(0.017)	(0.019)	(0.010)	(0.010)	(0.010)
square meter		-0.029***	-0.020***	-0.020***	-0.020***
		(0.003)	(0.003)	(0.003)	(0.002)
square meter^2		0.0002***	0.0001***	0.0001***	0.0001***
		(0.0000)	(0.0000)	(0.0000)	(0.0000)
unemp				-0.001	-0.002*
				(0.001)	(0.001)
I^{35}*unemp				0.002**	0.003***
				(0.001)	(0.001)
Fixed effects					
year	Yes	Yes	Yes	Yes	Yes
age deciles		Yes	Yes	Yes	Yes
quarter		Yes	Yes	Yes	Yes
district			Yes	Yes	Yes
district-year					Yes
Fit statistics					
Observations	20,587	20,587	20,587	20,587	20,587
R^2	0.811	0.867	0.928	0.928	0.931
Within R^2	0.268	0.482	0.517	0.517	0.526

Notes: The table reports the effect of the cap on building apartments below 35 square meters based on a difference-in-difference model (see Equation 1). The treatment group is apartments below 35 square meters, and the control group consists of apartments between 40 and 50 square meters. The time period is 2003-2021. Robust standard errors in parentheses are clustered by a three-digit postal code. Significance levels: * p<.1, ** p<.05, *** p<.01.

Table 5 Estimation results of price effect on apartments below 35 square meters in the inner city and outer city of Oslo.

Dependent Variable:	log(sqmprice)		
	(Oslo)	(Inner city)	(Outer city)
Variables			
$P^{2008} * I^{35}$	0.033***	0.032***	0.036***
	(0.006)	(0.007)	(0.010)
I^{35}	-0.027***	-0.030***	-0.023
	(0.010)	(0.011)	(0.017)
square meter	-0.020***	-0.026***	-0.008**
	(0.003)	(0.003)	(0.004)
square meter^2	0.0001***	0.0002***	0.000
	(0.000)	(0.000)	(0.000)
unemp	-0.001	-0.001	-0.003
	(0.001)	(0.001)	(0.002)
I^{35}*unemp	0.002**	0.0004	0.006***
	(0.001)	(0.001)	(0.002)
Fixed effects			
district	Yes	Yes	Yes
quarter	Yes	Yes	Yes
year	Yes	Yes	Yes
age deciles	Yes	Yes	Yes
Fit statistics			
Observations	20,587	12,283	8,304
R^2	0.928	0.929	0.928
Within R^2	0.517	0.571	0.450

Notes: The table reports the effect of the cap on building apartments below 35 square meters based on a difference-in-difference model (see Equation 1). The treatment group is apartments below 35 square meters, and the control group consists of apartments between 40 and 50 square meters. The time period is 2003-2021. The inner city is defined as the four districts covered by the regulation (Gamle Oslo, Grünerløkka, St. Hanshaugen, and Sagene), while the outer city is defined as the remaining 11 districts in Oslo. Robust standard errors in parentheses are clustered by a three-digit postal code. Significance levels: * p<.1, ** p<.05, *** p<.01.

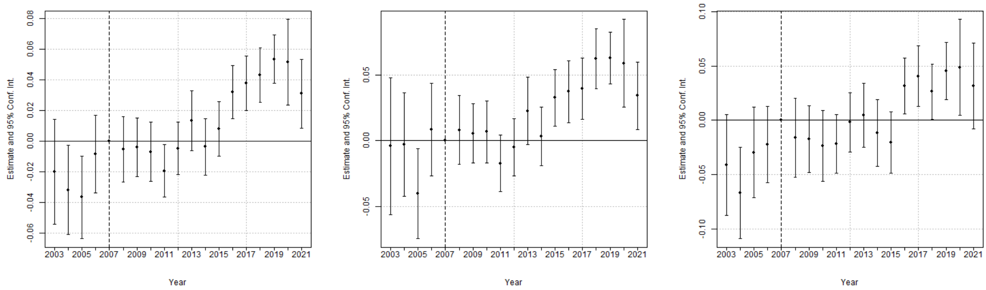
and is still not statistically significant (column 2). When district fixed effects are included (column 3), the effect of the regulation is estimated to be 3.3 percent for the whole of Oslo and is statistically significant at the 1 percent level. This indicates that the price per square meter for the apartments below 35 square meters increased 3.3 percent more than apartments between 40 and 50 square meters on average after the size regulation was implemented in 2008. This estimate remains unchanged when we add the unemployment rate (column 4), both as a separate term and interacted with the treatment variable, to control for how the local business cycle potentially affects the apartments in the treatment group differently than the apartments in the control group. We also add year-by-district fixed effects in column 5 to control for time-varying district-level heterogeneity, resulting in a slight increase in the estimated effect to 4 percent, which remains statistically significant at the 1 percent level.

We also divide the city into the inner city (the districts covered by the regulation) and the outer city (the districts not covered by the regulation) and run the models on the two areas separately. We choose the specification with control variables and district fixed effects (column 4 in Table 4) as our main specification.[14] The results are presented in Table 5. The effects of the regulation are measured to be 3.2 and 3.6 percent in the inner city and the outer city, respectively, and both coefficients are statistically significant at the 1 percent level. The inner city and the outer city also pass the parallel pretrend test when we test the areas separately (see Table A.4 in Appendix A).

The results obtained from the dynamic difference-in-difference model for the same specification, as shown in Table 5, are displayed in Figure 6. The results for Oslo as a whole are presented in panel (a), and those for the inner city and the outer city are presented in panels (b) and (c), respectively. The inner city and the outer city each has one interaction between treated and annual dummies that are statistically significant in the preperiod. It looks as though the relative prices of apartments below 35 square meters increased in 2006 and 2007 prior to the implementation of the regulation. These coefficients might be affected by the run-up to the great financial crisis in 2007-2009. We attempt to control for the business

[14]Detailed results with all specifications from Table 4 for the inner city and the outer city separately are shown in Table A.1 and Table A.2 in Appendix A.

Figure 6 Dynamic difference-in-difference price effect on apartments below 35 square meters.



(a) Oslo **(b)** Inner city **(c)** Outer city

Notes: The figure shows the estimated results of the interaction treated size and annual dummies from Equation 2 for the period 2003-2021. The treatment group comprises apartments below 35 square meters, and the control group consists of apartments between 40 and 50 square meters. The inner city is defined as the four districts covered by the regulation (Gamle Oslo, Grünerløkka, St. Hanshaugen, and Sagene), while the outer city is defined as the remaining 11 districts in Oslo. Standard errors are clustered by three-digit postal code.

cycle by including the local unemployment rate in our estimation, but we might not be able to control for all effects of the financial crisis. However, it is reassuring that the Wald tests for parallel trends in the preperiod do not reject parallel pretrends.[15]

For Oslo as a whole and the outer city, the interaction with treated apartments below 35 square meters and annual dummies are statistically significant from 2016 in the posttreatment period. For the inner city, the interactions are statistically significant from 2015. Since the building processes can take up to several years from the time when the general permission is given to the time when the construction is finished and it takes time before new apartments have an effect on the composition of size segments, it seems reasonable that the price effects of the regulation are visible some time after implementation.

5.2 Sensitivity checks

We conduct several sensitivity checks to test the robustness of our results and whether we can interpret them as causal effects of the apartment size regulation. We test the difference-

[15]The results are presented in Table A.4 in Appendix A. We cannot reject parallel trends in the preperiod at the 5 percent level in Oslo as a whole. The same applies for the inner city and the outer city separately.

in-difference model with placebo size groups and placebo cities.

One hypothesis could be that the increase in prices of small apartments could be caused by a general trend of increasing prices in all size segments, not by the apartment norm. One argument against this hypothesis is that our control group also consists of small apartments, with sizes between 40 and 50 square meters. Nevertheless, higher prices in the larger size segments might force more households to buy smaller apartments, which could lead to increased demand and relatively higher price growth for the smallest size segment.

Furthermore, we perform the analysis with small-sized placebo groups. Because the regulation placed restrictions on different size segments, we need to choose placebo size groups carefully. We test if the prices of apartments below 26 square meters rose more than those of apartments between 26 and 35 square meters. We perform a similar test for apartments between 40 and 44 square meters relative to apartments between 45 and 50 square meters. There are no statistically significant differences between the price increases in these placebo groups. We also test and find no effects in the placebo size segment measuring 60-69 square meters relative to 70-79 square meters. Detailed results are shown in Table A.5 and Figure A.5

We also perform estimations with placebo cities, Bergen and Trondheim, the second and the third largest cities after Oslo. In addition, we do estimations for Bærum and Lillestrøm, cities close to Oslo. Treatment and control groups are set equal to the treatment and control groups in our main specification, where the treatment group comprises apartments below 35 square meters, and the control group consists of apartments between 40 and 50 square meters. There are no significant effects in any of the placebo cities, except in Bergen, where there are measured negative price effects after 2007 on apartments below 35 square meters. However, Bergen does not pass the test for parallel pretrends. (See Table A.7 and Table A.8 for the results of this estimation and of the Wald test, respectively. Dynamic difference-in-difference for placebo cities are displayed in Figure A.6.)

6 Empirical results: Effect of apartment size regulation of large apartments

6.1 Price effect on the largest apartments

We explore whether the introduction of the minimum share of 50 percent of newly built large apartments in 2008, and the modification to 40 percent in 2013, had a price effect on these apartments. As can be seen in Figure 2, this led to a substantial increase in new apartments in this size segment in the regulated area, while the number of large apartments in the outer city was more stable between the preregulation and the postregulation periods. For a given demand, an increase in supply would lead to lower prices. However, a higher share of large apartments may lead to more stable living situations, enhancing residents' connection to the community and fostering a better quality of life. Additionally, for families with children, there may be certain thresholds where an increase in the number of families living in a neighborhood may augment the demand for other families. These two effects may lead to increased demand, which will pull in the direction of relatively higher prices of these apartments. As described in Section 3, we apply a difference-in-difference framework where apartments above 80 square meters in the inner city comprise the treatment group, and the control group consists of apartments in the equivalent size segment in the outer city. The estimation results are presented in Table 6 where the column header denotes the size segments based on which the model is estimated. For the whole size segment, 80+ and 80-99 square meters, we can reject parallel trends in the pretreatment period. For apartments above 100 square meters, we cannot reject parallel trends in the pretreatment period; therefore, we focus on this size segment. We find that apartments above 100 square meters increased 4.4 percent more in price on average in the inner city compared to the outer city after the implementation of the apartment size regulation. For these large apartments, it seems that the effect of the increase in demand surpassed the impact of increased supply.

To explore whether the increased demand stems from family buyers, we investigate

Table 6 Estimation results of price effect on apartments above 80 square meters.

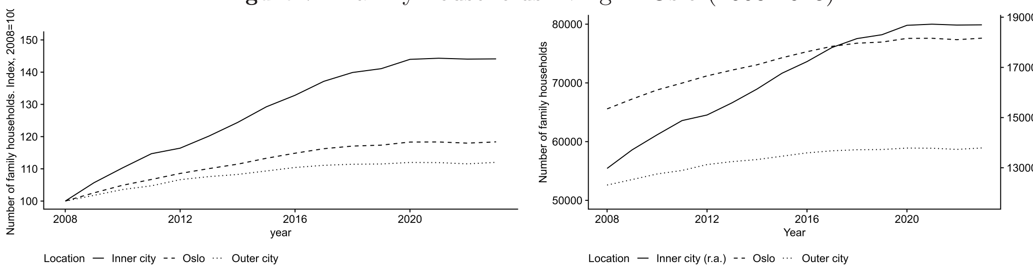
Dependent Variable:	log(sqmprice)		
Size segment:	(80+)	(80-99)	(100+)
Variables			
$P^{2008} * I^{inner}$	0.031***	0.027**	0.044***
	(0.011)	(0.012)	(0.016)
square meter	0.0009	0.002	0.003
	(0.001)	(0.013)	(0.002)
square meter^2	-0.00001**	-0.00002	-0.00002**
	0.000005	0.00008	0.000008
Fixed effects			
year	Yes	Yes	Yes
quarter	Yes	Yes	Yes
district	Yes	Yes	Yes
Fit statistics			
Observations	21,143	13,089	8,054
R^2	0.878	0.891	0.861
Within R^2	0.201	0.197	0.197

Notes: The table reports the effect of increased construction of apartments above 79 square meters, based on a difference in difference framework, see Equation 3. The treatment group comprises apartments in the inner city, regulated by the norm in the size group denoted by the column title and the control group is apartments in the outer city in the same size segment. The inner city is defined as the four districts covered by the regulation (Gamle Oslo, Grünerløkka, St. Hanshaugen, and Sagene), while the outer city is defined as the remaining 11 districts in Oslo. The time period is 2003-2021. Robust standard errors in parenthesis are clustered by a three-digit postal code. Significance levels: * p<.1, ** p<.05, *** p<.01.

whether the number of families *living* in the inner district and number of the *buyers* of the large apartments have increased.

6.2 Families *living* and *buying* apartments in Oslo

Figure 7 Family households living in Oslo (2008-2023).



(a) Index: 2008 = 100

(b) Number of households

Notes: This figure shows an index for family households (households with children who are not adults) living in Oslo and in the inner and outer city. In 2023 the number of family households in Oslo was 77,617 with 18,696 living in the inner city and 58,921 living in the outer city. The total number of households in Oslo was 365,777 with 125,439 living in the inner city and 240,338 living in the outer city. The index base year is 2008. The inner city is defined as the four districts covered by the norm (Gamle Oslo, Grünerløkka, St. Hanshaugen and Sagene), while the outer city is defined as the remaining 11 districts in Oslo.

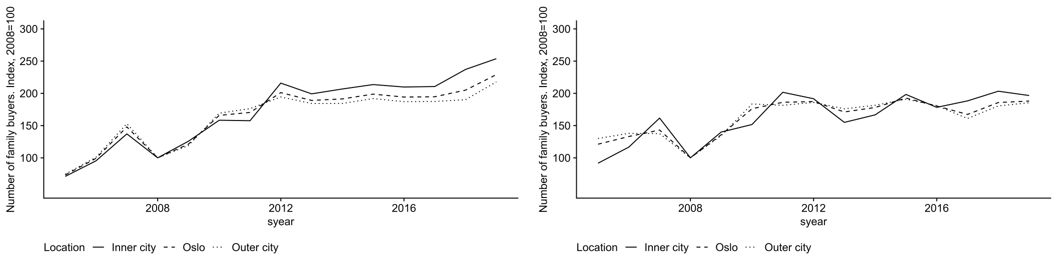
To study whether the goal of an increased number of families *living* in the inner city has been achieved, we start with aggregate data on household types per district in Oslo from 2008 to 2023.[16] The left panel of Figure 7 displays an index of the development in the number of family households in Oslo between 2008 and 2021. As we can observe, there is a notable increase in family households living in the inner city in the studied period. In the right panel of Figure 7, we also show the total number of households in the inner city (right axis), outer city and Oslo (left axis), to emphasise that although growth in the number of families in the inner city has been high over the last years, the level of family households is still low, compared to that of the outer city.

The increase in the number of families in the inner city can partially support the hypothesis that the apartment size regulation has had a positive effect on the share of families

[16]This data is downloaded from the municipality of Oslo's web page: https://www.oslo.kommune.no/statistikk/husholdninger/

living in the area. However, we lack access to the data on household types per district prior to the implementation of the regulation and therefore cannot observe the trend path in the preperiod.

Figure 8 Families buying apartments in Oslo, the inner city, and the outer city (2004– 2019). Index: 2008 = 100.



(a) Apartments between 80 and 100 square meters **(b)** Apartments above 100 square meters

Notes: This figure shows an the number of families with children (households with children who are minors) buying apartments in Oslo from 2004 to 2019. The left panel shows an index with 2008 as base year, while the right panel shows the total number of households. The inner city is defined as the four districts covered by the regulation (Gamle Oslo, Grünerløkka, St. Hanshaugen, and Sagene), while the outer city is defined as the remaining 11 districts in Oslo.

Figure 8 displays an index of the number of family *buyers* of the larger apartment sizes in the period 2004-2019 in Oslo, the inner, and the outer city. As we can see, the number of family buyers has more than doubled for the apartments between 80 and 99 square meters both in the inner and outer city. The increase is slightly larger in the inner city. The number of family buyers of apartments above 100 square meters has also increased substantially in the time period. It shows the number of families buying apartments in the inner city was increasing also before the apartment size regulation came into effect.[17]

In summary, there has been an increase in the number of families living in the inner city of Oslo over the period with the apartment size regulation. There has also been an increase in the number of families buying large apartments in the inner city, both before the regulation was implemented and after. These findings provide support to the hypothesis of

[17]The *share* of families buying the apartments in the larger size segment is presented in Figure B.4 in Appendix B. The share of family buyers is unchanged for apartments between 80 and 99 square meters and slightly increases for apartments above 100 square meters.

increased demand from families with children contributing to the price increase of largest apartments in the inner city. We should, however, be careful concluding that the increase in family households is a causal effect of the apartment size regulation, since the total number of households in the inner city has been increasing over this period, not only family households.

7 Conclusion

In this paper, we use transaction- and household-level data and a difference-in-difference framework to empirically investigate the effect of a Norwegian regulation (limiting the construction of small apartments and promoting that of large apartments) on the prices of apartments in the inner city of Oslo, the capital of Norway.

The regulation, introduced in 2008, mandated a minimum apartment size of 40 square meters, with at least 50 percent of newly built apartments required to exceed 80 square meters. In 2013, these restrictions were relaxed to 35 square meters and 40 percent, respectively. The aim of the regulation was to ensure the construction of larger apartments that could accommodate families with children, among other demographic groups.

We demonstrate a significant decline in newly built apartments below the minimum size requirement following the implementation of the regulation, both in the inner city covered by the regulation and the outer city not covered by the regulation. Furthermore, we find that prices of apartments below the minimum requirement has increased significantly by 3.3 percent after the regulation was implemented compared to apartments between 40 and 50 square meters (the control group).

We continue to show that despite an increase in the number of newly built apartments over 80 square meters, their prices rose in the inner city after the regulation was introduced. We show that the number of families living in the inner city increased after the regulation was implemented, as did the number of families buying apartments. This suggests that the positive price effect could be caused by an increase in demand for large apartments from families with children, in line with the intention of the regulation. However, it is important to interpret these results cautiously, as there has been a general increase in households of all types within the inner city, and the proportion of families remains relatively low compared to the outer city.

With this paper, we contribute to the growing literature investigating the price effects of government regulation of housing supply. The availability of data and the Norwegian policy change in 2008 allow a causal investigation of a particular regulation. Future research on supply-side policy reforms in other countries could provide more information about the external validity of our results.

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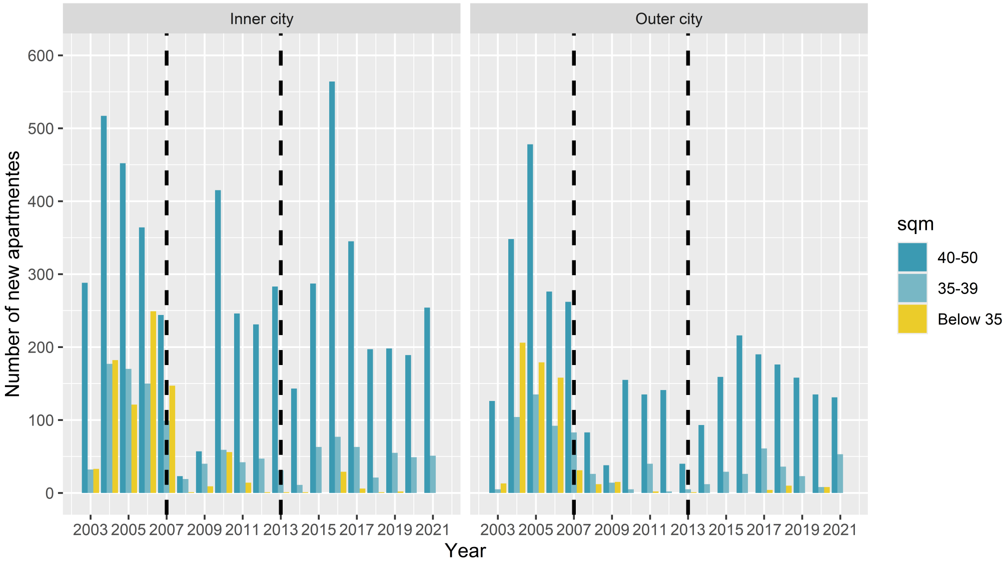
Appendix A: Additional charts and sensitivity price effects

Figure A.1. Number of new apartments by size (2003-2021).



Source: Samfunnsøkonomisk Analyse. **Notes:** The figure shows the number of newly built apartments at the time of sale in Oslo between 2003 and 2021. The inner city is defined as the four districts covered by the regulation (Gamle Oslo, Grünerløkka, St. Hanshaugen, and Sagene), while the outer city is defined as the remaining 11 districts in Oslo.

Table A.1. Estimation results: price effect on apartments below 35 square meters (inner city).

Dependent Variable:	log(sqmprice)				
	(1)	(2)	(3)	(4)	(5)
Variables					
$P^{2008} * I^{35}$	0.030***	0.032***	0.032***	0.032***	0.034***
	(0.011)	(0.007)	(0.007)	(0.007)	(0.007)
I^{35}	0.192***	-0.032***	-0.028***	-0.030***	-0.032***
	(0.011)	(0.011)	(0.010)	(0.011)	(0.012)
square meter		-0.029***	-0.026***	-0.026***	-0.026***
		(0.002)	(0.003)	(0.003)	(0.003)
square meter^2		0.0002***	0.0002***	0.0002***	0.0002***
		(0.000)	(0.000)	(0.000)	(0.000)
unemp				-0.001	-0.002
				(0.001)	(0.001)
I^{35}*unemp				0.0004	0.0008
				(0.001)	(0.001)
Fixed effects					
year	Yes	Yes	Yes	Yes	Yes
age deciles		Yes	Yes	Yes	Yes
quarter		Yes	Yes	Yes	Yes
district			Yes	Yes	Yes
district-year					Yes
Fit statistics					
Observations	12,283	12,283	12,283	12,283	12,283
R^2	0.887	0.916	0.929	0.929	0.930
Within R^2	0.403	0.554	0.571	0.571	0.575

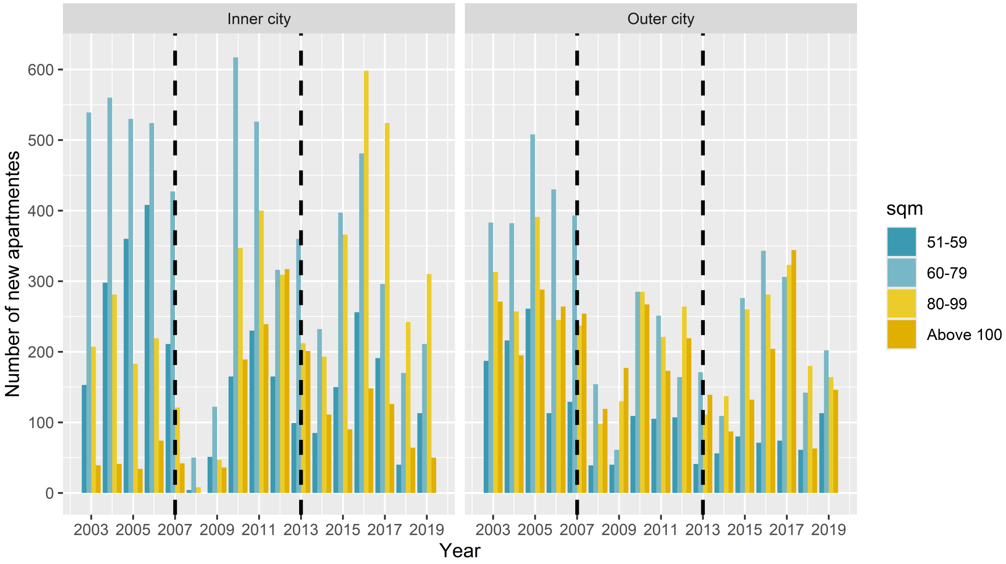
Notes: The table reports the effect of the cap on building apartments below 35 square meters based on a difference-in-difference model (see Equation 1). The treatment group comprises apartments below 35 square meters and the control group is apartments of 40-50 square meters. The time period is 2003-2021. Robust standard errors in parentheses are clustered by a three-digit postal code. Significance levels: * p < .1, ** p < .05, *** p < .01.

Table A.2. Estimation results: price effect on apartments below 35 square meters (outer city).

Dependent Variable:	log(sqmprice)				
	(1)	(2)	(3)	(4)	(5)
Variables					
$P^{2008} * I^{35}$	0.005	0.003	0.035***	0.036***	0.043***
	(0.023)	(0.016)	(0.010)	(0.010)	(0.009)
I^{35}	0.200***	-0.009	-0.002	-0.023	-0.032*
	(0.033)	(0.036)	(0.016)	(0.017)	(0.017)
square meter		-0.020***	-0.008**	-0.008**	-0.009***
		(0.006)	(0.004)	(0.004)	(0.003)
square meter^2		0.000	0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)	(0.000)
unemp				-0.003	-0.003
				(0.002)	(0.002)
I^{35}*unemp				0.006***	0.006***
				(0.002)	(0.002)
Fixed effects					
year	Yes	Yes	Yes	Yes	Yes
age deciles		Yes	Yes	Yes	Yes
quarter		Yes	Yes	Yes	Yes
bid			Yes	Yes	Yes
bid-year					Yes
Fit statistics					
Observations	8,304	8,304	8,304	8,304	8,304
R^2	0.722	0.830	0.928	0.928	0.933
Within R^2	0.190	0.504	0.449	0.450	0.457

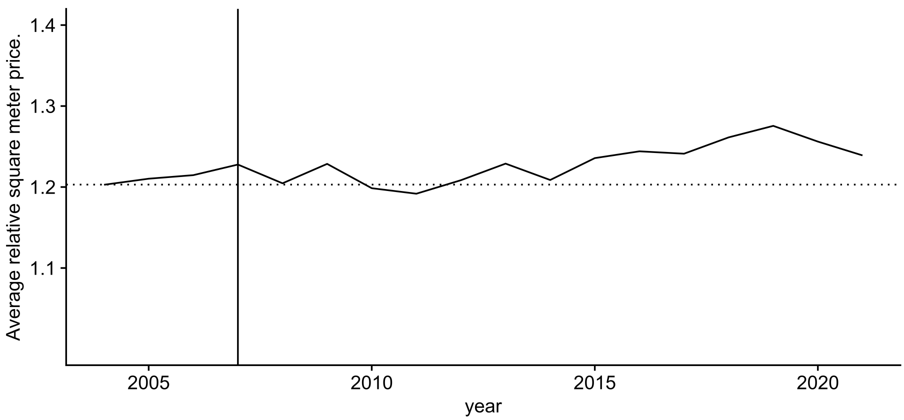
Notes: The table reports the effect of the cap on building apartments below 35 square meters based on a difference-in-difference model (see Equation 1). The treatment group comprises apartments below 35 square meters, and the control group consists of apartments between 40 and 50. The time period is 2003-2021. Robust standard errors in parentheses are clustered by a three-digit postal code. Significance levels: * p < .1, ** p < .05, *** p < .01.

Figure A.2. Number of new apartments by size (2003-2021).



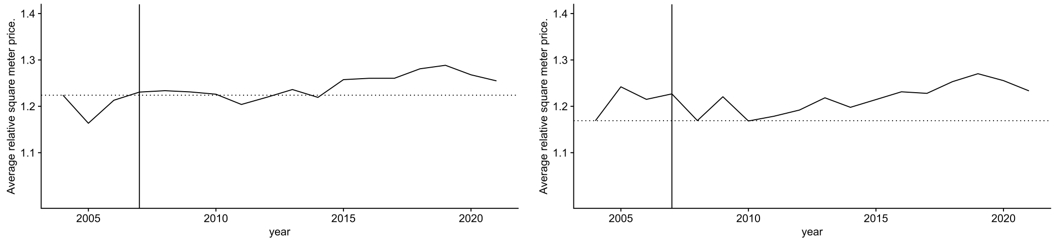
Source: Samfunnsøkonomisk Analyse. **Notes:** The figure shows the number of newly built apartments in Oslo between 2003 and 2021. The inner city is defined as the four districts covered by the regulation (Gamle Oslo, Grünerløkka, St. Hanshaugen, and Sagene), while the outer city is defined as the remaining 11 districts in Oslo.

Figure A.3. Relative square meter prices in Oslo (2004-2021)



Notes: This figure shows relative square meter prices between the treatment (< 35 square meters) and the control (40-50 square meters) size segments for apartments in Oslo from 2004 to 2021. The dotted line shows the starting point for the relative square meter price.

Figure A.4. Relative square meter prices in inner city and outer city (2004-2021).

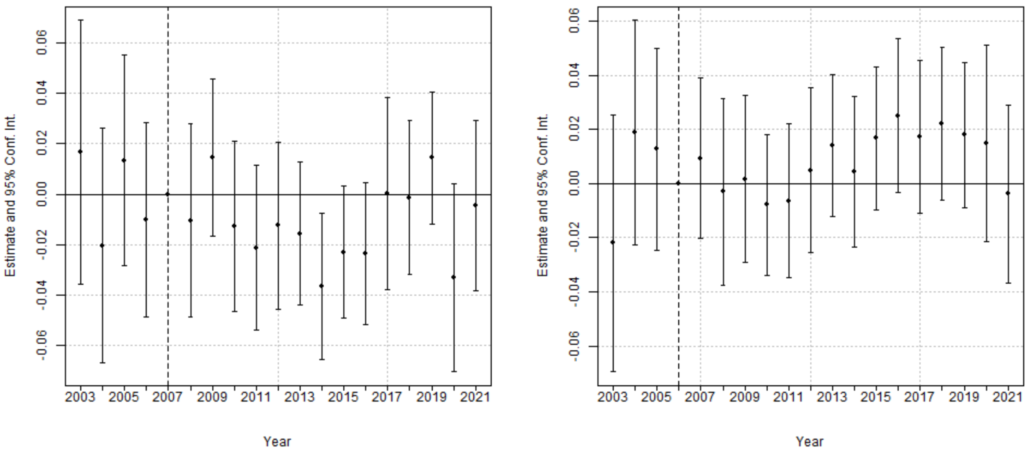


(a) Inner city (NOK 1000)

(b) Outer city (NOK 1000)

Notes: This figure shows relative square meter prices between the treatment (< 35 square meters) and the control (40–50 square meters) size segments for apartments in Oslo from 2004 to 2021. The dotted line shows the starting point for the relative square meter price. The inner city is defined as the four districts covered by the norm (Gamle Oslo, Grünerløkka, St. Hanshaugen, and Sagene), while the outer city is defined as the remaining 11 districts in Oslo.

Figure A.5. Dynamic difference-in-difference price effect on placebo sizes, small apartments in Oslo



(a) Treatment group: below 26 square meters.

(b) Treatment group: 40-44 square meters.

Notes: The figure shows the estimated results of the for the interaction-treated size and annual dummies from Equation 2 for the period 2003-2021. The treatment group is denoted in the caption. In panel (a), the treatment group is set at below 26 square meters, and the control group is set at 26–34. Of the 7365 observations, 1501 of these are 25 square meters and below. In panel (b), the treatment group comprises apartments between 40 and 44 square meters, and the control group consists of apartments between 45 and 50 square meters. Standard errors are clustered by a three-digit postal code.

Table A.3. Estimation results: price effect on small apartments, including interaction-treated apartments between 35 and 39 square meters (Oslo).

Dependent Variable:	log(sqmprice)
	Oslo
Variables	
$P^{2008} * I^{35}$	0.030***
	(0.006)
I^{35}	-0.026***
	(0.010)
$P^{2008-2013} * I^{35-39}$	-0.004
	(0.004)
I^{35-39}	0.001
	(0.005)
square meter	-0.020***
	(0.002)
square meter^2	0.0001***
	(0.000)
unemp	-0.0004
	(0.001)
I^{35}*unemp	0.002**
	(0.001)
Fixed effects	
age deciles	Yes
district	Yes
quarter	Yes
year	Yes
Fit statistics	
Observations	25,223
R^2	0.927
Within R^2	0.479

Notes: The table reports the price effect of the cap on building apartments below 35 square meters from 2008 and the additional cap on building 35–39-square-meter apartments between 2008 and 2012, based on a difference-in-difference model (see Equation 1). The treatment group comprises apartments below 35 square meters from 2008 to 2021 and 35–39-square-meter apartments between 2008 and 2012. The control group consists of 40–50-square-meter apartments. The time period is 2003-2021. Robust standard errors in parentheses are clustered by a three-digit postal code. Significance levels: * p < .1, ** p < .05, *** p < .01.

Table A.4. Results of Wald parallel-trends test.

Model: Apt. below 35 sqm		H0: Linear trends are parallel pre<2008
Model: Oslo	F(1,1)=	14.56
	Prob >	0.16
Model: Inner city	F(1,1)=	5.17
	Prob >	0.26
Model: Outer city	F(1,1)=	63.84
	Prob >	0.08
Model: Apt. above 79 sqm.		H0: Linear trends are parallel pre<2008
Model: 79 sqm+	F(1,1)=	1719
	Prob >	0.015
Model: 80 - 99 sqm	F(1,1)=	563
	Prob >	0.03
Model: 100 + sqm	F(1,1)=	22.3
	Prob >	0.13

Notes: The table reports the statistics of a Wald test of parallel trends prior to treatment. H0 states that linear trends prior to treatment are parallel. The tests are run with the command estat ptrends after didregress in Stata. The test statistics for the models estimating the effect on apartments under 35 square meters correspond to column 4 in Table 4 and Table 5, while the test statistics for apartments above 79 square meters correspond to Table 6.

Table A.5. Estimation results: placebo sizes.

Dependent Variable:	log(square meter price)		
	(1) T: sqm < 25	(2) T: sqm=40-44	(3) T: sqm= 60-69
Variables			
I^{25}	0.022*		
	(0.013)		
$P^{2008} * I^{25}$	-0.013		
	(0.010)		
I^{40-44}		-0.014	
		(0.010)	
$P^{2008} * I^{40-44}$		0.004	
		(0.007)	
I^{60-69}			0.027***
			(0.009)
$P^{2008} * I^{60-69}$			-0.002
			(0.006)
square meter	-0.011	-0.021	-0.023**
	(0.009)	(0.018)	(0.011)
square meter ^{2}	0.000	0.0001	0.0002**
	(0.0002)	(0.0002)	(0.000)
unemp	-0.0008	-0.0003	-0.006***
	(0.002)	(0.002)	(0.002)
I^{size} *unemp	0.001	-0.0004	-0.0009
	(0.002)	(0.001)	(0.001)
Fixed effects			
age deciles	Yes	Yes	Yes
district	Yes	Yes	Yes
quarter	Yes	Yes	Yes
year	Yes	Yes	Yes
Fit statistics			
Observations	7,365	13,222	30,249
R^2	0.939	0.920	0.884
Within R^2	0.307	0.230	0.193

Notes: The table reports results from Equation 1 for placebo size groups. In the first model, the treatment group is set to below 26 square meters, and the control group is set at 26–34 square meters. Of 7365 observations, 1501 are 25 square meters and below. In model (2), the treatment group comprises 40–44-square-meter apartments, and the control group consists of 45–50-square-meter apartments. In model (3), the treatment group consists of 60–69-square-meter apartments, and the control group comprises 70–79-square-meter apartments. Robust standard errors in parentheses are clustered by a three-digit postal code. Significance levels: * p < .1, ** p < .05, *** p < .01.

Table A.6. Results of Wald parallel-trends test, placebo sizes.

Model: T = below 26 square meters	F(1,1) =	45.2
	Prob >	0.09
Model: T = 40-44 square meters	F(1,1) =	15.4
	Prob >	0.16
Model: T = 60-69 square meters	F(1,1) =	53.2
	Prob >	0.09

Notes: The table reports the statistics of a Wald test of parallel trends prior to treatment. H0 states that linear trends prior to treatment are parallel. The tests are run with the command estat ptrends after didregress in Stata.

Table A.7. Estimation results: placebo cities. Price effect on apartments below 35 square meters

Dependent Variable:	log(sqmprice)			
	(Bergen)	(Trondheim)	(Bærum)	(Lillestrøm)
Variables				
$P^{2008} * I^{35}$	-0.027**	-0.014	0.035	0.004
	(0.010)	(0.015)	(0.025)	(0.044)
I^{35}	-0.004	0.017	-0.028	0.063
	(0.021)	(0.022)	(0.070)	(0.041)
square meter	-0.012***	-0.012	-0.024	-0.0002
	(0.004)	(0.008)	(0.016)	(0.003)
square meter^2	0.000	0.000	0.0002	0.000
	(0.000)	(0.000)	(0.0002)	(0.000)
unemp	-0.011***	0.002	0.002	
	(0.004)	(0.006)	(0.005)	
I^{35}*unemp	0.006*	-0.002	0.019	
	(0.003)	(0.004)	(0.009)	
Fixed effects				
age deciles	Yes	Yes	Yes	Yes
district	Yes	Yes	Yes	Yes
quarter	Yes	Yes	Yes	Yes
year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	4,730	4,410	1,070	998
R^2	0.837	0.845	0.952	0.928
Within R^2	0.424	0.455	0.378	0.434

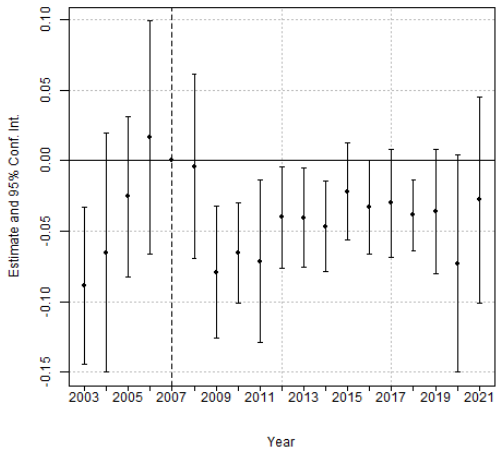
Notes: The table reports results from estimations using Equation 1 in placebo cities Bergen, Trondheim, Bærum, and Lillestrøm. Treatment group = 35 square meters, control group = 40–50 square meters. For Bergen, H0 of parallel trends prior to treatment can be rejected at the 5 percent level. Unemployment rates are not available for Lillestrøm since the municipality was established in 2020. The time period is 2003-2021. Robust standard errors in parentheses are clustered by a three-digit postal code. Significance levels: * p < .1, ** p < .05, *** p < .01.

Table A.8. Results of Wald parallel-trends test: placebo cities.

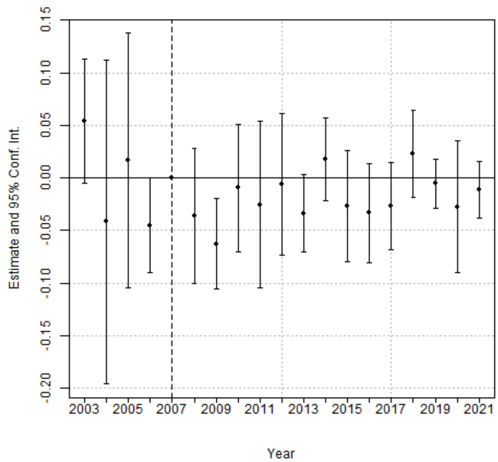
Mod. placebo Bergen	F(1,1)=	11120.9
	Prob >	0.00
Mod placebo Trondheim	F(1,1)=	62.0
	Prob >	0.08
Mod placebo Bærum	F(1,1)=	198.4
	Prob >	0.05
Mod placebo Lillestrøm	F(1,1)=	135423.5
	Prob >	0.00

Notes: The table reports the statistics of a Wald test of parallel trends prior to treatment. H0 states that linear trends prior to treatment are parallel. The tests are run with the command estat ptrends after didregress in Stata.

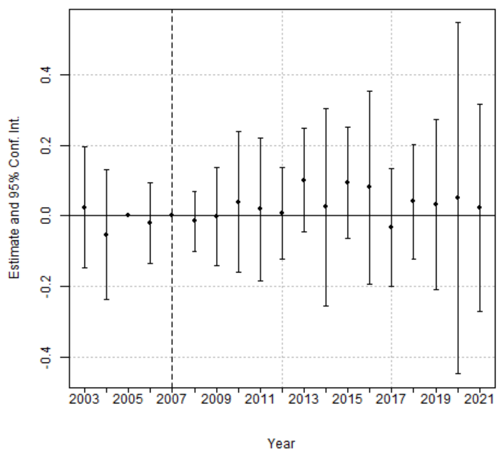
Figure A.6. Dynamic difference-in-difference price effect on placebo cities.



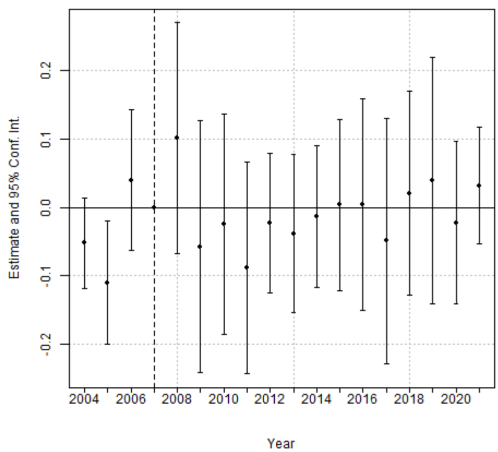
(a) Bergen



(b) Trondheim



(c) Bærum

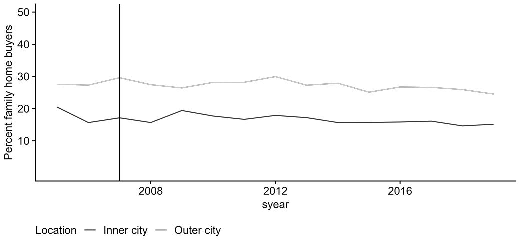


(d) Lillestrøm

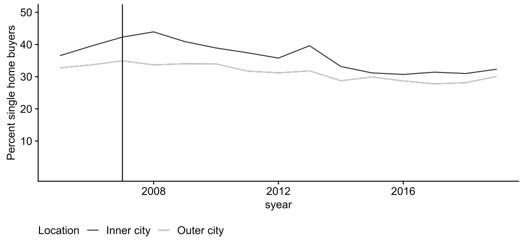
Notes: The figure shows the estimated results for the interaction-treated size and annual dummies from Equation 2 for the period 2003-2021 for placebo cities Bergen, Trondheim, Bærum, and Lillestrøm. The placebo city is denoted in the caption. Standard errors are clustered by a three-digit postal code.

Appendix B: Additional charts apartment buyers

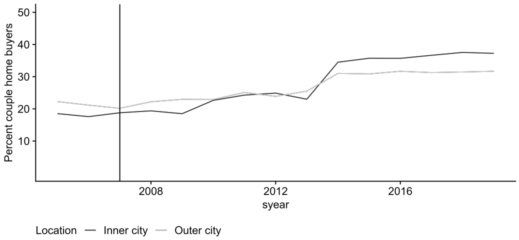
Figure B.1. Apartment buyers in Oslo (2004-2019). Type of household.



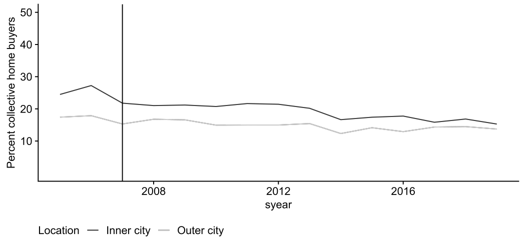
(a) Percent of family buyers



(b) Percent of single buyers



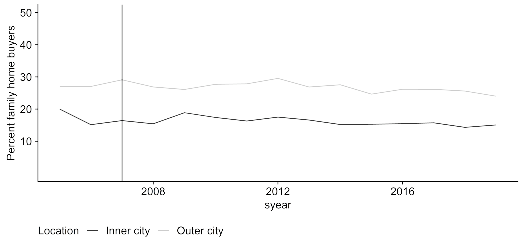
(c) Percent of couple buyers



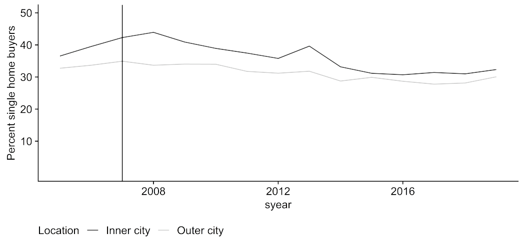
(d) Percent of multiperson buyers

Notes: The figure shows the annual percentages of apartments bought by families, single households, multiperson households, and couples without children in Oslo from 2004 to 2019. The inner city is defined as the four districts covered by the regulation (Gamle Oslo, Grünerløkka, St. Hanshaugen, and Sagene), while the outer city is defined as the remaining 11 districts in Oslo.

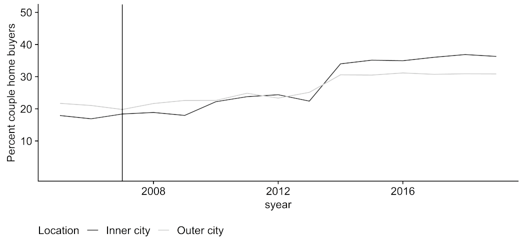
Figure B.2. Apartment buyers in Oslo (2004-2019) excluding households co-buying with parents.



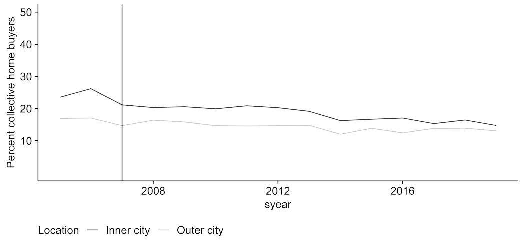
(a) Percent of family buyers



(b) Percent of single buyers



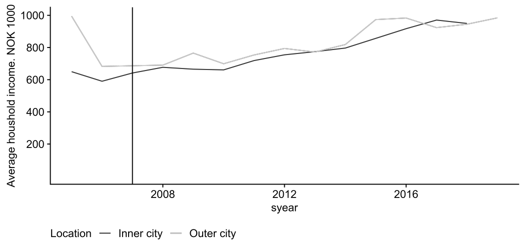
(c) Percent of couple buyers



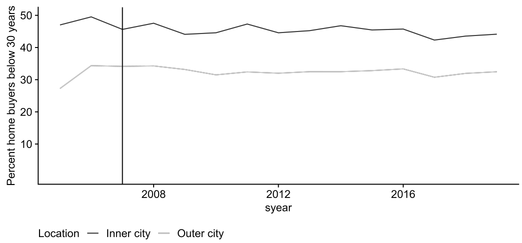
(d) Percent of multiperson buyers

Notes: The figure shows the annual percentages of apartments that are bought by families, single households, multiperson households, and couples without children in Oslo from 2004 to 2019. Households in which the age difference between the buyers is more than 20 years are assumed to be households buying together with a parent and are excluded from the dataset. The inner city is defined as the four districts covered by the regulation (Gamle Oslo, Grünerløkka, St. Hanshaugen, and Sagene), while the outer city is defined as the remaining 11 districts in Oslo.

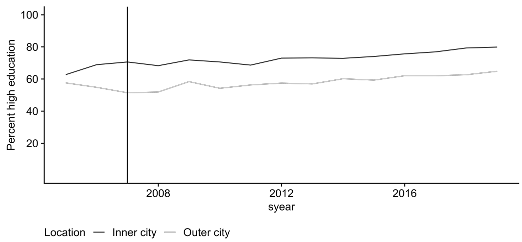
Figure B.3. Apartment buyers in Oslo (2004-2019), income, education and age.



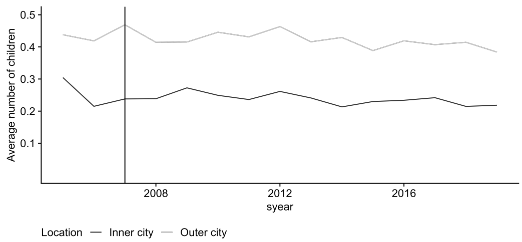
(a) Household income. NOK 1000.



(b) Percent of households below 30 years



(c) Percent of households with high education



(d) Average number of children

Notes: The inner city is defined as the four districts covered by the norm (Gamle Oslo, Grünerløkka, St. Hanshaugen, and Sagene), while the outer city is defined as the remaining 11 districts in Oslo.

Figure B.4. Percent of transactions bought by families (2004-2019).

(a) Apartments between 80 and 100 square meters

(b) Apartments over 100 square meters

Notes: This figure shows the shares of transactions made by families with children (households with children who were minors) in Oslo from 2004 to 2019. The inner city is defined as the four districts covered by the regulation (Gamle Oslo, Grünerløkka, St. Hanshaugen, and Sagene), while the outer city is defined as the remaining 11 districts in Oslo.

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Foto: Norges Bank

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Jeanette Fjære-Lindkjenn was born in Oslo, Norway in 1988. She holds a Master in Economics and Econometrics obtained from the University of Oslo, Norway in 2014.

The thesis consists of an introduction and four independent papers. The papers investigate the effects of specific regulations and policies in the Norwegian housing market. While grounded in the Norwegian context, the findings aim to contribute to the international literature on housing economics.

Paper I analyzes the 2017 introduction of a debt-to-income limit for residential mortgages in Norway. Using a combination of house transaction and tax data, the study finds that the regulation had a modest impact on household balance sheets. However, affected households experienced higher interest rates post-regulation.

Paper II (joint with Knut Are Aastveit, Ragnar Juelsrud, Markus Karlman, Karin Kinnerud, and Ella Getz Wold) reviews existing literature on mortgage regulations, highlighting their effectiveness in curbing household debt growth and house price inflation. Nonetheless, these tools entail significant costs, including reduced consumption smoothing and lower homeownership rates.

Paper III (joint with Nini Barth) explores house price dynamics across 14 Norwegian municipalities using a cointegrated vector error correction model (VECM). The analysis reveals substantial variation in the house price response to changes in user costs and indicates that larger deviations from model-implied prices increase the probability of housing market peaks.

Paper IV (joint with Nini Barth) examines a supply-side regulation that set a minimum size of 35 square meters for apartments in Oslo's inner city. The regulation sharply reduced the construction of smaller apartments, driving up their prices significantly.

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