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Macroprudential Policies and Housing Prices in Oslo

The effect of loan-to-value and debt-to-income regulations on the Oslo housing market

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Abstract

As a result of the severe consequences of the financial crisis in 2008, together with a recent rise in housing prices and household debt, Norway has imposed several macroprudential policies to limit the buildup of financial imbalances. The effect of such policies is yet to be understood sufficiently. This thesis examines the effect of the macroprudential policies imposed in Norway. Specifically, we investigate how loan-to-value regulations and debt-to-income regulations have influenced the selling price of dwellings in Oslo. Using high-resolution data on housing transactions in Oslo, we employ hedonic regression and regression discontinuity designs to uncover the effect of the regulations. We find that the loan-to-value regulation caused the selling price of dwellings to fall three weeks before the implementation of the policy. However, the effect is short lived, as the price development continued upwards only three months after the implementation. Further, we find that the debt-to-income regulation had a slower effect on the housing prices, causing the price development to fall three months after the policy was implemented. Moreover, using supplemental time series regression, we find that the debt-to-income policy may have had a more long-term effect on the housing market than the loan-to-value policy.

Preface

We would like to extend our deepest gratitude to Bjørnar Karlsen Kivedal and Andreas Eidspjeld Eriksen at Housing Lab, Oslo Metropolitan University, for their valuable guidance and supervision. This thesis would not have been realized without your profound knowledge and sincere input. Further, we would like to thank Andreas Jensen at Eiendomsverdi AS for supplying the data used in this study. We hope that you find this study informative, and that the findings and conclusions can facilitate better knowledge concerning macroprudential policies in Norway. Finally, we would like to thank family and friends for continuous support during this new and challenging process.

Contents

1 Introduction

The housing market is an integral part of a country's economy. Understanding how the housing market works and what causes the price of a dwelling to rise or fall is crucial to ensure a stable real economy and limiting financial imbalances. Historically, housing prices tend to experience a significant growth prior to financial crisis (Reinhart & Rogoff, 2008). The financial crisis in 2008 demonstrated how an extensive shock in the housing market could disrupt the global economy. The crisis originated from falling U.S. housing prices, which further led to higher mortgage default levels (Reinhart & Rogoff, 2008). The critical situation also highlighted that monetary policies which focused primarily on price stability did not necessarily ensure financial stability (Armstrong, Skilling, & Yao, 2019) and that there is a need for policies that go beyond the micro-based approach to financial supervision (Galati & Moessner, 2013). In addition to the severe consequences of the financial crisis, the housing market in several countries has, for the past years, experienced significant growth (OECD, 2021). Continually falling real interest rates have contributed to a steady price appreciation in housing prices (Turk-Ariss, 2015), while debt-to-income ratios among households in Norway have reached new heights (Finanstilsynet, 2017, 2020). As a result, together with the well-known consequences of the financial crisis, several countries have implemented different macroprudential policies to safeguard the economy from financial imbalances[1].

In July 2015, the Financial Supervisory Authority of Norway imposed several lending policies for responsible mortgage lending practices. The guidelines stated that banks could not issue mortgages that exceeded 85 percent of the dwelling's market value. In 2017, an additional regulation was imposed, stating that mortgages can not exceed five times the borrower's income (Boliglånsforskriften, 2016). These two policies are formally referred to as loan-to-value (LTV) and debt-to-income (DTI) policies. The effectiveness of these two policies has received significant attention in the media and in academic research. The policy debate is mainly focused on the implementation and effectiveness of such macroprudential policies as a tool to maintain financial stability and avoid macroeconomic imbalances to occur (Galati & Moessner, 2013).

[1] Examples of countries that have implemented various macroprudential polices are China, India, Singapore, Sweden, Poland, Greece, and Norway (Crowe, Dell'Ariccia, Igan, & Rabanal, 2013).

Recently, there has been an upsurge in academic research focusing on macroprudential policies. However, the findings are somewhat conflicting. Igan and Kang (2011) found that both DTI and LTV policies significantly impacted the Korean housing market, causing the housing prices to fall within a six-month period following the implementation. Wilhelmsson (2019), on the other hand, found no statistically significant effect of an LTV policy on the Swedish housing market. In addition, empirical findings suggest that macroprudential policies tend to influence various segments and geographical areas in the housing market differently (e.g., Armstrong et al., 2019; Laufer & Tzur-Ilan, 2021). Hence, the true causal effect of these policies is far from being adequately understood empirically (Galati & Moessner, 2013). Not only is it too early to tell what the long-term effect of the recently imposed macroprudential policies will be, but empirical work can be difficult due to the lack of established models of the interaction between the macroeconomy and the financial systems (Galati & Moessner, 2013).

In this paper, we will provide a better understanding of the causal effects of some of the macroprudential policies imposed in Norway. Specifically, we will examine how the LTV policy imposed in 2015 and the DTI policy imposed in 2017 have influenced the housing prices in Oslo. To our knowledge, no other studies have attempted to specifically uncover the effect of these policies on the price development in the Oslo housing market. For the past ten years, the Oslo housing market has experienced substantial price appreciation compared to the rest of Norway. With separate and stricter macroprudential policies in Oslo compared to the rest of the country, the price development in Oslo housing market still seems strong. Hence, in this paper we will attempt to uncover if the implemented policies have influenced the housing prices and examine if one of the policies has had a more substantial effect. Specifically, in this article, we ask: How has the LTV policy and the DTI policy influenced the selling price of dwellings in Oslo? The answer to this problem statement is an essential piece of the rapidly expanding literature on macroprudential policies. It will help clarify how the policies have influenced the housing prices in Oslo and provide a better basis of which to make future political decisions regarding the topic.

To answer this question, we will employ regression discontinuity designs using high-resolution data on housing transactions in Oslo for the period 2010-2020. We will examine if the housing prices have fallen shortly after the dates of implementation of the two policies, controlling for other effects. Furthermore, we will construct a price index for the housing market using hedonic regression and apply regression discontinuity design using the contructed index. We will also perform a time series regression with the index to uncover any long-term effects.

We find that the LTV policy imposed on July 1, 2015, had a short-term impact on housing prices. Specifically, the housing prices fell three weeks before implementation but continued to rise within three months after the date of implementation. The DTI policy imposed in January 1, 2017, on the other hand, had a more long-term effect on housing prices. The policy slowed down the price appreciation, causing a weaker price development. This effect, however, was not seen before three months after the policy implementation. The findings also indicate that there might have been a shift the buyers' preferences following the LTV policy implementation.

The rest of this paper is structured as follows. Section 2 provides some background on the macroprudential policies imposed in Norway and how the housing market has evolved over the past decade. In addition, the section presents previous findings from other studies as well as the methodological framework used to arrive at the mentioned conclusions. Section 3 presents the data used and how the data has been prepared for the analyses. Section 4 explains the econometric procedures, whereas Section 5 presents the results from the analyses and reviews the findings. The final section summarizes the findings and suggests alternative approaches to future studies.

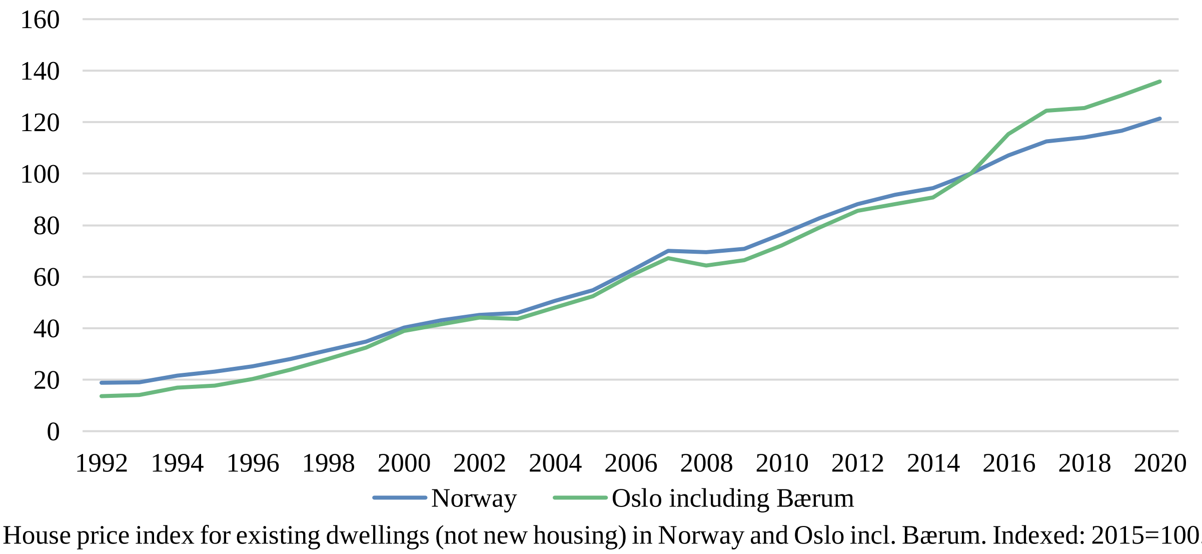
2 Background and theoretical framework

2.1 The Norwegian housing market

Since the second world war, the Norwegian government has implemented a particular strategy for the housing market. The strategy, also referred to as the Norwegian housing policy, advocates that households and individuals should be able to possess their own home, financed by their working income (Sørvoll, 2011). To accomplish this goal, the government has implemented various measures, such as building more affordable housing, allowing interest deduction on installments, and lowering real estate taxes. The Norwegian housing policy intends to help everyone attain affordable housing and is recognized as a quite successful policy. In 1960, the share of homeowners was 64 percent. By the early 1990s, the share of homeowners had increased to around 80 percent (Gulbrandsen, 2004; Sandlie & Gulbrandsen, 2017).

Even though the Norwegian housing policy benefits individuals and families who wish to own a home, the policy does contribute to a higher demand in the housing market. Furthermore, low interest rates following the financial crisis in 2008 have contributed to strong inflation in the housing market. The housing prices in Norway have for the past 10 years increased by around 67 percent, whereas housing prices in Oslo have increased by more than 100 percent (Eiendom Norge, 1/2021). Especially smaller dwellings have experienced extensive price growth[2]. The price for smaller dwellings in Oslo has appreciated by close to 150 percent during the last 10 years alone[3]. This price growth far exceeds the increase in household income and general inflation. As a result, entering the housing market financed by household income alone has become more challenging (Eiendomsverdi, 2019). Figure 1 below displays the development in the housing price index for existing dwellings from 1992 to 2020. From the graph, we can see that the Norwegian housing market and the Oslo housing market show similar growth. However, in 2015, the price index for Oslo accelerates upwards, leaving a notable gap between the Norwegian housing market and the Oslo housing market.

Figure 1: Price index for existing dwellings



House price index for existing dwellings (not new housing) in Norway and Oslo incl. Bærum. Indexed: 2015=100. Bærum is a neighboring county to Oslo and is often included when developing price indexes for the Oslo housing market. Numbers from Statistics Norway: (Statistics Norway, 2021b).

[2] Dwellings below 50 square meters are referred to as small dwellings, based on Table 5.
[3] The statistic is based on the data obtained from Eiendomsverdi AS.

In addition to the rising housing prices, the growth in household debt has been stronger than the income growth, resulting in record-high debt to income ratios (Finanstilsynet, 2017, 2020). According to an annual mortgage survey conducted by the Financial Supervisory Authority of Norway (FSA), the number of new loans which exceeds 5 times income fell substantially the following year after the DTI regulation implementation in 2017 (Finanstilsynet, 2020). However, the total DTI ratio for instalment loans has since picked up and was 20 percent higher in 2020 than in 2016 (Finanstilsynet, 2020). For the past 10 years alone, the average household debt in Norway has increased by more than 54 percent (Statistics Norway, 2021a). Figure 2 below shows the development in household debt for the past 17 years.

Figure 2[4]: Average household debt per income group

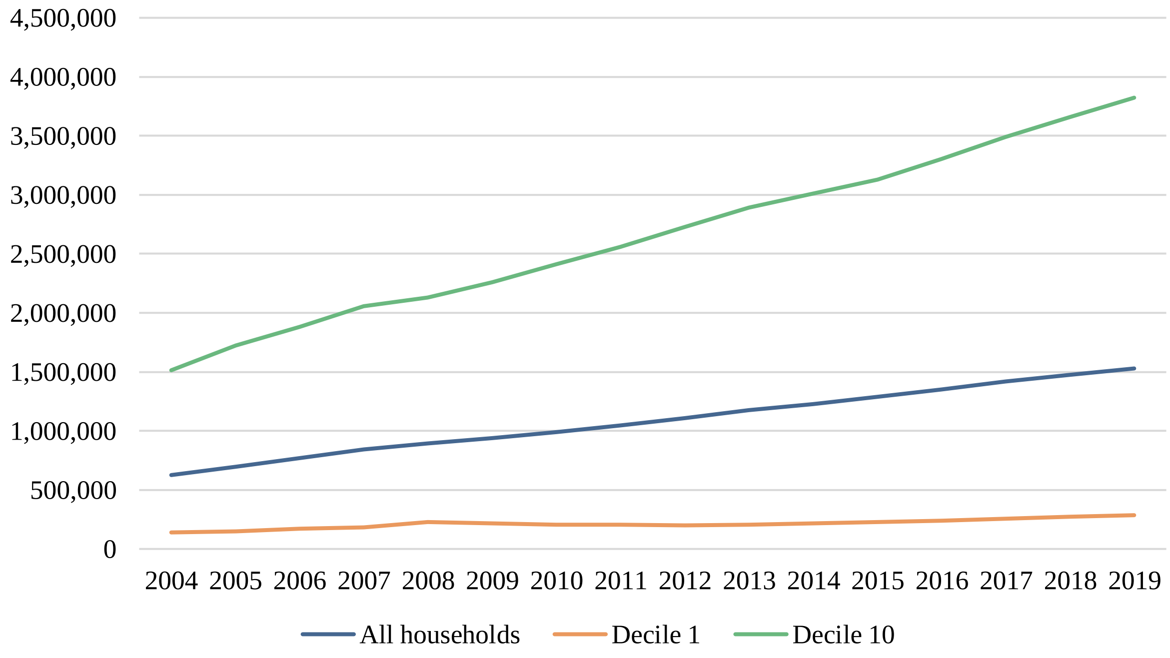


Figure 2 shows the average debt by income group for households in Norway. Decile 1 represents the lowest income group for households and Decile 10 represents the highest income group for households. Numbers are from Statistics Norway: (Statistics Norway, 2021a)

2.1.1 Macroprudential policies

The financial crisis in 2008 made it clear that a major shock in the housing market can greatly impact the international economy and create financial instability. In the wake of the crisis,

[4] All monetary values expressed in this paper are in Norwegian Krone (NOK).

several countries implemented different regulations to reduce the spiral effect of increasing housing prices and household debt. In addition to the increased attention for macroprudential policies caused by the financial crisis, other economic factors have also led to the need for stricter policies. Long-term real interest rates have consistently fallen over the past 30 years (Rachel & Smith, 2018), contributing to rising housing prices (Turk-Ariss, 2015). Macroprudential policies are intended to keep the mentioned trends under control. Specifically, the aim of such policies is “limiting the build-up of financial imbalances by moderating asset price and credit cycles.” (Armstrong et al., 2019, p. 88). Hence, such policies can be crucial to safeguard the financial markets and the real economy. Frequently used macroprudential policies are loan-to-value (LTV) restrictions and debt-to-income (DTI) restrictions. These two restrictions limit the maximum value of the mortgage compared to the value of the house (LTV) or to the buyer’s gross income (DTI). Other regulations such as amortization requirements and higher capital requirements are also frequently used (Crowe et al., 2013; Farelius & Billborn, 2016).

As a result of the increased risk for financial instability, in March 2010, FSA implemented a set of guidelines for responsible mortgage and loan practices. The guidelines recommended that mortgages should normally not exceed 90 percent of the market value of the house (Finansdepartementet, 2021b)This type of LTV policy is also widely used in other advanced economies (Crowe et al., 2013). To guarantee that the banks complied with the new guidelines, FSA further stated that violation of the guidelines could result in higher capital requirements for the banks. FSA’s objective was to improve the banks’ solidity and achieve better financial stability in the Norwegian economy. The LTV regulation was further tightened in December 2011, reducing the percentage from 90 percent to 85 percent (Finanstilsynet, 2011). Other regulations were also imposed and tightened, such as a stricter LTV limit on line of credit, and new regulations regarding amortization requirements. In addition, the banks had to take into account a potential five percentage point increase in the interest rate when assessing the borrower’s ability to repay the mortgage (Finanstilsynet, 2011).

Even though the new guidelines imposed in 2011 were strict and comprehensive, the banks still had the opportunity to deviate from the guidelines. Specifically, the guidelines stated that deviation from the LTV limits could be justified by additional collateral or a special risk assessment (Finanstilsynet, 2011). To further limit the banks' ability to deviate from the guidelines, the guidelines were formalized into official requirements in July 2015. These new, official requirements are referred to as Residential Mortgage Lending Regulations. As before, the new regulations included an LTV limit of 85 percent. However, the banks' flexibility was now tightened, and their ability to deviate from the regulations was now specified in a flexibility quota (Aastveit, Juelsrud, & Wold, 2020).

FSA further tightened the regulation in December 2016, adjusting the flexibility quota and imposing a DTI regulation. Specifically, banks could not grant loans if the customer's total loan value exceeded five times gross income, and only 10 percent of loans could deviate from the regulation. In Oslo, only 8 percent of loans could deviate from the regulation. In addition, the new regulation included an LTV limit for secondary housing in Oslo (Boliglånsforskriften, 2016). Although the new regulation was introduced in December 2016, it did not go into effect before January 2017. This mortgage lending regulation is still in effect today. Even though the same regulations for mortgage lending practices applies today, FSA has adjusted the regulation several times. In January 2021, a new regulation was imposed, replacing the old one. The 2021 regulation includes the same mortgage regulations as before, but now also regulates consumer loans (Finanstilsynet, 2021). The different mortgage regulations and their respective dates of implementation are summarized in Table 1. In this paper, we are focusing on the effect of the LTV regulation imposed in July 2015, and the DTI regulation imposed in January 2017, which are included in bold font in the table below.

Table 1: Macroprudential policies and respective dates of implementation

	Guidelines from FSA		Regulation (requirement)	
	2010 - March	2011 - December	2015 - July	2017 - January
LTV-limit	0.90	0.85	**0.85**	**0.85**
DTI-limit	3	-	**-**	**5**
LTV-limit for line of credit	0.75	0.70	0.70	0.60
Amortization requirement for loans with LTV >	-	0.70	0.70	0.60
Flexibility quota	-	-	0.10	0.1 (0.08 in Oslo)
Test for increased interest	-	5 pp	5 pp	5 pp
LTV-limit, secondary housing in Oslo	-	-	-	0.60

Table 1 displays the macroprudential policies implemented in Norway and the respective dates of implementation. The policies studied in this paper are highlighted in bold font. Source: (Boliglånsforskriften, 2016; Finansdepartementet, 2021a).

Even though changes have been made to the 2017 regulation since its implementation, the restrictions regarding LTV and DTI are still in use today. As seen from Table 1, the 2017 regulation contains stricter limits on LTV limits for loans with line of credit and interest-only loans. However, the biggest change from the 2015 regulation is the DTI limit, limiting mortgages to five times gross income. Some of the sections in the 2017 regulation are summarized in Appendix B.

2.1.2 The housing market after the new regulations

To put the DTI regulation into context, the median Norwegian household income before taxes in 2017 was 648,000 NOK (Statistics Norway, 2021a). The DTI regulation only allows a financial institution to issue a mortgage of five times the household's income. Which in this case will be 3,240,000 NOK. In 2017, the average price for a dwelling in Oslo was 4,561,374[5]. In other words, given an average Norwegian household income, the DTI regulation prevents the buyer from buying an averagely priced dwelling in Oslo. Figure 3 shows the price development for all dwellings in Oslo. Contrary to the price index from Statistics Norway presented in Figure 2 above, the index in Figure 3 contains only dwellings located within Oslo

[5] Data obtained from Eiendomsverdi AS.

and is based on the data supplied by Eiendomsverdi AS. The index exhibits a clear drop in 2017, before continuing to increase in 2018. This suggests that the DTI regulation imposed in January 2017 had a receding effect on the housing market in Oslo. The LTV regulation imposed in July 2015, did not seem to have had the same effect, as the price index continued to grow in 2015 and 2016. Only a small decline can be seen around the time of the LTV policy implementation. During the 11-year time period, the price index increases by 116 percent.

Figure 3: Price index for dwellings in Oslo

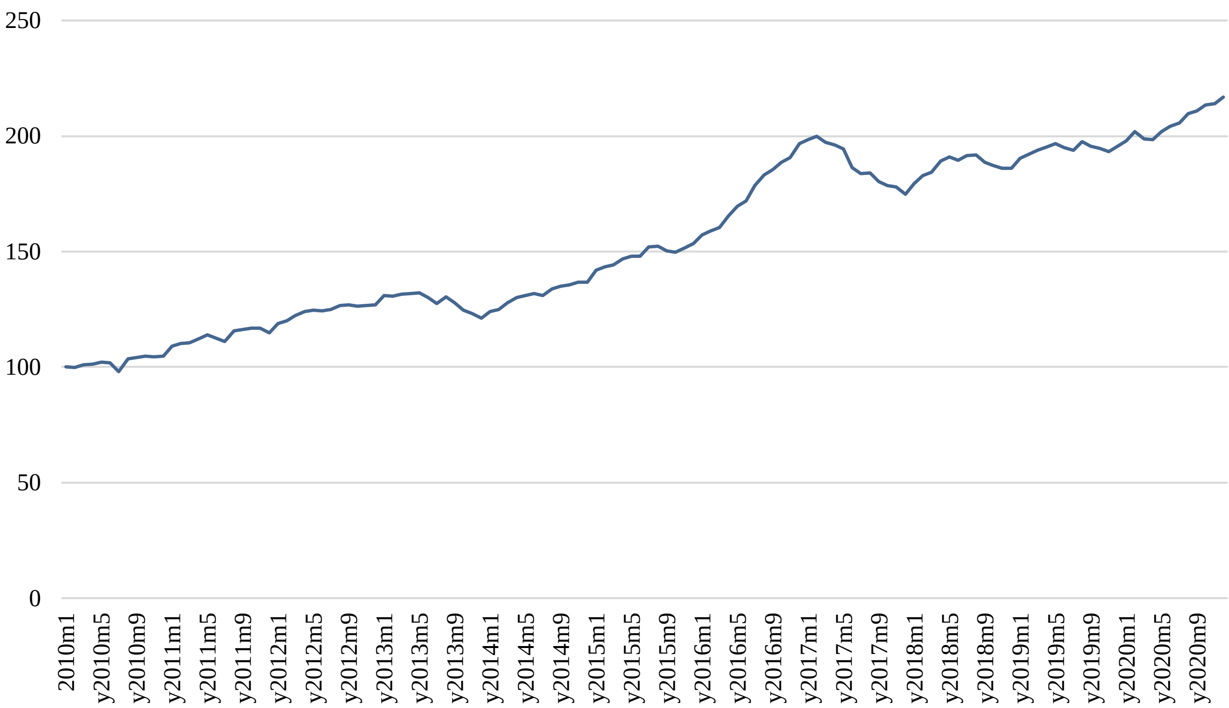


Figure 3 shows the price index for the Oslo housing market from January 2010 to December 2020, indexed at January 2010. The index is constructed using monthly dummy variables from the hedonic regression presented in section 4. Data used to construct the index is obtained from Eiendomsverdi AS.

2.2 Literature review and methodological framework

2.2.1 Previous findings

The total effect of macroprudential policies is not fully known (Igan & Kang, 2011). The international policy debate regarding the effectiveness of macroprudential policies is constantly fueled by new research and discoveries. However, investigating the impact of these policies is difficult, as the data that can be used is scarce and it may be too early to empirically understand the effect of the policies that where introduced only a few years ago (Galati & Moessner, 2013).

Furthermore, the methodologies used to examine the effects of the policies are diverse, and a there is a need for a more systematic approach that can evaluate the causal effects of macro prudential policies (Galati & Moessner, 2013).

Galati and Moessner (2013), a literature review of macroprudential policies, suggests that five different methodologies can be used when investigating the effect of macroprudential policies. The five methodologies are; (1) event studies of different cross sections of countries, (2) compiling assessments on the effectiveness of macroprudential policies, usually done by the authorities and external observers, (3) cross-country studies which apply a form of regression analyses, (4) macroeconomic models, such as dynamic stochastic general equilibrium (DSGE) models, that simulate the effect of macroprudential policies, and (5) using micro data to test the impact on banks. In this study, we use a combination of econometric models with high-resolution data from the housing market. Thus, this study does not necessarily fall in to one of the brackets discussed above, but applies a modern econometric approach by combining hedonic regression and regression discontinuity design.

The rapidly expanding literature on the topic of macroprudential policies presents several findings from various countries and municipalities, as well as cross country evidence. Wong, Fong, Li, and Choi (2011) examine the effect of LTV restrictions on Hong Kong, as well as evidence from 13 other economies. Hong Kong has a relatively long history of using LTV restrictions, with approximately 30 years of experience with this policy. Wong et al. (2011) find that the LTV policy in Hong Kong has stabilized the banking sector and helped banks navigate the boom-and-bust cycle of the housing market. The study also finds that LTV restrictions may impose liquidity constraints on the homebuyers, but this drawback can be limited by using mortgage insurance programs. In addition, cross-country evidence based on data from Hong Kong, Singapore and Korea, show mixed results of the effect of LTV on property market activities. Hence, the study suggests that the effect of LTV policy is more apparent in the household sector by limiting the household liquidity, and not as influential towards the activity in the housing market.

A study that focuses more on the effect of LTV policy on the activity in the housing market and the housing prices is Igan and Kang (2011). This study examines both the effect of LTV policy and DTI policy on the housing market in Korea. It finds that the number of dwellings sold drop right after a tightening of the LTV and DTI regulations. The decline in transaction activity occurs within a three-month window after the implementation of stricter policies. Additionally, the study discovers that the appreciation in housing prices slows down after the tightening of LTV and DTI, and that this effect is seen within a six-month period following the implementation of the regulations. The study suggests that price appreciation slows down more after LTV tightening than DTI tightening. In addition, the findings indicate that tighter loan eligibility criteria decrease future expectations regarding the housing market.

A more recent study, Armstrong et al. (2019), examines the effect of LTV restrictions on the New Zealand housing market. During the time period 2013 to 2016, the Reserve Bank of New Zealand implemented three rounds of LTV restrictions. The first LTV restriction was imposed in 2013 and had a significant effect on the house price inflation in Auckland and the rest of New Zealand. Using the difference-in-difference method, the study compares dwellings that were exempt from the new LTV restrictions with dwellings that were affected by the new policy. The LTV restriction imposed in 2013 had a 2.4 percent moderating effect on the housing price inflation during a one-year period after the implementation. The study also suggests that the effect of LTV was stronger in the rest of New Zealand than in the largest city, Auckland. An important finding from the study is that the effect of LTV-restrictions on housing prices is short-lived, and that if the macroprudential authority wishes to achieve a lasting effect of the policy, it is necessary to either continue tightening the LTV restriction or seek other policies, such as debt-to-income measures. In addition, if the housing market experiences strong price growth during the period right before the LTV implementation, the effect of the LTV policy will be weak and short-lived.

Wilhelmsson (2019) examines the effect of LTV-restrictions and amortizations requirement on the Swedish housing market. The study is similar to this paper, as it uses a hedonic RD design to discover if the implantation of lending regulations has had any effect on housing prices. Sweden has been practicing lending regulations for the past decade and has implemented and tightened the LTV restriction during this period. In 2010, an LTV restriction of 0.85 was introduced, followed by an amortization requirement in 2016. The study finds no statistically significant effect of the LTV policy. A proposed explanation is that the banks already implemented different LTV measures before 2010. The amortization requirement had a larger influence on the housing market. An average decline of eight percent was seen during a short-term period after the implementation. Research on the Norwegian housing market and the effect of LTV policies also exists. Aastveit et al. (2020) examine the effect of mortgage regulations on households in Norway. The study finds that households affected by an LTV regulation lower their debt uptake and become more financially robust when facing adverse shocks. However, the findings also suggest that households tend to deplete their liquid assets to secure the loan, which suggests an ambiguous effect of LTV policies.

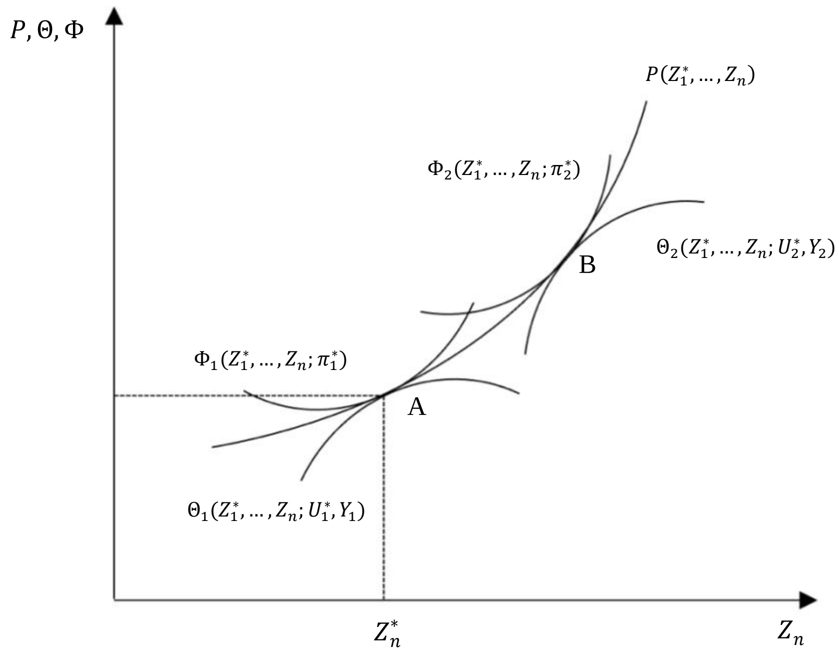
2.2.2 The hedonic pricing method

To analyze the effect of the imposed regulations in 2015 and 2017 on the housing market, it is useful to estimate a price index for the Oslo housing market from 2010 to 2020. One could argue that taking the average price of all the housing transactions and look at the monthly change would be sufficient. However, the housing market does not consist of homogenous products. All dwellings consist of different characteristics and attributes, such as number of bedrooms, number of square meters, geographic location and year built. These characteristics are often referred to as observable heterogeneity. Unobservable heterogeneity refers to aspects such as lighting conditions, views from the dwelling, and age of the kitchen. Due to the different characteristics of each dwelling, the housing market consists of *heterogeneous* products. Heterogeneity makes it difficult to compare dwellings sold in one specific period with dwellings sold in a different period. For example, due to seasonal differences, large homes might be more popular during a certain period, which will result in a spike in the average housing price that period. Hence, adjusting for the heterogeneity when estimating the housing prices is crucial. In other words, to develop a reliable price index for the housing market, we need a model that can accommodate the different characteristics of each home.

Hedonic pricing is a widely used method when estimating the price of a product based on its fundamental characteristics. Rosen (1974) was the first to explain and conceptualize the theory of hedonic pricing. The hedonic model is based on the idea that a product's value is derived from its characteristics and attributes. By decomposing the product, in this case the dwelling, into its inherent characteristics, one can also find the contributory value of each attribute (Rosen, 1974). This idea yields the basis for hedonic regression, which is used to estimate how different attributes affect the product price. Hedonic regression is widely used in economic research (e.g. McCluskey & Borst, 2007; Owusu-Ansah, 2011; Wilhelmsson, 2000), and is regarded as one of the best methods when calculating an index for the housing market (Eurostat, 2013). Statistics Norway has been using a hedonic pricing model to develop price indices for the housing market since 1992 (Lillegård, 1994; Takle, 2012). In the following, we will give a brief summary of the hedonic model, based on Rosen (1974).

Assume that the market price of a dwelling can be described by a price function consisting of different characteristics (attributes) for the dwelling. Rosen's hedonic model consists of a vector with *n* objective attributes: $\mathrm{Z} = (\mathrm{Z}_1, \dots, \mathrm{Z}_\mathrm{n})$. $\mathrm{P}(\mathrm{Z})$ is the hedonic price function which shows the relation between market price P and attributes Z. In the case of housing attributes, $\mathrm{Z}_1, \dots, \mathrm{Z}_\mathrm{n}$ can for example represent square meters, number of bedrooms, floor number and geographical location. Figure 4 below shows the relation between the market price for a dwelling and its attributes in market equilibrium. The graph illustrates how the hedonic price function $\mathrm{P}(\mathrm{Z})$ is formed.

Figure 4: The hedonic model and market equilibrium



Hedonic model with market equilibrium. A and B represent points where market equilibrium is achieved. The price function P(Z) is the line formed between the two pints. The Y-axis represents the selling price P, as well as the offer function Φ, and the bid function Θ. The X-axis represents the number of attributes, Z. Figure 4 is based on the figure presented in Osland (2001).

The bid function $\Theta(Z; U, Y)$ represents consumers' maximum willingness to pay for different dwellings with different attributes, given a fixed utility and income, while P(Z) is the minimum price the consumer must pay. Θ is referred to as the consumption decision and is an important factor when explaining market equilibrium. Consumers maximize their utility given a certain income, meaning $\max U(Z, X; \alpha)$ subject to a non-linear budget constraint $Y = P(Z) + X$. In other words, a consumer's utility is maximized when $\Theta(Z^*; U^*, Y) = P(Z^*)$. U is individual utility, Z is the dwelling's attributes, X is all other goods than the dwelling treated as numeraire, Y is the income measured in the number of units of X, and α is parameter referring to the consumer's preferences. When maximizing the utility, consumers will adjust so that the marginal substitution effect between Z and X is equal to the implicit price on Z. $\Theta(Z; U, Y)$ is illustrated as an indifference curve in Figure 4 above. Here, at an optimal location where the bid function and the offer function are tangent, $P(Z^*)$ is formed. Along the indifference curve Θ, households are indifferent. Consumers minimize the price they pay for a dwelling while maximizing the number of attributes. Graphically, this means that the consumer's indifference curve will be pushed down towards the x-axis in Figure 4. We have drawn two indifference curves, with optimal points at A and B, which represent one dimension of the equilibrium.

Contrary to consumers, the suppliers want to sell at the highest price possible, given a combination of attributes. This side of the equilibrium is referred to as the production decision, and completes the hedonic model, allowing us to find market equilibrium. As with the consumption decision, the dwellings offered contain different variations of attributes, Z. Assume that the market consists of many small companies that sell dwellings and adjust the combination of attributes in each dwelling to maximize their profits. The profit function can be expressed as, $\pi = M * P(Z) - C(M, Z;\ \beta)$, where M(Z) is the supply of housing with attributes Z and P(Z) is the hedonic price function. The cost function C represents the cost of producing the dwellings, and β is a parameter adjusting for input costs and production technology within the company. Companies want to maximize the production function to maximize profit. Similar to the transformation of the utility function for consumers, we can transform the profit function into an offer function $\Phi\ (Z; \pi, \beta)$. This function is the production decision's counterpart to the consumer's indifference curve explained above. The offer function is defined as the minimum price the producers are willing to accept for dwellings with different attributes, to achieve a constant profit level and given that the optimal amount of housing is produced (Osland, 2001).

Market equilibrium is achieved when the bid function and offer function are tangent. The consumer will try to achieve the lowest possible price given a fixed number of attributes. Hence, the demand function will gravitate down towards the x-axis. On the contrary, the producer wants to achieve the highest possible price given a fixed number of attributes. Hence, the producer will choose the indifference curve that is positioned as high as possible. When the two functions meet and become tangent, market equilibrium is achieved. It is the interaction between producers and consumers in the market that yields the hedonic price function P(Z). In Figure 4, two interactions, point A and B, have been drawn to illustrate the effect.

In reality, however, thousands of consumers and producers try to maximize their utility, and form P(Z). It is also important to note that the hedonic model explained above assumes that the suppliers produce and sell dwellings. The data used in this study, however, only contain existing dwellings, which are not produced by the suppliers. Hence, the model above is not formally adapted the second-hand market. Nevertheless, the model is still suited for the needs in this study, as the hedonic approach allows us to control for the heterogeneity in the housing market when developing a housing price index. The specification we use in the hedonic regression allows us to obtain monthly and weekly time variables. These variables are used to construct the price index, which provides information about the price development in the housing market over the past 11 years. In addition to the price index, we will need to conduct econometric analyses of the selling price of dwellings before and after the implementation of the macroprudential policies. The econometric model chosen is regression discontinuity design.

2.2.3 Regression discontinuity design

In social sciences, the goal is often to determine the causal effect of a treatment on a certain outcome of interest (Cattaneo, Idrobo, & Titiunik, 2019). In experiments where the treatment of interest is randomly assigned, this process can be rather simple and uncomplicated. However, when the treatment of interest cannot be randomly assigned, for example due to practical and ethical reasons, finding the true causal effect may be challenging. Hence, using methods to make it appear as if the treatment of interest is randomly assigned in non-experimental settings, such as in quasi-experiments, are especially promising (Cattaneo, Idrobo, et al., 2019; Stock & Watson, 2015).

Difference-in-difference estimation and instrumental variables are frequently used methods when examining treatment effects in non-experimental settings. In certain cases, however, the difference-in-difference method is not feasible, as it requires a control group not subject to any treatment. In some natural experiments, however, no such control group exists. An alternative research design applied in order to analyze treatment effects in non-experimental settings where a non-treated group is not necessary, is regression discontinuity (RD) design (Cattaneo, Idrobo, et al., 2019). RD designs were first introduced by Thistlethwaite and Campbell (1960), but did not receive much attention in economics until the late 2000s (Lee & Lemieux, 2010). One of the reasons for the recent upsurge in research using RD design is that the causal inferences from RD designs can potentially be recognized as more credible than those from natural experiment strategies, such as instrumental variables and difference-in-difference (Lee & Lemieux, 2010).

The main idea of RD designs is to determine the treatment effect in a non-experimental setting where the sorting of treatment and control group is based on whether an assignment variable (running variable) exceeds a known cutoff point (Lee & Lemieux, 2010). In other words, receiving treatment depends on whether an observable variable (the running variable) passes a known threshold value (Stock & Watson, 2015). In our case, the threshold value, from now on referred to as the cutoff value, is the date of implementation of the macroprudential policy. If the dwellings were sold on or after the date of implementation, the dwellings would receive treatment in the form of being subject to the new macroprudential policy. If the dwellings were sold before the date of implementation (before the cutoff), the dwellings would not receive treatment. Formally, a simple RD design can be expressed as:

$$Y = \alpha + D\tau + X\beta + \varepsilon, \qquad D = \begin{cases} 0 & if\ X < c \\ 1 & if\ X \geq c \end{cases} \tag{1}$$

where Y is the outcome of interest, X is the running variable, and D is a dummy variable for whether the running variable exceeds the cutoff, c. The coefficient τ is then the estimated causal effect of X exceeding the cutoff (Lee & Lemieux, 2010). RD design compares observations close to the cutoff, where the ones below the cutoff are used as counterfactuals to the ones above the cutoff (Calonico, Cattaneo, Farrell, & Titiunik, 2019). The idea of RD designs is that we can compare observations below and above cutoff as if they were randomly assigned. In practice, researchers often use local polynomial estimation, weighing the observations close to the cutoff higher than the ones further away (Calonico et al., 2019). It is important to note that model (1) above is linear in the assignment (running) variable X. In practice, however, an RD model is often not linear, and the estimation of RD designs are usually viewed as a nonparametric problem (Hahn, Todd, & Van der Klaauw, 2001; Lee & Lemieux, 2010). One of the main advantages of RD designs is that the RD results can be shown graphically. Model (1) is expressed graphically in Figure 5 below. If there is a causal effect of X at the cutoff, the outcome variable Y is discontinuous at the cutoff. This discontinuity can be seen in the graph, where the population regression line makes a jump at the cutoff, c. The discontinuity is the estimated causal effect, τ. Using similar scatter plots to visually illustrate the causal effect of X adds transparency to the RD findings and is an essential part of RD designs (Cattaneo, Idrobo, et al., 2019; G. W. Imbens & Lemieux, 2008).

Figure 5: Hypothetical RD plot

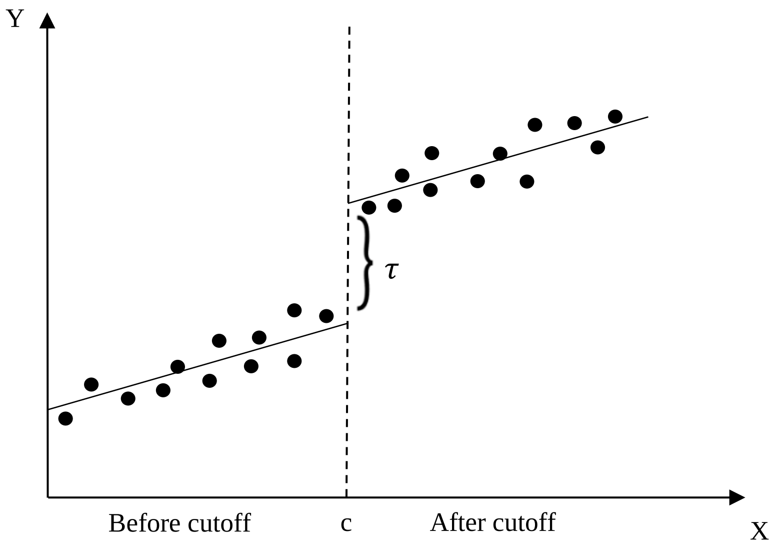


Figure 5 shows a hypothetical RD-plot for a linear regression. When the running variable X reaches the cutoff point, c, the population regression line makes a jump up and becomes discontinuous. The size of the jump corresponds with the coefficient τ. The figure is based on (Lee & Lemieux, 2010).

Another key feature of the RD design is that the probability of receiving treatment changes instantly when X = c (Cattaneo, Idrobo, et al., 2019). In other words, the chance of receiving treatment jumps from 0 to 1 when X reaches the cutoff. This assumption is used in *sharp* RD designs and allows us to estimate the local causal effect. However, an important element of RD design is the fact that being assigned to the treatment group is not always the same as actually receiving treatment. If this is the case, *fuzzy* RD design can be used. This method is an alternative to sharp RD design and is based on the idea that compliance with treatment assignment is imperfect (Cattaneo, Idrobo, et al., 2019). In fuzzy RD design, the probability of receiving treatment does not change from 0 to 1 when X reaches the cutoff. The probability only increases. This imperfection between treatment and assignment is solved by using an instrumental variable, which indicates if the cutoff is crossed (Stock & Watson, 2015). Further interpretation of fuzzy RD designs is beyond the scope of this paper. Hence, we will mostly focus on sharp RD design when examining the effect of macroprudential policies. However, in section 4.2 we will explain why a fuzzy RD approach could be interesting to apply in this study, and why we have focused on the sharp RD design instead.

Another issue that arises when implementing RD design is the selection of the bandwidth, which is the smoothing parameter. The bandwidth determines the window where the global fit is conducted (Cattaneo, Idrobo, et al., 2019). In other words, the bandwidth specifies the number of bins used on either side of the cutoff. In our RD model, the running variable is the date of sale. Hence, if we select a bandwidth of 150, we conduct a local polynomial regression with X running from c- 150 days, and c + 150 days. Choosing a bandwidth too narrow may lead to an imprecise estimation. On the other hand, choosing a bandwidth too wide may cause the regression to include observations very far from the cutoff, which leads to the comparison on both sides of the cutoff being less credible since we are no longer only comparing observations right before and right after the cutoff (Lee & Lemieux, 2010). Thus, a key decision when implementing RD design is the choice of bandwidth (G. Imbens & Kalyanaraman, 2012).

A useful addition to the RD design is to include covariates. Very often, researchers augment their RD model with various covariates to account for different measures, such as demographic and socioeconomic variables (Calonico et al., 2019). Including covariates that are correlated to the outcome of interest can improve the precision of the analysis (G. W. Imbens & Lemieux, 2008). The covariates work as a type of control variables and are useful to eliminate sample biases that occur in the original specification, especially when including observations that are further from the cutoff. Using a larger bandwidth and including observations further from the cutoff can sometimes result in biased estimates (G. W. Imbens & Lemieux, 2008). The covariates help eliminate the bias that occurs from these observations. An important condition is that the covariates should be continuous at $X = c$.

3 Data

3.1 About the data

The data used in this study contains housing transactions from Oslo. The dataset is provided by Eiendomsverdi AS (EV), a company that tracks information about the Norwegian real estate market and collects transaction data from real estate agencies and the Norwegian Land Registry. Since we are focusing on the housing market in Oslo and the effect of the mortgage regulations imposed in 2015 and 2017, the data contains information on housing transactions in Oslo from 2010 to 2020. The data covers approximately 70 percent of all transactions in Oslo and is considered highly reliable and accurate. The remaining 30 percent are other types of transactions, such as bequests and sales that are not listed on the market. The total number of transactions included in the dataset are 209,731. It is important to note that most dwellings are sold more than once during our selected time period.

To achieve an accurate estimate of the price development in the housing market with the hedonic regression, we need several relevant housing attributes included in the regression. The data includes nine different variables that work as attributes in the regression. These are the number of bedrooms, size of the living area, floor number, type of ownership, postal code, property type, year built, and date of sale. Other variables included in the data that are not used in the hedonic regression but provides info about the transactions are unit ID, selling price, list price, date of registration in the Land Registry, common debt, lot size, city district, and size of utility floor space. Table 2 shows all the different variables and their meaning, highlighting the variables used in the hedonic regression and the RD design.

Table 2: Variables included in the dataset

Variable	**Description of variable**
Unit ID	ID-number for each dwelling
List Price	Sellers suggested retail price
Date of Registration	The date when the dwelling was registered for sale
Common Debt	Amount of shared debt in the property (included in the selling price)
Lot Size	Size of the lot
Utility Floor Space	Number of square meters available for usage
Selling Price	**Actual sales price**
Date of Sale	**The date when the dwelling was sold**
Bedrooms	**Number of bedrooms**
Living Area	**Number of square meters of living area**
Floor	**Floor number**
Ownership	**Type of ownership (cooperative and self-owned)**
Property Type	**Property type (apartment, single family home, semi-detached or row house)**
Postal Code	**Postal code for dwelling**
City District	**Which city district in Oslo the dwelling is located**
Build Year	**Year of which the building/dwelling was built**

Table 2 describes the different variables in the data. The variables written in bold are used in the hedonic regression. The remaining variables provides info about the transaction and dwellings but are not necessarily useful in the regression. For example: common debt is already included in the total selling price. Lot size is rarely useful when estimating dwellings in Oslo, as most of the dwellings are apartments. Utility floor space provides in many cases the same information as living area. Hence, we find it sufficient to include only one space variable. Date of registration does not influence the selling price and is not a useful substitution for date of sale. City district is not necessary since postal code is a more precise geographical attribute. However, as seen later in the study, we will include City District as a covariate in the RD design.

3.2 Trimming and cleaning

Even though the data obtained from EV is considered highly reliable and is a good representation of the real estate market in Oslo, there are some issues with the data. Missing observations, double registrations, unrealistic values, and irrelevant information occurs throughout the dataset. For example, the variable bedroom has some missing values we deem problematic. Of the 209,731 observations included in the dataset, only 123 dwellings are considered zero-bedroom dwellings. In other words, only 123 dwellings are registered with zero bedrooms. Zero-bedroom dwellings usually contain a bed in the living room or in a cubicle. Zero-bedroom dwellings are quite common in Oslo. Hence, it does not make sense that the data only includes 123 zero-bedroom dwellings. To achieve a hedonic model with the highest possible accuracy, we are dependent on reliable observations that form a representative selection. Due to the problem mentioned above, along with other problematic observations and values, we have devoted considerable time to cleaning and trimming the dataset. In this section we will explain the cleaning process, emphasizing why we have done the different changes in the dataset. Table 3 shows the different steps conducted to cleaning the dataset.

Table 3: Steps taken to clean the dataset

Step	Data size	Removals	Description of removed observations
Initial	209,731		
1	196,311	13,420	Properties with ownership other than self-ownership and cooperative
2	191,310	5,001	Selling price below 825,000 NOK and above 14 mill NOK
3	188,321	2,989	Living Area under 22 sqm and over 242 sqm
4	184,425	3,896	Repairing zero-bedroom dwellings
5	184,092	333	Dwellings with bedroom value above 5
6	181,431	2,661	Dwellings with floor below one and above 10
7	181,334	97	Properties with same selling date and HouseID
8	181,295	39	Duplicates and unrealistic values

Table 3 explains the steps taken to clean the dataset. In total, 28,436 observations are removed.

As mentioned earlier, the initial number of observations is 209,731. Included in this number are dwellings with different types of ownership forms. In Norway, there are two main types of ownership forms, self-owned[6] and cooperative. Self-owned simply means that the buyer fully owns the property. Hence, this type of ownership tends to be the most desirable and may cause the selling price to be higher. However, when buying a self-owned property, a purchase tax of 2.5 percent incurs. Cooperative on the other hand means that the buyer essentially becomes a shareholder in the cooperative and buys the rights to live in the property. When buying a property with this ownership model, the buyer does not need to pay the 2.5 percent purchase tax. The desired ownership type depends on the buyer's preferences and is somehow subjective. However, the two ownership types tend to affect the selling price. Therefore, it is necessary to include ownership form as a variable in the hedonic regression.

In step one in Table 3 we remove transactions that are listed with other types of ownership forms than self-owned and cooperative. These ownership forms make up a very small minority

[6] Real estate economics and studies related to the Norwegian real estate market usually use the word "owner-occupier" instead of "self-owned" (e.g. Barlow, 1990; Larsen, 2018). We believe "owner-occupier" is a too narrow description of the ownership form, as it can be perceived as the owner is only allowed to live in (occupy) the dwelling, and not rent it out. The ownership form, however, allows for rental. Hence, we use the word "self-owned" for this ownership form.

of the total listed dwellings in Norway and are often subject to special agreements and rental restrictions. Since the price of these dwellings can vary due to the individual agreements and restrictions for the property, we choose to remove these dwellings. This results in a reduction in the dataset of 13,420 observations.

In step two we trim the data to remove extreme values. Data trimming is commonly used in statistics when dealing with outliers and extreme values. Using this method improves the statistical accuracy and assist us in choosing which observations to move. We trim the data on the one and 99 percentile, removing the bottom and top one percent of the data regarding the selling price. This results in removing all dwellings that are sold for more than 14,000,00 and all dwellings sold for less than 825,000. This step reduces the dataset by 5,001 observations. In step three we also use data trimming to remove extreme values, trimming the data on the one and 99 percentile. We remove everything below 22 square meters and everything above 242 square meters. This results in a reduction of 2,989 observations.

In step four we adjust the number of zero-bedroom dwellings. As mentioned earlier, the number of dwellings registered with zero bedrooms is extremely low. One explanation may be that several of the zero-bedroom dwellings have missing values for bedroom. Instead of being registered with zero bedrooms, the dwellings have been registered with no value. Hence, the bedroom value for most of the zero-bedroom dwellings may be missing. This can explain why the dataset only includes 123 zero-bedroom dwellings. To repair this problem, we can replace the missing value with the number zero, since we assume that most observations with a missing value for the number of bedrooms are zero-bedroom dwellings. However, this would mean replacing *all* observations that have a missing value for bedroom with zero, and not *only* the observations that actually are zero-bedroom dwellings. Some of the dwellings not registered with a value for the number of bedrooms may be larger apartments that most likely contain more than one bedroom. Hence, we must account for the size of the apartment when replacing a missing value with zero.

Only the smaller dwellings are most likely to have zero bedrooms. To resolve the issue, we replace all missing values with zero, given that the apartment is smaller than 35 sqm[7]. This process changes the number of zero-bedroom dwellings from 123 to 6,310, before trimming the variable. Dwellings larger than 34 sqm and that are registered with a missing value for bedroom are omitted from the dataset. This results in 3,893 observations being removed. In step five we trim the bedroom variable on the one and 99 percentile, removing all observations with bedroom value higher than five. Table A.2 in Appendix A shows the final distribution of the bedroom variable. In step six we trim the data on the one and 99 percentile to remove extreme values for floor. This results in all floor numbers below one and above 10 being removed, and a reduction of 2,661 observations. Steps seven and eight are simple data cleaning processes, such as removing duplicates, removing observations with a registered build year of zero, and postal codes that are not found in Oslo.

Each step is intended to provide us with a dataset that is more representative of the real estate market in Oslo, while also making our hedonic regression more reliable as a pricing model for a given dwelling. In addition to the changes described above, a few additional adjustments are necessary before using the data for analysis. First, the variable indicating the floor number still poses a problem. Apartments listed for sale are usually registered with a floor number. An apartment located on the fifth floor tend to be higher priced than an identical apartment located on the first floor, since living on the first floor in Oslo tend to be less attractive. If we were to only look at apartments and no other property types, such as single-family homes, the floor variable would be an interesting variable to include in the regression. An apartment with a higher floor number would most likely affect the selling price positively and push the price upwards. However, with other property types, the floor variable creates a problem. Many of the single-family homes and semi-detached homes do not have any specific floor number. Most often, these homes are located on the ground, spanning across two or three floors. Hence, assigning a specific floor-number to these property types is not easy. This is also apparent in the data, where most of the property types other than apartments have no registered floor number at all. Removing the observations with no floor number would mean losing all

[7] After examining the data and the observations with missing values for bedroom, 35 sqm is chosen as the threshold. Most of the dwellings with a lower selling price and a missing value for bedroom are assumed to be zero-bedroom dwellings. Most of the dwellings that are over 35 sqm and registered with a missing value for bedroom tend to be sold at a considerably higher price, indicating that the dwelling is not a zero-bedroom dwelling, but a dwelling with one or several bedrooms. Hence, using 35 sqm as the threshold seems appropriate.

observations registered as a single-family home, semi-detached home, or row house. On the other hand, simply not using floor in the regression would mean losing an important housing attribute that contributes to pricing a dwelling. To get around this problem, we have created a dummy variable indicating whether an apartment is located on the first floor. The variable takes value of one if the observation is an apartment located on the first floor, and the value of zero if the observation is an apartment not located on the first floor or any other property types. Including the variable in the regression allows us to adjust for floor number without losing any observations.

Second, the variable for build year contains values that do not necessarily affect the selling price in the hedonic model. For example, a building that is built in 1976 is most likely not considered more expensive than a building built in 1974. Hence, including build year as a discrete variable in the hedonic regression may not be the best approach. Furthermore, several observations may have conflicting values for build year due to uncertainty of the actual build year. To address this problem, we have created dummy variables for build year eras. The chosen time periods are based on Lillegård (1994)[8], which uses the time period before 1945, between 1945 and 1959, between 1960 and 1969, between 1970 and 1982, and after 1982. Since our data is newer than what Lillegård (1994) used, we have added an extra time period from 1983-1999, resulting in the last time period including all dwellings built later than 2000.

3.3 Description of the final data

The data cleaning process above reduces the total number of observations by 13.6 percent. Despite a relative sizable reduction of 28,436 observations, we are still left with 181,295 observations, which should be more than sufficient for a hedonic price regression. Summary statistics about the final data are displayed in Table 4.

[8] Lillegård (1994) also uses a hedonic model on the real estate market.

Table 4: Distribution of selected variables

	Mean	Median	Standard deviation	Minimum	Maximum
Selling price	3,874,799	3,300,000	2,117,109	825,000	14,000,000
Living Area	73	66	35	22	242
Price per living area	55,218	52,500	20,220	6,429	191,549
Bedrooms	2	2	1	0	5
Build Year	1958	1960	37	1597	2020

Table 4 presents distributions of the variables Selling price, Living Area, Price per living area, bedrooms and build year.

As seen above, the average selling price based on the past 11 years is close to 4,000,000 while the average living area space is 73 sqm. The minimum and maximum values reflect the data cleaning process with the one percent trimmed mean, with the lowest selling price being 825,000 and the highest being 14,000,000. A frequently used characteristic when describing and pricing a dwelling is the price per square meter. Hence, we have included this as the price per living area. The average price per living area is 55,218, which is close to other estimates done by real estate companies in Norway[9]. The oldest dwelling is from 1597, while the newest was built in 2020[10].

Table 5 below shows the distribution of the variable for living area. The three intervals small, medium, and large are based on the City Council of Oslo's regulations for new buildings[11]. Originally, the City Council's intervals are divided into 5 intervals, where interval 1 is from 22 sqm to 34.9 sqm, interval 2 is from 35 sqm to 50 sqm, interval 3 is from 50 sqm to 79.9 sqm, interval 4 is from 80 sqm to 99.99 sqm and interval 5 is from 100 sqm and above. However, due to practical reasons, our intervals are merged into 3 intervals. Thus, small dwellings in our analyses include both interval 1 and 2, and range from 22 sqm to 50 sqm. Medium dwellings include interval 3 and range from 50 sqm to 80 sqm. Large dwellings include interval 4 and 5 and includes all dwellings larger than 81 sqm. Medium dwellings are subject to the most transactions and make up almost 50 percent of the final data.

[9] The average price per square meter in Oslo in January 2018 was 66,001. Since the data in this study contains transactions from 2010 to 2020, it seems correct that our price per sqm estimate is a bit lower. Source: (Eiendom Norge, 1/2018)

[10] The observations in the dataset only consist of dwellings that are *sold* and *built*. Future contracts for dwellings that are projected to be built later than 2020 are not included.

[11] The intervals are obtained from a regulation proposed by the City Council of Oslo in 2013 (Oslo kommune bystyre, 2013)

Table 5: Small, medium, and large dwellings

Living area distribution	Observations	Percent
Small (22 sqm to 50 sqm)	42,748	23.58
Medium (51 sqm to 80 sqm	86,278	47.59
Large (81 sqm and above)	52,269	28.83

Table 8 summarize the intervals used when dividing dwellings into small, medium, and large.

Additional information about the variables and the data can be found in appendix A. Table A.1, A.2, and A.3 in appendix A provide information about the city districts, the bedroom variable, and property type respectively. Figure A.1 shows the development in mean selling price per sqm, together with the number of transactions each year.

4 Empirical approach

4.1 Hedonic regression

The hedonic model allows us to account for the different characteristics of each dwelling and adjust for the heterogeneity in the housing market. Furthermore, we can use the model to create a price index for the housing market in Oslo, which can be used to examine the price development before and after the implementation of the DTI and LTV regulations. Hedonic pricing models are widely used in real estate economics (e.g. Anglin, Rutherford, & Springer, 2003; Benson, Hansen, Schwartz, & Smersh, 1998; Sohn, Kim, Kim, & Li, 2020), and can provide a reliable estimation of the price development. A simple approach when estimating the selling price of a dwelling is to use the dwelling's size and number of bedrooms. One can argue that these two attributes affect the price of a dwelling the most, and that its sufficient to only include these two variables in the hedonic regression. However, there are several other attributes and characteristics about a dwelling that can affect the price, such as build year, geographical location, floor number, and property type. Lillegård (1994) includes 13 different variables in a hedonic regression, accounting for characteristics such as distance from city center, if the dwelling comes with parking space, number of bathrooms, and property type. We use a similar functional form of the hedonic model as Lillegård (1994), adjusted for some differences between the data sets used.

4.1.1 Functional form

The price index for the housing market does not exhibit a linear development, as seasonal fluctuations and market activity can cause the housing prices to rise or fall. Nor is there always a linear relationship between a dwelling's price and its attributes. For example, the relation between price and living area can be perceived as diminishing. Hence, using a logarithmic specification of the variable for living area allows us to accommodate for the non-linear relationships between the dependent and independent variable. Lillegård (1994) uses natural logarithm for the variables for selling price, living area, number of bathrooms, and number of rooms. Our data set, however, is not identical to Lillegård (1994). We use number of bedrooms instead of number of rooms. In addition, the number of bathrooms is not included in our sample at all. Further, we use a linear specification for bedrooms instead of logarithm. Because of the one percent trimmed mean, the highest number of bedrooms in our dataset is 5. We assume that from 0 to 5, the relation between price and number of bedrooms is close to linear. This is also confirmed by the adjusted R squared, which is higher when using a linear specification for bedrooms instead of a logarithmic specification. Hence, we only use a logarithmic specification for selling price and living area, while the rest of the variables have a linear specification. The model used is as follows:

$$\begin{aligned} ln(Selling\ Price_{i,t}) &= \beta_0 + \beta_1 ln(Living\ Area_i) + \beta_2 Bedroom_i + \beta_3 \ln\,(Living\ Area_i) \\ &* Bedroom_i + \beta_4 Apartment\ First\ Floor_i + \beta_5 Ownership_i \\ &+ \gamma_2 Postal\ Code_{2,i} + \cdots + \gamma_{413} Postal\ Code_{413,i} + \theta_2 Property\ Type_{2,i} + \cdots \\ &+ \theta_4 Property\ Type_{4,i} + \vartheta_2 Build\ Year_{2,i} + \cdots + \vartheta_6 Build\ Year_{6,i} \\ &+ \delta_2 Month_{2,t} + \cdots + \delta_{132} Month_{132,t} + \varepsilon_{i,t} \end{aligned} \tag{2}$$

where the dependent variable represents the selling price of the dwelling with a logarithmic specification. $ln(Living\ Area)$ is the variable for living area with a logarithmic specification, while $Bedroom$ represents the number of bedrooms. We have also included an interaction term, $\ln(LivingArea)\ * Bedroom$, that takes into consideration the fact that simply increasing the number of bedrooms without increasing the living area does not necessarily increase the selling price. With a given number of square meters it is limited how many bedrooms the dwelling can have. Hence, simply increasing the number of bedrooms with a given number of square meters can affect the price negatively. The interaction term includes this effect in the pricing model. $Apartment\ First\ Floor$ represents a dummy variable for whether the dwelling is an apartment located on the first floor, or not. The next dummy variable, $Ownership$, represents the type of ownership, which is either cooperative or self-owned.

To avoid multicollinearity, the first dummy variable for the remaining attributes with a binary specification begins with the number two. To adjust for the geographical differences between the dwellings, Lillegård (1994) uses distance to city center as a variable, as well as different zones which includes city districts. However, our dataset includes postal codes for each dwelling. Hence, we use $Postal\ Code$ as our geographical attribute. This form of geographical data is much more precise than distance from city center and may lead to a better price estimate. In our data there are 413 unique postal codes. Hence, we have included 412 postal code dummies to avoid multicollinearity. $Property\ Type$ are dummy variables that represent the property type of the dwelling, which is either apartment, single family home, row house, or semi-detached house. $Build\ Year$ are dummy variables for the different building year intervals. ε represents the residual and is assumed to be independently and normally distributed with mean zero. The letter i represents each transaction, while the letter t represents which date the transaction occurred.

Finally, $Month$ is a time dummy variable which indicate which month and year the dwellings were sold in. The time dummies prevent the confusion of price changes due to the passage of time and price changes due to attributes of the dwelling (Lillegård, 1994). Lillegård (1994) uses quarterly time dummies in the hedonic regression. However, to achieve an even more precise estimate and to accommodate for monthly changes, we use monthly time dummies instead of quarterly time dummies. In addition to the described model above, we also estimate a new hedonic regression with weekly time dummies instead of monthly time dummies, holding everything else the same. The weekly time dummies are used when employing an RD design analysis with the price index.

4.1.2 Price index

The hedonic model described above provides us with the estimated effect of the different attributes on the selling price. However, we are more interested in examining the monthly price *development* from 2010 to 2020 to better understand the effect of the DTI and LTV policy. The estimated monthly dummy variables can be used to develop a price index, as they indicate the monthly effect on the selling price. January 2010 is used as the reference variable in the hedonic regression. Hence, this month is given the value 0. Since the selling price has a logarithmic specification, and the monthly time dummies have a linear specification, we use the exponential value of the coefficient to achieve the statistics used to construct the price index. The exponential value of 0 is 1. Hence, January 2010 receives the value 1, which means that our price index is indexed at January 2010. The rest of the coefficients for the monthly time dummies are also transformed using the exponential value ($e^{coefficient}$). In addition, all the final numbers are multiplied with 10, so that the price index is indexed at January 2010 with a value of 100 instead of one. Finally, we estimate a new hedonic regression with weekly time dummies instead of monthly.

4.2 Regression discontinuity design

The price index described above provides useful information about the price development in the Oslo housing market and gives an indication of the effect of the macroprudential policies. However, simply studying the price index is not enough to arrive at a valid statistical conclusion. Hence, the main findings in this study will be derived from the RD analyses. RD design allows us to compare observations below and above the cutoff as if they were randomly assigned, weighing the observations close to the cutoff higher than the ones further away (Calonico et al., 2019). Our belief is that the housing prices may have dropped following the implementation of the macroprudential policies. RD design allows us to compare housing prices before and after the dates of implementation. Furthermore, the housing attributes used in the hedonic regression can also be used as covariates in the RD design, allowing us to control for the heterogeneity in the housing market. In the following, we will first explain the steps taken when employing a sharp RD design. Then, we will discuss supplementary RD designs which are used to provide a better understanding of the causal effect of the macroprudential policies.

4.2.1 Cutoff

The first step when conducting the RD analyses is choosing the cutoff. Since the LTV policy was implemented on July 1, 2015, and the DTI policy was implemented on January 1, 2017, it makes sense to use both dates as two different cutoffs. However, a problem that can occur when implementing sharp RD designs is that the findings may only indicate the short-term effect. Sharp RD designs only look for any potential discontinuities *at* cutoff, comparing observations right before and right after the cutoff. As with other economic policies, the true effect of the macroprudential policies may not occur before several months after the date of implementation. Hence, the sharp RD design may not yield any statistically significant effect of the policies. In addition, another important aspect of the Norwegian housing market that can contribute to delaying the effect of the macroprudential policy is the use of pre-qualification letters. Before bidding on a dwelling, the potential buyer needs to obtain a pre-qualification letter from the bank. This letter states how much the buyer will receive in mortgage and the maximum bid price. These letters are usually valid for 3 months, which means that if a buyer received a pre-qualification letter the day before the new policy was imposed, the buyer would not be subject to the new policies before three months later. At this time, the buyer would need to renew the pre-qualification letter, which would then be adjusted for the new policies.

Due to the potential delayed effect of the policies, the sharp RD design may fail to capture the causal effect. One solution is to increase the bandwidth. This may help us achieve a better estimate of the long-term effect as the RD design will include observations further from the cutoff. However, using a broad bandwidth also has its downsides, as we will explain in the next section. Thus, to account for the potential delayed effects from the pre-qualification letters we will conduct RD analyses with several different cutoffs. In addition to July 1, 2015, and January 1, 2017, we will use three additional cutoffs in both 2015 and 2017, where the first cutoff is three weeks before the implementation of the new policy[12]. We also use cutoffs at one and a half months and three months after both policy implementations to account for delayed effects. Larsen (2018), a similar study to this, also applies several cutoff dates in the RD analysis. This strategy is used to avoid the findings being dependent on a too specific and narrow RD model.

[12] The government issued a press release on December 14, 2016, informing that a new DTI policy would be imposed on January 1, 2017 (Finansdepartementet, 2016). The date of this press release indicates when the market received the news about the new policy; almost three weeks before. Hence, we use a cutoff three weeks before policy implementation, both for the RD analysis in 2015 and in 2017.

4.2.2 Bandwidth

The second step when conducting the RD analyses is choosing the correct bandwidth. The results from the RD analyses are highly sensitive to the chosen bandwidth (G. W. Imbens & Lemieux, 2008), which makes choosing a bandwidth an important part of RD designs. The bandwidth determines the window where the global fit is conducted (Cattaneo, Idrobo, et al., 2019), and will in our study indicate the number of days from the selected cutoff. In modern empirical work with RD designs, a common strategy when choosing a bandwidth is using a mean square error (MSE) optimal bandwidth (Calonico, Cattaneo, & Farrell, 2020b). However, this optimal bandwidth choice is invalid for inference (Calonico et al., 2020b). A solution to this problem is employing robust-biased corrected inference methods, which yield valid robust-biased confidence intervals and inference methods even though the MSE-optimal bandwidth is used (Calonico, Cattaneo, & Farrell, 2020a; Calonico et al., 2020b; Calonico, Cattaneo, & Titiunik, 2014). Hence, we employ an RD-package in Stata which implements local polynomial RD point estimators *with* robust biased-corrected confidence intervals and inference procedures[13]. We compute the MSE-optimal bandwidth with equal bandwidth on both sides of the cutoff[14]. This procedure is done with both July 1, 2015, and January 1, 2017, used as cutoffs.

With cutoff at July 1, we obtain an optimal bandwidth of 295 days before and after cutoff. This bandwidth is used for all the RD estimates regarding the LTV policy implemented in 2015. With cutoff at January 1, 2017, we obtain an MSE-optimal bandwidth of 225 days before and after cutoff. This bandwidth is used for all the RD estimates regarding the DTI policy implemented in 2017. The optimal bandwidth is calculated with a polynomial order of one. Calculating the bandwidth with a higher polynomial order would yield a too wide bandwidth, with the bandwidth passing 1,000 days before and after cutoff. If we were to use such a wide bandwidth, the regression would compare a dwelling sold many years before the cutoff with a dwelling sold many years after the cutoff. This would make the comparison of dwellings around the cutoff less credible (Lee & Lemieux, 2010), and we would most likely not find the estimated causal effect of the macroprudential policies. Hence, when calculating the

[13] The Stata package is called rdrobust, and uses local polynomial RD point estimators with robust biased-corrected confidence intervals and inference procedures developed in Calonico et al. (2014), Calonico, Cattaneo, and Farrell (2018), Calonico et al. (2019), and Calonico et al. (2020b). A detailed description of the package can be found in Calonico, Cattaneo, Farrell, and Titiunik (2017).

[14] A companion command to rdrobust is rdbwselect, which offers data-driven bandwidth selection. We use this command to compute the optimal bandwidth, given the covariates and a polynomial order of one. A description of the command can be found in Calonico et al. (2017).

bandwidth, we use a polynomial order of 1. In the main RD analyses, however, we use a higher polynomial order to get a better fit between the data and the RD model.

4.2.3 Covariates

Augmenting the RD model by including covariates allows us to control for effects other than the running variable that might affect the selling price of a dwelling. From the hedonic regression, we know that several housing attributes influence the selling price. Hence, when conducting an RD analysis where the outcome of interest is the selling price of a dwelling, it makes sense to include these attributes in the RD model. There does not seem to be broad consensus around how exactly covariates should be included in RD designs (Calonico et al., 2019). Calonico et al. (2019) advocates the use of simple covariates above and below the cutoff, as long as there is no treatment effect on the covariates at the cutoff. In other words, we will include most of the housing attributes as covariates, on the condition that the covariates themselves do not exhibit any discontinuity at cutoff. This assumption forces us to omit or change some of the dummy variables used in the hedonic regression.

Dummy variables take the value of either one or zero, which can disturb the estimates from the RD analysis. For example, in our RD analysis we use days as the running variable. Including monthly time dummies as covariates would yield biased estimates, since the running variable and the monthly dummies are correlated. In addition to leaving out monthly dummies, the dummy variables for property type, ownership and build year have been omitted. As a substitute, we have used a discrete specification for these variables. The dummy variables for postal codes are substituted with a discrete variable for city district. Even though the dummy variables used in the hedonic regression are better control variables for the selling price than the discrete alternatives, the results from RD design become more reliable as the dummy variables no longer can disturb the estimates. To further assure that the covariates are specified correctly, we have used the results from the hedonic regression and ranked city district and estate types with respect to their effect on selling price. This means that even though the previous dummy variables now are included as linear and discrete variables, the regression interprets the relationship between the dependent variable, and city district and property type as linear and increasing. Even though a discrete specification for these variables is not optimal, we deem the advantages to outweigh the problems with including such variables.

4.2.4 Final specification

In addition to the mentioned specifications above, some other econometrical decisions must be discussed before employing the RD model. First, an important factor when implementing RD designs is choosing the local polynomial estimators. In practice, researchers usually choose a bandwidth, followed by a local linear regression using observations that lie within the chosen bandwidth (Calonico et al., 2019). We use a polynomial order of four to get a better fit between the data and the model. Gelman and Imbens (2019) argue that higher polynomial orders can be troublesome in certain contexts (Pei, Lee, Card, & Weber, 2020). However, higher polynomial orders may have a smaller bias than lower orders (Hahn et al., 2001; Pei et al., 2020). Wilhelmsson (2019), a study that examines a similar dataset as the one used in this study, uses a polynomial order of four in his RD analyses. Using a lower polynomial order could potentially cause most of the estimated coefficients to be statistically significant, as a lower polynomial order may be perceived as a less strict assumption. A higher polynomial order, however, may potentially be too strict, resulting in none of the coefficients being statistically significant. Hence, we believe using the same polynomial order as Wilhelmsson (2019) is a balanced and appropriate specification.

Second, the weighing of the observations is determined by the chosen kernel. The kernel, together with the bandwidth, localize the regression fit near the cutoff (Calonico et al., 2019). We use a triangular kernel, which linearly downweighs the observations. In other words, the observations closest to the cutoff are weighted higher than the ones further from cutoff. The RD model can be expressed as:

$$lnSellingPrice = \alpha + \tau Policy + \beta_1 Date + \beta_2 Policy * Date + Z\theta + \varepsilon \qquad (3)$$

where the dependent variable is the selling price with a logarithmic specification. $Policy$ captures the effect of crossing the chosen cutoff. $Date$ represents the running variable, which is the date of sale. The interaction term allows for a different slope of the regression line before and after cutoff. θ is a vector, representing the different covariates used. ε is the error term. Since we are looking for any discontinuities in the population regression line at cutoff, we are interested in estimating the coefficient τ. It is important to note that model (3) above is a simplified specification of the applied RD model. For the analyses, we use a polynomial order of four, with different slopes on each side of the cutoff, as well as a triangular kernel and MSE-bandwidth. For simplicity, these specifications are not expressed in model (3).

4.2.5 Supplementary RD designs

In addition to the main RD analyses discussed above, we will employ some alternative RD designs to further explore the data and the effect of the macroprudential policies. First, we will use the same RD specification above on different segments in the housing market. We will explore small, medium, and large dwellings separately, examining whether the macroprudential policies have had a different impact on the three segments. Second, we will look for any discontinuities in the hedonic price index. The idea is to use the price index with weekly time dummies in an RD analysis, discovering if there are any discontinuities at the chosen cutoffs. We construct a price index with weekly observations using the method explained in section 4.1.2. Then we use the index as the dependent variable in an RD analysis, with weeks as the running variable. Larsen (2018) also uses a price index with weekly observations as the outcome of interest in an RD analysis. A major advantage when using this method is the simplicity of the RD design. The hedonic price index is already adjusted for the heterogeneity in the housing market. Hence, there is no need to include any covariates in the RD analysis. The index itself takes into account the different attributes. Using the index as the outcome of interest may also provide results that can indicate a more long-term effect, as there are fewer observations closer to cutoff. For this analysis, we will use four of the eight cutoffs mentioned previously. Specifically, we will use the dates of implementation, which are July 1, 2015, and January 1, 2017, in addition to cutoffs three months after the policy implementation.

The third additional RD design approach is to conduct a regression kink (RK) design. RK designs were first mentioned by Nielsen, Sørensen, and Taber (2010), and is an extension of the initial RD designs. As with RD designs, RK designs is based on the idea that a variable of interest receives treatment based on a known assignment rule (Card, Lee, Pei, & Weber, 2017). However, RK designs are used to examine a potential kink at cutoff, instead of a jump. The idea is to compare the slope of the regression population line before the cutoff with the slope after the cutoff. Hence, instead of examining the jump at the cutoff, RK design examines the slope change (Card et al., 2017). This method may contribute to getting a better understanding of the long-term effects of the macroprudential policies. If there is a significant change in the slope of the population regression line, the findings will indicate that the policies have caused the housing price development to either increase or fall. As with RD design, RK design involves using a local polynomial regression, comparing observations close to the cutoff (Card et al., 2017).

Finally, we will implement a fuzzy RD design. As discussed in section 2.2.3, being assigned to the treatment group, in this case all observations above the cutoff, is not always the same as actually receiving treatment. In other words, the probability of being subject to the new lending regulations may not change from 0 to 1 when the running variable reaches the cutoff. For example, the pre-qualification letters discussed previously may cause the buyer to be affected by the new policies three months after the implementation. Sharp RD design, however, assumes that all observations above cutoff receives treatment, and that the probability of receiving treatment jumps from 0 to 1 at the cutoff. Hence, this form of imperfect compliance between treatment assignment may suggest that results from sharp RD design can be called into question. Such imperfect incompliance can be solved by using an instrumental variable that indicates if the cutoff is crossed (Stock & Watson, 2015), which is the case in fuzzy RD designs. In these designs, we include a valid instrumental variable[15] to denote whether treatment was actually received when the running variable reached cutoff (Cattaneo, Idrobo, & Titiunik, 2018). An apparent drawback with our study is that we do not have any data that indicates if the housing transactions actually where subject to the new policies or not. Hence, we do not have an intrumental variable that can indicate whether treatment was received after cutoff. An alternative is to use a dummy variable that indicates if the running variable has reached cutoff. In other words, the instrumental variable in our fuzzy RD design indicates if the date of sale is before or after cutoff. This approach, however, yields identical results as our sharp RD design. Thus, we use a sharp RD design for the rest of the analyses in this study. In section 5.2.6 we display the results from our fuzzy RD analysis and demonstrate that the results from the sharp RD design and the fuzzy RD design are the same.

4.3 Time series analysis

To get an even better understanding of the long-term effect of the macroprudential policies and obtain findings that can support the potential results from the RD design with index and RK

[15] An instrumental variable is considered a valid instrument when; (1) the variable is relevant: $corr\ (Z_i, X_i) \neq 0$, and (2) the variable is exogenous: $corr\ (Z_i, u_i) = 0$ (Stock & Watson, 2015, p. 472).

design, we will employ a time series analysis of the housing price index. To conduct a time series analysis, one could simply study the development of the average monthly price in the housing market and look for trends. However, as argued repeatedly in this paper, average housing prices do not account for the heterogeneity in the housing market. Hence, we use our constructed housing price index with weekly time dummies to identify any stochastic trends in the times series. A trend is a long-term movement over time, and a time series variable can fluctuate around this trend (Stock & Watson, 2015). When working with time series data, non-stationarity, generated e.g. by stochastic trends, can cause the estimated autoregressive coefficient in an AR(1) model to be biased toward zero. In addition, the t-statistic may have a non-normal distribution (Stock & Watson, 2015). Furthermore, building a forecasting model without accounting for trends may cause the forecasted estimates to be wrong.

However, we are not interested in conducting a time series regression to *forecast* the price development in the housing market. We are only interested in discovering whether the index (Y) has a positive stochastic trend during the time period 2010 to 2020, or if the macroprudential policies has caused the index to have a reduced positive trend after the policy implementations in 2015 and 2017. More precisely, we will study if Y_t's distribution changes during the 11-year period. This may tell us if the price index has had a positive trend before the policies were introduced, followed by a weaker positive trend after. Also notice that we are only testing for stochastic trends, not deterministic trends. A deterministic trend would mean that the price index for example would increase by 2 percent every year, which would mean that the index would exhibit a linear trend. It is hard to believe that the price index would linearly increase every year. As we have shown in figures 1 and 3 earlier, the housing price index does not exhibit a linear growth. Hence, we focus on stochastic trends, which are random and varies over time (Stock & Watson, 2015). The following discussion is based on Stock and Watson (2015)'s description of time series regression.

To detect if the index Y contains a stochastic trend, we can test of unit root. A unit root occurs when an AR(1) model contains $\beta_1 = 1$. Consider the following AR(1) model:

$$Y_t = \beta_0 + \beta_1 Y_{t-1} + u_t \tag{4}$$

where Y_t is the series, u_t is the error term, and t refers to the selected time period that is forecasted. Y_{t-1} represents the first lag of the series. Since only one lag is used as a regressor (Y_{t-1}), the model is a first-order autoregression. If the series Y_t follows an AR(1) model with $\beta_1 = 1$, then then series contains a unit root, which means that it contains a stochastic trend. If the series follow an AR(1) model with $|\beta_1| < 1$ and u_t is stationary, then the series does not contain a unit root, and the series does not contain a stochastic trend. Using this theoretical framework, we can determine if the series contains a stochastic trend by testing if $\beta_1 = 1$.

In our study, the series we would like to examine is the housing price index constructed with the hedonic regression with weekly time dummies. We will run two Dickey-Fuller tests to determine if the index contains a unit root, i.e. contains a stochastic trend. The first test will simply test if the index is stationary or non-stationary. Here, the null hypothesis is that the series contains a unit root, and the alternative is that the series is stationary. In the second test, we will specifically test for trend. Here, the null hypothesis is the same as the previous test, but the alternative is that the series is stationary around a *trend*. In other words, we are testing for stationarity *and* trend-stationarity. We expect that the 2017 DTI policy had a dampening effect on the housing market, which means that the index would most likely be trend-stationary before 2017, and only stationary after 2017 since the positive trend has been eliminated or dampened by the DTI policy. The variable "Weeks" is declared as a time series variable, which means that the second week in 2010 is considered as the first lag, third week in 2010 is considered the second lag, and so forth. We split the dataset into three separate intervals. First, to uncover the effect of the LTV policy, we run the Dickey-Fuller test on the time series data from 2010 – 2015, and from 2015 - 2017. Then, we run the same tests for the time frame 2010 – 2017, and from 2017 – 2020, to uncover the effect of the DTI policy.

5 Findings

Our findings are derived from the three main analyses described above. First, the estimated relations in the hedonic model are used to construct the price index for the housing market. Second, the results from the RD designs will provide evidence of the short-term effect of the implementation of macroprudential policies in question, as well as more long-term effects from the supplemental RD and RK designs. Finally, to get an even better understanding of the long-term effects we will look for stochastic trends in the data and examine if there is a difference in trend-stationarity before and after policy implementation. We find that the LTV policy had a dampening but short-term effect on the selling price of dwellings, and that this effect was seen three weeks before the date of implementation. The DTI policy seems to have had a more long-term effect, causing lower price appreciation three months after the policy was imposed.

5.1 Hedonic regression

Even though the main results from this study are obtained from the RD analysis of the housing transaction data, the hedonic model yields supplementary information about the general price development over the past 11 years. In addition, the price index can also be used separately in an RD analysis, examining potential discontinuities in the price index itself. Using model (2) presented in section 4.1.1, we obtain the regression results presented in Table A.4 in Appendix A. In addition to displaying the results from the hedonic regression for all dwellings in Oslo, Table A.4 also contains three additional columns with results for subsets of the housing market. The selected subsets are small, medium, and large dwellings[16]. Hence, we obtain four different regression models, where the standard errors are expressed within the columns, and the number of asterisks represent a 10 percent, five percent, and one percent significance level respectively. The regression for "all dwellings" in column one has an adjusted R squared of 0.893. This suggests that our model can explain almost 90 percent of the variance in selling price. In addition, the relatively high adjusted R squared indicates that the model has an appropriate specification and is a reliable estimate of housing prices.

Looking more closely at the estimates in column one, we can see that all the housing attributes are statistically significant at a five percent significance level, except for a few postal codes and monthly dummy variables. Non-significant postal codes and monthly time dummies are to be expected, since using such detailed variables is extremely specific. For example, one postal code may include only a handful of dwellings. The estimation of the coefficient for this postal code may then be very unspecific due to the lack of observations. In addition, a non-significant postal code may also indicate that it has no statistically significant effect on the price since the selling price for the dwellings within the postal code is close to the average selling price in Oslo. In regard to the non-significant monthly dummy variables, it is important to note that

[16] The intervals are based on Table 5 in section 3.3

only the first four months are not statistically significant. Since we are estimating the price development right before 2015, as well as the years after, we do not consider the non-significant monthly dummies for 2010 as a problem.

Table A.5 in Appendix A, contain the results from a similar hedonic regression, where weekly dummy variables have been used instead of monthly dummy variables. As with the regression with monthly dummy variables, the weekly dummy variables also contain non-significant coefficients the first year. However, we do not consider these non-significant coefficients as a problem since the RD analysis of the price index uses a bandwidth that does not include these coefficients.

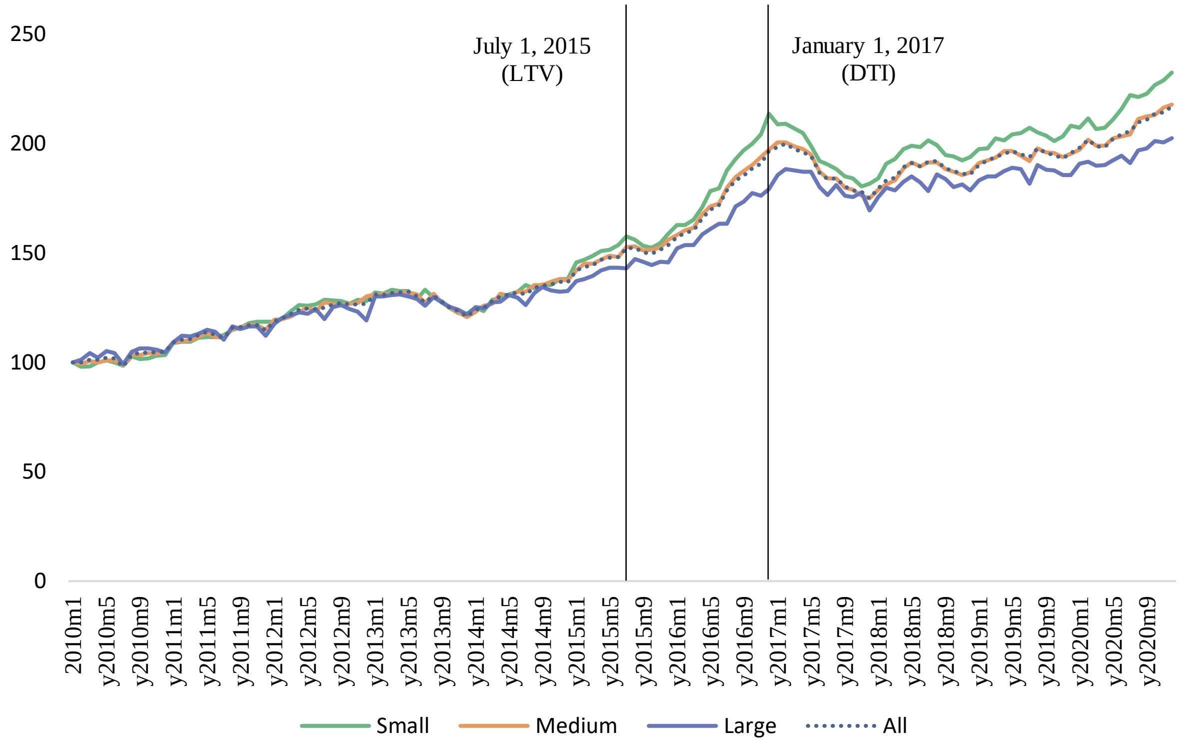
Looking more closely at each attribute's coefficient in the column for all dwellings in Table A.4, one can see that the variable for living area has an estimated coefficient of 0.646. This suggests that a one percent increase in living area size will result in a 0.646 percent increase in selling price, given no change in the other variables. The bedroom variable has a coefficient of -0.086, which indicates an 8.6 percent decrease in selling price when an extra bedroom is added, given no change in the other variables. This confirms our previous belief, that simply increasing the number of bedrooms without increasing the size of the dwelling causes the selling price to fall. The interaction term which adjusts for this effect, however, is positive. Since we are only using the coefficients from the monthly and weekly time dummies further on in the study, we will not give a detailed interpretation of each coefficient in table A.4 and A.5.

It is worth noticing, however, that the coefficients vary between being positive and negative when comparing the different size segments. For example, the coefficient for the bedroom variable is positive for small and large dwellings, while it is negative for medium and all dwellings. These differences are explained by the interaction term between the living area and bedroom variable. Notice that the interaction term is positive when the bedroom variable is negative, and vice versa. For example, the total effect of one extra bedroom given small dwellings, and a living area of 35 square meters, would be 0.026[17]. If we estimate the effect of one extra bedroom given a medium sized dwelling of 52 square meters, we get 0.040[18].

5.1.1 Price index

From the hedonic regression discussed above, we are only interested in the monthly and weekly time dummies. The coefficients for each monthly time dummy from 2010 to 2020 is used to construct the price index. Figure 5 below show price indices for small, medium, and large dwellings in Oslo, as well as all dwellings combined.

Figure 5: Price indices for small, medium, large, and all dwellings



Price indices for small, medium, large, and all dwellings in Oslo, indexed at January 2010. Selected time-period is January, 2010 to December, 2020. Small (22 sqm – 50 sqm), medium (51 sqm – 80 sqm), large (81 sqm +). All dwellings contain all sizes. Price for small dwellings increase by 132 percent, medium dwellings increase by 117 percent, and large dwellings increase by 100 percent. All dwellings increase in price by 115 percent. Vertical lines included for policy dates; July 1, 2015 and January 1, 2017.

Figure 5 shows a steady growth in selling price for all three dwelling types. Small dwellings, which range from 22 sqm to 50 sqm, are the ones with the highest price appreciation over the

[17] Calculation: $0.166 + (-0.0394 * \ln(35)) = 0.02592$
[18] Calculation: $-0.0211 + (0.0155 * \ln(52)) = 0.04014$

sample. From January 2010 to December 2020, the selling price for these dwelling types have appreciated by more than 130 percent. Medium dwellings, ranging from 51 sqm to 80 sqm, and large dwellings, containing all dwellings larger than 81 sqm, have increased in price by 117 percent and 100 percent respectively. All dwellings in total, represented by the dotted line, have increased by 116 percent. One can also see from the graph that the three dwelling types have a very similar price development from 2010 to 2015. However, from 2015, the price development for small and medium dwellings accelerates, creating a gap between the small, medium, and large dwellings. The dates of implementation of the LTV and DTI policies have also been marked by two vertical lines in Figure 5.

From a pure visual investigation, Figure 5 suggests that the DTI regulation had a relatively strong effect on the housing market, pushing the housing prices downwards. After the implementation of the LTV regulation in July 2015, there was a small decrease in price, before the price development continued upwards. Hence, it seems that the DTI regulation had a stronger effect on the price development than the LTV regulation. After the implementation of the DTI policy in January 2017, the price index fell for all three dwelling types. The price index for all dwellings fell by 12.4 percent during a one-year period after the implementation of the DTI policy. The LTV regulation seems to have had a minor effect on the housing market, only dampening inflation for a few months. Specifically, the price index for all dwellings fell by only 1.3 percent shortly after the LTV implementation. Even though the price indices presented suggests a strong price depreciation after the implementation of DTI, we cannot say for sure that the true causal effect came from this macroprudential policy. Hence, in the next section we will present the results from the RD design, which will provide a better view of the estimated causal effect of the LTV and DTI policies.

5.2 Regression discontinuity design

In the following sections, we will present the results from the different RD analyses. First, we will examine the results of the RD analysis of the 2015 LTV policy implementation and the results from the RD analysis of the 2017 DTI implementation. Second, we will examine the results from the segment analyses of small, medium, and large dwellings. Third, we will present the RD analysis of the hedonic price index. Finally, we will uncover potential kinks in the transaction data with the RK design, as well as illustrate the effect of a fuzzy RD design. For all the following RD analyses, we will uncover if the coefficient τ presented in model (3), is statistically significant. The null hypothesis is that no discontinuity exists at the cutoff, while the alternative is that there is discontinuity at the cutoff. In other words, we can reject the null hypothesis of no discontinuity if the coefficient is statistically significant[19].

Before including several cutoffs, we will present the results from the sharp RD design for July 1, 2015 and January 1, 2017, with no covariates, as well as six additional regressions where one additional covariate is included for each regression. By including covariates, we can control for effects other than the running variable that might affect the selling price of a dwelling. Furthermore, this allows us to obtain the causal effect on price, ceteris paribus. However, it might be interesting to see what the total effect of the macroprudential policies is without including any covariates. The LTV and DTI policy may have caused a shift in the buyers' preferences. For example, the stricter LTV ratios imposed in 2015 may have caused buyers to demand smaller dwellings. If this was the case, the total effect on selling price would be that the price would drop right after the LTV implementation since smaller dwellings became more popular. The same idea can be applied to the other housing attributes. In other words, when not controlling for covariates, we can obtain the total effect on selling price. However, the total effect on price may simply be a result of changed preferences among buyers. Thus, to isolate the effect on selling price while holding everything else equal, we can include the housing attributes as covariates. Since the covariates may be confounding variables, we may expect to see a smaller discontinuity in the RD analysis when covariates are included compared to when no covariates being included.

The results from the RD analyses with and without covariates are shown in Table 6 below. We can see that the coefficient for July 1, 2015, show a statistically significant decrease in selling price following the LTV implementation when no covariates are included. This suggests that the total effect on price when not controlling for any changed preferences among buyers, is negative. Further, we can see that the covariates included influence the coefficients and the respective statistics. Including the bedroom variable as a covariate reduces the absolute value of the coefficient, which suggests that the number of bedrooms accounts for some of the

[19] In the following, will not present the null hypothesis and alternative hypothesis for each RD analysis conducted, as the hypotheses are the same for all the RD analyses. The RK design uses the same hypothesis, only substituting discontinuity with a kink at the cutoff.

variation in selling price. The covariate that accounts for living area, greatly affects the coefficient and the standard error. The coefficient changes from negative to positive when the living area variable is included. In addition, the coefficient is no longer statistically significant. This may indicate that the LTV restriction caused a shift in preferences for what type of dwellings buyers demand. The coefficient remains positive for all other covariates included.

Table 6: Results from RD analyses of policy implementation with different covariates

July 1, 2015

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Coefficient	-0.139***	-0.051**	0.009	0.014	0.014	0.014	0.017
Std error	0.030	0.025	0.022	0.019	0.018	0.018	0.015
z-value	-4.622	-2.014	0.409	0.739	0.755	0.756	1.142
p-value	0.000	0.044	0.683	0.460	0.450	0.450	0.253
Bandwidth	295	295	295	295	295	295	295
Order-est.	4	4	4	4	4	4	4

January 1, 2017

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Coefficient	-0.041	-0.022	-0.017	-0.013	-0.022	-0.021	-0.038
Std error	0.054	0.048	0.041	0.036	0.036	0.036	0.027
z-value	-0.758	-0.459	-0.417	-0.373	-0.600	-0.579	-1.435
p-value	0.448	0.646	0.677	0.709	0.548	0.563	0.151
Bandwidth	225	225	225	225	225	225	225
Order-est.	4	4	4	4	4	4	4

(1)	No covariates
(2)	Bedroom
(3)	Bedroom, ln(Living Area)
(4)	Bedroom, ln(Living Area), Ownership
(5)	Bedroom, ln(Living Area), Ownership, ln(Build Year)
(6)	Bedroom, ln(Living Area), Ownership, ln(Build Year), Property Type
(7)	Bedroom, ln(Living Area), Ownership, ln(Build Year), Property Type, City District

Table 6 shows results from seven different RD designs with two cutoffs (July 1, 2015, and January 1, 2017) and different covariates. The dependent variable is the selling price with a logarithmic specification, and the running variable is the date of sale. The regressions with July 1, 2015, as cutoff are estimated with an MSE-optimal bandwidth of 295, while the regression with January 1, 2017, as cutoff are estimated with an MSE-optimal bandwidth of 225. A polynomial order of four, and a triangular kernel is used for all estimates. All RD analyses in this study has been conducted using the rdrobust package in Stata based on Calonico et al. (2017).

When looking at the results from the January 1, 2017 cutoff, the covariates also influence the coefficients. However, the coefficients remain negative, as well as not statistically significant. From the estimates displayed in Table 6, we can arrive at two conclusions. First, the LTV policy imposed in July 2015 had a significant impact on the selling price of dwellings, causing the prices to fall shortly after the policy was imposed. Second, the negative effect disappears when including covariates, which suggests that the decrease in price may have been a result of changed preferences among buyers. Even though the covariates greatly affect the outcome of the RD analyses, we have established from the hedonic regression that the variables are important control variables when analyzing the selling price in the housing market. Furthermore, this study tends to uncover the effect of macroprudential policies on housing prices, not on buyers' preferences. Hence, we include all covariates in the remaining RD designs to isolate the effect of selling price, ceteris paribus.

5.2.1 The 2015 LTV regulation

Table 7 below contains the main results from the RD analysis of the LTV implementation in 2015, with four different cutoffs. The first cutoff is set to three weeks before July 1. August 15 is one and a half months after the implementation, while October 1 is three months after. An optimal bandwidth of 295 is used, based on the MSE-optimal bandwidth approach. To get a better fit between the RD-model and the data, we have used a polynomial order of 4 in the main RD analysis. Finally, a triangular kernel is used. The number of observations refers to the number of total observations in the dataset before and after the chosen cutoff. The effective number of observations refers to the actual number of observations within the given bandwidth. The number used on each side of the cutoff varies between 6,000 and 9,000 observations.

Table 7: Results from RD analyses of the 2015 LTV policy

	June 10	July 1	August 15	October 1
Coefficient	-0.051***	0.017	-0.017	0.003
Std error	0.011	0.015	0.010	0.011
z-value	-4.875	1.142	-1.590	0.235
p-value	0.000	0.253	0.112	0.814

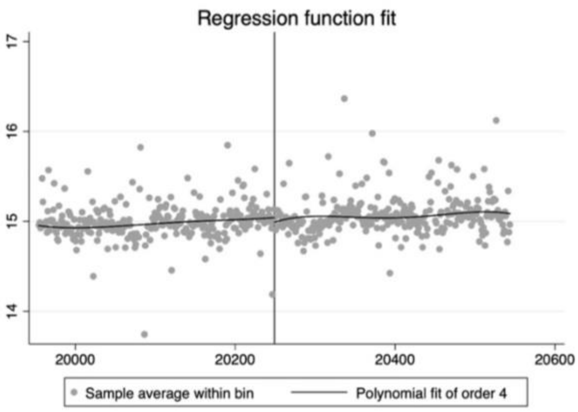
	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff
No of obs.	87,309	93,504	88,580	92,233	89,686	91,127	92,687	88,126
Eff no of obs.	14,709	12,565	14,602	12,586	13,236	14,048	14,226	12,938
Bandwidth	295	295	295	295	295	295	295	295
Order-est.	4	4	4	4	4	4	4	4

The table presents the main results from a sharp regression discontinuity design. Four cutoffs have been used, where the first cutoff on June 10 is three weeks before the introduction of LTV policy on July 1. The dependent variable is ln(Selling Price), and the running variable is the date of sale. An MSE-optimal andwidth of 295 days has been used before and after cutoff, with a polynomial order of four and a triangular kernel. All six covariates have been used, namely Bedroom, ln(Living Area), Ownership, ln(Build Year), Property Type, and City District. The only statistically significant coefficient at a five percent significance level is for June 10, where there is a decrease in selling price.

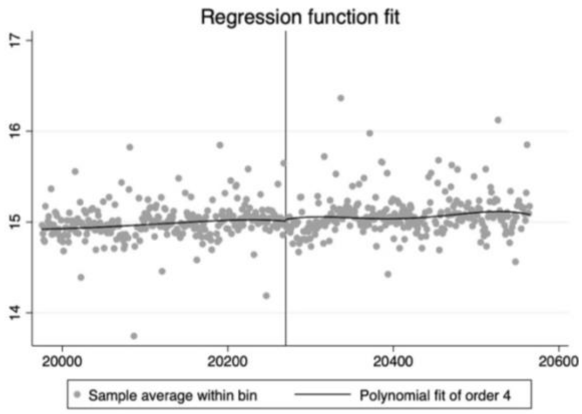
The only coefficient that is statistically significant at the five percent level is the coefficient for the June 10 cutoff. In the previous section, we established that the coefficient for July 1 was statistically significant when no covariates where included, while not being statistically significant when all the covariates where included. Now, however, when using all the covariates and several cutoff dates, the effect of the policy seems to be seen already three weeks *before* the implementation. This may suggest that the LTV policy had a dampening effect on the housing prices three weeks before the policy took effect. The remaining cutoffs used yield no statistically significant coefficients at a five percent significance level. To summarize, the results from the RD analyses suggest that the LTV policy had a negative effect on the selling price of dwellings on June 10 when controlling for covariates. When no covariates are included, the analyses from the previous section in Table 6 indicate that a negative effect on selling price was seen at the date of the policy implementation, while also indicating a shift in the buyers' preferences. These findings suggest that the policy had a dampening effect on the housing prices three weeks before implementation, while also causing a shift in preferences at the date of implementation, which again caused the housing prices to fall.

A major advantage of RD designs is that the results can be illustrated graphically. Using RD plots compliments the RD analysis conducted above, and adds transparency to the results (Cattaneo, Idrobo, et al., 2019). Figure 6 contains a graphical presentation of the findings in Table 7 above. Figure 6 a) shows the price development within 295 days of the first cutoff, June 10. The graph confirms the statistically significant coefficient found above. At the cutoff, there is a significant jump down, creating a discontinuity in the regression population line. In section 5.3 we will conduct several specification tests to examine whether the discontinuity found on June 10 is a result of the LTV implementation, or if the result from the RD analysis is influenced by other measures, such as bandwidth selection and covariate selection. Figure 6 c) also show a small discontinuity at cutoff. Figure 6 b) and d) do not show any significant discontinuities at the cutoff, which again confirms the findings in Table 7 above. Even though the use of RD plots is informative and helpful to understand the concept of RD design, one should not draw final conclusions from these plots as graphs can give a wrong impression of the true result (Lee & Lemieux, 2010). Hence, we only use the RD plots to illustrate the effect of the macroprudential policies. The statistical results used to form a conclusion are retrieved from tables.

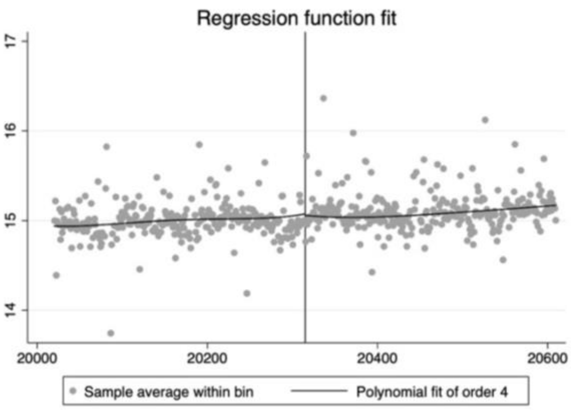
Figure 6: RD plots for the 2015 LTV policy



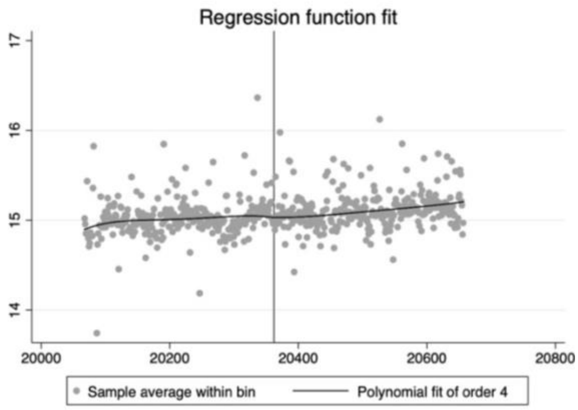
a) June 10



b) July 1



c) August 15



d) October 1

Figure 6 graphically illustrates the sharp regression discontinuity analysis for all dwellings in 2015. Figure a) – d) show the development in price before and after the cutoffs in June, July, August, and October respectively. The dependent variable is ln(Selling price), and the running variable is days[20]*. Bandwidth of 295 is used, with a polynomial order of four and triangular kernel. All six covariates have been used. The significant coefficient from Table 7 can be seen in a), where there is a larger jump (discontinuity) at the cutoff.*

5.2.2 The 2017 DTI regulation

For the RD analysis of the 2017 DTI implementation, we also use four different cutoffs, as well as including all six covariates. The first cutoff is set to December 11, which is three weeks before the implementation of the DTI policy. As with the RD analyses discussed in the previous section, cutoffs a month and a half and three months after policy implantation is used to control for delayed effects. Table 8 below displays the results of the RD analysis. We use an MSE-optimal bandwidth of 225 days before and after the cutoff, a polynomial order of four, and a triangular kernel. The first two coefficients are negative, indicating a decrease in price after the cutoffs. As with the RD analysis conducted above, there was a small drop in price three weeks before the DTI implementation. However, none of the coefficients are statistically significant. Hence, we cannot say with statistically certainty that the prices dropped or increased given the four cutoffs.

[20] The X-axis represents the date of sale. 20270 corresponds to July 1, 2015. The specific number that corresponds to the specific date used as cutoff is not relevant, as the date is expressed explicitly below each plot.

Table 8: Results from RD analyses of the 2017 DTI policy

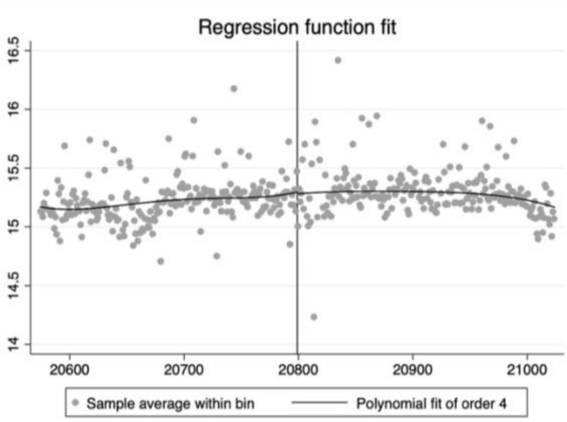
	December 11	January 1	February 15	April 1
Coefficient	-0.016	-0.038	0.003	0.007
Std error	0.017	0.027	0.013	0.012
z-value	-0.900	-1.435	0.209	0.607
p-value	0.368	0.151	0.835	0.544

	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff
No of obs.	112,145	68,668	112,338	68,475	114,091	66,722	116,166	64,647
Eff no of obs.	10,399	9,040	9,679	9,527	8,655	10,414	9,531	10,477
Bandwidth	225	225	225	225	225	225	225	225
Order-est.	4	4	4	4	4	4	4	4

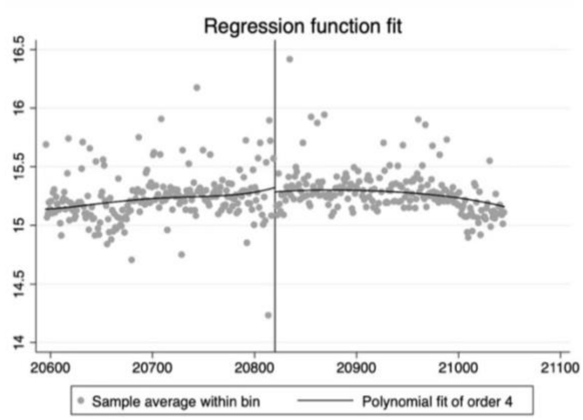
Table 8 presents the main results from a sharp regression discontinuity design. Four cutoffs have been used. The dependent variable is ln(Selling Price), and the running variable is the date of sale. Bandwidth of 225 days has been used before and after cutoff, with a polynomial order of four and a triangular kernel. All six covariates have been used, namely Bedroom, ln(Living Area), Ownership, ln(Build Year), Property Type, and City District.

Figure 7 below displays the RD results graphically. Figure 7 b) show a small discontinuity at the cutoff. This confirms the findings in the table above, even though the discontinuity is not statistically significant. Figures 7 a), c), and d) on the other hand show smaller or no discontinuity at the cutoffs. The findings are somewhat conflicting with the indications obtained from the hedonic price index in section 5.1.1. The price indices for small, medium, large, and all dwellings displayed in Figure 5 show a large drop in price following the 2017 DTI implementation. We expected to see a similar result from the RD design, with a negative and statistically significant coefficient. The results from the RD design may suggest that the effect of the DTI policy was not seen instantly, and that the DTI policy caused a more long-term effect not captured by the RD design. For example, the pre-qualification letters may have delayed the effect of the policy.

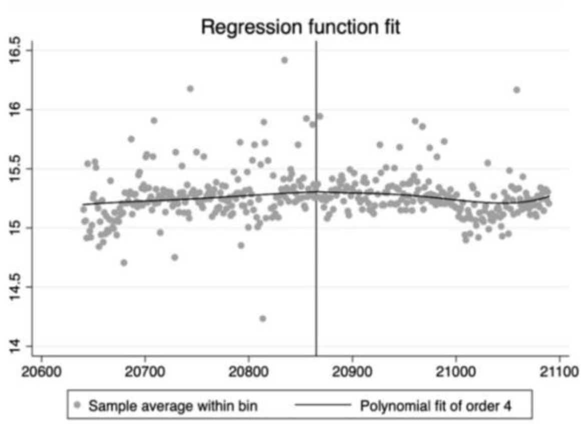
Figure 7: RD plots for the 2017 DTI policy



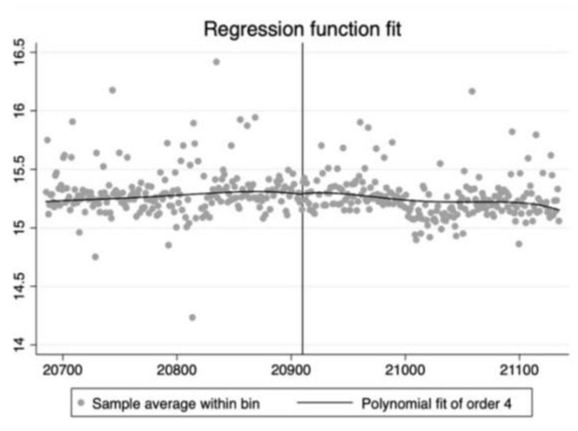
a) December 11



b) January 1



c) February 15



d) April 1

Figure 7 graphically illustrates the sharp regression discontinuity analysis for all dwellings in 2017. Figure a) – d) show the development in price before and after the cutoffs in December 2016, January 2017, February 2017, and April 2017 respectively. The dependent variable is ln(Selling price), and the running variable is days[21]. Bandwidth of 225 is used, with a polynomial order of four and triangular kernel. All six covariates have been used. None of the discontinuities are statistically significant.

The findings in the previous sections both supports and contradicts some the findings from previous academic research. Igan and Kang (2011) suggest that the LTV policy had a stringer effect than the DTI policy in the housing prices in Korea. From the analyses done so far in this study, we obtain a similar indication. Only the LTV policy seems to have had a significant effect on the housing prices. On the other hand, Igan and Kang (2011) also found that LTV policy slowed down the price appreciation in the Korean housing market within six months

[21] The X-axis represents the date of sale. 20820 corresponds to January 1, 2017. The specific number that corresponds to the specific date used as cutoff is not relevant, as the date is expressed explicitly below each plot.

after policy implementation. Our findings, however, indicate that the LTV policy imposed in Norway had a more immediate effect, causing the prices to fall three weeks before the policy was implemented. In the next sections, we will conduct further analyses to uncover any effects not captured by the RD analyses above.

5.2.3 Small, medium, and large dwellings

In addition to studying the housing market as a whole and looking at all dwellings in Oslo simultaneously, it can be informative to conduct separate RD analyses for small, medium, and large dwellings. We use the same intervals based on the size of living area as the intervals presented in Table 5. The results from the RD analysis of the 2015 LTV policy is shown in Table 9 below. The results from the RD analysis of the 2017 DTI policy can be found in Table A.6 in Appendix A. The RD analysis of the 2015 LTV implementation confirms our previous findings. All three dwelling types had a significant reduction in selling price three weeks before the policy implementation. The selling price for small dwellings had a larger discontinuity, with a coefficient of -0.064, compared to the coefficients for medium and large dwellings, which had values of -0.043 and -0.050 respectively. Hence, the LTV policy seems to have had a stronger effect on smaller dwellings than medium and large dwellings. This suggests that the buyers of smaller dwellings, such as first-time buyers and individuals buying alone, were more affected by the LTV policy than the buyers of medium and large dwellings. The LTV policy can potentially prevent someone with less equity to obtain a mortgage. Households with more equity are still most likely able to obtain a mortgage, even though the mortgage might be smaller. In regard to the 2017 DTI policy implementation, the segment findings in Table A.6 support the previous findings. None of the coefficients are statistically significant. Hence, we cannot say with statistical certainty that the DTI policy has had any effect in the different dwelling types.

Table 9: Results from RD analyses of different segments

Small dwellings

	June 10	July 1	August 15	October 1
Coefficient	-0.064***	0.013	-0.034*	-0.001
Std error	0.018	0.026	0.018	0.022
z-value	-3.533	0.508	-1.928	-0.065
p-value	0.000	0.612	0.054	0.949

	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff
No of obs.	19,810	22,796	20,117	22,489	20,524	22,082	21,274	21,332
Eff no of obs.	3,382	3,169	3,363	3,158	3,216	3,346	3,548	3,095
Bandwidth	295	295	295	295	295	295	295	295
Order-est.	4	4	4	4	4	4	4	4

Medium dwellings

	June 10	July 1	August 15	October 1
Coefficient	-0.043***	0.024	-0.019	-0.007
Std error	0.014	0.018	0.014	0.015
z-value	-3.095	1.341	-1.355	-0.447
p-value	0.002	0.180	0.175	0.655

	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff
No of obs.	41,726	44,292	42,318	43,700	42,824	43,194	44,177	41,841
Eff no of obs.	6,949	5,884	6,919	5,895	6,266	6,605	6,641	6,092
Bandwidth	295	295	295	295	295	295	295	295
Order-est.	4	4	4	4	4	4	4	4

Large dwellings

	June 10		July 1		August 15		October 1	
Coefficient	-0.050**		-0.005		0.023		0.010	
Std error	0.022		0.040		0.025		0.023	
z-value	-2.337		-0.137		0.916		0.450	
p-value	0.019		0.891		0.360		0.653	
	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff
No of obs.	25,773	26,416	26,145	26,044	26,338	25,851	27,236	24,953
Eff no of obs.	4,378	3,512	4,320	3,533	3,754	4,097	4,037	3,751
Bandwidth	295	295	295	295	295	295	295	295
Order-est.	4	4	4	4	4	4	4	4

Table 9 presents the results from a sharp regression discontinuity design on small, medium, and large dwellings. Four cutoffs have been used. The dependent variable is ln(Selling Price), and the running variable is the date of sale. Bandwidth of 295 days has been used before and after cutoff, with a polynomial order of four and a triangular kernel. All six covariates have been used.

5.2.4 RD design with index

An alternative to conducting an RD analysis on the transaction data is to use the price index as the outcome of interest. Larsen (2018) uses an RD design with both a hedonic time-dummy index and a weekly average price as the outcome variables. Similarly, we use our hedonic price index with weekly time dummies to examine if there is a discontinuity in the index around the time of policy implementation. We have chosen four different cutoffs, with weeks as the running variable. Table 10 below displays the results from the RD analysis with the index. We have conducted the RD analysis of the four cutoffs with MSE-optimal bandwidths, in addition to a bandwidth that reflects the previous used bandwidths. In the previous RD analyses, we have used a bandwidth of 295 days for cutoffs regarding the 2015 LTV policy, and a bandwidth of 225 days for cutoffs regarding the 2017 DTI policy. In the index analysis, we use weeks as the running variable. The number of weeks that correspond to 295 and 225 days are 44 and 32 weeks. The MSE-optimal bandwidth is 31 and 37 weeks for the two policies respectively. From Table 10 we can see that none of the coefficients are statistically significant. Hence, we cannot say for sure that there is a discontinuity at the cutoffs. However, the findings may *indicate* that the DTI policy had a negative impact on the selling price three months after the implementation. This may contribute to our belief that the effect of the DTI policy was not seen before a few months after the day of implementation. Still, we cannot interpret these results with any statistical certainty.

Table 10: Results from RD analyses with the price index as the outcome of interest

2015 (LTV)

	July 1		**October 1**	
	MSE optimal	**42 weeks**	**MSE optimal**	**42 weeks**
Coefficient	9.167	13.769	8.313	9.404
Std error	12.781	10.61	9.015	7.703
z-value	0.717	1.300	0.922	1.221
p-value	0.473	0.194	0.356	0.222

	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff
No of obs.	286	286	286	286	299	273	299	273
Eff no of obs.	30	31	41	42	30	31	41	42
Bandwidth	31	31	41	41	31	31	41	41
Order-est.	4	4	4	4	4	4	4	4

2017 (DTI)

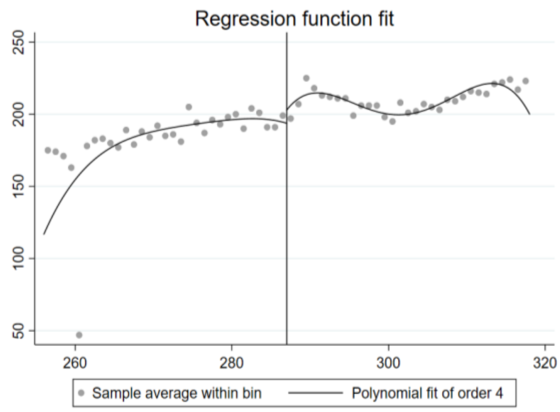
	January 1		**April 1**	
	MSE optimal	**32 weeks**	**MSE optimal**	**32 weeks**
Coefficient	51.220	64.668	-6.092	-4.302
Std error	68.023	77.665	11.584	13.443
z-value	0.753	0.833	-0.526	-0.320
p-value	0.451	0.405	0.599	0.749

	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff
No of obs.	364	208	364	208	376	196	376	196
Eff no of obs.	36	37	31	32	36	37	31	32
Bandwidth	37	37	32	32	37	37	32	32
Order-est.	4	4	4	4	4	4	4	4

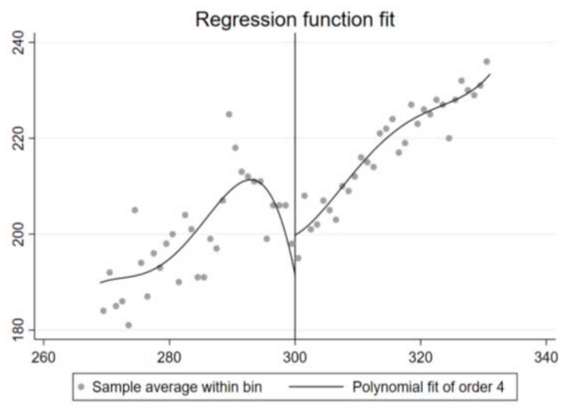
Results from the RD design with the price index as the outcome of interest, and weeks as the running variable. MSE-optimal bandwidth has been used, which yields a bandwidth of 31 for 2015 and 37 for 2017. In addition, bandwidths which reflect 295 and 225 days have been used. These are 42 weeks and 32 weeks respectively. The running variable ranges from 1-450, where 1 is the first week in January 2010. The regressions are estimated with a triangular kernel and a polynomial order of four.

Figure 8 below shows the RD plots with the index as the dependent variable and weeks as the running variable. Using the index as the dependent variable one can clearly see the fluctuations in price. Figure a) and b) show RD-plots with cutoffs at July 1, 2015 and October 1, 2015 respectively. One cannot see any clear discontinuity in either of the graphs. Figure c) shows a much clearer discontinuity in the price index. However, if we recall the results from the main RD analysis of the DTI policy in Table 8, the coefficient for January 1 was negative. Figure c) show a *positive* jump at the cutoff. This contradicts the findings in Table 8 above. However, the previous findings with January 1 as a cutoff yielded no statistically significant coefficient. Furthermore, we cannot say with any statistically certainty that the positive jump in the price index is statistically significant.

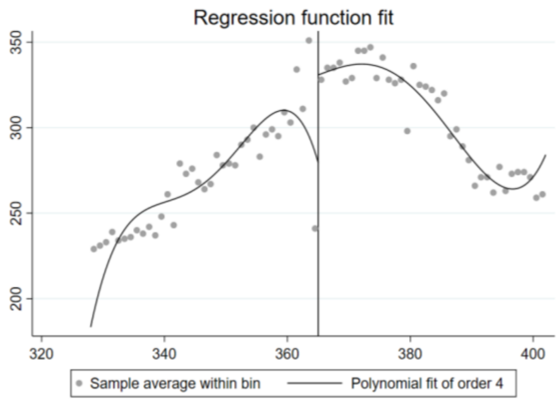
Figure 8: RD plots with index



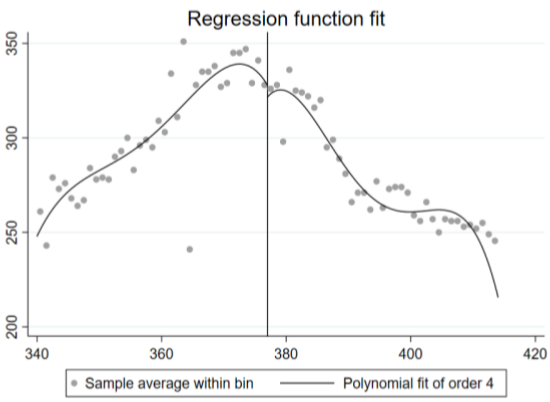
a) July 1



b) October 1



c) January 1



d) April 1

RD-plots with the price index as the outcome of interest, and weeks as the running variable. MSE-optimal bandwidth has been used for each graph, which yields a bandwidth of 31 for 2015 and 37 for 2017. The running variable ranges from 1-450, where 1 is the first week in January 2010. The graphs are estimated with a triangular kernel and a polynomial order of four.

It is important to note that the chosen specification of the RD design can potentially affect the results. For all the RD analyses conducted in this study, we use a polynomial order of four and a triangular kernel. As previously argued, we believe such a specification is appropriate for our analyses. However, we can use Figure 8 above to illustrate that a different polynomial order would potentially have led to a completely different result. For example, looking at Figure 8 d), we can see that the population regression line drops right before the cutoff. The observations on the left side of the cutoff, however, seems to be close to linear. If we were to use a linear specification, the population regression line would be linear and most likely continue upwards instead of falling right before cutoff. The discontinuity between the two population regression lines at the cutoff would then become larger, and we might have obtained a statistically significant and negative coefficient, indicating a decrease in price development at the cutoff. The same logic can be applied to the other plots in Figure 8. This illustrates the sensitivity of RD designs and their specification. Based on our previous discussion on the selected polynomial order, however, we believe that a polynomial order of four is the correct specification for the RD analyses conducted in this study.

5.2.5 Regression kink design

The final RD design that can provide a better understanding of the long-term effects of the lending regulations is the regression kink (RK) design. In Table 11 below we display the results from the RK design. The coefficient no longer represents a jump at cutoff. Instead, it shows the change in the regression population line before and after cutoff. We use the same covariates and bandwidths as the ones we use in the main RD design. The polynomial order, however, is now one. Since we are looking for a change in slope, and the objective is to achieve a better understanding of the long-term effects, a linear population regression line will better describe the long-term effect of the policies, i.e. if the price development increases or decreases before or after the policy implementation. As with the RD analysis of the index, we have used four different cutoffs.

Table 11: Results from RK design

	July 1, 2015	October 1, 2015	January 1, 2017	April 1, 2017
Coefficient	-0.0005***	0.00028**	0.00025	-0.0007***
Std error	0.00014	0.00012	0.00021	0.00019
z-value	-3.5599	2.3248	1.1704	-3.71096
p-value	0.000	0.020	0.242	0.000

	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff
No of obs.	88,580	92,233	92,687	88,126	112,338	68,475	116,166	64,647
Eff no of obs.	14,602	12,586	14,226	12,938	9,679	9,527	9,531	10,477
Bandwidth	295	295	295	295	225	225	225	225
Order-est.	1	1	1	1	1	1	1	1

The table shows the results from the sharp RK design. Four cutoffs have been used, two for each policy implementation. For the July and October cutoffs, a bandwidth of 295 has been used, together with a polynomial order of one and a triangular kernel. For the January and April cutoffs, a bandwidth of 225 has been used, together with a polynomial order of one and a triangular kernel. All covariates are included in the regression.

Of the four RK analyses, three yield statistically significant results. First, the coefficient for July 1, 2015, is negative and statistically significant. This indicates that the positive trend in selling price seen before July 2015 dropped after the implementation of the LTV policy. Second, the coefficient for October 1, 2015, is positive and statistically significant. This result indicates that the price development picked back up and continued to increase within a three-month period after the LTV implementation. This finding suggests that the effect of the LTV policy is short-lived. The finding also supports our previous estimates from the sharp RD designs with several cutoffs, where we obtained a significant decrease in price three weeks before the date of implementation and at the date of implementation. This, together with the RK estimates, suggests that the LTV policy had a short-term effect on the housing prices, since the housing prices fell around the date of policy implementation and increased three months later. The third coefficient in Table 11, which represents the date of the DTI policy implementation, is not statistically significant. Hence, the DTI policy does not seem to have had an immediate effect on the price development. The last coefficient is negative and statistically significant. This indicates that the effect of the DTI policy was not seen before three months after the policy implementation.

These results support some of the findings done in previous studies. Armstrong et al. (2019) found that the effects of the LTV policy implemented in New Zealand was short lived. Igan and Kang (2011) find that the price appreciation slows down within six months after the LTV and DTI policy implementation. This study shows similar findings. However, we have also established that the LTV policy and DTI policy had different effects on the price development in the housing market. The findings from the RK design, together with indications from the RD design with the price index, suggest that the effect of the DTI policy was not seen before three months after implementation. The main RD analyses in section 5.2.1 suggest that the effect of the LTV policy was seen three weeks before implementation and at the date of implementation, and that the effect was short lived since the RK estimates indicate an increase in price development three months later.

5.2.6 Fuzzy RD design

All the analyses above are based on a *sharp* RD design. However, as argued previously in this paper, the problem statement posed in this paper can also be studied using a *fuzzy* RD design. When there is imperfect compliance between treatment assignment, fuzzy RD can be a better approach. Fuzzy RD design uses an instrumental variable that indicates if the cutoff is crossed (Stock & Watson, 2015). We do not have any data or variables that indicates if the housing transactions where subject to the new policies or not. Thus, the only variable we can use that indicates if the cutoff has been crossed is a dummy variable that indicates if the running variable has reached the cutoff. In other words, the instrumental variable in our fuzzy RD design indicates if the date of sale is before or after cutoff. As argued previously, this approach yields identical results as the sharp RD design. To illustrate this assertion, we have conducted a fuzzy RD design for two of the cutoff dates. The results are displayed in Table 12 below. If we compare the results from the table below with the results in tables 7 and 8, we can see that all the findings are identical. Hence, given the data and variables used in this study, using a fuzzy RD design does not provide any additional information about the effects of the LTV and DTI policy.

Table 12: Results from a fuzzy RD design

	July 1, 2015		January 1, 2017	
Coefficient	0.017		-0.038	
Std error	0.015		0.027	
z-value	1.142		-1.435	
p-value	0.253		0.151	
	Before cutoff	After cutoff	Before cutoff	After cutoff
No of obs.	88,580	92,233	112,338	68,475
Eff no of obs.	14,602	12,586	9,679	9,527
Bandwidth	295	295	225	225
Order-est.	4	4	4	4

Table 12 shows the results from a fussy RD design. For the July cutoff, a bandwidth of 295 has been used, together with a polynomial order of four and a triangular kernel. For the January cutoff, a bandwidth of 225 has been used, together with a polynomial order of four and a triangular kernel. All covariates are included in the regression. The results are identical to the results from the sharp RD design.

An alternative to the approach above is to use an instrumental variable in the fuzzy RD design that indicates how many days have passed since the implementation of the macroprudential policy. If, for example, 100 days have passed, it is more likely that the housing transactions at this point are subject to the new lending regulations. Constructing such an instrumental variable, however, is beyond the scope of this paper. For future research on this topic, such implementation of instrumental variables for a fuzzy RD design may be a promising approach.

5.3 Specification tests

One of the main advantages of RD designs is that the mechanism used to assign treatment is known and observable, allowing the researcher to be objective when separating treatment groups from control groups (Cattaneo, Idrobo, et al., 2019). However, using an exogenous cutoff to determine treatment and control groups is not always enough to guarantee that the RD results are valid. For example, when the DTI policy was implemented on January 1, 2017, the housing transactions before this date where exempt from DTI regulation. Hence, buyers and sellers, especially buyers, in the housing market may have wished to complete the transaction before January 1. This might have led to a considerable rise of transactions in December, followed by a drop in transactions in January. This can influence the distribution of the assignment variable around cutoff, which can further manipulate the results from the RD design. One approach to asses if there is a possibility of manipulation of the assignment variable is to check its distribution and density (Lee & Lemieux, 2010). In addition to conducting a density test, Cattaneo, Idrobo, et al. (2019) discusses five empirical validation tests to evaluate the reliability of the RD results. In the following four sections we will conduct four of these five tests.

5.3.1 Covariate test

One of the most important tests when assessing the reliability and validity of RD designs involves testing all covariates the same way as the outcome of interest (Cattaneo, Idrobo, et al., 2019). Lee and Lemieux (2010) also suggests conducting a parallel RD analysis on the covariates as an important step when interpreting the RD results. The main idea behind this test is to identify any discontinuities in the covariates that may influence the results from the main RD analysis. If the covariates correlate strongly with the outcome of interest around the cutoff and are discontinuous at the cutoff, the discontinuity in the covariate will affect the discontinuity in the outcome of interest. In our case, we know that the housing attributes used in the hedonic regression statistically affect the selling price of the dwellings. Thus, if one of the covariates exhibit a discontinuity at the cutoff, the discontinuity in selling price may simply be a result of the covariate's discontinuity. This may result in the RD results from the main analyses being called into question. In addition, we have previously established that the LTV policy may have caused a shift in the buyers' preferences. This shift can potentially be captured by a covariate test. If there is a strong shift in preferences, some of the covariates may exhibit a discontinuity at the cutoff.

Table 13 below displays the results from the covariate tests for all eight cutoff dates. We can see that there are three statistically significant coefficients with July 1 as cutoff. This further confirms our previous belief, that there was a clear shift in the buyers' preferences at the time of the LTV policy implementation. From the main RD analyses with all covariates included (tables 7 and 8), we only obtained one statistically significant coefficient, which was the June 10 cutoff. The covariate test below shows no discontinuities in the covariates at the June 10 cutoff, which strengthens our conclusion of a price drop on June 10.

Table 13: Covariate test

2015 (LTV)

	June 10, 2015		July 1, 2015		August 15, 2015		October 1, 2015	
	Coefficient	**p-value**	**Coefficient**	**p-value**	**Coefficient**	**p-value**	**Coefficient**	**p-value**
Bedroom	0.040	0.456	-0.303***	0.000	0.283***	0.000	-0.133**	0.024
ln(Living Area)	0.020	0.401	-0.193***	0.000	0.138***	0.000	-0.074***	0.006
Ownership	0.031	0.565	-0.138*	0.07	0.133**	0.027	-0.030	0.628
ln(Build Year)	0.001	0.416	-0.001	0.544	0.003**	0.029	0.000	0.951
Property Type	-0.004	0.913	-0.158***	0.000	0.109***	0.001	-0.078*	0.059
City District	-0.414	0.115	-0.059	0.871	0.212	0.446	0.418	0.163

2017 (DTI)

	December 11, 2016		January 1, 2017		February 15, 2017		April 1, 2017	
	Coefficient	**p-value**	**Coefficient**	**p-value**	**Coefficient**	**p-value**	**Coefficient**	**p-value**
Bedroom	0.053	0.596	-0.072	0.599	-0.017	0.804	-0.026	0.695
ln(Living Area)	-0.030	0.497	-0.034	0.546	-0.016	0.632	-0.022	0.457
Ownership	0.032	0.758	-0.048	0.741	-0.095	0.218	-0.077	0.275
ln(Build Year)	0.000	0.815	-0.004	0.24	-0.001	0.375	-0.000	0.884
Property Type	-0.062	0.338	0.089	0.229	-0.006	0.893	-0.009	0.847
City District	-0.110	0.828	0.567	0.432	-0.254	0.501	-0.420	0.222

Table 13 displays the results from the covariate tests, with cutoffs at June 10, 2015, July 1, 2015, August 15, 2015, October 1, 2015, December 11, 2017, January 1, 2017, February 15, 2017, and April 1, 2017. For the 2015-cutoffs, a bandwidth of 295 has been used. For the 2017-cutoffs a bandwidth of 225 has been used. A polynomial order of four and a triangular kernel has been used.

Some of the covariates show discontinuities at the August 15 and October 1 cutoffs. None of the sharp RD analyses yielded any statistically significant coefficients at these two cutoffs. However, the RK design indicated a positive development in price at the October 1 cutoff. Since the same covariates where used in the RK design, the results from the RK design should be interpreted somewhat more carefully, as the covariates may have influenced the outcome. In addition, the significant coefficients in Table 13 above may also indicate that the shift in preferences among buyers continued within three months after the LTV implementation.

5.3.2 Density test

The second specification test examines the density of the running variable. The idea is to assess the possibility of any manipulation of the running variable (Lee & Lemieux, 2010), and reveal if the number of observations below the cutoff is significantly different from the number of observations above cutoff. If the units examined are not able to precisely manipulate the value of the score they receive, the number of observations just above and below the cutoff should be approximately the same (Cattaneo, Idrobo, et al., 2019). In our case, the number of housing transactions just above and below the cutoff should be approximately the same. If, for example, the buyers in the housing market received the news of new lending regulations a few weeks before the date of implementation, they might have attempted to buy a dwelling before the date of implementation so that they would not be subject to the new regulations. This might have caused a significantly difference in the number of dwellings sold before and after the date of implementation.

To examine this hypothesis, we employ a continuity-based approach to test the density of the running variable. As with the RD designs used previously, this approach tests if there are any discontinuities in the regression population line. However, we now use number of observations as the outcome of interest, instead of the selling price. The running variable is still the date of sale. The idea of this test is to test if there are any discontinuities in the population regression line when comparing the number of transactions before the cutoff with the number of transactions after the cutoff. An alternative to this approach would be to run a McCrary-test (Lee & Lemieux, 2010; McCrary, 2008). However, for simplicity, we use a method developed by Cattaneo, Jansson, and Ma (2019) and (Cattaneo, Jansson, & Ma, 2021), which is an companion command to the already used rd-package in Stata.

In table 14 and 15 we display the results from the density test. With cutoff at June 10, 2015, we can see that there was a significant drop in the number of observations after the cutoff. This may suggest that there was a drop in the number of housing transactions after the implementation of the LTV policy in July 15. The June 11 cutoff and the October 1 cutoff also show a significant drop in the number of observations at the cutoff, while the August 15 cutoff yields an increase in the number of transactions at the cutoff. The density tests for the DTI policy imposed in 2017, which are displayed in Table 15, also yield statistically significant discontinuities. Only the February 15 cutoff show no discontinuity at the cutoff, which indicates that the number of transactions were similar before and after February 15. The fact that seven of the eight density tests indicate that there was a statistically significant difference in the number of transitions before and after cutoff suggests that the result from the main RD design can be called into question. However, it is not surprising that we find such large differences in the number of transactions around these dates. To illustrate this point, we can examine the density plots in Figure 9 below.

Table 14: Density tests for the 2015 LTV policy

Density test	**June 10**	**July 1**	**August 15**	**October 1**
Coefficient	-5.877***	-25.390***	16.028***	-4.619***
p-value	0.000	0.000	0.000	0.000

	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff
No of obs.	87,649	93,646	88,923	92,372	90,031	91,264	93,037	88,258
Eff no of obs.	14,91	12,632	14,794	12,635	13,284	14,08	14,272	12,986
Order-est.	4	4	4	4	4	4	4	4

Density tests of the running variable date of sale with cutoffs at June 10, 2015 July 1, 2015, August 15, 2015, and October 1, 2015. The dependent variable is the number of observations, while the running variable is the number of days. The test uses a bandwidth of 295, a polynomial order of four, and a triangular kernel. All cutoffs yield a statistically significant discontinuity at cutoff, which indicates that the number of observations before and after both cutoffs are significantly different.

Table 15: Density tests for the 2017 DTI policy

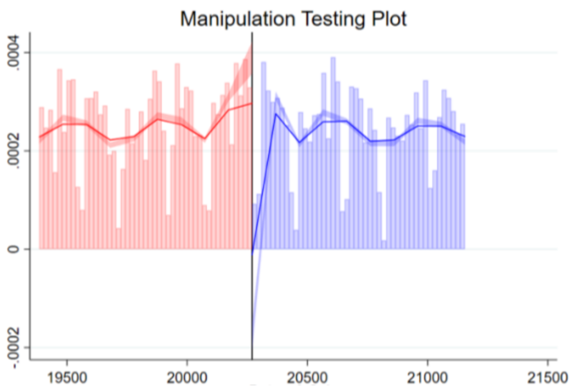
Density test	**December 11**	**January 1**	**February 15**	**April 1**
Coefficient	-13.910***	-3.427***	-1.300	-11.741***
p-value	0.000	0.000	0.194	0.000

	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff
No of obs.	112,542	68,753	112,735	68,56	114,490	66,805	116,566	64,729
Eff no of obs.	10,429	9,091	9,71	9,622	8,709	10,482	9,565	10,509
Order-est.	4	4	4	4	4	4	4	4

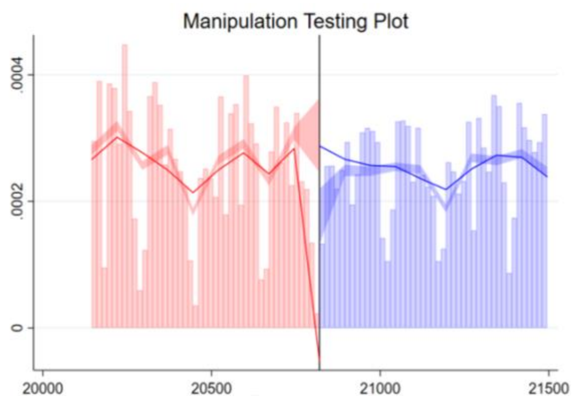
Density tests of the running variable date of sale with cutoffs at December11, 2016, January 1, 2017, February 15, 2017, and April 1, 2017. The dependent variable is the number of observations, while the running variable is the number of days. The test uses a bandwidth of 225, a polynomial order of four, and a triangular kernel All cutoffs yield a statistically significant discontinuity at cutoff, except for February 15.

Figure 9 a) and b) contain the RD plots for the density tests for July 1, 2015, and January 1, 2017, respectively. The plots confirm the findings in table 14 and 15 above, with clear discontinuities in the population regression lines at the cutoffs. However, they also provide a better understanding of the cyclical activity in the housing market. The number of transactions fluctuate within the given bandwidth, and not only at the given cutoff. This suggests that the activity in the housing market is volatile and is subject to seasonal differences. In other words, the outcome of interest, which in this case is the number of transactions, will most likely vary from day to day. Hence, it is not surprising that we find a significant discontinuity in the number of observations before and after the cutoff.

Figure 9: Density plots for July 1, 2015 and January 1, 2017



a) July 1, 2015



b) January 1, 2017

Density plots with July 1, 2015, and January 1, 2017 used as cutoffs. Bandwidth for July is set to 295, and bandwidth for January is set to 225. A triangular kernel and a polynomial order of four is used. The test shows a statistically significant difference in number of dwellings sold before and after both cutoffs.

It is also important to note that a triangular kernel is used. The kernel is the smoothing parameter and will in this case weigh the observations closest to the cutoff higher than the ones further from the cutoff. This effect can be seen in both the density plots below. The regression population exhibits small triangular variations further from cutoff. Close to the cutoff, however, the variations are weighted higher, which enhances the effect of the variations. In other words, the effect of the variation in the activity in the housing market is enhanced close to the cutoff, creating a discontinuity at the cutoff. Even though the chosen kernel and the volatility in the housing market may have been contributory factor to the statistically significant density tests, the tests still indicate that the results from the RD analyses should be interpreted somewhat more carefully.

5.3.3 Bandwidth test

The third test we will conduct is a bandwidth test. The results from the RD design are highly sensitive to the chosen bandwidth (G. W. Imbens & Lemieux, 2008). Thus, it is important to test the sensitivity with different bandwidth choices. G. W. Imbens and Lemieux (2008) suggests using bandwidths twice or half the size of the originally chosen bandwidth. We employ this approach for all eight cutoffs. The original bandwidth for the 2015 cutoffs was 295 days. Hence, we use 148 and 590 as bandwidths for these dates. The original bandwidth for the 2017 (and December 2016) dates was 225. For this test then, we use 113 and 450 for those cutoffs. The regression used is the same as previously. The outcome of interest is still the selling price with a logarithmic specification, and the running variable is the number of days. All covariates are included, and we use triangular kernel and a polynomial order of four. Table 16 below displays the results from the bandwidth test.

Table 16: Results from bandwidth test

	Jun 10, 2015		**July 1, 2015**	
Band-width	**148**	**590**	**148**	**590**
Coeffi-cient	-0.033**	-0.022***	-0.009	0.047***
Std error	0.015	0.008	0.020	0.010
z-value	-2.137	-2.714	-0.433	4.843
p-value	0.033	0.007	0.665	0.000

	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff
No of obs.	87,309	93,504	87,309	93,504	88,580	92,233	88,580	92,233
Eff no of obs.	8,296	7,228	26,861	25,653	8,468	6,939	27,195	25,267
Order-est.	4	4	4	4	4	4	4	4

	Aug 15, 2015		October 1, 2015	
Band-width	**148**	**590**	**148**	**590**
Coeffi-cient	-0.013	0.002	-0.005	-0.012
Std error	0.014	0.008	0.017	0.008
z-value	-0.887	0.257	-0.270	-1.512
p-value	0.375	0.797	0.787	0.130

	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff
No of obs.	89,686	91,127	89,686	91,127	92,687	88,126	92,687	88,126
Eff no of obs.	7,230	6,586	27,272	26,059	7,519	5,772	28,277	25,368
Order-est.	4	4	4	4	4	4	4	4

	December 11, 2016		Jan 1, 2017	
Band-width	**113**	**450**	**113**	**450**
Coeffi-cient	-0.017	0.004	0.007	0.005
Std error	0.023	0.012	0.064	0.013
z-value	-0.716	0.316	0.110	0.347
p-value	0.474	0.752	0.912	0.729

	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff
No of obs.	112,145	68,668	112,145	68,668	112,338	68,475	112,338	68,475
Eff no of obs.	5,505	4,024	20,264	18,637	4,435	4,480	19,308	19,574
Order-est.	4	4	4	4	4	4	4	4

	February 15, 2017		April 1, 2017	
Band-width	**113**	**450**	**113**	**450**
Coeffi-cient	-0.006	-0.003	0.029*	0.009
Std error	0.018	0.009	0.017	0.009
z-value	-0.323	-0.272	1.739	1.008
p-value	0.747	0.786	0.082	0.314

	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff
No of obs.	114,091	66,722	114,091	66,722	116,166	64,647	116,166	64,647
Eff no of obs.	3,775	5,336	18,746	19,915	4,024	5,019	19,915	20,460
Order-est.	4	4	4	4	4	4	4	4

Bandwidth test for the main RD design. The test uses a bandwidth half the size and twice the size of the original bandwidth. All covariates are included. A polynomial order of four and a triangular kernel has been used.

From the main RD analyses in section 5.2.1 and 5.2.2, the only statistically significant coefficient was the coefficient with the June 10 cutoff. From the table above, we can see that the cutoff at June 10 still yields statically significant coefficients, even though the bandwidth has been changed. In addition, one of the coefficients for the July 1 cutoff is now statistically significant. This indicates that there was a statistically significant jump in price on July 1, given a bandwidth of 590 days. This contradicts our previous finding in Table 6. There, we employed an RD design without covariates and obtained a statistically significant decrease in selling price on July 1. Now, we see an increase in selling price at the cutoff, given a broader bandwidth and all covariates included.

One explanation for the positive coefficient for July 1 is that the bandwidth now includes observations very far from the cutoff. The previous findings have indicated that the LTV had a short-term effect on the housing market. The effect was most likely seen three weeks before the policy was implemented, and the housing prices continued to rise shortly after. This short-term effect can also be seen in the constructed price index shown previously. The index exhibits a small decline around the date of implementation, before continuing upwards. When using a bandwidth of 590 days, we include observations that are located 1.6 years away from the cutoff. This means that the last observation in the regression is located 3.2 years away from the first observation. During this three-year period, there was a clear price appreciation in the housing market, as confirmed by the hedonic price index. Hence, the small decline in price seen right before the date of implementation is not enough to "outweigh" the price appreciation seen over those three years. On the contrary, when using such a broad bandwidth, the RD estimates indicate a positive increase in price at the cutoff.

In a sense, using a broad bandwidth can help us uncover the long-term effects of the policies, as observations further from the cutoff are included. However, using a broad bandwidth may lead to the comparison on both sides of the cutoff being less credible since we are no longer only comparing observations right before and right after the cutoff (Lee & Lemieux, 2010). Furthermore, no other coefficient than the ones already discussed are statistically significant. Hence, the test does not provide any further information about the effect of the macroprudential policies. The test does, however, suggest that the originally statistically significant coefficient uncovered in the main RD analyses is not robust to different bandwidths, and should be interpreted somewhat more carefully.

5.3.4 Placebo cutoffs

The final specification test we will employ uses artificial or placebo cutoff dates. The idea behind this test it to check for potential discontinuities at other cutoff dates than the ones already used. Continuity away from the original cutoff dates is neither necessary nor sufficient for the original results to be valid. However, finding evidence of discontinuity away from cutoff can potentially cause doubt on the original RD results (Cattaneo, Idrobo, et al., 2019). Cattaneo, Idrobo, et al. (2019) suggests using placebo cutoffs that are one, two, and three units away from the original cutoff. In this study, however, we have already used several cutoffs to account for delayed effects. Using a cutoff one day away from the original dates, for example January 2, will not provide us with any useful information. First, the date is so close to the original date, that any differences in selling price will most likely not occur in such a short time period. Second, due to delayed effects, we have already used cutoffs further from the original date. Hence, a placebo cutoff should be even further from the original cutoff dates than the once already used.

Larsen (2018), which also applies a form of RD analyses on housing market data, uses placebo cutoffs that are four months from the original cutoff. Since our extra cutoff is three months away from the date of policy implementation, we believe using the same approach will adequately control for potential discontinuities away from the cutoff. We use cutoff dates four months away from July 1, 2015, and January 1, 2017. The placebo cutoffs and the respective RD results are displayed in Table 17 below. Like before, we are looking for any discontinuities. All covariates have been used in the RD design, as well as a triangular kernel and a polynomial order of four. None of the placebo cutoffs used yield any statistically significant coefficients. Hence, the outcome of interest does not exhibit any discontinuities at the chosen placebo cutoff points.

Table 17: Results from placebo test

	March 1, 2015		November 1, 2015		August 1, 2017		May 1, 2017	
Coeffi-cient	0.00757		-0.00015		0.01424		0.01266	
Std error	0.01195		0.01142		0.02185		0.01189	
z-value	0.6336		-0.0129		0.652		1.0647	
p-value	0.526		0.99		0.514		0.287	
	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff
No of obs.	81,260	99,552	94,274	86,538	105,743	75,069	117,353	63,459
Eff no of obs.	13,304	14,865	15,459	12,367	9,618	9,376	9,084	10,461
Bandwidth	295	295	295	295	225	225	225	225
Order-est.	4	4	4	4	4	4	4	4

Table 17 shows the results from the placebo test. Four cutoffs have been used to test for potential discontinuities away from the original main cutoffs. March 1, 2015, and November 1, 2015, are four months before and after July 1 (date of LTV policy) respectively. August 1, 2016, and May 1, 2017, are four months before and after January 1, 2017 (date of DTI policy) respectively. The same RD design as previously has been used; bandwidth of 295 days for the 2015 cutoffs and a bandwidth of 225 for the 2016 and 2017 cutoff, triangular kernel, all covariates, and a polynomial order of four. No discontinuities are found.

5.4 Stationarity test

To achieve an even better understanding of the long-term effects, especially from the DTI policy, we supplement the RD analyses with a time series regression. We use a Dickey-Fuller (DF) test to examine if the price index contains any stochastics trends before and after the date of the DTI implementation. Figure 10 shows the development of the price index with weeks as the running time variable. Two vertical lines have been added to the graph to illustrate the date of the LTV and DTI policy implementation. The graph clearly shows a positive trend in price. However, after January 2017, the index falls before growing less rapidly than before the DTI implementation.

Figure 10: Line plot for time-series data

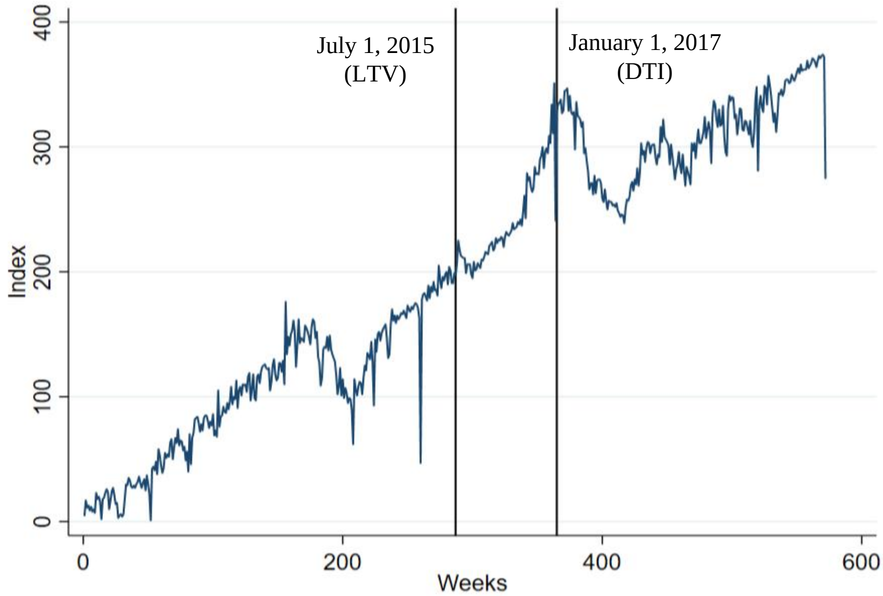


Figure 10 shows a line plot for time-series data, with the time-series being the price index from January 2010 to December 2020. The Y-axis represents the index, and the X-axis represent weeks. Two vertical lines have been added to illustrate which week the DTI policy and LTV policy was implemented.

The DF tests will reveal if the less rapid trend seen after 2017 is stationary or trend-stationary. If the index is stationary after the 2017 DTI policy implementation, we can conclude that the policy may have dampened the positive *trend* seen before the policy implementation. In a DF-test, the null hypothesis is that the time series contains a unit root (i.e. is non-stationary), and the alternative is that the series is stationary. When testing for tend-stationarity, the null hypothesis is that the time series contain a unit root, and the alternative is that the time series is trend-stationary (i.e. stationary around a trend). We have tested the potential effect of both the LTV policy and the DTI policy. Table 18 and 19 below show the results from the DF-tests for the two time-periods 2010 to 2015 and 2015 to 2017 respectively. The two time periods represent the time periods before and after the LTV policy implementation. For both time-periods, we have tested for stationarity and trend-stationarity. The same process has been done for the time periods 2010 to 2017, and 2017 to 2020, to test the effect of the DTI policy. The results from these two tests are found in Table 20 and 21 below.

Table 18: DF-test of LTV with time period from 2010-2015:

	Coefficient	p-value	1 % critical value	5% critical value	10 % critical value
Z(t) with no trend	-2.482	0.120	-3.457	-2.879	-2.570
Z(t) with trend	-7.049***	0.000	-3.989	-3.429	-3.130
L1 without trend	-0.042**	0.014			
L1 with trend	-0.299***	0.000			
Trend	0.185***	0.000			
No of obs	285				

Results from the Dickey-Fuller test for the time period January 2010 (week 1) to June 2015 (week 27). The Dickey-Fuller statistic (Z(t)) is not statistically significant when not controlling for any trend. Hence, the time series is non-stationary before July 1, 2015. When controlling for a trend, however, Z(t) is statistically significant for a 10 %, five %, and one % significance level. This indicates that the index is trend-stationary before July 1, 2015.

Table 19: DF-test of LTV with time period from 2015-2017:

	Coefficient	p-value	1 % critical value	5% critical value	10 % critical value
Z(t) with no trend	-1.693	0.435	-3.542	-2.908	-2.589
Z(t) with trend	-3.474**	0.042	-4.091	-3.473	-3.164
L1 without trend	-0.053*	0.095			
L1 with trend	-0.354***	0.001			
Trend	0.278***	0.003			
No of obs	77				

Results from the Dickey-Fuller test for the time period July 2015 (week 27) to December 2016 week (52). The Dickey-Fuller statistic (Z(t)) is not statistically significant when not controlling for any trend. Hence, the time series is non-stationary before January 2017. When controlling for a trend, however, Z(t) is statistically significant for a 10 % and five % significance level. This indicates that the index is trend-stationary before and after July 1, 2015 (until January 2017).

The findings in table 18 and 19 above indicate that the index is non-stationary before and after the implementation of the LTV policy in July 2015. The DF-statistic (Z(t)) in both tables is not statistically significant when not controlling for any trend. Thus, we fail to reject the null hypothesis of unit root. When controlling for trend, however, the Z(t) is statistically significant at a five percent significance level, and we reject the null hypothesis of stationary with an alternative of trend-stationarity. This indicates that for the time periods before and after the LTV implementation, the index is trend-stationary. In other words, the positive trend seen before July 2015 does not diminish after the LTV policy was imposed. The findings from the DF test of the DTI policy are seen below.

Table 20: DF-test for 2010-2017:

	Coefficient	p-value	1 % critical value	5% critical value	10 % critical value
Z(t) with no trend	-1.988	0.292	-3.451	-2.875	-2.570
Z(t) with trend	-7.729***	0.000	-3.985	-3.425	-3.130
L1 without trend	-0.021**	0.048			
L1 with trend	-0.285***	0.000			
Trend	0.188***	0.000			
No of obs	363				

Results from the Dickey-Fuller test for the time period January 2010 (week 1) to December 2016 (week 52). The Dickey-Fuller statistic (Z(t)) is not statistically significant when not controlling for any trend. However, when controlling for a trend, Z(t) is statistically significant for a 10 %, five %, and one % significance level.

Table 21: DF-test for 2017-2020:

	Coefficient	p-value	1 % critical value	5% critical value	10 % critical value
Z(t) with no trend	-2.928**	0.042	-3.474	-2.883	-2.573
Z(t) with trend	-3.806**	0.016	-4.004	-3.436	-3.136
L1 without trend	-0.082***	0.004			
L1 with trend	-0.142***	0.000			
Trend	0.048**	0.017			
No of obs	207				

Results from the Dickey-Fuller test for the time period January 2017 (week 1) to December 2020 (week 52). The Dickey-Fuller statistic (Z(t)) is statistically significant both with and without a trend. This indicates that the price index does contain a weaker trend after the 2017 DTI policy implementation.

In Table 20, we see the results from the DF-test with a time period from 2010 to 2017. When not controlling for any trend, we fail to reject the null hypothesis of unit root. With trend, however, the null hypothesis is rejected, which indicates that the index is trend-stationary before January 2017. The main finding from the DF-tests, however, is seen in Table 21. In this case, Z(t) is statistically significant both with and without trend. In other words, the index is stationary when not controlling for any trend. This suggests that the trend seen before January 2017 has diminished, and that the DTI policy may have had a dampening effect on the price development in the housing market. This supports the previous indications from the RD analyses, that the DTI policy had a more long-term effect on the price development in the Oslo housing market.

5.5 Summary of results

As a result of the broad use of different econometric models, the findings above may appear somewhat overwhelming and incomprehensible. Hence, it might be beneficial to summarize the results before arriving at a final conclusion. First, from the hedonic regression and the constructed housing price index we find that the housing prices in Oslo appreciated by 116 percent during the 11-year period studied in this paper. Furthermore, the price index fell by 1.3 percent shortly after the implementation of the LTV policy in 2015. The DTI policy seems to have a had a stronger effect, as the price index fell by 12.4 percent after the DTI implementation in 2017. Second, we find that the LTV policy caused the selling price in the housing market to drop when the LTV policy was imposed on July 1, 2015. This finding, however, is derived from a sharp RD analysis with no covariates included to control for other effects. When including covariates in the regression, we find no drop in selling price on July 1. This may suggest that the LTV policy caused a shift in the buyers' preferences, which resulted in a decrease in selling price. The covariate test conducted in section 5.3.1 supports this finding, as there are clear discontinuities in several of the covariates on July 1. Hence, we cannot conclude that the LTV policy had a causal effect on the housing price when controlling for other effects. However, the findings suggest that the policy has shifted the buyers' preferences, which again may have caused the price to fall on July 1.

Third, from the sharp RD analysis with several cutoffs and all covariates included, we find that the LTV policy had a significant effect on the selling price on June 10, three weeks before the policy went into effect. This suggests that market expectations caused the prices to fall already three weeks before the policy went into effect. Further, the sharp RD analyses yield no statistically significant effect on selling price when examining the effect of the DTI policy imposed in 2017. Fourth, the RK analyses indicate that there was a significant change in price development on July 1, causing the positive trend in selling price seen before July 2015 to drop after the implementation of the LTV policy. The price development picked back up on October 1, which indicates that the effect of the LTV policy was short lived. Further, the RK analyses indicated a significant decrease in the price development on April 1, 2017, three months after the DTI policy. This may indicate that the effect of the DTI policy was not seen before three months after the policy went into effect.

Finally, using time series regression, we find that the positive trend in the housing market may have diminished after the DTI policy went into effect. This finding, together with the non-statistically significant coefficients from the sharp RD analyses of the DTI policy, may indicate that the effect of the DTI policy is not captured by the sharp RD design, which focuses more on the short-term effect. Using time series regression, however, we find that the DTI policy influenced the housing prices in Oslo. By also factoring in the results from the RK analyses, which revealed that there was a drop in price development three months after the DTI policy went into effect, the findings may indicate that the DTI policy had delayed and more long-term effect on the housing market than the LTV policy.

The specification tests indicate that the selected covariates and bandwidths, as well as the density of the running variable, may have influenced some of the results. Furthermore, the chosen kernel and polynomial order may also have influenced the outcome of the tests. If we were to use a different specification, such as changing the polynomial order, we would most likely obtain significantly different results. This suggests that RD designs should be employed with care and with an appropriate specification. However, despite some uncertainty around the specifications, the results above point towards two main outcomes of the macroprudential policies. First, the effect of the LTV policy on housing prices in Oslo was seen three weeks before the date of implementation and was short lived. Second, the DTI policy caused a more long-term effect on the housing prices which was not seen before three months after the date of implementation.

6 Conclusion

The financial crisis in 2008, as well as existing academic research (e.g. Muellbauer & Murphy, 2008; Reinhart & Rogoff, 2008), has highlighted the importance of a stable housing market. The severe consequences of the crisis caused traditional macroeconomic policies to reach their limits (Crowe et al., 2013). As a result, together with a more recent rise in housing prices and debt-to-income ratios, Norway has imposed several macroprudential policies to limit the buildup of financial imbalances. The consequences of such policies, however, are far from being sufficiently understood empirically (Galati & Moessner, 2013). In this study, we have attempted to provide a better understanding of the effect of macroprudential polices on the housing prices in Oslo. Based on the hedonic pricing method and RD designs, as well as supplemental time series regression, we identify two main effects caused by the LTV and DTI policies. First, the LTV policy imposed on July 1 1, 2015, had an immediate and short-term effect on the housing prices in Oslo. Specifically, the housing prices fell three weeks before the implementation of the policy but continued to rise within three months after. Second, the DTI policy imposed on January 1, 2017, had a more long-term effect on the housing prices. The effect, however, was not seen before three months after policy implementation.

The findings clearly indicate that the policies influenced the housing prices in Oslo. Previous academic research from other countries show both similar and conflicting results. Igan and Kang (2011) found that price appreciation slows down more after an LTV tightening than a DTI tightening. Our findings, however, indicate that the DTI policy may have had a more lasting effect on the price appreciation. Armstrong et al. (2019) suggests the effect of LTV restrictions on housing prices is short-lived, and that it is necessary to either continue tightening the LTV restriction or seek other policies, such as DTI measures, if one wishes to achieve a lasting effect of the polices. Similarly, our findings indicate that the effect of the LTV policy was short-lived, and that the DTI policy imposed a year and a half later helped ensure a more stable price growth for the following years. Thus, our study is yet another contribution to the rapidly expanding literature on the topic of macroprudential policies. However, to our knowledge, we are the first to specifically uncover the effects of LTV and DTI policies on the housing prices in Oslo. Understanding the effects of these policies on the housing market is crucial to maintain financial stability. Furthermore, the findings from this study, together with previous academic research on the topic, will provide a better basis on which to make future political decisions regarding the topic.

Future research should continue to improve upon the methodological framework used to examine the effects of macroprudential policies. RD designs with other specifications than the ones used in this study should be examined, as the results may be sensitive to the chosen specification. As a result of the potential delayed effects of the pre-qualification letters, fuzzy RD design is a promising method to employ to achieve a better understanding of the true causal effect of the macroprudential policies. Furthermore, our findings indicate that there was a shift in the buyers' preferences as a result of the LTV policy. This supplementary finding can provide an exciting basis for future research on buyers' changed preferences, which will further provide better knowledge of the housing market and help ensure financial and macroeconomic stability in the future.

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Appendix A

Table A.1: City districts

City District	Observations	Percent
Grünerløkka	23,222	12.81
Sagene	19,536	10.78
Gamle Oslo	18,376	10.14
Frogner	18,241	10.06
Østensjø	13,539	7.47
St. Hanshaugen	12,929	7.13
Alna	12,654	6.99
Nordstrand	11,009	6.07
Nordre Aker	8,907	4.91
Vestre Aker	8,147	4.49
Bjerke	7,936	4.38
Ullern	7,346	4.05
Søndre Nordstrand	6,876	3.79
Grorud	6,443	3.55
Stovner	5,774	3.18
Sentrum	318	0.18
Marka	42	0.02

Table A.1 shows the number of observations and percentage of the different city districts.

Table A.2: Bedrooms

Bedrooms	Observations	Percent
0	5,948	3.28
1	61,344	33.84
2	69,732	38.46
3	34,994	19.30
4	7,768	4.28
5	1,509	0.83

Table A.2 summarize the bedroom variable. Only dwellings with number of bedrooms from zero to five are included due to the one-percent trimmed mean.

Table A.3: Property types

Property type	Observations	Percent
Single family home	5,881	3.24
Apartment	162,105	89.42
Row house	8,022	4.42
Semi-detached	5,287	2.92

Table A.3 summarizes the property type variable. Apartments make up a clear majority of the dwellings included in the dataset.

Figure A.1: Development in price per living area and transactions

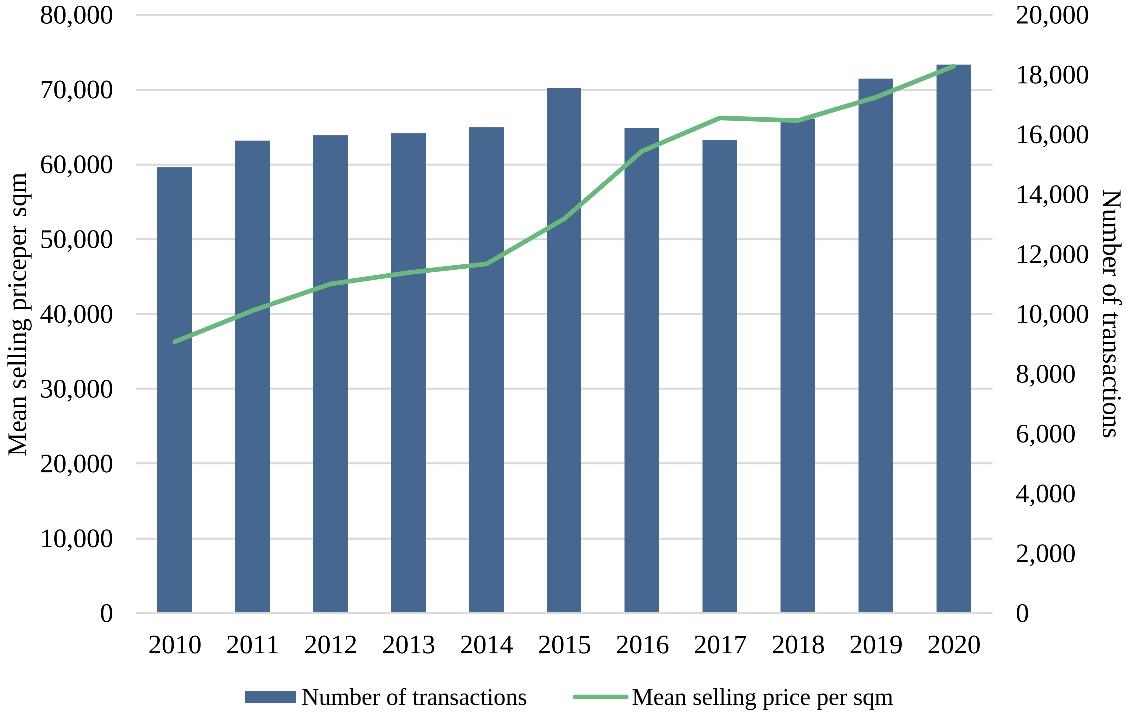

Figure A.1 displays the number of transactions from 2010 to 2020, as well as the development in mean selling price per square meter. The blue columns show the number of transactions, and the green line shows the mean selling price per sqm.

Table A.4: Hedonic regression results with monthly dummy variables

	(1) All	(2) Small	(3) Medium	(4) Large
ln(Living Area)	0.646***	0.576***	0.684***	0.862***
	(0.002)	(0.009)	(0.015)	(0.012)
Bedroom*ln(Living Area)	0.0251***	-0.0394***	0.0155*	-0.0560***
	(0.001)	(0.009)	(0.008)	(0.003)
Bedroom	-0.0860***	0.166***	-0.0211	0.273***
	(0.004)	(0.032)	(0.034)	(0.016)
Apartment on first floor	-0.0357***	-0.0342***	-0.0380***	-0.0410***
	(0.001)	(0.002)	(0.001)	(0.002)
Ownership (Cooperative)	-0.113***	-0.135***	-0.108***	-0.0855***
	(0.001)	(0.002)	(0.002)	(0.003)
Postal Code (2)	0.741***		0.749***	0.732***
	(0.084)		(0.164)	(0.030)
Postal Code (3)	1.039***		1.204***	0.918***
	(0.122)		(0.168)	(0.01)
.				
.				
.				
Postal Code (412)	0.109		0.423***	-0.00283
	(0.082)		(0.162)	(0.02)
Postal Code (413)	0.0379			-0.067***
	(0.178)			(0.012)
Property Type (Apartment)	-0.107***	-0.0966*	-0.180***	-0.116***
	(0.003)	(0.056)	(0.021)	(0.004)
Property Type (Row House)	-0.0232***	0.0131	-0.0692***	-0.0519***
	(0.003)	(0.062)	(0.023)	(0.003)
Property Type (Semi-detached)	-0.0313***	0.00703	-0.0256	-0.0546***
	(0.003)	(0.090)	(0.024)	(0.003)
Build Year (1945 to 1959)	-0.0135***	0.0182***	0.00408	-0.0193***
	(0.002)	(0.004)	(0.003)	(0.004)
Build Year (1960 to 1969)	-0.0112***	0.00875***	-0.00963***	-0.0148***
	(0.002)	(0.003)	(0.003)	(0.004)
Build Year (1970 to 1982)	-0.00459**	-0.000351	0.0225***	-0.0111***
	(0.002)	(0.004)	(0.003)	(0.003)
Build Year (1983 to 1999)	0.0256***	0.0211***	0.0291***	0.0396***
	(0.002)	(0.004)	(0.003)	(0.003)
Build Year (post 2000)	0.0560***	0.0165***	0.0511***	0.104***
	(0.002)	(0.003)	(0.003)	(0.004)
2010 month 2	-0.00193	-0.0213*	-0.00613	0.00969
	(0.007)	(0.012)	(0.010)	(0.015)
2010 month 3	0.01000	-0.0195*	0.00540	0.0416***
	(0.007)	(0.012)	(0.009)	(0.014)
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2020 month 11	0.761***	0.828***	0.771***	0.695***
	(0.007)	(0.011)	(0.009)	(0.013)
2020 month 12	0.774***	0.843***	0.778***	0.705***
	(0.008)	(0.013)	(0.011)	(0.018)
_cons	11.39***	11.77***	11.07***	10.57***
	(0.081)	(0.066)	(0.168)	(0.057)
N	181,295	42,748	86,278	52,269
R^2	0.893	0.821	0.843	0.892
adj. R^2	0.893	0.819	0.843	0.891

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.01

Table A.4 shows the results from the hedonic regression. Four regressions have been estimated; small, medium, large, and all dwellings. The standard errors are expressed within the columns, and the number of asterisks represent a 10 percent, five percent, and one percent significance level respectively. For the dummy variable "Apartment First Floor", the reference variable is all apartments not located on the first floor and all other property types. For the dummy variable "Ownership", the reference variable is self-owned. The postal code dummy variables range from two to 413. Postal code 1 is omitted and used as the reference variable. For the dummy variable "Property Type", the reference variable is single family home. For the dummy variables for "Build Year", the reference variable is the time-period before 1945. There are 132 time dummy variables, where the first variable represents the first month in 2010, and the last represents the 12th month in 2020. The first month in 2010 is omitted and used as the reference variable in the regression. Total observations for regression (1) is 181,295, which reflects the number of observations remaining after the data cleaning process explained in section 3.2.

Table A.5: Hedonic regression results with weekly dummy variables

	(1) All	(2) Small	(3) Medium	(4) Large
ln(Living Area)	0.647***	0.575***	0.686***	0.862***
	(0.002)	(0.009)	(0.015)	(0.012)
Bedroom*ln(Living Area)	0.0250***	-0.0381***	0.0148*	-0.0561***
	(0.001)	(0.009)	(0.008)	(0.003)
Bedroom	-0.0858***	0.161***	-0.0183	0.273***
	(0.004)	(0.032)	(0.034)	(0.016)
Apartment on first floor	-0.0356***	-0.0340***	-0.0378***	-0.0408***
	(0.001)	(0.002)	(0.001)	(0.002)
Ownership (Cooperative)	-0.113***	-0.135***	-0.108***	-0.0859***
	(0.001)	(0.002)	(0.002)	(0.003)
Postal Code (2)	0.730***		0.722***	0.713***
	(0.084)		(0.157)	(0.037)
Postal Code (3)	1.045***		1.201***	0.949***
	(0.122)		(0.160)	(0.044)
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Postal Code (412)	0.101		0.405***	-0.0207
	(0.082)		(0.155)	(0.030)
Postal Code (413)	0.0224			-0.121***
	(0.179)			(0.028)
Property Type (Apartment)	-0.106***	-0.103*	-0.180***	-0.116***
	(0.003)	(0.056)	(0.020)	(0.004)
Property Type (Row House)	-0.0232***	0.00913	-0.0697***	-0.0520***
	(0.003)	(0.062)	(0.023)	(0.003)
Property Type (Semi-detached)	-0.0310***	-0.000338	-0.0255	-0.0547***
	(0.003)	(0.090)	(0.024)	(0.003)
Build Year (1945 to 1959)	-0.0134***	0.0176***	0.00369	-0.0192***
	(0.002)	(0.004)	(0.003)	(0.004)
Build Year (1960 to 1969)	-0.0113***	0.00850**	-0.0101***	-0.0153***
	(0.002)	(0.003)	(0.003)	(0.004)
Build Year (1970 to 1982)	-0.00459**	-0.000298	0.0224***	-0.0115***
	(0.002)	(0.004)	(0.003)	(0.003)
Build Year (1983 to 1999)	0.0254***	0.0219***	0.0285***	0.0394***
	(0.002)	(0.004)	(0.003)	(0.003)
Build Year (post 2000)	0.0562***	0.0165***	0.0512***	0.104***
	(0.002)	(0.003)	(0.003)	(0.004)
2010 week 2	0.0255	0.00561	0.0878*	-0.0285
	(0.030)	(0.036)	(0.046)	(0.053)
2010 week 3	0.0163	-0.00228	0.0832*	-0.0520
	(0.030)	(0.034)	(0.046)	(0.049)
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2020 week 51	0.787***	0.849***	0.839***	0.710***
	(0.032)	(0.036)	(0.051)	(0.050)
2020 week 52	0.630***	0.877***	0.654***	0.679***
	(0.052)	(0.038)	(0.159)	(0.076)
_cons	11.38***	11.80***	11.01***	10.61***
	(0.085)	(0.075)	(0.167)	(0.076)
N	181,295	42,748	86,278	52,269
R^2	0.894	0.823	0.845	0.894
adj. R^2	0.893	0.819	0.843	0.892

Standard errors in parentheses
* p<0.10, ** p<0.05, *** p<0.01

Table A.5 shows the results from the hedonic regression. Four regressions have been estimated; small, medium, large, and all dwellings. The standard errors are expressed within the columns, and the number of asterisks represent a 10 percent, five percent, and one percent significance level respectively. For the dummy variable "Apartment First Floor", the reference variable is all apartments not located on the first floor and all other property types. For the dummy variable "Ownership", the reference variable is self-owned. The postal code dummy variables range from two to 413. Postal code 1 is omitted and used as the reference variable. For the dummy variable "Property Type", the reference variable is single family home. For the dummy variables for "Build Year", the reference variable is the time-period before 1945. There are 132 time dummy variables, where the first variable represents the first month in 2010, and the last represents the 12th month in 2020. The first month in 2010 is omitted and used as the reference variable in the regression. Total observations for regression (1) is 181,295, which reflects the number of observations remaining after the data cleaning process explained in section 3.2.

Table A.6: Results from RD analyses of different segments in 2017

Small dwellings

	December 10	January 1	February 15	April 1
Coefficient	-0.005	-0.080*	-0.011	0.011
Std error	0.026	0.041	0.024	0.020
z-value	-0.203	-1.942	-0.471	0.542
p-value	0.839	0.052	0.638	0.588

	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff
No of obs.	25,973	16,633	26,025	16,581	26,446	16,160	26,954	15,652
Eff no of obs.	2,576	2,129	2,404	2,328	2,142	2,561	2,233	2,548
Bandwidth	225	225	225	225	225	225	225	225
Order-est.	4	4	4	4	4	4	4	4

Medium dwellings

	December 10	January 1	February 15	April 1
Coefficient	-0.005	-0.013	0.001	0.036
Std error	0.020	0.037	0.017	0.017
z-value	-0.247	-0.341	0.054	2.081
p-value	0.805	0.733	0.957	0.037

	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff
No of obs.	53,338	32,680	53,436	32,582	54,288	31,730	55,311	30,707
Eff no of obs.	4,854	4,369	4,513	4,595	4,103	4,973	4,567	4,987
Bandwidth	225	225	225	225	225	225	225	225
Order-est.	4	4	4	4	4	4	4	4

Large dwellings

	December 10	January 1	February 15	April 1
Coefficient	-0.078*	-0.013	0.017	-0.018
Std error	0.043	0.059	0.029	0.026
z-value	-1.822	-0.214	0.594	-0.701
p-value	0.068	0.831	0.552	0.484

	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff	Before cutoff	After cutoff
No of obs.	32,834	19,355	32,877	19,312	33,357	18,832	33,901	18,288
Eff no of obs.	2,969	2,542	2,762	2,604	2,410	2,880	2,731	2,942
Bandwidth	225	225	225	225	225	225	225	225
Order-est.	4	4	4	4	4	4	4	4

Table A.6 presents the results from a sharp regression discontinuity design on small, medium, and large dwellings. Four cutoffs have been used. The dependent variable is ln(Selling Price), and the running variable is the date of sale. Bandwidth of 225 days has been used before and after cutoff, with a polynomial order of four and a triangular kernel. All six covariates have been used.

Appendix B

The 2017 mortgage lending regulation

Some of the sections in the 2017 regulation are reproduced and translated below[22].

§ 1. Scope

The regulations apply to financial institutions offering mortgages secured on residential property.

§ 3. Debt servicing capacity

The financial institution shall calculate a borrower's ability to service the loan based on the borrower's income and all relevant expenses, including interest, loan instalments and normal living expenses.

When assessing the borrower's servicing capacity, the financial institution shall factor in an interest rate increase of 5 percentage points from the prevailing interest rate level. In the case of fixed interest loans, a corresponding interest rate increase must be factored in from the end of the fixed-rate period. If the borrower lacks sufficient resources to meet normal living expenses after a 5 percentage point interest rate increase, the loan shall not be granted.

§ 4. Debt-to-income ratio

A mortgage shall not be granted if the borrower's overall debt exceeds five times annual income.

§ 5. Loan-to-value ratio

At the time of granting, instalment loans secured on residential property shall not exceed 85 per cent of the property's appraised value, which cannot be higher than its prudently assessed market value.

[22] Source: *(Boliglånsforskriften, 2016; Finanstilsynet, 2019)*

The first subsection does not apply to mortgages secured on a second home in the municipality of Oslo. At the time of granting, such instalment loans shall not exceed 60 per cent of the property's value, calculated in accordance with the first subsection.

At the time of granting, interest-only loans (lines of credit) shall not exceed 60 per cent of the property's value, calculated in accordance with the first subsection.

All loans secured on residential property shall be included in the calculation of the loan-to-value ratio, including joint debt in housing cooperatives and jointly owned properties.

§ 8. Flexibility

Financial institutions may grant loans in breach of one or more of the requirements of Sections 3, 4, 5 and 7 for up to 10 per cent of the value of total loans granted each quarter.

The first subsection does not apply to mortgages secured on residential property in the municipality of Oslo. Each quarter, financial institutions may grant mortgages secured on residential property in the municipality of Oslo that do not meet one or more of the requirements of Sections 3, 4, 5 and 7 for up to 8 per cent of the value of total mortgages granted in the municipality of Oslo, alternatively for up to NOK 10 million.