WORKING PAPER

EFFECTIVENESS OF A SOFT LTV LIMIT

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Effectiveness of a Soft LTV Limit

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Abstract

This paper evaluates the effectiveness of a soft Loan-to-Value (LTV) limit as a borrower-based macroprudential tool. Our analysis is based on detailed loan-level data from a major Belgian bank, covering all new mortgage originations between 2016 and 2021. Using the 2020 Belgian LTV recommendations as a quasi-natural experiment, we analyze how a non-binding framework that allows tolerance margins affects mortgage lending behavior. We develop a hybrid approach combining machine-learning (ML) predictions of counterfactual treatment status with a difference-in-differences (DiD) and triple-differences (DDD) design. The ML model, trained on pre-policy data, identifies borrowers likely to exceed the 90% LTV threshold in absence of the reform, allowing consistent treatment classification across periods. Our results show that the introduction of a soft LTV limit leads to a significant decline in average LTV ratios and in the share of high-LTV loans (higher than 90%), with stronger effects among constrained borrowers. The adjustment occurs gradually, reflecting banks' progressive adaptation to supervisory expectations rather than abrupt credit rationing. The reduction in leverage is primarily achieved through higher down payments, which leads to lower monthly repayments and thus lower credit risk. When focusing on the exceptions we find convincing evidence that banks especially favor first-time-buyers, since they remain significantly more present in the above 90% mortgage segment compared to non-FTB. Differences in age, savings, and income also lead to differentiated treatment effects, indicating that banks apply the soft limits in a risk-sensitive and targeted manner. These findings demonstrate that soft borrower-based measures can achieve prudential objectives similar to hard caps without exacerbating credit exclusion. From a policy perspective, the Belgian experience highlights that supervisory guidance can effectively curb excessive leverage while maintaining mortgage accessibility.

1 Introduction

Since the Great Financial Crisis (GFC) policymakers realize that monitoring the developments in the residential real estate markets is important. Since bank loans for house purchase are an important determinant of evolutions in the real estate market, excessive mortgage lending may be a source of vulnerability for the financial sector (Jordà et al., 2016; Mian & Sufi, 2011). Consequently, bank supervisors closely observe these market dynamics to intervene proactively. This has led to the implementation of a wide range of macroprudential policies around the world (Akinci & Olmstead-Rumsey, 2018; Cerutti et al., 2017). Borrower-based measures have become the

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most popular type of macroprudential policies. They focus on the behavior of borrowers, aiming to directly target credit growth and leverage among households.

Over the last 15 years, several countries have introduced limits on the Loan-To-Value (LTV) of mortgage loans, implying that households can only borrow a certain percentage of the value of the property they want to acquire. Evidence from countries that implemented hard LTV limits such as the Netherlands (van Bekkum et al., 2024) and Portugal (Abreu et al., 2024), shows that such caps are effective in reducing average LTV ratios and dampening credit growth. However, these policies may entail distributional effects, particularly disadvantaging first-time buyers (FTBs) and lower-income households that typically rely on higher leverage to enter the housing market. Hard limits, by definition, leave no room for banks to exercise discretion, which can increase the risk of credit rationing or a shift toward unregulated lending channels. For this reason, several countries instead adopt a soft limit. In general, soft limits take the form of thresholds combined with tolerance margins, allowing a predefined share of loans to exceed the threshold[1]. Belgium is among the countries applying a soft LTV limit. In 2019, the National Bank of Belgium (NBB) announced regulatory expectations in which they demand Belgian banks to limit the LTV of new mortgages to 90% for owner-occupied and 80% for buy-to-let loans, with the possibility to exceed these threshold for a given percentage of their loan portfolio. The Belgian approach is designed to achieve similar risk-reducing objectives while allowing for selective exceptions based on borrower risk profiles. FTBs, for example, benefit from more generous tolerance margins than repeat buyers. Whether such flexibility enhances or undermines policy effectiveness remains an open empirical question.

This paper contributes to this debate by providing a causal evaluation of a soft LTV limit framework. Our analysis is based on detailed loan-level data from a major Belgian bank, covering all of their new mortgage origination between 2016 and 2021 and containing extensive borrower, loan, and property information. We exploit the Belgian reform as a quasi-natural experiment to investigate how a non-binding, discretionary framework affects lending behavior across borrower types and over time. Specifically, we assess whether or not the policy succeeds in reducing LTV ratios, how it influences the share of highly leveraged loans, and which borrower groups benefit from the tolerance margins. We combine Machine Learning (ML) prediction techniques with difference-in-differences (DiD) and triple-differences (DDD) estimations. The key challenge in identifying the causal impact of borrower-based measures lies in the endogeneity of treatment status: once the regulation is in place, observed LTVs are constrained by the policy itself. Therefore, we employ a ML model trained on pre-policy data to predict which borrowers would have exceeded the 90% threshold in absence of the policy. This predictive algorithm allows us to classify loans ex ante as *Treated* (likely constrained) or *Control* (likely unconstrained) and to maintain consistent treatment definitions before and after the reform. By combining these predictions with a DiD setup, we isolate the effect of the new limits from concurrent market developments. Our approach thus extends the empirical macroprudential literature in two relevant ways. First, we introduce a hybrid ML-DiD framework that provides a credible counterfactual treatment assignment in repeated cross-sectional data. Second, we apply this framework to a non-binding policy context, where compliance depends on banks' internal risk assessments and strategic behavior rather than strict enforcement.

The results reveal several key findings. First, the introduction of soft LTV limits was followed by a significant decline in both average mortgage LTV ratios and the share of loans with LTVs above 90%. Constrained borrowers

[1] An exception is Israel, where a soft limit was introduced in 2010 that did not involve tolerance margins. Instead, banks were subject to higher capital requirements for loans exceeding the threshold (Tzur-Ilan, 2023).

experience significantly larger reductions than unconstrained ones, about 2 percentage points (pps) more in average LTV and roughly 4 pps more in the share of high-LTV loans. These effects emerge gradually, reflecting banks' progressive adaptation to the new supervisory expectations rather than an abrupt policy-induced cutoff. Second, the reduction in LTV is primarily achieved through higher down payments, combined with lower monthly repayments, decreasing the credit risk. Third, the heterogeneity analysis shows that the flexible design mitigates some of the adverse distributional effects typically associated with hard limits. As intended by the policy, constrained First-Time-Buyers (FTB) are more likely to benefit from the tolerance margins, maintaining access to high-LTV credit. Differences in age, savings, and income also laed to differentiated treatment effects, indicating that banks apply the soft limits in a risk-sensitive and targeted manner.

Taken together, these results suggest that soft borrower-based measures can achieve the desired prudential outcomes without inducing credit rationing. From a policy perspective, this finding is particularly relevant for jurisdictions concerned about balancing financial stability and housing affordability. The Belgian experience highlights that macroprudential effectiveness does not necessarily require strict binding rules and that supervisory expectations can serve as an effective complement to formal regulatory instruments.

The remainder of the paper is organized as follows. Section 2 describes the institutional background of the Belgian mortgage market and outlines the design of the Belgian soft LTV recommendation. Section 3 reviews the related literature on borrower-based macroprudential instruments. Section 4 presents the data and variable construction, while Section 5 details the empirical methodology. Section 6 reports the prediction results and the construction of the *Treated* indicator. Section 7 discusses the main results on the overall effectiveness of the soft limits, whereas Sections 8, 9, and 10 examine, respectively, the heterogeneous effects across borrower types, the policy's impact on other loan and property characteristics, and the treatment of "pockets of risk"[2]. Section 11 presents robustness checks and Section 12 concludes.

2 Institutional background

The Belgian real estate market displays several distinctive characteristics. First, it did not suffer as much as other euro area countries from the Great Financial Crisis (GFC). Despite a modest increase in mortgage defaults (peaking at only 1.4%) the market did not experience substantial corrections and even remained slightly overvalued (National Bank of Belgium, 2012, 2020). Second, the default rates on Belgian mortgage loans have historically been very low, never going above 1.4% since 2006 and even reaching as low as 0.9% in 2019 (National Bank of Belgium, 2020). This resilience can be linked to the high rate of owner-occupied housing (well above 70%) in Belgium (National Bank of Belgium, 2012, 2020). Third, the mortgage market is highly competitive, which is reflected by narrow commercial margins and a growing tendency to secure loans through mandates rather than traditional mortgage registrations (National Bank of Belgium, 2022). Finally, Belgian households have a high preference for fixed-rate mortgages. This feature, combined with the possibility to refinance a mortgage loan at a small cost, is an additional cause of the slim commercial margins on mortgage loans.

The period of low-for-long interest rates resulted in an easier access to mortgage loans sparking high demand for residential property and subsequently driving up real estate prices and increasing overvaluation. The easier

[2] "Pockets of risk" is the term used by the National Bank of Belgium (NBB) to describe loans that combine a high LTV (above 90%) with a high DSTI (above 50%).

access to mortgage loans also entailed a reduction in overall credit quality which, according to the macroprudential supervisor, was not combined with an adequate risk-based pricing (National Bank of Belgium, 2020). After a first warning in 2016[3], the European Systemic Risk Board (ESRB) issued a recommendation to the competent Belgian authorities (the NBB) in 2019 concerning vulnerabilities in the real estate market. Highlighting concerns such as overvalued house prices, escalating household debt, loose credit standards, and continuous growth in mortgage credit, the ESRB emphasized the need for borrower-based regulatory measures (European Systemic Risk Board, 2019).

Responding to this recommendation, the National Bank of Belgium (NBB) issued regulatory expectations on LTV limits, including specific tolerance margins. The exact thresholds and tolerance margins are presented in Table 1. In general the LTV is capped at 90% for owner-occupied properties, with defined tolerance margins. Acknowledging the importance of maintaining mortgage access for first-time buyers (FTBs), the NBB set less stringent LTV expectations allowing up to 35% of new FTB mortgages to exceed 90% LTV, and 5% to exceed an LTV of 100%. Conversely, Buy-to-Let (BTL) loans, which are more susceptible to defaults, are subject to stricter LTV limits (<80%). Additionally, the NBB has identified and addressed "pockets-of-risk", focusing on loans that exhibit both high LTV and high Debt-Service-To-Income (DSTI) ratios, as these are particularly vulnerable to default risk. In this paper we focus on the Owner-Occupied mortgage loans and how they are impacted by the LTV limits. We do not consider BTL loans since these loans concern real estate transactions for investment purposes, hence these are a different type of borrowers with different incentives and risk profiles.

Table 1: Supervisory expectations introduced by the National Bank of Belgium (NBB) in January 2020

Limit	**Loan Type**		**Threshold**	**Tolerance margins**
LTV	Owner-Occupied	FTB	90%	35% with LTV >90% 5% with LTV >100%
		Other OO	90%	20% with LTV >90% 0% with LTV >100%
	BTL		80%	10% with LTV >80% 0% with LTV >90%
Pockets-of-risk	All loans		LTV < 90% & DSTI < 50%	5%
			LTV < 90% & DTI < 9%	

[3]In 2016, Belgium was one of eight countries to receive a warning (ESRB/2016/06) from the European Systemic Risk Board (ESRB) following a Union-wide assessment of vulnerabilities in residential real estate markets. The ESRB highlighted rapid growth in both house prices and mortgage lending, already high and rising household indebtedness, and an increasing share of mortgagors potentially vulnerable to adverse economic conditions or developments in the Belgian housing market.

3 Literature

LTV caps have become a widely adopted tool in the macroprudential policy toolkit aimed at curbing excessive credit growth, reducing systemic risk and promoting financial stability. Several studies have explored the impact on mortgage lending, housing markets and borrower behavior. Our research adds to this literature by examining the impact of Belgium's 2020 introduction of a soft LTV limit using a machine-learning supported difference-in-difference framework. This paper contributes to two strands of the literature. First, it adds to the growing body of research on borrower-based macroprudential policies (Abreu et al., 2024; Acharya et al., 2022; Akinci & Olmstead-Rumsey, 2018; Alam et al., 2025; Armstrong et al., 2019; Cerutti et al., 2017; Claessens et al., 2013; de Araujo et al., 2020; De Veirman & De Jong, 2025; Dirma & Karmelavičius, 2025; Grodecka, 2020; Higgins, 2024; Hodula et al., 2023, 2025; Kinghan et al., 2022; Kuttner & Shim, 2016; Poghosyan, 2020; Tzur-Ilan, 2023; van Bekkum et al., 2024) by providing new evidence on the performance of soft LTV limits. Second, it advances the empirical methodology by combining ML-based counterfactuals with quasi-experimental inference, aligning with recent calls for integrating data-driven tools in policy evaluation (Athey & Imbens, 2019; Mullainathan & Spiess, 2017).

A number of cross-country studies have established the effectiveness of borrower-based tools in containing credit and housing booms (Akinci & Olmstead-Rumsey, 2018; Alam et al., 2025; Cerutti et al., 2017; Kuttner & Shim, 2016; Poghosyan, 2020). They show that LTV and DSTI limits are successful in reducing credit and housing price growth, especially in boom periods. Claessens et al. (2013) emphasize the broader systemic motivation for borrower-based regulations as they can help mitigate the procyclicality of credit and reduce the risk of household financial distress. They also underline the importance of the timing and calibration of the policies, they are best implemented in boom periods and adjusted to the country's circumstances. As shown by Morgan et al. (2019), the characteristics of financial institutions also play a role in the effectiveness of the LTV policies, e.g. LTV caps constrain mortgage credit more effectively in banks with higher exposure to non-performing loans. Alam et al. (2025) find evidence of nonlinear effects on household credit, with effects being smaller for larger tightenings, possibly due to policy leakage.

Recent research has increasingly used administrative and loan-level data to estimate the causal impacts of LTV regulations, typically focusing on single-country analyses that fully account for country-specific features. Such country-specific analyses are relevant because the design and implementation of LTV limits vary substantially across jurisdictions, including differences in thresholds, bindingness, the use of hard versus soft caps and, in the case of soft limits, the size of the tolerance margins (Mokas & Giuliodori, 2023). This type of granular data makes it possible to analyze heterogeneous borrower responses. Table 2 summarizes the various countries that introduced LTV limits and highlights key studies employing loan- or household-level data.

Table 2: Micro-Level Empirical Studies of LTV Limits by Country

Country	Policy Description	References
Sweden	**Hard** LTV limit introduced in July 2010. No formal DSTI limits, although banks routinely assess both LTV and DSTI when evaluating applications.	Grodecka (2020)
Israel	**Soft** LTV limit introduced in October 2010, **hard** LTV limits in November 2012. Some borrower categories (e.g., investors) faced stricter limits. No DSTI limits.	Tzur-Ilan (2023)
Netherlands	**Hard** LTV cap introduced in August 2011, gradually decreased from 106% to 100% LTV over the subsequent years. No formal DSTI cap. Very specific/limited exceptions.	De Veirman and De Jong (2025) and van Bekkum et al. (2024)
Lithuania	**Soft** LTV cap introduced in November 2011, in combination with DSTI limits. Later more stringent rules were introduced for specific cases.	Dirma and Karmelavičius (2025)
Brazil	**Hard** LTV cap introduced in September 2013. No formal DSTI limit but use of stricter criteria as well.	de Araujo et al. (2020)
New Zealand	**Soft** LTV limit introduced in October 2013 and adjusted in 2015 & 2016. Exemption for new builds. Exact thresholds and tolerance margins depend on owner-occupied status and whether it is in Auckland (following the adjustments in 2015 & 2016)	Armstrong et al. (2019)
Ireland	**Soft** LTV limit and LTI limit introduced in February 2015. With limited exceptions for FTBs and second-time buyers. Exact limit differs depending on property price, the higher the price the stricter the limit.	Acharya et al. (2022), Higgins (2024), and Kinghan et al. (2022)
Czech Republic	**Soft** LTV cap gradually introduced since June 2015, and later DSTI and DTI caps. Enforcement was strengthened over time. In 2020 there was an easing of the borrower-based measures.	Hodula et al. (2023, 2025)
Portugal	**Hard** LTV cap at 90% introduced in July 2018. Part of a broader macroprudential package including DSTI guidelines.	Abreu et al. (2024)
Belgium	**Soft** LTV cap introduced in 2020 (90% for general borrowers, with tolerance margins). DSTI and/or DTI limit in certain cases. See Table 1 for more info.	this paper

Notes: This table summarizes several recent empirical studies using loan-level (micro) data to assess the effectiveness of LTV limits across countries. DSTI and DTI denote debt-service-to-income and debt-to-income limits, respectively. This list is not exhaustive.

A consistent finding across these studies is that average LTV ratios decrease following the introduction of LTV caps, although the impact varies considerably across contexts. For instance, van Bekkum et al. (2024) identify an average decline of 4.9 percentage points (pp), whereas Kinghan et al. (2022) report a more modest reduction of 1.4 pp. Additionally, several studies document pronounced bunching just below the regulatory thresholds (Tzur-Ilan, 2023; van Bekkum et al., 2024). The literature diverges regarding the mechanisms behind these observed reductions in LTV ratios. In principle, reductions in LTV can result either from increased down payments, purchases of less expensive properties, or a combination of both. Most studies find evidence supporting both mechanisms simultaneously (de Araujo et al., 2020; Tzur-Ilan, 2023; van Bekkum et al., 2024). However, exceptions exist; notably, Kinghan et al. (2022) find no significant evidence indicating that borrowers shift toward cheaper properties. Higgins (2024) goes further by demonstrating heterogeneity across income groups: wealthier borrowers tend to increase their down payments, whereas less affluent borrowers more often opt for less expensive properties. Additionally, Armstrong et al. (2019) find that house price inflation in New Zealand decreased following the introduction of LTV limits. However, they stress that the size and duration of this effect depend on prevailing macroeconomic conditions, noting in particular that the impact of LTV limits is more muted during periods of strong house price growth.

Next to LTV, other loan characteristics are impacted as well. Effects are found on interest rates, maturities and DSTI. Contrary to initial expectations, several studies document an increase in interest rates for constrained borrowers (Abreu et al., 2024; de Araujo et al., 2020; Tzur-Ilan, 2023). de Araujo et al. (2020) attribute this finding to banks increasingly adopting risk-based pricing strategies, as LTV restrictions heighten banks' awareness of borrower risks, leading to higher interest charges despite lower LTV ratios. In addition, some studies identify an increase in DSTI ratios and loan maturities for constrained borrowers, likely driven by higher interest rates (Abreu et al., 2024; Tzur-Ilan, 2023). While Acharya et al. (2022) observe a reallocation of credit from low- to high income borrowers and Kinghan et al. (2022) and Higgins (2024) find indications that the effects of the limits differ across the income distribution in Ireland, Tzur-Ilan (2023) findings suggest that there was no change in the distribution of borrower's observable characteristics (such as income) in Israel after the implementation of the limits. Both Acharya et al. (2022) and Tzur-Ilan (2023) identify spatial reallocation, with borrowers shifting from urban to more rural areas. Tzur-Ilan (2023) notes movement away from central business districts toward neighborhoods with lower socioeconomic conditions, while Acharya et al. (2022) find that borrowers leave urban areas, where LTVs are typically close to the limit, and relocate to rural areas where LTVs tend to be lower. Acharya et al. (2022) further show that this reallocation slowed house price growth in "hot" markets and Higgins (2024) report similar declines in postal codes where many borrowers previously exceeded loan-to-income (LTI) limits.

In addition, some studies express concerns related to potential unintended consequences of LTV caps, such as increased risk-taking by borrowers and lenders compensating for restricted mortgage lending. Both van Bekkum et al. (2024) and de Araujo et al. (2020) find that mortgage arrears declined significantly, supporting the hypothesis that loans originating after the policy change were, on average, less risky. However, while van Bekkum et al. (2024) find that households do not turn towards other types of credit to compensate lower mortgage debt in the Netherlands, Tzur-Ilan (2023) states that, in Israel, constrained borrowers turned towards unregulated forms of credit (e.g. consumer credit) which are typically more expensive and risky. Additionally, in Ireland, Acharya et al. (2022) observe a risk-shifting effect since banks that are more affected by the constraints shift towards holdings of securities and corporate credit to seek higher yields.

Lastly, a broader debate arises regarding the relative effectiveness of different borrower-based measures. In practice, LTV limits are often introduced alongside other constraints. Hodula et al. (2023) argue that in Czechia DSTI and DTI limits had a greater impact than LTV caps on the loan amounts and on incentivizing the banks to apply risk-based pricing. Additionally, Grodecka (2020) shows, using Swedish data, that when borrowers are already constrained by DSTI limits, the marginal impact of LTV caps is limited. Dirma and Karmelavičius (2025) further highlight the value of combining multiple borrower-based instruments, since they find that while LTV are effective to manage loss given default, DSTI caps are more suitable to address mortgage default risk. Although the precise impacts vary across countries and policy designs, these studies suggest that policymakers may benefit from deploying a broader mix of borrower-based tools rather than relying on LTV limits alone.

4 Data

For our research we use loan-level data from 87342 mortgage loans granted by a major Belgian bank[4] over the period 2016-2021[5]. The available dataset consists of information about mortgage loans at origination. It includes loan characteristics, borrower characteristics and information about the property. Buy-To-Let (BTL) loans are excluded, we only focus on loans with the purpose of acquiring/building a property designed to be occupied by the owner. Loans with a maturity less than 2 years are excluded. A list of variables as well as the summary statistics for the non-categorical variables can be found in Tables 3 and 4. We argue that this dataset is representative for the entire Belgian population. The bank is active in all three Belgian regions (Flanders, Brussels, Wallonia) and the distribution of the loan characteristics closely relates to those published by the National Bank of Belgium (NBB) covering the entire mortgage market. This close alignment suggests that our sample is representative of the Belgian mortgage market as a whole, allowing us to generalize the findings.

To preserve confidentiality some of the borrower-related variables are provided in the form of buckets instead of the exact (continuous) values (see Table 3). To simplify our analysis these bucketed variables are transformed into continuous variables[6]. We also construct three binary indicators used throughout the analysis: *Young*, *lowIncome*, and *lowSavings*. The *Young* dummy equals 1 for borrowers aged 35 years or younger, *lowIncome* equals 1 for borrowers with a (combined) monthly net income below €3,000 (measured as the amount that arrives on their account), *lowNAI* equals 1 for borrowers with a (combined) Net Available Income below €1,500 and *lowSavings* equals 1 for borrowers holding €30,000 or less in savings with the observed bank.

[4]The Belgian banking market is characterized by four large players, which among each other account for +/- 80% mortgage lending

[5]In subsequent graphical representations we show data up to 2023, but we restrict the empirical analysis to the period up to 2021 to avoid confounding influence from rising monetary policy rates and mortgage rates from 2022 onward.

[6]For instance, borrower age is provided as categories (<25, 25–30, 30–35, . . . , 65<), which we transform into midpoints (22.5, 27.5, 32.5, . . . , 67.5). This transformation allows us to capture age effects with a single continuous variable rather than multiple dummies.

Table 3: Overview of Variables Used in the Analysis

Variable	Description	Scale / Levels	Variable Type	Related To
DSTI	Debt-Service-to-Income ratio (monthly debt payments divided by monthly income)	Percentage	Continuous	Borrower
LTV	Loan-to-Value ratio (loan amount divided by property value)	Percentage	Continuous	Loan
Interest	Annual interest rate on the loan	Percentage	Continuous	Loan
Maturity	Loan term until full repayment	Months	Continuous	Loan
Age	Age of the primary borrower	10 buckets	Bucketed	Borrower
Savings	Total savings (all borrowers combined)	26 buckets	Bucketed	Borrower
Income	Gross monthly income (all borrowers combined)	14 buckets	Bucketed	Borrower
NAI	Net Available Income after debt payments	14 buckets	Bucketed	Borrower
LoanSize	Total amount borrowed	21 buckets	Bucketed	Loan
Value	Value of the collateral property	21 buckets	Bucketed	Collateral
Segment	Client segment (low-savings retail, high-savings retail, privilege, private&wealth)	4 categories	Categorical	Borrower
Profession	Employment type of the borrower	4 categories	Categorical	Borrower
Risk	Internal risk classification assigned by the bank	13 categories	Categorical	Borrower
LoanPurpose	Purpose of the loan (purchase and building)	2 categories	Categorical	Loan
Region	Region where the collateral is located	3 categories	Categorical	Collateral
Province	Province of the collateral	10 categories	Categorical	Collateral
Municipality	Anonymized municipality of the collateral	581 categories	Categorical	Collateral
FTB	First-Time Buyer	0/1	Dummy	Borrower
Cosigner	Presence of a co-borrower on the loan	0/1	Dummy	Borrower
Fixed	Fixed rate loan (versus variable rate)	0/1	Dummy	Loan
House	Property is a house (versus apartment)	0/1	Dummy	Collateral

Table 4: Summary Statistics for Borrower and Loan Characteristics Before and After the Soft LTV Limits (Training 2016–2018, Pre 2019, Post 2020–2021)

	Age	Income	NAI	Savings	FTB	Cosigner	LoanSize	Maturity	Interest	Fixed	LTV	DSTI	Value
Training period (2016–2018)													
count	42376	42376	42376	42376	42376	42376	42376	42376	42376	42376	42376	42376	42376
mean	36.66	3200	1759	56284	0.74	0.56	188301	220.73	1.88	0.88	77.54	40.22	273940
std	10.88	1432	1013	89995	0.44	0.50	112700	58.82	0.50	0.33	24.32	13.11	156881
min	22.50	1750	500	0	0.00	0.00	25000	25.00	0.27	0.00	13.00	14.00	25000
25%	27.50	1750	1125	7500	0.00	0.00	125000	180.00	1.60	1.00	62.00	32.00	175000
50%	32.50	3125	1625	22500	1.00	1.00	175000	240.00	1.85	1.00	85.00	39.00	225000
75%	42.50	3750	2125	67500	1.00	1.00	225000	240.00	2.10	1.00	100.00	47.00	325000
max	67.50	8000	6000	550000	1.00	1.00	1100000	360.00	4.62	1.00	113.00	81.00	1100000
Pre period (2019)													
count	18008	18008	18008	18008	18008	18008	18008	18008	18008	18008	18008	18008	18008
mean	35.62	3321	1782	55517	0.75	0.55	216941	245.51	1.64	0.97	79.46	39.83	306841
std	10.47	1461	1020	85673	0.44	0.50	120131	58.01	0.42	0.16	23.32	12.25	171637
min	22.50	1750	500	0	0.00	0.00	25000	27.00	0.50	0.00	13.00	16.00	25000
25%	27.50	2250	1125	7500	0.00	0.00	125000	227.59	1.37	1.00	66.00	32.00	175000
50%	32.50	3125	1625	22500	1.00	1.00	225000	240.00	1.62	1.00	88.00	38.71	275000
75%	42.50	4250	2375	67500	1.00	1.00	275000	300.00	1.85	1.00	100.00	46.75	375000
max	67.50	8000	6000	550000	1.00	1.00	1100000	306.00	4.38	1.00	109.00	74.00	1100000
Post period (2020–2021)													
count	26958	26958	26958	26958	26958	26958	26958	26958	26958	26958	26958	26958	26958
mean	36.05	3523	1942	82899	0.72	0.53	222271	251.68	1.43	0.99	76.23	36.99	333636
std	10.72	1609	1119	96462	0.45	0.50	132337	60.11	0.37	0.09	21.92	10.92	191828
min	22.50	1750	500	0	0.00	0.00	25000	25.00	0.42	0.00	14.00	14.88	25000
25%	27.50	2250	1125	22500	0.00	0.00	125000	240.00	1.17	1.00	63.00	29.76	225000
50%	32.50	3375	1625	52500	1.00	1.00	225000	280.00	1.38	1.00	83.00	36.00	275000
75%	42.50	4250	2625	97500	1.00	1.00	275000	300.00	1.65	1.00	91.00	43.00	375000
max	67.50	8000	6000	550000	1.00	1.00	1100000	301.00	3.18	1.00	100.00	67.00	1100000

Notes: This table reports descriptive statistics for all mortgage loans in the sample, subdivided into three periods: the training sample (2016–2018), the pre-policy period (2019), and the post-policy period (2020–2021). Monetary values are expressed in euros. Maturity is expressed in months. For each variable, the table shows the count, mean, standard deviation, minimum, quartiles, and maximum. *LoanSize* and *Value* denote the loan amount and property value, respectively; *DSTI* is the debt-service-to-income ratio; *LTV* is the loan-to-value ratio; *NAI* is net available income; *Cosigner* equals one if a loan has a co-borrower; and *Fixed* equals one for fixed-rate contracts.

5 Methodology

5.1 Difference-in-differences methodology

The objective of our analysis is to quantify the impact of the LTV restrictions on the LTV ratio itself as well as on other loan characteristics, including interest rates, maturity, loan amount, property value, and DSTI. To assess the causal effect of the reform, we employ a difference-in-differences (DiD) specification (Equation 1).

$$Y_i = \beta_0 + \beta_1 \cdot Post_t + \beta_2 \cdot Treated_i + \beta_3 \cdot Post_t \cdot Treated_i \; (+\sum_{n=1}^{N} \gamma_n \cdot Controls_{n,i}) + \pi_r + \omega_t + \epsilon_m \tag{1}$$

The dependent variable Y_i alternately denotes the LTV ratio, a dummy for LTV > 90 (*LTVabove90*), or other loan characteristics of interest. The variable $Post_t$ equals one after the implementation of the policy, while $Treated_i$ is a dummy equal to one for household i predicted to prefer an LTV above 90% in the absence of the policy. All specifications include region (or municipality) fixed effects (π_r) and time (year–quarter) fixed effects (ω_t), with standard errors clustered at the municipality level.

The validity of the DiD relies on two assumptions: (i) in the absence of the policy, treated and control groups would have followed parallel trends, and (ii) neither households nor banks anticipated the reform in ways that would bias pre-treatment dynamics. Because our setting involves a repeated cross-section, rather than panel data, it raises concerns regarding group consistency over time. To address this potential issue, we include borrower controls, following Kinghan et al. (2022). Alternative approaches include matching techniques (Tzur-Ilan, 2023) or restricting the sample to loans close to the regulatory threshold, as in De Araujo et al. (2020).

To investigate whether banks use their tolerance margins to selectively grant high-LTV loans to specific borrower types, we employ two complementary strategies: (i) a triple-differences (DDD) specification on the full sample, and (ii) DiD regressions on subsamples based on borrower characteristics.

The DDD specification takes the following form:

$$\begin{aligned} Y_i = \; & \beta_0 + \beta_1 \cdot Post_t + \beta_2 \cdot Treated_i + \beta_3 \cdot Post_t \cdot Treated_i + \beta_4 \cdot Z + \beta_5 \cdot Post_t \cdot Z + \beta_6 \cdot Treated_i \cdot Z \\ & + \beta_7 \cdot Post_t \cdot Treated_i \cdot Z (+\sum_{n=1}^{N} \gamma_n \cdot Controls_{n,i}) + \pi_r + \omega_t + \epsilon_m \end{aligned} \tag{2}$$

where Z_i denotes a borrower characteristic of interest (e.g., FTB status, age group, or income). The coefficient of interest, β_7, captures whether the treatment effect differs across borrower types. It is important to note that with our data only the intensive margin[7] can be analyzed. We do not observe rejected loan applications, nor can we identify households who may have refrained from applying due to the stricter lending expectations. This limitation underscores that the observed effects likely represent a lower bound of the policy's overall impact.

[7] Only changes in loan conditions among households who actually obtain a mortgage can be observed.

5.2 Treated versus Control: prediction methods

Implementing a DiD with repeated cross-sections requires four groups: pre-policy treated, pre-policy control, post-policy treated, and post-policy control. The main challenge is classifying loans in the post-policy period, since observed LTVs are constrained by the regulatory cap. Accordingly, we predict which loans would have exceeded the 90% LTV threshold had the policy not been introduced. Prediction models are trained on pre-policy data up to the end of 2018, while 2019 serves as a buffer year to ensure consistent treatment of pre- and post-policy observations, since the DiD analysis covers the 2019–2021 period and the policy started in 2020[8].

The literature proposes several approaches to construct counterfactual treatment status. de Araujo et al. (2020), following Botosaru and Gutierrez (2018), apply propensity score matching based on wage and location fixed effects. Tzur-Ilan (2023) and Abreu et al. (2024) use OLS regressions to predict counterfactual LTV ratios using borrower income (and age in Tzur-Ilan (2023)), classifying loans as *Treated* if the predicted LTV exceeds the threshold.

We first adopt the regression approach, following methodologies applied by Abreu et al. (2024) and Tzur-Ilan (2023). Using pre-policy data (2016–2018), we regress LTV on age and income, consistent with prior studies, and subsequently extend the specification with additional covariates. As an alternative, we also estimate a binary model of the probability of surpassing the 90% LTV threshold:

$$LTV_i = \alpha_0 + \alpha_1 \cdot Age_i + \alpha_2 \cdot Income_i + \alpha_3 \cdot Savings_i + ... + \pi_r + \omega_t + \mu \tag{3}$$

$$Pr(LTV > 90)_i = \alpha_0 + \alpha_1 \cdot Age_i + \alpha_2 \cdot Income_i + \alpha_3 \cdot Savings_i + ... + \pi_r + \omega_t + \mu \tag{4}$$

Predicted values from these models are then used to classify loans in 2019–2022 as *Treated* or *Control*.

To improve classification accuracy, we prefer to apply ML methods, which generally outperform linear models (Mullainathan & Spiess, 2017). We use Random Forests, XGBoost, and LightGBM models, trained on 2016–2018 data. Using a fixed training window and generating predictions for both 2019 and 2020–2021 ensures that all counterfactuals are produced by a single model, eliminating the risk that specification differences would drive the results. Two classification approaches are possible: (i) predicting the LTV and assigning treated status when it exceeds 90%, or (ii) predicting the probability of exceeding the threshold and selecting an optimal cutoff based on the match between actual and predicted treatment status in 2019. We adopt the second approach since it provides greater flexibility. Importantly, our predictive models rely only on variables unaffected by the policy introduction. Since loan characteristics may change in response to the policy, we restrict predictors to borrower characteristics (age, income, savings, FTB status, etc.) and geographical controls (province and regional dummies[9]). We further assume stability in loan purpose and property type.

Since treatment status is predicted rather than observed, some misclassification is unavoidable. Consequently, coefficients on the *Treated* dummy and its interactions may represent lower-bound estimates of the true policy effects. This attenuation bias is mitigated by using ML models, which yield higher predictive accuracy than the linear approaches, thereby improving the reliability of the resulting DiD estimates.

[8]Although the policy was only introduced in 2020, and data up to 2019 could be used for training, doing so would imply using observed (true) treatment status for 2019 but predicted status for the post-policy years. This inconsistency could lead to results that reflect model variation rather than policy effects. Therefore, we use predicted treatment status for both pre- and post-policy periods.

[9]This means that we assume that borrowers may relocate within but not across provinces.

6 Prediction results

6.1 Regression method

Following Abreu et al. (2024) and Tzur-Ilan (2023), we first generate counterfactual treatment classifications using a regression approach. The baseline specification includes borrower age and income[10], together with region and province fixed effects. We then extend the model by adding additional borrower characteristics: savings, FTB status, cosigner status, and a property-type dummy.

Figures 1 and 2 present the distributions of the predicted LTVs and probabilities of exceeding the 90% threshold (*LTVabove90*) in 2019 for the observations with an actual LTV above 90% and those with an actual LTV below 90%. The extended model improves the predictions, showing the importance of using all the available borrower information. Nonetheless, the predicted distributions of "above 90" and "below 90" observations remain broadly overlapping. This lack of separation suggests that regression-based predictions alone do not always provide a reliable distinction between the *Treated* and *Control*.

Figure 1: Distribution of predicted LTV and predicted Pr[LTV>90] for 2019 (pre-policy) conditional on the actual LTV being above (orange) or below (blue) 90%; regression method; basic setup

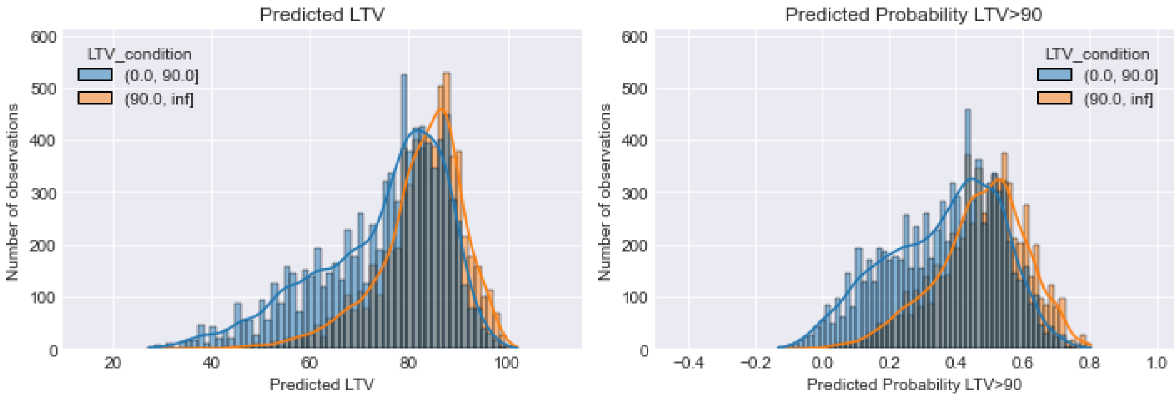
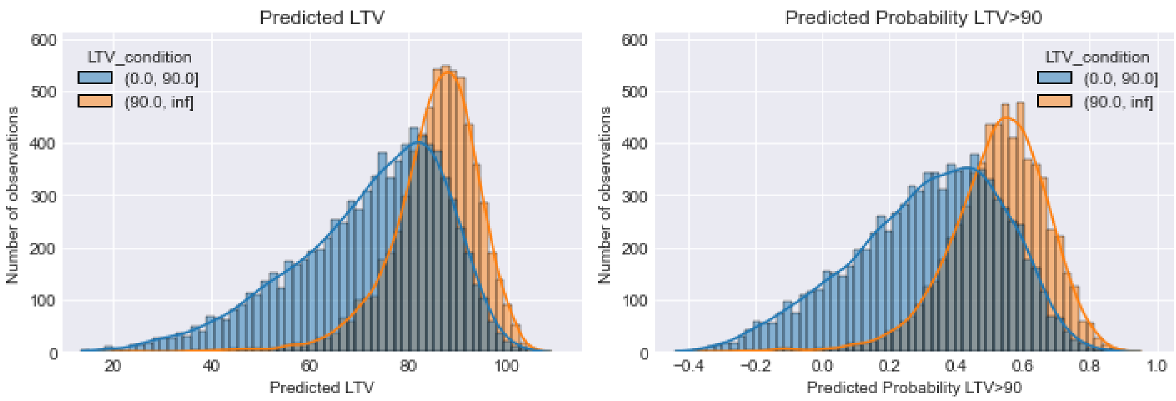


Figure 2: Distribution of predicted LTV and predicted Pr[LTV>90] for 2019 (pre-policy) conditional on the actual LTV being above (orange) or below (blue) 90%; regression method; with additional covariates



[10]The quadratic terms of Age and Income are included

6.2 ML method

We evaluate several prediction models[11] (Random forest, XGBoost and LightGBM[12]) and select the one with the best predictive performance (in terms of RMSE, MAE, MAPE) for use in subsequent analysis. The resulting performance metrics can be found in Table 5. The XGBoost and LightGBM models are very similar in terms of performance and clearly outperform the Random Forest model. As the metrics for the training and test set are the least different for the LightGBM model, indicating almost no overfitting, we choose to continue with those predictions. Importantly, both models identify a consistent set of key predictors (see Appendix B, Figure 13), and as shown in the robustness checks, our results remain stable across model specifications.

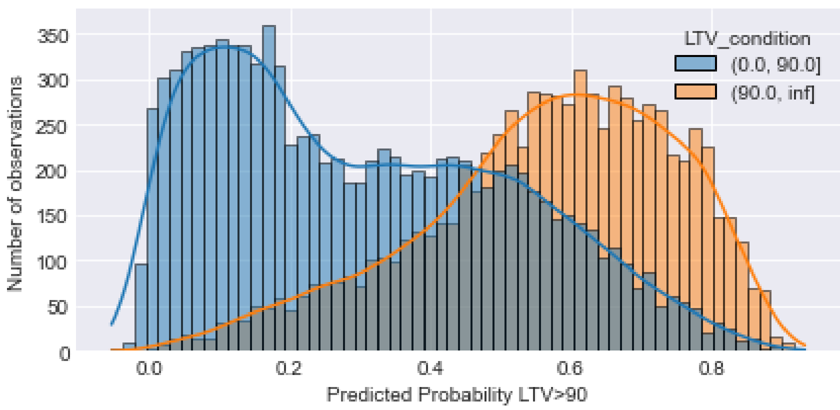
Table 5: Performance of Machine Learning Models Predicting the Probability of LTV > 90%

	Predicting:Probability LTV>90					
	Model 1: Random Forest		Model 2: XGBoost		Model 3: LightGBM	
	Training set	Test set	Training set	Test set	Training set	Test set
R^2	0.379	0.278	0.314	0.294	0.298	0.291
RMSE	0.386	0.422	0.407	0.417	0.412	0.418
MAE	0.333	0.377	0.345	0.359	0.351	0.361
MAPE	751×10^{12}	812×10^{12}	779×10^{12}	768×10^{12}	793×10^{12}	765×10^{12}

Notes: This table reports the predictive performance of three machine learning algorithms (Random Forest, XGBoost, and LightGBM) used to estimate the probability that a mortgage loan's LTV exceeds 90%. Models are trained on 2016–2018 data and evaluated on an out-of-sample 2019 test set. Reported metrics are the coefficient of determination (R^2), root mean squared error (RMSE), mean absolute error (MAE), and mean absolute percentage error (MAPE). Higher R^2 and lower RMSE/MAE/MAPE reflect better predictive accuracy.

Figure 3 plots the distribution of predicted probabilities of exceeding the 90% threshold for the pre-policy year 2019, conditional on the actual observed outcomes being above or below 90%. Although some overlap remains, the distributions are much more clearly separated than in the regression case (Figures 1–2), with natural cutoff points emerging around 0.4–0.6. This illustrates the superior discriminatory power of the ML approach.

Figure 3: Distribution of predicted LTV and predicted Pr[LTV>90] for 2019 (pre-policy) conditional on the actual LTV being above (orange) or below (blue) 90%; ML method



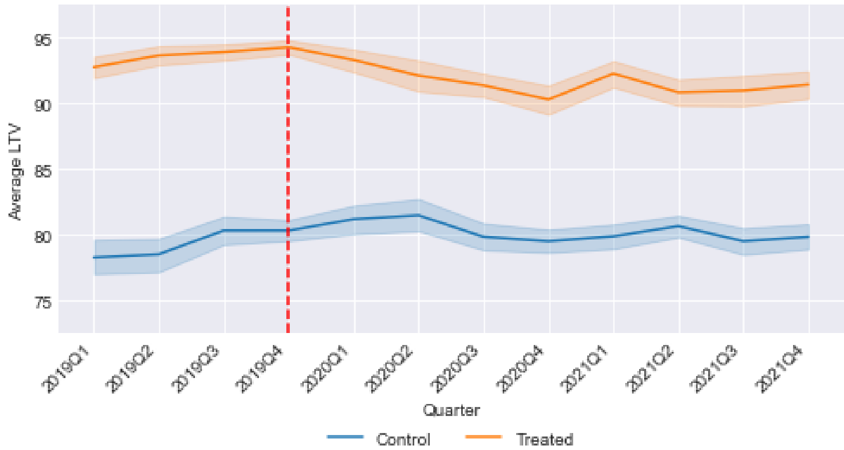
To obtain a classification, we need to impose two thresholds: (i) one that assigns the binary Treated indicator, and (ii) one that excludes loans with very low predicted probabilities, which are fundamentally different from near-threshold borrowers and thus not suitable controls. The derivation of these thresholds is detailed in Appendix B.

[11] Each model has been optimized using hyperparameter tuning and cross-validation.

[12] We use the *HistGradientBoostingRegressor*, which is inspired by *LightGBM* and easily implemented using the scikit-learn package in Python, for simplicity we refer to it as *LightGBM*

Figure 4 illustrates the resulting groups and the evolution of their observed LTVs over time. Loans with predicted probabilities below 0.2 are excluded, those between 0.2 and 0.5 are assigned to the *Control* group, and those at or above 0.575 are considered *Treated*, meaning that loans with a probability between 0.5 and 0.575 are excluded as well. An overview of the resulting number of observations per treatment status, period and borrower type can be found in the Appendix in Table 19.

Figure 4: Evolution average observed LTV for the *Treated* and *Control* groups



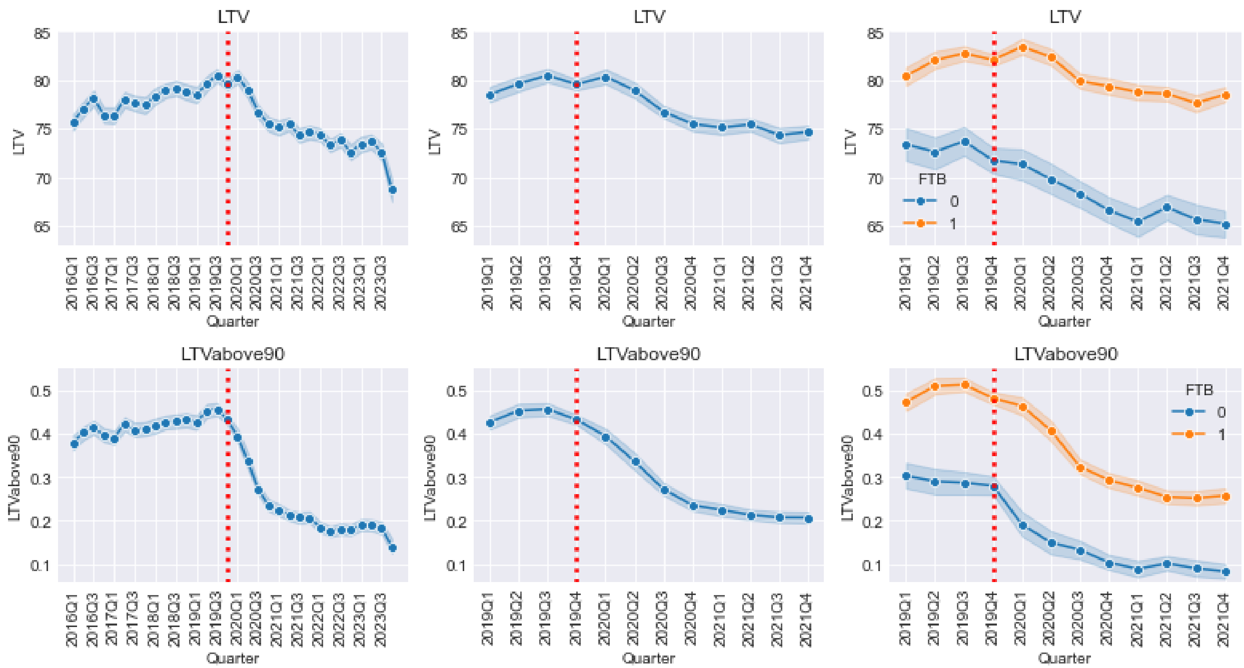
7 Effectiveness of the LTV limit: main effects

7.1 Did the average LTV decrease?

The primary objective of the regulatory recommendations is to reduce the share of new mortgages issued with an LTV above 90%. This, however, provides only an indirect assessment of the policy's effectiveness. A more direct evaluation should consider whether the loan distribution shifted away from the high-risk, highly leveraged, above-90% segment (*LTVabove90*). By analyzing both the continuous LTV measure and the share of loans above 90%, we can more precisely evaluate whether banks adjust their lending practices in line with the supervisory expectations. Therefore, both *LTV* and *LTVabove90* are used in subsequent analyses.

First we examine the overall evolution of mortgage LTV ratios graphically. Figure 5 provides descriptive evidence for the full sample, including observations excluded from the regressions. It illustrates a marked decline in high-LTV lending: in the first column the evolution of both *LTV* and *LTVabove90* over 2016–2023 reveals a consistent downward trend since the introduction of the NBB expectations. To avoid contamination from the inflation surge and the rise of both the ECB policy rate and long term interest rates since the Ukrainian war, we restrict our empirical analysis to 2019-2021, shown in the second column. We observe that the decline of both variables was immediate but gradual. The third column depicts the evolution of *LTV* and *LTVabove90* for FTBs versus non-FTBs, showing that FTBs are in general allowed to borrow at higher LTVs than non-FTBs. Both groups show a decline since the introduction of the soft limits, but the decline in LTV seems to be more pronounced for non-FTBs.

Figure 5: Evolution of *LTV* and *LTVabove90* over 2016-2023 and over the focus period 2019-2021



A simple regression with only a *Post* dummy and borrower controls confirms these patterns. The coefficient for *Post* in Table 6 indicates an average decline of roughly 2 to 3 percentage points (pps) in mortgage LTV ratios during the two years following the policy implementation. A quarterly analysis (Appendix C.1, Table 23) shows that the decline starts in 2020Q1 with about 1–2 pps to gradually reach -4 pps in the last quarters of 2021. A similar pattern emerges for *LTVabove90*, for which an average decline of 16-21 pps can be observed. The progressive reduction suggests that banks require an adjustment period to achieve full compliance, as the new NBB requirements have to be integrated into their loan origination processes. Banks need to incentivize their branch network to apply the new rules, typically by adjusting the commissioning of the branches.

Table 6 further reports coefficients for borrower control variables. Some general patterns emerge. First, FTBs and younger borrowers obtain, on average, higher LTVs. Banks often use a mortgage as a captive product to attract clients with a future cross-sell potential. The positive coefficient for the cosigner dummy means that banks allow higher LTVs when there is a second borrower, as these loans exhibit lower credit risk for the banks since the partners share the debt servicing. The positive and significant coefficient on the income variable suggests that lower-income households face tighter LTV constraints, in line with their lower repayment capacity.

The overall decline in LTVs implies that borrowers face either higher down payment requirements or reduced loan amounts. This results in lower mortgage credit risk for banks, which is beneficial for financial stability. At the macroprudential level, the measure supports the NBB's objectives of reducing household credit burdens and mitigating the risk of excessive increases in housing prices caused by the buildup of higher-risk mortgage loans in the banking system.

Table 6: Regression of *LTV* and *LTVabove90* on a *Post* dummy including borrower characteristics; 2019-2021

	LTV				LTVabove90			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post	-2.987***	-3.009***	-2.013***	-2.174***	-0.210***	-0.211***	-0.164***	-0.168***
	(0.255)	(0.244)	(0.254)	(0.244)	(0.008)	(0.008)	(0.008)	(0.008)
FTB			7.547***	7.509***			0.222***	0.219***
			(0.345)	(0.349)			(0.009)	(0.009)
cosigner			2.416***	2.099***			0.066***	0.058***
			(0.362)	(0.345)			(0.009)	(0.009)
Age			1.010***	1.004***			0.008***	0.007***
			(0.111)	(0.111)			(0.003)	(0.003)
$(\text{Age})^2$			-0.021***	-0.021***			-0.000***	-0.000***
			(0.002)	(0.002)			(0.000)	(0.000)
Inc/1000			3.436***	4.145***			0.098***	0.112***
			(0.441)	(0.439)			(0.011)	(0.011)
$(\text{Inc}/1000)^2$			-0.298***	-0.338***			-0.009***	-0.009***
			(0.046)	(0.046)			(0.001)	(0.001)
Sav/10000			-0.623***	-0.587***			-0.033***	-0.033***
			(0.055)	(0.058)			(0.001)	(0.002)
$(\text{Sav}/10000)^2$			0.006***	0.005***			0.001***	0.001***
			(0.001)	(0.001)			(0.000)	(0.000)
Region FE	x	-	x	-	x	-	x	-
Municipality FE	-	x	-	x	-	x	-	x
Observations	22,681	22,678	22,681	22,678	22,681	22,678	22,681	22,678
R^2	0.011	0.057	0.161	0.203	0.055	0.096	0.154	0.190
R^2 Within	0.006	0.007	0.157	0.160	0.044	0.045	0.145	0.144

Notes: This table reports ordinary least squares (OLS) regressions of loan-to-value (*LTV*) ratios and an indicator for high-LTV loans (*LTVabove90*) on a post-policy dummy (*Post*), equal to 1 for loans originated after the 2020 LTV recommendation. The sample includes the *Control* and *Treated* loans granted between 2019 and 2021. Columns (1)–(2) include only fixed effects; columns (3)–(4) add borrower controls (age, income, savings and their squared terms, plus cosigner and FTB status). Standard errors are clustered at the municipality level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

7.2 Impact on the constrained borrowers

In several countries, LTV limits are implemented as hard limits: no exceptions are permitted for the targeted borrowers and all loans that would otherwise exceed the threshold are forced below it. In such a setting, borrowers most likely to exceed the limit (the *Treated*) would experience sharp reductions in LTV, while others (the *Control*) would remain largely unaffected, yielding clear DiD estimates. Belgium, however, adopted a different approach by introducing a soft limit, allowing banks to grant a certain share of loans above the 90% threshold. This institutional design implies that constrained (*Treated*) borrowers do not have to be uniformly pushed below the threshold, banks can decide which borrowers to include in the exception quota. As a result, the policy effect on constrained borrowers could be weaker and less clear-cut than under a hard limit.

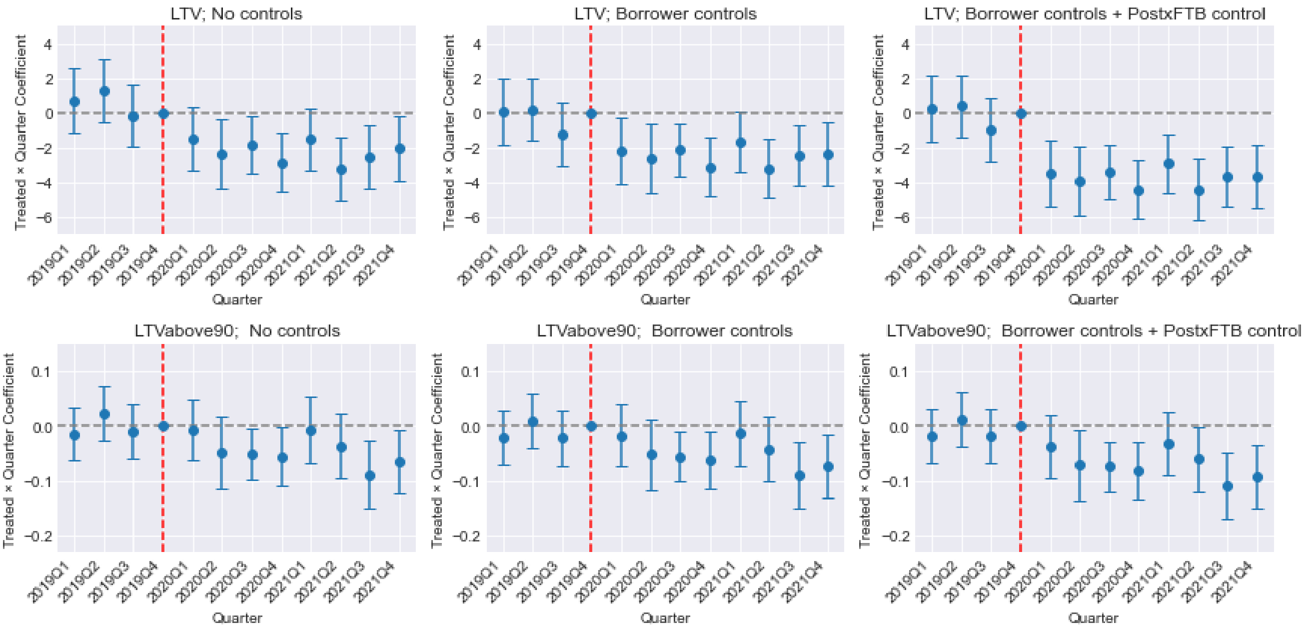
Table 7 reports the DiD estimates, where the interaction term *Post* × *Treated* captures the differential effect of the policy on *Treated* borrowers relative to the *Controls*. The coefficient is negative and significant across all specifications, indicating that borrowers with a high ex ante probability of exceeding the 90% threshold experience stronger reductions in LTV. In the more saturated models, *Treated* borrowers show a 2 pps larger decline in average LTV and a 4 pp greater drop in the likelihood of exceeding 90% relative to *Controls*. Figure 6 is an event-study plot of the quarterly DiD coefficients and demonstrates that the parallel trend assumption holds. The negative treatment effect is clearest in the specifications including a *Post* × *FTB* interaction, suggesting the presence of important dynamics linked to the FTB status.

Table 7: DiD regression; dependent variable: LTV and $LTVabove90$; 2019-2021

	LTV				LTVabove90			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × Treated	-2.594***	-2.604***	-2.228***	-2.210***	-0.043***	-0.046***	-0.042***	-0.045***
	(0.438)	(0.429)	(0.414)	(0.404)	(0.012)	(0.012)	(0.012)	(0.012)
Borrower Controls	-	-	x	x	-	-	x	x
Time FE	x	x	x	x	x	x	x	x
Region FE	x	-	x	-	x	-	x	-
Municipality FE	-	x	-	x	-	x	-	x
Observations	22,681	22,678	22,681	22,678	22,681	22,678	22,681	22,678
R^2	0.127	0.163	0.197	0.234	0.184	0.215	0.203	0.234
R^2 Within	0.115	0.109	0.186	0.184	0.127	0.122	0.148	0.144

Notes: This table reports difference-in-differences (DiD) estimates of the effect of the 2020 soft LTV recommendation on mortgage LTV ratios and the probability of exceeding 90% ($LTVabove90$). The interaction term (*Post* × *Treated*) captures the differential change for constrained (Treated) versus unconstrained loans following the policy. Columns (1)–(4) use LTV as the dependent variable; columns (5)–(8) use $LTVabove90$. Specifications differ in the inclusion of borrower controls (age, income, savings and their squared terms, plus cosigner and FTB status) and in the fixed effects (region or municipality). All models include time fixed effects, and standard errors are clustered at the municipality level. Results with additional controls appear in Table 36 (Appendix). *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Figure 6: Plot DiD coefficients; dependent variable: *LTV* and *LTVabove90*; 2019Q4 as benchmark (borrower controls; Time FE & Region FE)



8 Targets and exceptions: the heterogeneous effects

8.1 Differential impact based on borrower characteristics

A central feature of the Belgian LTV recommendation is its soft limit design: banks are allowed to exceed the 90% cap for a limited share of their mortgage portfolio (up to 35% above 90% for FTBs), provided these exceptions can be justified. This naturally raises the question of which borrower groups are granted access to these higher-LTV loans. Policymakers anticipated that this flexibility would preserve credit access for groups such as FTBs and young households, who might otherwise be disproportionately constrained.

To empirically identify which borrower groups benefit from this exception mechanism, we extend the baseline DiD specification to a triple-differences (DDD) framework with interaction terms of the form *Post* × *Treated* × *Borrower Characteristic*[13]. We estimate these models using both *LTV* and *LTVabove90*[14] as dependent variables. The *Post* × *Treated* term captures the general differential effect of the policy on Treated borrowers relative to the Controls, while *Post* × *Treated* × *Borrower Characteristic* identifies whether the treatment effect varies systematically with specific borrower characteristics, such as FTB status, age or income. The *Post* × *Borrower Characteristic* interaction serves mainly as a control, absorbing time-varying group differences, though we hypothesize that it may also reflect indirect policy-induced changes in risk-based pricing. Tables 8 and 9 allow us to interpret the individual coefficients and identify the average treatment effect on the treated. Tables 10 and 11 summarize the implied pre- and post-policy outcomes across borrower subgroups by aggregating the relevant coefficients, thereby offering an overview of how different groups evolve over time relative to each other.

We first examine the regressions including the triple interaction with *FTB* (Table 8, regressions (1) and (4)). The *Post* × *Treated* coefficient is negative and highly significant for both dependent variables: *LTV* declines by roughly 3.7 pps, and *LTVabove90* by about 21 pps. These effects are notably larger than those obtained from the simpler DiD model in Table 7 (approximately 2 pps and 4 pps, respectively), confirming that the dynamics depend on the FTB status. However, the triple interaction *Post* × *Treated* × *FTB* yields diverging results, the coefficient is large and positive for *LTVabove90*, but statistically insignificant for the continuous *LTV*. This indicates that Treated FTBs were far less affected by the tightening than Treated non-FTBs in terms of exceeding the 90% threshold, while their average leverage did not change significantly. Quantitatively, the share of above-90% loans among treated non-FTBs fell by around 21 pps, compared with only 3.5 pps for treated FTBs (relative to the controls). The lack of heterogeneity in the continuous LTV suggests that banks mainly nudge non-FTB loans from just above to just below the 90% limit, thereby reducing the proportion of high-LTV loans without materially changing the overall distribution of LTVs. Table 10 confirms that non-FTBs experience a larger decrease in both LTV and LTVabove90, with Treated non-FTBs being the most impacted.

Regressions (2) and (5) in Table 8 analyze heterogeneity by age. Results indicate that while Treated Young borrowers suffer a stronger decrease in LTV than other Treated borrowers, this does not impact their presence in the high-LTV loans. The non-significant *Post* × *Treated* term in (2) combined with a negative *Post* × *Treated* × *Y* term, implies that most of the treatment effect on the treated (+/- -3.1 pps) is carried by the Treated Young borrowers, leaving other Treated borrowers mostly unaffected[15]. Additionally, Treated borrowers that are

[13] All constituent terms and lower-order interactions are included in the regressions.

[14] The continuous *LTV* captures overall leverage, including variation far below 90%, while *LTVabove90* directly captures loans exceeding the regulatory threshold, i.e. the "exception" group.

[15] Nevertheless, when considering the other coefficients (*Post* and *Post* × *Y*) and Table 11, it is shown that most groups (whether

cosigning also seem to get slightly higher LTVs and relatively more high-LTV loans compared to other Treated borrowers (see *Post* $\times$ *Treated* $\times$ *cosigner* in regressions (3) and (6)).

Additional heterogeneity emerges in Table 9. Although modest in magnitude and only significant at the 10% level, regression (1) suggests that Treated low-income borrowers face a larger treatment effect in terms of LTV than other Treated borrowers (-2.8 pps versus -1.4 pps). In regression (3) and (6), the triple interaction with low-savings (*Post* $\times$ *Treated* $\times$ *LS*) indicates that Treated borrowers with relatively low savings obtain, on average, higher LTVs. However, this effect is confined to the below-90% range as no effect is found in the regressions with *LTVabove90* as dependent. This points towards banks giving relatively more leeway to Treated borrowers with low savings compared to other Treated borrowers, without increasing the risk too much since the loans remain in the lower LTVs. Table 11 confirms that the relatively higher LTV levels for Treated low-savings borrowers does not translate into higher absolute LTVs post-policy[16].

Overall, the results indicate that the soft-limit design of the Belgian LTV expectations leads to a differentiated tightening rather than a uniform one. Banks use their exception capacity mainly to preserve high-LTV access for first-time, cosigning and higher-income borrowers. The sharp drop in the share of loans above 90%, alongside only modest changes in average LTVs, suggests that adjustments are concentrated around the regulatory threshold. These patterns show that the soft-limit framework enables banks to make choices based on their own risk perceptions.

Finally, while we cannot causally attribute them to the reform (as explained earlier), the results point to a broader shift in bank behavior. We hypothesize that by combining an LTV cap with a limited exception mechanism, the policy prompts banks to reconsider their overall approach to risk differentiation. In practice, this translates into a more granular form of risk-based pricing, where banks not only enforce the regulatory threshold but also reasses which borrower types are deemed sufficiently safe to deserve higher LTVs. This plausible adjustment is reflected in the *Post* $\times$ *Borrower Characteristic* coefficients: on average, FTBs, younger borrowers, those with higher NAI, and households with higher savings are more likely to be granted higher LTV ratios in the post-policy period.

A potential concern is that analyzing FTBs and non-FTBs together may obscure important dynamics, since regulatory tolerance margins were explicitly more generous for FTBs. We therefore also explore the heterogeneity of effects within these subgroups in the following sections.

they are Young, Treated, both or neither) experience a decreased LTV and/or a decreased relative presence in the high-LTV loans.

[16]The increase is offset by the highly significant and negative coefficient for the *Post* $\times$ *LS* interaction

Table 8: DDD regression with **FTB**, **Young** and **Cosigner**; dependent variable: *LTV* and *LTVabove90*; 2019-2021

	LTV			LTVabove90		
	(1)	(2)	(3)	(4)	(5)	(6)
Post	-3.944***	-2.456***	-0.060	-0.172***	-0.151***	-0.150***
	(0.559)	(0.590)	(0.545)	(0.013)	(0.012)	(0.012)
Treated	9.553***	13.477***	10.106***	0.280***	0.295***	0.293***
	(0.831)	(0.738)	(0.573)	(0.028)	(0.019)	(0.016)
Post × Treated	**-3.712*** **	-0.452	**-2.794*** **	**-0.211*** **	-0.012	**-0.064*** **
	(1.149)	(0.960)	(0.659)	(0.038)	(0.026)	(0.020)
Post × FTB	5.175***			0.057***		
	(0.692)			(0.015)		
Treated × FTB	0.869			0.032		
	(0.889)			(0.030)		
Post × Treated × FTB	0.287			**0.176*** **		
	(1.238)			(0.042)		
Post × Young		3.429***			0.032**	
		(0.729)			(0.016)	
Treated × Young		-2.361***			0.031	
		(0.882)			(0.024)	
Post × Treated × Young		**-3.058*** **			-0.035	
		(1.073)			(0.030)	
Post × cosigner			-0.930			0.032**
			(0.676)			(0.015)
Treated × cosigner			-1.242*			0.011
			(0.681)			(0.020)
Post × Treated × cosigner			1.367			**0.049***
			(0.868)			(0.027)
Borrower Controls	x	x	x	x	x	x
Region FE	x	x	x	x	x	x
Observations	22,681	22,681	22,681	22,681	22,681	22,681
R^2	0.198	0.171	0.195	0.201	0.195	0.199
R^2 Within	0.195	0.167	0.191	0.191	0.186	0.190

Notes: This table reports triple-differences (DDD) estimates of the heterogeneous impact of the 2020 soft LTV recommendation on loan-to-value (*LTV*) ratios and the probability of exceeding 90% (*LTVabove90*) across borrower groups with different financial capacity. The interaction term (*Post × Treated × FTB/Young/Cosigner*) measures the additional policy effect for first-time buyers (*FTB*), young borrowers (*Young*), and borrowers applying with a co-signer (*Cosigner*). The *Treated* dummy equals one for loans predicted (using pre-policy data) to exceed a 90% LTV absent the policy. All regressions include borrower controls, region and time fixed effects. Standard errors are clustered at the municipality level. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Table 9: DDD regression with **low Income (LI)**, **low NAI** and **low Savings (LS)**; dependent variable: *LTV* and *LTVabove90*; 2019-2021

	LTV			LTVabove90		
	(1)	(2)	(3)	(4)	(5)	(6)
Post	-0.845*	0.218	1.680***	-0.109***	-0.093***	-0.092***
	(0.434)	(0.467)	(0.414)	(0.011)	(0.012)	(0.011)
Treated	8.608***	10.303***	9.406***	0.296***	0.322***	0.323***
	(0.512)	(0.514)	(0.571)	(0.014)	(0.015)	(0.019)
Post × Treated	**-1.352****	**-2.223*****	**-1.526****	-0.017	-0.028	0.024
	(0.541)	(0.566)	(0.622)	(0.016)	(0.017)	(0.021)
Post × LI	0.705			-0.052***		
	(0.747)			(0.015)		
Treated × LI	1.782**			0.004		
	(0.700)			(0.018)		
Post × Treated × LI	**-1.435***			-0.034		
	(0.851)			(0.023)		
low NAI		1.689***			0.029**	
		(0.575)			(0.015)	
Post × low NAI		-1.440**			-0.080***	
		(0.608)			(0.017)	
Treated × low NAI		-1.611**			-0.045**	
		(0.630)			(0.018)	
Post × Treated × low NAI		-0.082			-0.035	
		(0.795)			(0.028)	
Post × LS			-5.737***			-0.108***
			(0.699)			(0.015)
Treated × LS			-0.248			-0.033
			(0.751)			(0.023)
Post × Treated × LS			**1.988****			-0.039
			(0.899)			(0.026)
Borrower Controls	x	x	x	x	x	x
Region FE	x	x	x	x	x	x
Observations	22,681	22,681	22,681	22,681	22,681	22,681
R^2	0.195	0.196	0.198	0.199	0.201	0.198
R^2 Within	0.191	0.192	0.194	0.190	0.192	0.189

Notes: This table reports triple-differences (DDD) estimates of the heterogeneous impact of the 2020 soft LTV recommendation on loan-to-value (*LTV*) ratios and the probability of exceeding 90% (*LTVabove90*) across borrower groups with different financial capacity. The interaction term (*Post × Treated × LI/low NAI/LS*) measures the additional policy effect for borrowers with low income (*LI*), low net available income (*low NAI*), and low savings (*LS*). The *Treated* dummy equals one for loans predicted (using pre-policy data) to exceed a 90% LTV absent the policy. All regressions include borrower controls, region and time fixed effects. Standard errors are clustered at the municipality level. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Table 10: DDD regression: Overview of the net effect per group on LTV; 2019-2021

		LTV					
	X =	FTB	Young	low income	low NAI	cosigner	low savings
Treated X	Pre	90.8	92.2	86.1	87.1	88.3	85.7
		(- 2.1)	**(- 2.4)**	**(- 4.0)**	**(- 3.9)**	**(- 2.3)**	**(- 7.9)**
	Post	88.7	89.8	82.1	83.2	86.0	77.8
Treated non-X	Pre	88.7	90.3	87.0	87.5	87.5	84.7
		(- 7.7)	**(- 2.7)**	(- 2.4)	(- 2.2)	**(- 2.6)**	**(- 4.1)**
	Post	81.0	87.6	84.6	85.3	84.8	80.6
Control X	Pre	77.6	80.1	73.4	76.2	75.7	74.7
		(+ 1.1)	(- 0.8)	(- 1.3)	(- 1.7)	(- 0.9)	**(- 4.2)**
	Post	78.7	80.9	72.1	74.5	74.8	70.5
Control non-X	Pre	77.6	76.1	75.6	74.3	74.8	73.7
		(- 4.5)	**(- 2.7)**	(- 1.3)	(- 0)	(- 0)	(+ 1.8)
	Post	73.1	73.4	74.3	74.3	74.8	75.5
Observations		22,681	22,681	22,681	22,681	22,681	22,681

Note: Values are derived from triple-DiD regressions with borrower controls. Reported levels are obtained by summing the intercept and all relevant main and interaction coefficients. For simplicity, dummy indicators are used in place of continuous borrower controls, and no fixed effects are included. Level changes for continuous controls are not shown. Differences between the "Pre" and "Post" periods appear in brackets. See Tables 24–35 in the Appendix for details on the summation procedure and the corresponding level changes of the controls.

Table 11: DDD regression: Overview of the net effect per group on *LTVabove90*; 2019-2021

		LTVabove90					
	X =	FTB	Young	low income	low NAI	cosigner	low savings
Treated X	Pre	0.73	0.67	0.63	0.62	0.65	0.68
		(- 0.15)	(- 0.13)	**(- 0.21)**	**(- 0.18)**	(- 0.15)	**(- 0.19)**
	Post	0.58	0.54	0.42	0.44	0.50	0.49
Treated non-X	Pre	0.66	0.63	0.63	0.65	0.65	0.60
		(- 0.38)	**(- 0.16)**	(- 0.12)	(- 0.10)	**(- 0.21)**	(- 0.09)
	Post	0.28	0.47	0.51	0.55	0.44	0.51
Control X	Pre	0.39	0.30	0.27	0.29	0.30	0.33
		(- 0.12)	(- 0.12)	**(- 0.16)**	**(- 0.17)**	(-0.15)	**(- 0.20)**
	Post	0.27	0.18	0.11	0.12	0.15	0.13
Control non-X	Pre	0.32	0.30	0.27	0.27	0.30	0.24
		(- 0.18)	**(- 0.16)**	(- 0.12)	(- 0.10)	(- 0.15)	(- 0.09)
	Post	0.14	0.14	0.15	0.17	0.15	0.15
Observations		22,681	22,681	22,681	22,681	22,681	22,681

Note: Values are derived from triple-DiD regressions with borrower controls. Reported levels are obtained by summing the intercept and all relevant main and interaction coefficients. For simplicity, dummy indicators are used in place of continuous borrower controls, and no fixed effects are included. Level changes for continuous controls are not shown. Differences between the "Pre" and "Post" periods appear in brackets. See Tables 24–35 in the Appendix for details on the summation procedure and the corresponding level changes of the controls.

8.2 Dynamics within the (non-)FTB group

The previous regressions highlight that borrower characteristics play a key role in how the policy is transmitted. The most pronounced pattern emerges for the FTBs, indicating that banks use the extra room they are given to exceed the 90% LTV cap for this group. The question, however, is whether this flexibility is applied uniformly across FTBs, or whether banks further differentiate within this population. To explore this heterogeneity, we first run a DiD regression separately for the two subgroups (Table 12). For both subgroups the average treatment effect is considerably larger than when we apply the DiD to the entire group (see Table 7). We then continue with two complementary strategies. First, we estimate DDD regressions separately for FTBs and non-FTBs (Table 13). Second, we run DDD regressions on the full sample but with borrower-type dummies focused on the potentially more vulnerable FTB subgroups: young FTBs, low-income FTBs, and low-savings FTBs (Table 14).

Table 13 shows that within the FTB group, low-savings Treated borrowers (see *Post × Treated × LS* in regression(3)) appear to get some flexibility through a smaller treatment effect, suggesting that banks seek to avoid excluding borrowers with limited capacity to raise down payments. Among non-FTBs the pattern is reversed: Treated low-savings borrowers face relatively larger LTV reductions compared to other Treated non-FTBs. This asymmetry highlights how the policy effectively prioritizes FTBs, allowing banks to grant them greater flexibility than non-FTBs. Table 14 further refines this picture by focusing on the most at-risk groups (Young-FTBs, low-income FTBs and low-savings FTBs). Among Treated borrowers, young FTBs face the largest treatment effect in terms of LTV reduction[17] and Treated low-income FTBs also experience more pronounced declines in LTV. In contrast, Treated low-savings FTBs benefit from relatively softer treatment effects.

Taken together, these findings indicate that while the policy's exception mechanism preserves higher-LTV access for FTBs, as intended, banks exercise discretion in determining which FTB subgroups benefit most. Vulnerable borrowers with limited savings, are partly shielded, whereas young and low-income Treated FTBs have to adjust.

Table 12: DiD regression: FTBs and non-FTBs separately; dependent variable: *LTV* and *LTVabove90*; 2019-2021

	LTV				LTVabove90			
	Only FTB		Only non-FTB		Only FTB		Only non-FTB	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × Treated	-3.677***	-3.703***	-4.037***	-4.198***	-0.051***	-0.054***	-0.211***	-0.205***
	(0.473)	(0.466)	(1.150)	(1.237)	(0.014)	(0.014)	(0.038)	(0.040)
Borrower Controls	x	x	x	x	x	x	x	x
Time FE	x	x	x	x	x	x	x	x
Region FE	x	-	x	-	x	-	x	-
Municipality FE	-	x	-	x	-	x	-	x
Observations	17,320	17,310	5,361	5,324	17,320	17,310	5,361	5,324
R^2	0.177	0.221	0.128	0.240	0.187	0.225	0.121	0.212
R^2 Within	0.167	0.165	0.106	0.112	0.130	0.127	0.050	0.051

Notes: This table reports separate DiD regressions for FTBs and non-FTBs. The key interaction term, *Post × Treated*, measures the differential change in loan-to-value (LTV) ratios and in the probability of exceeding 90% LTV among borrowers predicted to be constrained by the 2020 soft LTV recommendation. All regressions include borrower controls, time fixed effects, and either region or municipality fixed effects. Standard errors are clustered at the municipality level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

[17] Yet the overall change for this group is muted, since the coefficient on *Post × Young FTB* is strongly positive, offsetting much of the negative effect.

Table 13: DDD regressions with **Young (Y)**, **low income (LI)** and **low savings (LS)**; dependent variable: *LTV* and *LTVabove90*; 2019-2021; on FTBs and non-FTBs separately

	LTV						LTVabove90					
	Only FTBs			Only non-FTBs			Only FTBs			Only non-FTBs		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post	-0.563	2.402***	3.612***	-4.416***	-4.502***	-3.411***	-0.135***	-0.047***	-0.068***	-0.170***	-0.183***	-0.150***
	(0.866)	(0.625)	(0.484)	(0.715)	(0.659)	(0.831)	(0.019)	(0.017)	(0.013)	(0.014)	(0.015)	(0.020)
Treated	15.108***	11.112***	10.729***	10.525	6.243***	4.356**	0.283***	0.313***	0.351***	0.324	0.288***	0.123
	(0.857)	(0.631)	(0.618)	(6.508)	(1.066)	(2.050)	(0.022)	(0.017)	(0.020)	(0.199)	(0.034)	(0.079)
Post × Treated	**-2.422****	**-4.087*****	**-3.195*****	-0.687	**-2.424****	1.195	-0.024	**-0.047****	0.001	-0.179	**-0.213*****	0.059
	(1.107)	(0.686)	(0.674)	(8.543)	(1.234)	(2.328)	(0.029)	(0.020)	(0.023)	(0.262)	(0.042)	(0.090)
Post × Y	2.694***			0.589			0.045**			-0.045		
	(1.015)			(1.161)			(0.022)			(0.030)		
Treated × Y	-2.743***			-2.448			0.053**			-0.069		
	(0.995)			(6.579)			(0.026)			(0.200)		
Post × Treated × Y	-1.783			-3.337			-0.030			0.006		
	(1.226)			(8.643)			(0.034)			(0.263)		
Post × LI		-1.946**			1.674			-0.106***			0.006	
		(0.913)			(1.447)			(0.021)			(0.029)	
Treated × LI		-0.255			3.158			-0.014			-0.041	
		(0.813)			(2.123)			(0.021)			(0.063)	
Post × Treated × LI		1.018			**-5.858***			-0.003			0.032	
		(1.001)			(3.457)			(0.028)			(0.094)	
Post × LS			-6.897***			-1.922*			-0.116***			-0.073***
			(0.928)			(1.083)			(0.021)			(0.025)
Treated × LS			-0.496			3.348			-0.071***			0.184**
			(0.881)			(2.159)			(0.025)			(0.082)
Post × Treated × LS			**3.413*****			**-5.389****			-0.012			**-0.308*****
			(1.089)			(2.680)			(0.030)			(0.100)
Borrower Controls	x	x	x	x	x	x	x	x	x	x	x	x
Region FE	x	x	x	x	x	x	x	x	x	x	x	x
Observations	17,320	17,320	17,320	5,361	5,361	5,361	17,320	17,320	17,320	5,361	5,361	5,361
R^2	0.146	0.174	0.180	0.098	0.121	0.118	0.177	0.182	0.179	0.114	0.114	0.118
R^2 Within	0.141	0.169	0.175	0.095	0.118	0.116	0.164	0.170	0.166	0.110	0.109	0.113

Notes: This table presents triple-differences (DDD) regressions estimating heterogeneous effects of the 2020 soft LTV recommendation across borrower types, separately for first-time buyers (FTBs) and non-FTBs. The interaction term *Post × Treated × Y/LI/LS* captures whether the policy impact differs for younger borrowers (Y), low-income (LI), or low-savings (LS) households relative to others. All regressions include borrower-level controls and region fixed effects. Standard errors are clustered at the municipality level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 14: DDD regressions with **Young-FTBs (Y-FTB)**, **lowIncome-FTBs (LI-FTB)** and **lowSavings-FTBs (LS-FTB)**; dependent variable: *LTV* and *LTVabove90*; 2019-2021

	LTV			LTVabove90		
	(1)	(2)	(3)	(4)	(5)	(6)
Post	-2.526***	-1.084***	0.145	-0.159***	-0.119***	-0.118***
	(0.500)	(0.405)	(0.349)	(0.012)	(0.011)	(0.010)
Treated	8.915***	8.893***	8.666***	0.242***	0.284***	0.294***
	(0.624)	(0.477)	(0.523)	(0.018)	(0.014)	(0.016)
Post × Treated	**-1.615****	**-1.420*****	**-1.834*****	**-0.065*****	-0.014	-0.006
	(0.753)	(0.508)	(0.565)	(0.022)	(0.015)	(0.018)
Y-FTB	-4.570***			-0.121***		
	(0.822)			(0.020)		
Post × Y-FTB	4.757***			0.064***		
	(0.751)			(0.017)		
Treated × Y-FTB	2.160***			0.100***		
	(0.783)			(0.022)		
Post × Treated × Y-FTB	**-2.709*****			0.012		
	(0.928)			(0.028)		
LI-FTB		-0.763			-0.029*	
		(0.729)			(0.017)	
Post × LI-FTB		1.435*			-0.040***	
		(0.747)			(0.015)	
Treated × LI-FTB		1.293*			0.024	
		(0.684)			(0.019)	
Post × Treated × LI-FTB		**-1.612***			-0.035	
		(0.856)			(0.024)	
LS-FTB			-1.977***			0.012
			(0.732)			(0.016)
Post × LS-FTB			-3.507***			-0.063***
			(0.850)			(0.019)
Treated × LS-FTB			1.108			-0.013
			(0.785)			(0.022)
Post × Treated × LS-FTB			**2.159****			-0.004
			(1.002)			(0.027)
Borrower Controls	x	x	x	x	x	x
Region FE	x	x	x	x	x	x
Observations	22,681	22,681	22,681	22,681	22,681	22,681
R^2	0.198	0.195	0.199	0.201	0.199	0.199
R^2 Within	0.194	0.192	0.195	0.191	0.190	0.190

Notes: This table reports triple-differences (DDD) regressions. The variables *Young-FTB*, *LowIncome-FTB*, and *LowSavings-FTB* are dummy indicators equal to 1 for borrowers who are both first-time buyers (FTBs) and respectively young, low-income, or low-savings. The coefficients on *Post × Treated × Y-FTB/LI-FTB/LS-FTB* measure whether the effect of the 2020 soft LTV recommendation differs for these specific FTB subgroups compared with other borrowers. All regressions include borrower-level controls and region fixed effects. Standard errors are clustered at the municipality level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

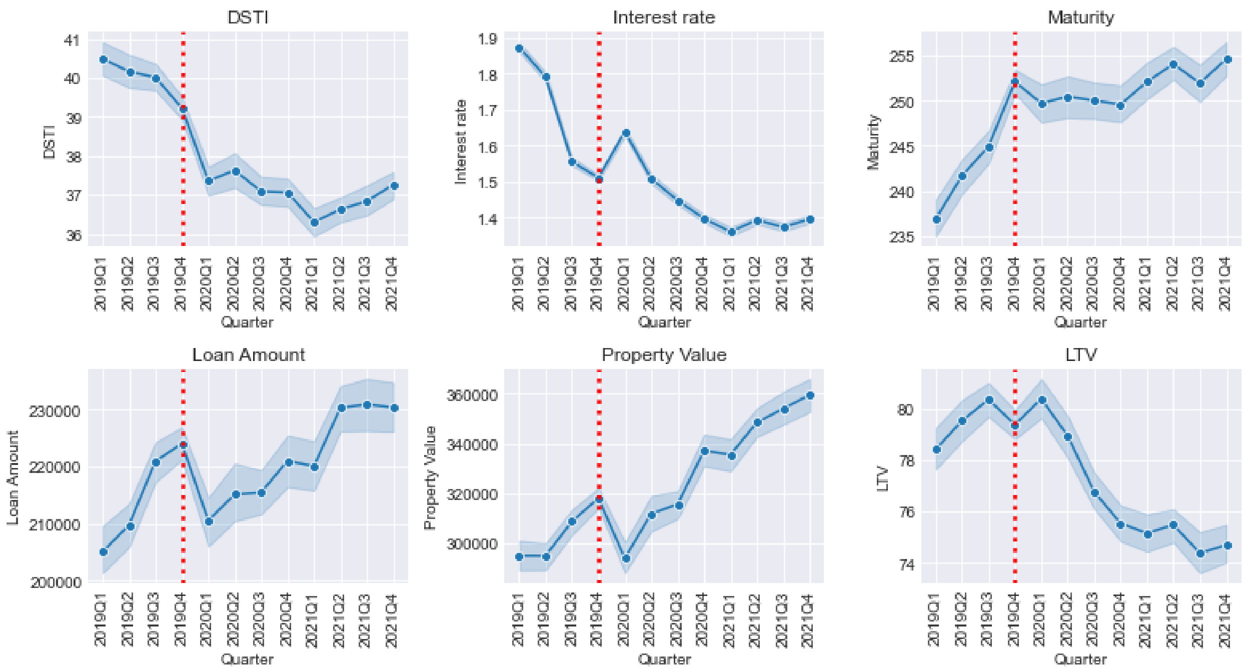
9 Impact on loan and property characteristics

9.1 Main effects

As borrowers adjust to lower LTV ratios, other loan and property characteristics may also change. A reduction in LTV requires households to either contribute more of their own funds or to purchase a less expensive property. The latter can occur through selecting a smaller or lower-quality dwelling (e.g., fewer amenities, poorer condition) or by relocating to less desirable areas. In principle, a lower LTV should also translate into a lower interest rate, since the loan becomes less risky for the lender.

Descriptive evidence in Figure 7 shows that DSTI and interest rates were already declining prior to the policy change. Loan maturities were trending upward before the reform and then stabilize. Loan amounts and property values have been rising over time, consistent with nominal growth. Although interesting, these general patterns do not isolate the causal impact of the policy. For this, we turn towards a DiD analysis.

Figure 7: Evolution of the loan and property characteristics over the focus period 2019-2021



The DiD estimates in Table 15 show that DSTI, interest rates, and loan amounts are significantly affected by the LTV expectations. Relative to the Controls, Treated borrowers exhibit lower DSTIs, higher interest rates and lower loan amounts. This suggests that the lower LTVs among the Treated are obtained by a relative increase in down payment rather than a decrease in property value. This finding is in line with what Kinghan et al. (2022) found in Ireland, but in contrast with evidence from Brazil, Israel and the Netherlands where both loan amounts and property prices declined (de Araujo et al., 2020; Tzur-Ilan, 2023; van Bekkum et al., 2024). Additionally, although overall interest rates were declining, Treated borrowers face relatively higher rates as a compensation for the, on average, higher LTV and hence higher risk, which is in line with what Abreu et al. (2024), de Araujo et al. (2020), and Tzur-Ilan (2023) found. This supports the hypothesis of more pronounced risk-based pricing since the reform. The relative decline in DSTI is in contrast with Abreu et al. (2024) and Tzur-Ilan (2023), who observed higher DSTIs in Portugal and Israel. While they argue that the relatively higher interest rates lead to relatively higher DSTIs, our findings suggest that the decrease in loan amounts was strong enough to compensate the higher borrowing cost.

Finally, while Treated borrowers are not pushed toward cheaper properties, this does not imply that all constrained borrowers increase their down payments. An alternative interpretation is sample selection, namely that only borrowers with sufficient financial means to raise additional equity are able to remain in the market. More liquidity-constrained households, those unable to meet higher down payment requirements, may instead postpone homeownership.

Table 15: Dynamic Effects of the Soft LTV Limits on Loan and Property Characteristics (Quarterly DiD Estimates, 2019–2021)

	DSTI (1)	interest rate (2)	Maturity (3)	ln(Amount) (4)	ln(Value) (5)	LTV (6)
Treated	1.230***	0.145***	1.769	0.017	-0.171***	10.300***
	(0.386)	(0.013)	(1.503)	(0.015)	(0.013)	(0.559)
2019Q1 × Treated	-0.498	-0.035*	2.222	0.006	-0.042**	0.968
	(0.662)	(0.021)	(2.803)	(0.029)	(0.021)	(1.075)
2019Q2 × Treated	-0.348	-0.018	2.396	0.014	-0.010	0.381
	(0.654)	(0.020)	(2.699)	(0.028)	(0.024)	(0.996)
2019Q3 × Treated	-0.836	0.009	-1.507	-0.012	-0.027	-0.507
	(0.612)	(0.020)	(2.566)	(0.023)	(0.020)	(1.006)
2020Q1 × Treated	-0.516	0.008	-0.780	-0.018	0.016	**-3.097*** **
	(0.603)	(0.020)	(2.786)	(0.027)	(0.024)	(1.094)
2020Q2 × Treated	**-1.226***	0.027	0.240	-0.034	0.020	**-3.198*** **
	(0.659)	(0.023)	(2.716)	(0.029)	(0.025)	(1.096)
2020Q3 × Treated	**-1.826*** **	**0.080*** **	2.076	**-0.061** **	-0.007	**-3.064*** **
	(0.595)	(0.020)	(2.271)	(0.027)	(0.021)	(0.866)
2020Q4 × Treated	**-2.780*** **	**0.069*** **	3.790	**-0.071** **	0.012	**-4.419*** **
	(0.570)	(0.020)	(2.459)	(0.028)	(0.022)	(0.912)
2021Q1 × Treated	**-2.745*** **	**0.086*** **	4.188*	**-0.069*** **	-0.029	**-2.976*** **
	(0.546)	(0.022)	(2.400)	(0.027)	(0.023)	(0.899)
2021Q2 × Treated	**-2.927*** **	**0.076*** **	2.001	**-0.090*** **	-0.010	**-3.759*** **
	(0.597)	(0.019)	(2.238)	(0.024)	(0.020)	(0.916)
2021Q3 × Treated	-0.718	**0.063*** **	1.092	-0.027	0.021	**-3.556*** **
	(0.667)	(0.020)	(2.520)	(0.026)	(0.022)	(0.921)
2021Q4 × Treated	**-2.287*** **	**0.066*** **	1.041	**-0.094*** **	-0.016	**-3.949*** **
	(0.633)	(0.021)	(2.397)	(0.027)	(0.023)	(0.942)
Borrower Controls	x	x	x	x	x	x
Time FE	x	x	x	x	x	x
Region FE	x	x	x	x	x	x
Time × FTB FE	x	x	x	x	x	x
Observations	22,681	22,681	22,681	22,681	22,681	22,681
R^2	0.157	0.279	0.318	0.295	0.471	0.201
R^2 Within	0.129	0.138	0.299	0.277	0.425	0.190

Notes: This table reports quarterly difference-in-differences (DiD) estimates of the impact of the soft LTV limits on loan and property characteristics. Each column corresponds to a separate regression for the dependent variable listed in the header. The interaction terms (*Quarter × Treated*) measure the difference in outcomes between constrained (*Treated*) and unconstrained loans relative to 2019Q4, which serves as the baseline quarter. Coefficients in bold refer to the post-policy period (2020Q1–2021Q4). All regressions include borrower controls (age, income, savings and their squared terms, plus cosigner and FTB status), time, region, and *time × FTB* fixed effects. Standard errors are clustered at the municipality level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

9.2 Heterogeneous effects

The previous results suggest that the adjustment to the LTV recommendation was not uniformly distributed across all Treated borrowers. Certain borrower groups were more likely than others to face tighter constraints or benefit from exceptions. To explore these differences, we further examine the heterogeneity across borrower types with respect to other loan and property characteristics. The corresponding estimates are reported in Table 16.

A general pattern emerges: lower LTV ratios are typically associated with smaller loan amounts, while property values remain broadly unaffected. This mirrors the results in Table 15 and suggests that adjustments occur primarily through borrowers' financing choices rather than shifts in housing demand. Among young Treated borrowers, the stronger treatment effect (negative coefficient for LTV) is accompanied by a significantly larger decline in DSTI relative to other Treated borrowers. This indicates that lower leverage translates into lower repayment burdens and, consequently, lower credit risk. For low-income Treated borrowers, although the decline in LTV is only marginally significant, it is likewise associated with a relatively lower DSTI. Interestingly, Treated borrowers with low savings exhibit relatively higher LTVs, yet without a corresponding rise in DSTI.

Table 16: Differential treatment across borrower types based on DDD regression; dependent variable: loan & property characteristics; 2019-2021

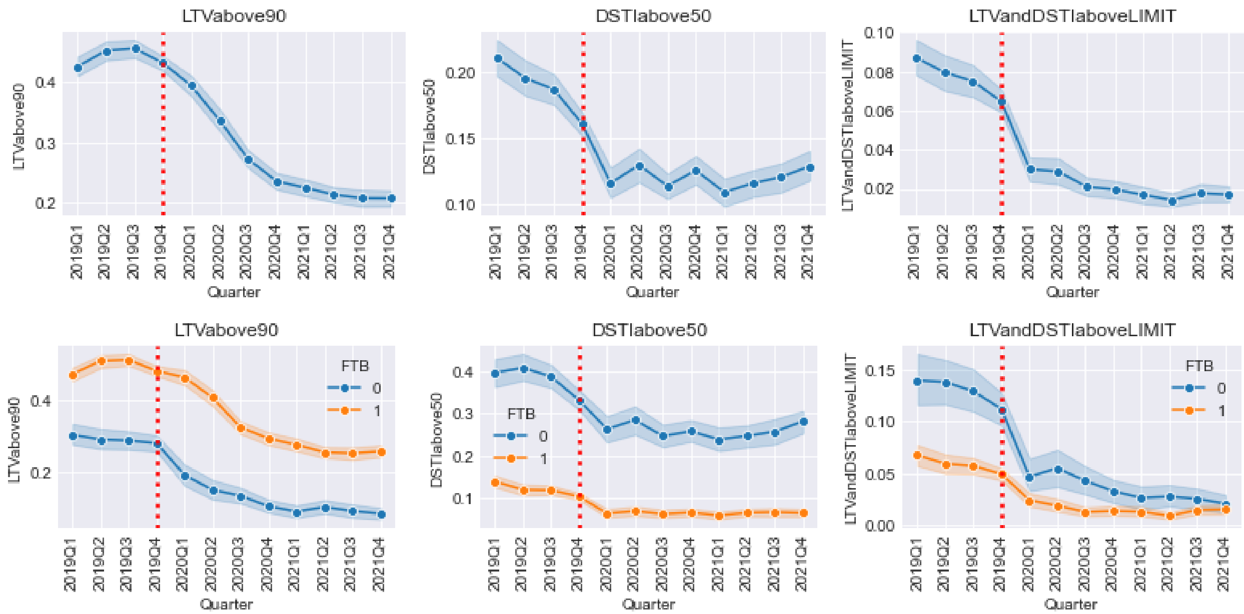
	DSTI (1)	Interest (2)	Maturity (3)	ln(Amount) (4)	ln(Value) (5)	LTV (6)
Post × Treated × FTB	-0.828 (1.012)	0.039 (0.033)	-7.121* (3.705)	-0.147** (0.062)	-0.043 (0.032)	0.244 (1.242)
Post × Treated × Young	-1.679** (0.728)	0.019 (0.024)	-1.444 (3.008)	-0.061* (0.031)	0.046* (0.025)	-3.238*** (1.064)
Post × Treated × LowIncome	-1.797*** (0.531)	-0.023 (0.020)	-3.581 (2.349)	-0.063** (0.027)	-0.006 (0.020)	-1.521* (0.841)
Post × Treated × LowSavings	0.717 (0.607)	-0.016 (0.020)	2.334 (2.527)	0.099*** (0.028)	-0.021 (0.021)	1.866** 0.898)
Borrower Controls	x	x	x	x	x	x
Time FE	x	x	x	x	x	x
Region FE	x	x	x	x	x	x
Observations	22,681	22,681	22,681	22,681	22,681	22,681

Notes: This table reports coefficients from triple-difference (DDD) regressions estimating the heterogeneous effects of the soft LTV limits across borrower groups. The dependent variables are indicated in column headers. Each specification includes borrower controls (age, income, savings, presence of cosigner, FTB status), time fixed effects, and region fixed effects. Standard errors are clustered at the municipality level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

10 Extension: Pockets-of-Risk

The riskiest mortgage loans are those that combine a high LTV ratio and a high DSTI ratio, a category the NBB labels "pockets-of-risk" (PoR). Given their elevated probability of default, the NBB introduced stricter supervisory expectations for such loans. Whereas the general LTV recommendation permits exceptions of up to 35% for first-time buyers (FTBs) and 20% for non-FTBs, loans with both LTVs above 90% and DSTIs above 50% face a tighter limit: no more than 5% of a bank's mortgage portfolio may exceed these thresholds. As shown in Figure 8, banks adhere to this requirement and even respond more conservatively than requested, reducing the share of PoR loans from roughly 8% before the recommendation to about 2% thereafter.

Figure 8: Evolution of *LTVabove90*, *DSTIabove50* and *LTVandDSTIaboveLIMIT* (PoR) over 2019-2021



In what follows we examine this adjustment more formally. We begin with a standard DiD specification. Table 17 reports the results, including borrower controls. In regressions (1)–(3), the coefficient on *Post* × *Treated* is small and statistically insignificant, suggesting no average treatment effect. However, once we allow for heterogeneity by borrower type by including the interaction *Post* × *FTB*, the picture changes markedly. The coefficient on *Post* × *Treated* turns negative and highly significant, corresponding to a reduction of roughly 3 pps in the likelihood of exceeding the "pockets-of-risk" threshold.

Motivated by these findings, we estimate the DiD specification separately for FTBs and non-FTBs (regressions (7)-(12) of Table 17). The results confirm the heterogeneity: while the treatment effect is small for FTBs (around 1–2 pps), it is much larger for non-FTBs (approximately 11-12 pps). This pattern indicates that the tightening of "pockets-of-risk" lending disproportionately affects non-FTBs, consistent with the policy's design to preserve credit access for FTBs. The effects on the quarterly level for the different groups are shown in Figure 9. Another way of confirming this heterogeneity is by estimating a DDD with *FTB*. In regression (1) of Table 18, the coefficient on *Post* × *Treated* × *FTB* is positive and highly significant, confirming that FTBs experience a substantially smaller treatment effect than non-FTBs. The positive triple interaction nearly offsets the negative coefficient on *Post* × *Treated*, indicating that it is primarily the non-FTBs who drive the contraction in "pockets-of-risk" lending. By contrast, triple interactions with other borrower characteristics yield no significant effects.

Table 17: DiD regressions: all mortgages + FTBs and non-FTBs separately; dependent variable: *LTVandDSTIaboveLimit*; 2019-2021

	LTVandDSTIaboveLIMIT											
	All						Only FTBs			Only non-FTBs		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post	-0.059***			-0.117***			-0.036***			-0.102***		
	(0.004)			(0.009)			(0.004)			(0.009)		
Treated	0.011*	0.014**	0.016**	0.027***	0.029***	0.032***	0.015**	0.018***	0.021***	0.121***	0.123***	0.125***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.007)	(0.029)	(0.030)	(0.030)
FTB	-0.059***	-0.059***	-0.059***	-0.111***	-0.111***	-0.112***						
	(0.005)	(0.005)	(0.005)	(0.010)	(0.010)	(0.010)						
cosigner	-0.004	-0.005	-0.006	-0.003	-0.005	-0.005	0.005	0.004	0.003	-0.029***	-0.033***	-0.034***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.011)	(0.011)	(0.011)
Age	0.006***	0.006***	0.006***	0.006***	0.007***	0.006***	0.007***	0.007***	0.007***	0.005	0.006	0.006
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.004)	(0.004)	(0.004)
$(\text{Age})^2$	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000**	-0.000**	-0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
(Inc/1000)	0.006	0.006	0.006	0.004	0.004	0.005	-0.006	-0.007	-0.006	0.042***	0.042***	0.044***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.012)	(0.012)	(0.013)
$(\text{Inc}/1000)^2$	-0.000	-0.000	-0.000	0.000	0.000	0.000	0.001	0.001	0.001	-0.003**	-0.003**	-0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
(Sav/10000)	-0.003***	-0.002***	-0.002***	-0.003***	-0.002***	-0.002***	-0.002***	-0.002**	-0.002**	-0.005***	-0.005***	-0.005***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
$(\text{Sav}/10000)^2$	0.000*	0.000	0.000	0.000**	0.000	0.000	0.000	0.000	0.000	0.000***	0.000**	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Post × Treated	-0.004	-0.003	-0.007	**-0.028*** **	**-0.027*** **	**-0.033*** **	**-0.015** **	**-0.014** **	**-0.019*** **	**-0.112*** **	**-0.114*** **	**-0.116*** **
	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.032)	(0.032)	(0.033)
Post × FTB				0.088***	0.087***	0.088***						
				(0.011)	(0.011)	(0.011)						
Intercept	0.019			0.048**			-0.051**			0.003		
	(0.023)			(0.024)			(0.022)			(0.082)		
Time FE	-	x	-	-	x	-	-	x	-	-	x	-
Region FE	-	x	-	-	x	-	-	x	-	-	x	-
Time × Region FE	-	-	x	-	-	x	-	-	x	-	-	x
Observations	22,681	22,681	22,681	22,681	22,681	22,681	17,320	17,320	17,320	5,361	5,361	5,361
R^2	0.037	0.038	0.039	0.043	0.044	0.045	0.018	0.019	0.021	0.059	0.063	0.066
R^2 Within	-	0.018	0.018	-	0.024	0.024	-	0.004	0.004	-	0.019	0.020

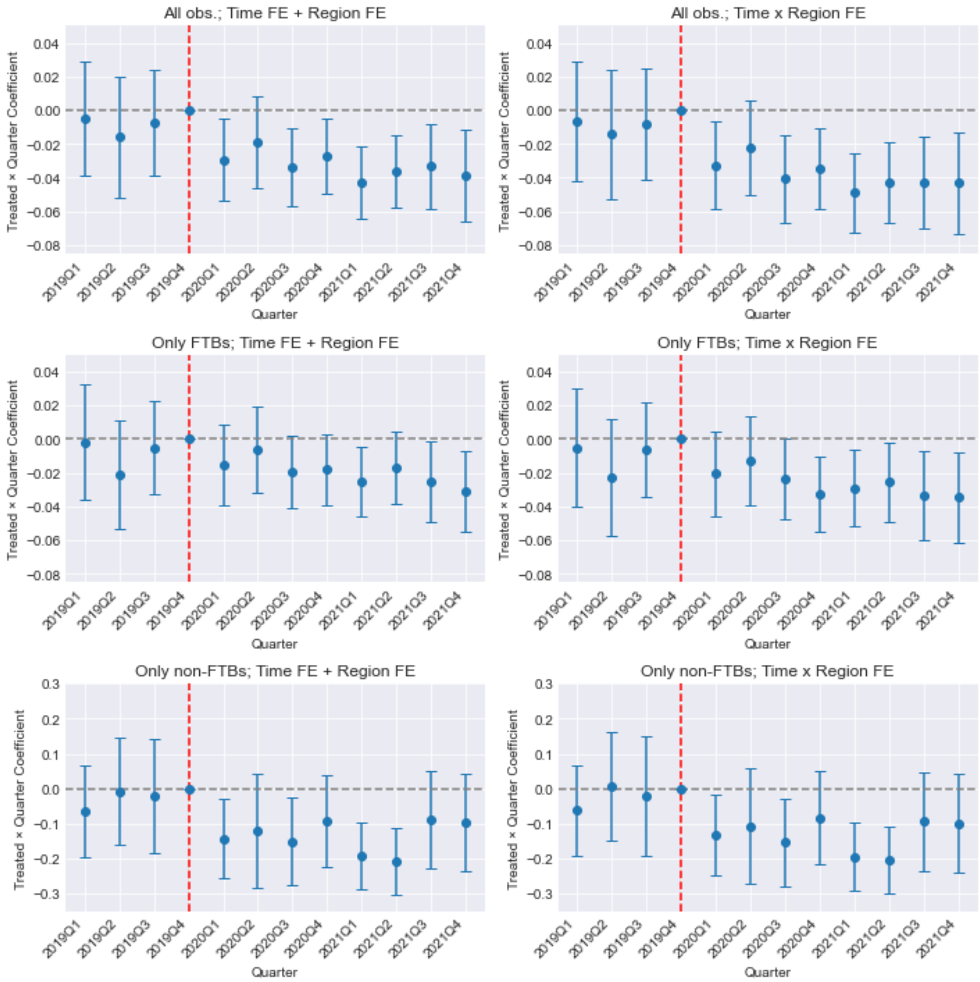
Notes: This table reports difference-in-differences (DiD) estimates of the effect of the 2020 soft LTV recommendation on the incidence of "pockets of risk" (*LTVandDSTIaboveLimit*), defined as loans with both LTV above 90% and DSTI above 50%. Columns (1)–(6) use the full sample, while columns (7)–(9) and (10)–(12) restrict the estimation to first-time buyers (FTBs) and non-FTBs, respectively. The key interaction term (*Post × Treated*) captures the differential change in the probability of "pockets of risk" lending for constrained (*Treated*) versus unconstrained loans after the policy implementation. Regressions include either time and region fixed effects or time × region fixed effects. Standard errors are clustered at the municipality level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 18: DDD regressions with **FTB**, **Young**, **low income** and **low savings**; dependent variable: *LTVandDSTIaboveLIMIT*; 2019-2021

	LTVandDSTIaboveLIMIT			
	(1)	(2)	(3)	(4)
Post	-0.102***	-0.119***	-0.108***	-0.091***
	(0.009)	(0.009)	(0.010)	(0.011)
Treated	0.137***	0.048***	0.044***	0.039***
	(0.028)	(0.013)	(0.009)	(0.012)
Post × Treated	**-0.110*****	**-0.033****	**-0.036*****	**-0.026****
	(0.032)	(0.014)	(0.009)	(0.012)
Treated × FTB	-0.123***			
	(0.028)			
Post × Treated × FTB	**0.096*****			
	(0.033)			
Post × Young		0.006		
		(0.009)		
Treated × Young		-0.021		
		(0.014)		
Post × Treated × Young		0.007		
		(0.016)		
Post × low income			-0.025***	
			(0.008)	
Treated × low income			-0.025**	
			(0.012)	
Post × Treated × low income			0.013	
			(0.013)	
Post × low savings				-0.050***
				(0.009)
Treated × low savings				-0.019
				(0.015)
Post × Treated × low savings				0.020
				(0.016)
Borrower Controls	x	x	x	x
Region FE	x	x	x	x
Time × FTB FE	x	x	x	x
Observations	22,681	22,681	22,681	22,681
R^2	0.047	0.043	0.044	0.045
R^2 Within	0.047	0.042	0.043	0.044

Notes: This table reports triple-differences (DDD) estimates of heterogeneous effects of the soft LTV recommendation on the likelihood of jointly high LTV and DSTI loans ("pockets of risk"). The interaction *Post × Treated × FTB/Young/low income/low savings* captures the differential impact of the policy for each subgroup relative to other borrowers. The *Treated* dummy identifies loans predicted (using pre-policy data) to exceed a 90% LTV ratio absent the policy. All regressions include borrower controls, region fixed effects, and time × FTB fixed effects. Standard errors are clustered at the municipality level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Figure 9: Plot DiD coefficients; dependent variable: $LTVandDSTIaboveLIMIT$; 2019-2021; with Time x FTB FE



11 Robustness checks

To assess the stability of our findings, we perform a range of robustness checks addressing potential concerns related to treatment classification, model specification, and the validity of our identifying assumptions. Across all variants, the estimated effects remain statistically and economically significant (see Appendix C, Tables 37 - 46).

First, we confirm the consistency of our predictions through the use of alternative ML models. Predictions based on the XGBoost model, instead of our LightGBM model, give similar results. Second, we assess the robustness of our classification (*Treated* versus *Control*) to alternative cutoff rules. In particular, we vary the probability threshold used to define treatment status in the ML-based approach and re-estimate the DiD models under these alternative definitions. The results are stable across a broad range of cutoffs, confirming that our findings are not sensitive to the precise classification rule. Second, we assess specification robustness. Including different fixed effects, or clustering standard errors at broader geographic levels, yields similar results. Additionally, excluding 2020, which corresponds to the covid-19 pandemic quarters, increases the average effects as it was the transition period and banks were working towards full compliance. Excluding quarters closest to the policy implementation (2019Q3-2020Q2) also does not alter the results. Finally, we perform a placebo test. Applying the model to pre-policy data (pretending the reform occurred at the beginning of 2019Q3) produces insignificant coefficients. Overall, the effects of the Belgian soft LTV limits are robust across alternative classifications, specifications and samples.

12 Conclusion

Low interest rates may cause increases in housing prices, typically fueled by generous supply of mortgage loans to households by banks. During the low-for-long interest rate era caused by accommodative monetary policy in the euro area following the great financial crisis and the sovereign debt crisis, there were indications that housing markets became stretched in terms of valuation. As a result, the ESRB issued recommendations to the macroprudential authorities in several countries, including Belgium, to implement measures to contain housing market dynamics. In 2019, the National Bank of Belgium (NBB) announced regulatory expectations in which they demanded Belgian banks to limit the LTV of new mortgages to 90% for owner-occupied and 80% for buy-to-let loans. The NBB decided to treat the thresholds as a soft limit, with for example a tolerance margin of 35% for FTBs. Banks have to justify these exceptions in a comply or explain framework, but the rules are not binding.

This paper investigates whether or not the LTV regulation is effective in curbing risky mortgage lending. The main focus and contribution of the paper, however, is in researching the social consequences of the LTV framework. Upon the announcement of the LTV rules, the macroprudential authority indicated that they expected the banks to use the tolerance margins to avoid excluding vulnerable borrowers from the housing market. Did the banks apply the room for exceptions uniformly across their lender population or did they target specific groups of borrowers? Do banks support the access to housing by FTB and young households? How do banks treat poorer borrowers? Do we find evidence that new borrowers are forced to buy cheaper houses?

First, we demonstrate that, after the introduction of LTV limits in Belgium, the average LTV on mortgage loans decreases and the proportion of loans with LTVs exceeding 90% declines significantly, indicating that the policy is effective. In our analysis, we find that banks lower the loan amount in order to achieve the desired LTV distribution. Since we do not find a treatment effect on house values, we conclude that new borrowers are not forced to buy cheaper houses, but instead increase their down payments to achieve a lower LTV. The relative decrease in loan amounts is generally associated with a lower DSTI, implying a lower repayment burden. Hence, the objectives of the macroprudential authority to decrease household mortgage leverage and increase the creditworthiness of mortgages in the bank books are achieved.

In terms of social consequences, we document a number of important findings related to the soft LTV limit design. Banks use the tolerance margin to favor FTB, as intended by the macroprudential authority, as we find evidence that they remain significantly more present among loans with LTVs above 90% compared to non-FTB. Additionally, despite the general decrease in LTVs for low-saving borrowers, banks alleviate this burden by offsetting the treatment effect for constrained FTBs with low-savings. In contrast, low-income constrained borrowers, especially those who are not FTBs, suffer a stronger decrease in LTVs than other constrained borrowers.

A probable reason for most of the choices the banks made concerning the exceptions is the cross-sell potential. Banks consider mortgage loans as a captive product. When they can sell a mortgage to a new customer, the borrower has to maintain an account at the bank on which the salary is paid and from which the service of the mortgage loan is done over the entire maturity. When these customers take housing-related insurance from the same bank or when they manage their investments from that bank, the bank collects fees and other non-interest revenues that increase the total return on the mortgage. Hence, banks have an incentive to favor FTB because the lifetime return on such customers may be substantial.

Taken together, these findings suggest that soft macroprudential limits can strike a balance between enhancing financial stability and maintaining access to credit for potentially vulnerable borrower groups. This provides a policy-relevant alternative to hard limits, particularly in systems aiming to safeguard both prudential soundness and social inclusiveness in mortgage lending.

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van Bekkum, S., Gabarro, M., Irani, R. M., & Peydró, J. L. (2024). The real effects of borrower-based macroprudential policy: Evidence from administrative household-level data. *Journal of Monetary Economics*. https://doi.org/10.1016/j.jmoneco.2024.103574

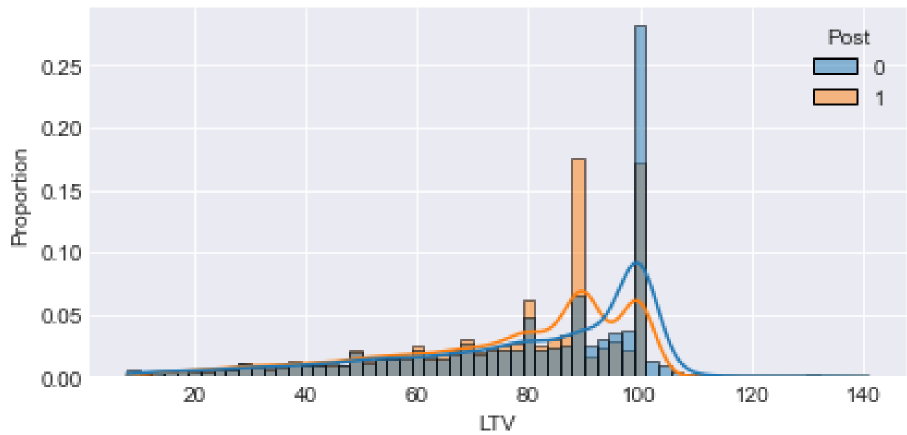
13 Appendix

13.1 Appendix A: Data

Table 19: Overview number of observations per group; 2019-2021

		Numbers of observation					
	X =	FTB	Young	low income	low NAI	cosigner	low savings
Treated X	Pre	4,013	3,619	1,956	1,962	2,701	3,651
	Post	3,773	3,340	1,742	1,554	2,430	2,515
Treated non-X	Pre	316	710	2,373	2,367	1,628	678
	Post	273	706	2,304	2,492	1,616	1,531
Control X	Pre	3,339	2,561	2,476	2,749	2,643	2,727
	Post	6,195	5,281	3,962	4,378	4,852	2,943
Control non-X	Pre	1,808	2,586	2,671	2,398	2,504	2,420
	Post	2,964	3,878	5,197	4,781	4,307	6,216
Observations		22,681	22,681	22,681	22,681	22,681	22,681
Observations Pre		9,476	9,476	9,476	9,476	9,476	9,476
Observations Post		13,205	13,205	13,205	13,205	13,205	13,205
Observations Treated		8,375	8,375	8,375	8,375	8,375	8,375
Observations Control		14,306	14,306	14,306	14,306	14,306	14,306

Figure 10: Evolution distribution of LTV Pre versus Post; 2019-2021



13.2 Appendix B: Predictions

Figure 11 presents the feature importances of the selected model. As expected, savings, age, income, FTB status, and net available income are among the strongest predictors, followed by geographic dummies and property type. Figure 12 validates the model's predictive power by plotting average observed LTVs across five 20%-wide probability bins. The positive relationship confirms that higher predicted probabilities correspond to higher realized LTVs.

The next step is to determine appropriate cutoffs: one to exclude observations too far below the LTV policy threshold, and another to separate the *Treated* from the *Controls*. The exclusion cutoff removes loans with predicted probabilities far below the 90% threshold, which are not comparable to those around it. Additionally, we need a clear distinction between *Controls* (loans likely to remain below 90% LTV in absence of the policy) and *Treated* loans (those most likely to exceed the 90% threshold if the policy had not changed).

Examining equally spaced bins, we find that average LTVs for probability ranges 0.8–1 and 0.6–0.8 exceed 90% before 2020, justifying their classification as *Treated*. Conversely, probabilities between 0 and 0.2 consistently yield low LTVs, supporting their exclusion. Probabilities in the 0.2–0.4 range correspond to average LTVs well below 90%, making them good controls, while those between 0.4 and 0.6 are closer to the threshold and require closer inspection. Additionally, we cross-check these choices by looking at the conditional distributions of predicted probabilities for the year 2019 (see Figure 3). Based on this distribution, the ideal threshold for separating treated from controls appears to lie between 0.4 and 0.6. In summary, two key choices remain: (1) confirming 0.2 as the exclusion cutoff and (2) selecting a *Treated–Control* cutoff within 0.4–0.6.

Figure 13 shows that loans with probabilities between 0.15–0.2 behave similarly to those below 0.15, confirming 0.2 as the exclusion threshold. Determining the exact *Treated–Control* cutoff is less straightforward. While Figure 3 suggests a 0.5 threshold, a closer look reveals that loans with probabilities between 0.5–0.575 still have average LTVs below 90% in 2019. Excluding this range increases classification accuracy in both groups (see Tables 20–22). Accordingly, the final *Control* group includes observations with probabilities between 0.2 and 0.5, while the *Treated* group consists loans with probabilities above 0.575.

Figure 11: Feature importance of predictions for Pr[LTV>90]; training 2016-2018

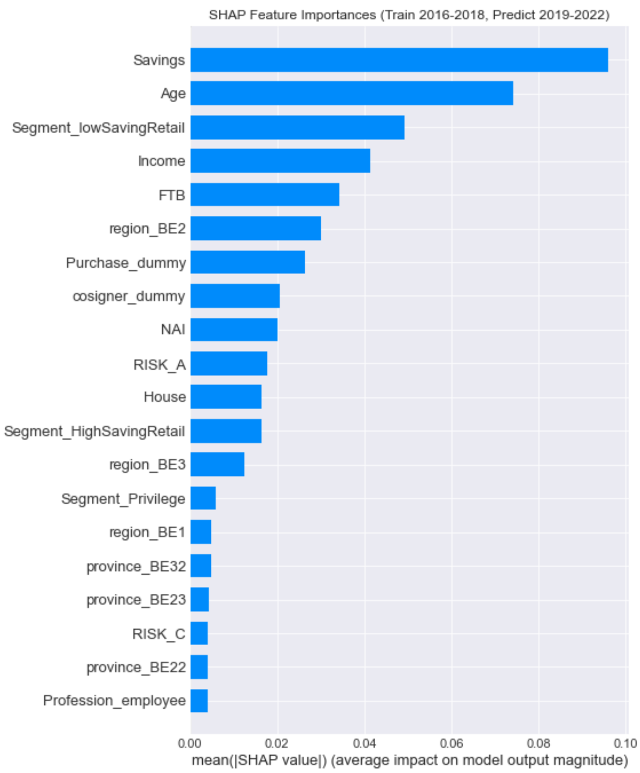


Figure 12: Evolution average LTV for 5 equally spaced probability ranges

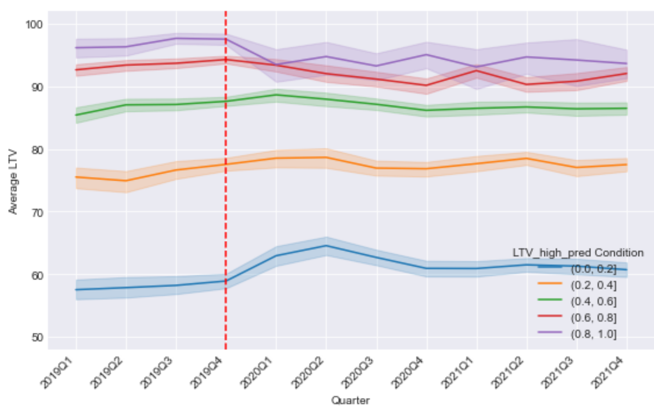


Figure 13: Evolution average LTV for different probability ranges

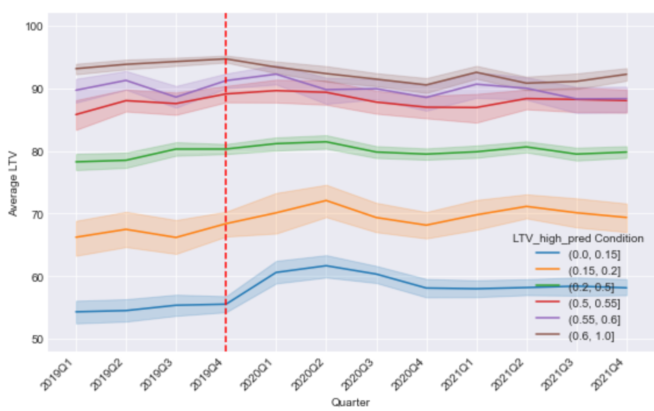


Figure 14: Evolution average LTV for the "Treated", "Control" and "Excluded"

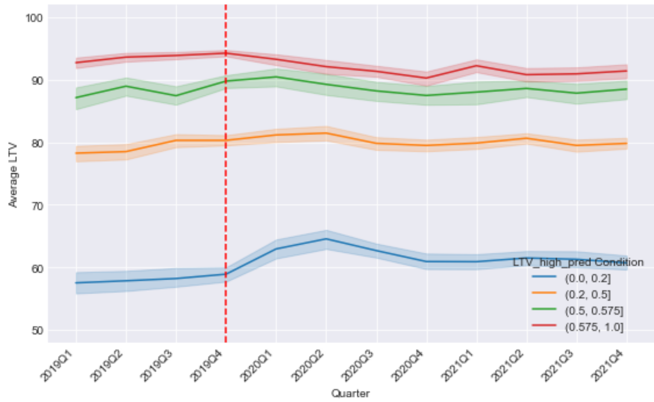


Table 20: Numbers of observations correctly & wrongly predicted for 2019 (threshold: 0.575)

	# Observations	
	Actual LTV<90	Actual LTV>90
Predicted LTV<90	8334 (66.7% correct)	4160
Predicted LTV>90	1159	4355 **(79.0% correct)**

Note: Percentage corresponds to the proportion of correctly identified observations within the Treated group and within the Control group separately

Table 21: Numbers of observations correctly & wrongly predicted for 2019 (threshold: 0.50)

	# Observations	
	Actual LTV<90	Actual LTV>90
Predicted LTV<90	7578 **(72.6% correct)**	2859
Predicted LTV>90	1915	5656 (74.7% correct)

Note: Percentage corresponds to the proportion of correctly identified observations within the Treated group and within the Control group separately

Table 22: Numbers of observations correctly & wrongly predicted for 2019 (threshold: 0.50-0.575)

	# Observations	
	Actual LTV<90	Actual LTV>90
Predicted LTV<90	7578 **(72.6% correct)**	2859
Predicted LTV>90	1159	4355 **(79.0% correct)**

Note: Percentage corresponds to the proportion of correctly identified observations within the Treated group and within the Control group separately

13.3 Appendix C: Regressions

13.3.1 Appendix C.1: Additional regressions

Table 23: Regression with quarter dummies (2019Q4 as benchmark); dependent variables: *LTV* and *LTVabove90*; 2019-2021

	LTV				LTVabove90			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
2019Q1	-1.879***	-1.915***	-1.143**	-1.227**	-0.036**	-0.041***	-0.022	-0.028**
	(0.583)	(0.570)	(0.551)	(0.535)	(0.015)	(0.015)	(0.014)	(0.014)
2019Q2	-1.264**	-1.317**	-0.560	-0.679	-0.014	-0.014	-0.001	-0.003
	(0.550)	(0.550)	(0.519)	(0.512)	(0.015)	(0.015)	(0.015)	(0.015)
2019Q3	0.066	-0.008	0.296	0.179	0.001	0.000	0.000	-0.001
	(0.502)	(0.494)	(0.488)	(0.478)	(0.013)	(0.013)	(0.012)	(0.012)
2020Q1	-1.162**	-1.324***	-0.416	-0.697	-0.091***	-0.093***	-0.066***	-0.070***
	(0.516)	(0.509)	(0.482)	(0.473)	(0.016)	(0.016)	(0.015)	(0.015)
2020Q2	-1.950***	-2.118***	-0.992*	-1.294**	-0.139***	-0.144***	-0.103***	-0.111***
	(0.586)	(0.586)	(0.552)	(0.550)	(0.018)	(0.018)	(0.017)	(0.017)
2020Q3	-3.387***	-3.426***	-2.160***	-2.355***	-0.193***	-0.195***	-0.148***	-0.153***
	(0.526)	(0.523)	(0.475)	(0.473)	(0.015)	(0.014)	(0.013)	(0.013)
2020Q4	-4.523***	-4.576***	-3.134***	-3.334***	-0.251***	-0.253***	-0.198***	-0.203***
	(0.523)	(0.525)	(0.498)	(0.503)	(0.015)	(0.015)	(0.014)	(0.015)
2021Q1	-4.158***	-4.293***	-2.695***	-2.981***	-0.252***	-0.255***	-0.196***	-0.202***
	(0.573)	(0.570)	(0.526)	(0.521)	(0.016)	(0.016)	(0.015)	(0.015)
2021Q2	-3.689***	-3.624***	-2.717***	-2.895***	-0.258***	-0.257***	-0.212***	-0.216***
	(0.524)	(0.510)	(0.490)	(0.479)	(0.014)	(0.014)	(0.014)	(0.014)
2021Q3	-4.698***	-4.704***	-2.921***	-3.116***	-0.272***	-0.273***	-0.207***	-0.212***
	(0.530)	(0.537)	(0.486)	(0.493)	(0.016)	(0.016)	(0.016)	(0.016)
2021Q4	-4.580***	-4.580***	-2.951***	-3.164***	-0.277***	-0.276***	-0.213***	-0.218***
	(0.566)	(0.568)	(0.513)	(0.517)	(0.013)	(0.013)	(0.013)	(0.013)
Borrower Controls	-	-	x	x	-	-	x	x
Region FE	x	-	x	-	x	-	x	-
Municipality FE	-	x	-	x	-	x	-	x
Observations	22,681	22,678	22,681	22,678	22,681	22,678	22,681	22,678
R^2	0.014	0.060	0.163	0.204	0.065	0.105	0.161	0.196
R^2 Within	0.010	0.010	0.159	0.161	0.054	0.055	0.151	0.151

Note: * p<0.1 ; ** p<0.05; *** p<0.01; Standard errors clustered at municipality level.

Triple Difference-in-difference summing up the coefficients for easier interpretation:

Table 24: Overview addition of coefficients for interpretability Triple DiD with FTB; 2019-2021

Intercept	**+ Treated**	**+ FTB + FTBxTreated**	**+ Post + PostxTreated + PostxFTB + PostxTreatedxFTB**	**Resulting LTV**
Intercept 77.594	**Treated** + 11.124	**FTB** + 2.045	**Pre** + 0	90.763
			Post - 4.490 - 3.136 + 5.591	88.728
		non-FTB + 0	**Pre** + 0	88.718
			Post - 4.490 - 3.206	81.022
	Control + 0	**FTB** + 0	**Pre** + 0	77.594
			Post - 4.490 + 5.591	78.695
		non-FTB + 0	**Pre** + 0	77.594
			Post - 4.490	73.104

Note: Young = + 5.058, low income = - 1.383, cosigner = + 1.214, low savings = - 2.155;
For simplicity the regressions with no FEs have been used

Table 25: Overview addition of coefficients for interpretability Triple DiD with FTB; 2019-2021

Intercept	**+ Treated**	**+ FTB + FTBxTreated**	**+ Post + PostxTreated + PostxFTB + PostxTreatedxFTB**	**Resulting LTVabove90**
Intercept 0.322	**Treated** + 0.335	**FTB** + 0.071	**Pre** + 0	0.728
			Post - 0.183 - 0.195 + 0.062 + 0.169	0.581
		non-FTB + 0	**Pre** + 0	0.657
			Post - 0.183 - 0.195	0.279
	Control + 0	**FTB** + 0.071	**Pre** + 0	0.393
			Post - 0.183 + 0.062	0.272
		non-FTB + 0	**Pre** + 0	0.322
			Post - 0.183	0.139

Note: Young = + 0.022, low income = - 0.040, cosigner = + 0.022, low savings = - 0;
For simplicity the regressions with no FEs have been used

Table 26: Overview addition of coefficients for interpretability Triple DiD with Young (Y); 2019-2021

<table>
<tr><th>Intercept</th><th>+ Treated</th><th>+ Y
+ YxTreated</th><th>+ Post + PostxTreated
+ PostxY + PostxTreatedxY</th><th>Resulting LTV</th></tr>
<tr><td rowspan="8">Intercept
76.125</td><td rowspan="4">Treated
+ 14.194</td><td rowspan="2">Young
+ 3.938 - 2.043</td><td>Pre
+ 0</td><td>92.214</td></tr>
<tr><td>Post
- 2.757 + 3.569 -3.206</td><td>89.820</td></tr>
<tr><td rowspan="2">non-Young
+ 0</td><td>Pre
+ 0</td><td>90.319</td></tr>
<tr><td>Post
- 2.757</td><td>87.562</td></tr>
<tr><td rowspan="4">Control
+ 0</td><td rowspan="2">Young
+ 3.938</td><td>Pre
+ 0</td><td>80.063</td></tr>
<tr><td>Post
- 2.757 + 3.569</td><td>80.875</td></tr>
<tr><td rowspan="2">non-Young
+ 0</td><td>Pre
+ 0</td><td>76.125</td></tr>
<tr><td>Post
- 2.757</td><td>73.368</td></tr>
<tr><td colspan="5">Note: FTB = + 4.076, low income = - 1.482, cosigner = + 1.085, low savings = - 2.094;
For simplicity the regressions with no FEs have been used</td></tr>
</table>

Table 27: Overview addition of coefficients for interpretability Triple DiD with Young (Y); 2019-2021

<table>
<tr><th>Intercept</th><th>+ Treated</th><th>+ Y
+ YxTreated</th><th>+ Post + PostxTreated
+ PostxY + PostxTreatedxY</th><th>Resulting
LTVabove90</th></tr>
<tr><td rowspan="8">Intercept
0.301</td><td rowspan="4">Treated
+ 0.325</td><td rowspan="2">Young
+ 0.044</td><td>Pre
+ 0</td><td>0.670</td></tr>
<tr><td>Post
- 0.160 + 0.034</td><td>0.544</td></tr>
<tr><td rowspan="2">non-Young
+ 0</td><td>Pre
+ 0</td><td>0.626</td></tr>
<tr><td>Post
- 0.160</td><td>0.466</td></tr>
<tr><td rowspan="4">Control
+ 0</td><td rowspan="2">Young
+ 0</td><td>Pre
+ 0</td><td>0.301</td></tr>
<tr><td>Post
- 0.160 + 0.034</td><td>0.175</td></tr>
<tr><td rowspan="2">non-Young
+ 0</td><td>Pre
+ 0</td><td>0.301</td></tr>
<tr><td>Post
- 0.160</td><td>0.141</td></tr>
<tr><td colspan="5">Note: FTB = + 0.129, low income = - 0.040, cosigner = + 0.022, low savings = - 0;
For simplicity the regressions with no FEs have been used</td></tr>
</table>

Table 28: Overview addition of coefficients for interpretability Triple DiD with lowIncome (LI); 2019-2021

Intercept	**+ Treated**	**+ LI** **+ LIxTreated**	**+ Post + PostxTreated** **+ PostxLI + PostxTreatedxLI**	**Resulting LTV**
Intercept 75.587	**Treated** + 11.407	**low income** - 2.171 + 1.256	**Pre** + 0	86.079
			Post - 1.291 - 1.067 -1.657	82.064
		non-LI + 0	**Pre** + 0	86.994
			Post - 1.291 - 1.067	84.636
	Control + 0	**low income** - 2.171	**Pre** + 0	73.416
			Post - 1.291	72.125
		non-LI + 0	**Pre** + 0	75.587
			Post - 1.291	74.296

Note: FTB = + 4.536, Young = + 5.059, cosigner = + 1.206, low savings = - 2.187;
For simplicity the regressions with no FEs have been used

Table 29: Overview addition of coefficients for interpretability Triple DiD with lowIncome (LI); 2019-2021

Intercept	**+ Treated**	**+ LI** **+ LIxTreated**	**+ Post + PostxTreated** **+ PostxLI + PostxTreatedxLI**	**Resulting** **LTVabove90**
Intercept 0.272	**Treated** + 0.361	**LI** + 0	**Pre** + 0	0.633
			Post - 0.121 - 0.043 - 0.046	0.423
		non-LI + 0	**Pre** + 0	0.633
			Post - 0.121	0.512
	Control + 0	**LI** + 0	**Pre** + 0	0.272
			Post - 0.121 - 0.043	0.108
		non-LI + 0	**Pre** + 0	0.272
			Post - 0.121	0.151

Note: FTB = + 0.125, Young = + 0.018, cosigner = + 0.020, low savings = - 0;
For simplicity the regressions with no FEs have been used

Table 30: Overview addition of coefficients for interpretability Triple DiD with low NAI (LNAI); 2019-2021

Intercept	**+ Treated**	**+ LNAI + LNAIxTreated**	**+ Post + PostxTreated + PostxLNAI + PostxTreatedxLNAI**	**Resulting LTV**
Intercept 74.309	**Treated** + 13.215	**LNAI** + 1.909 - 2.298	**Pre** + 0	87.135
			Post - 2.230 - 1.674	83.231
		non-LNAI + 0	**Pre** + 0	87.524
			Post - 2.230	85.294
	Control + 0	**LNAI** + 1.909	**Pre** + 0	76.218
			Post - 1.674	74.544
		non-LNAI + 0	**Pre** + 0	74.309
			Post - 0	74.309
Note: FTB = + 4.458, Young = + 5.050, low income = - 1.515, cosigner = + 1.200; For simplicity the regressions with no FEs have been used				

Table 31: Overview addition of coefficients for interpretability Triple DiD with low NAI (LNAI); 2019-2021

Intercept	**+ Treated**	**+ LNAI + LNAIxTreated**	**+ Post + PostxTreated + PostxLNAI + PostxTreatedxLNAI**	**Resulting LTVabove90**
Intercept 0.266	**Treated** + 0.383	**LNAI** + 0.032 - 0.058	**Pre** + 0	0.623
			Post - 0.099 - 0.082	0.442
		non-LNAI + 0	**Pre** + 0	0.649
			Post - 0.099	0.550
	Control + 0	**LNAI** + 0.032	**Pre** + 0	0.289
			Post - 0.099 - 0.082	0.117
		non-LNAI + 0	**Pre** + 0	0.266
			Post - 0.099	0.167
Note: FTB = + 0.123, Young = + 0.020, low income = - 0.022, low savings = + ?, cosigner = + 0.0; For simplicity the regressions with no FEs have been used				

Table 32: Overview addition of coefficients for interpretability Triple DiD with cosigner (C); 2019-2021

<table>
<tr><th>Intercept</th><th>+ Treated</th><th>+ C
+ CxTreated</th><th>+ Post + PostxTreated
+ PostxC + PostxTreatedxC</th><th>Resulting LTV</th></tr>
<tr><td rowspan="8">Intercept
74.794</td><td rowspan="4">Treated
+ 12.664</td><td rowspan="2">cosigner
+ 2.080 - 1.210</td><td>Pre
+ 0</td><td>88.328</td></tr>
<tr><td>Post
- 2.679 - 1.223 + 1.566</td><td>85.992</td></tr>
<tr><td rowspan="2">no-cosigner
+ 0</td><td>Pre
+ 0</td><td>87.458</td></tr>
<tr><td>Post
- 2.679</td><td>84.779</td></tr>
<tr><td rowspan="4">Control
+ 0</td><td rowspan="2">cosigner
+ 2.080</td><td>Pre
+ 0</td><td>76.874</td></tr>
<tr><td>Post
- 1.223</td><td>75.651</td></tr>
<tr><td rowspan="2">no-cosigner
+ 0</td><td>Pre
+ 0</td><td>74.794</td></tr>
<tr><td>Post
- 0</td><td>74.769</td></tr>
<tr><td colspan="5">Note: FTB = + 4.527, Young = + 5.048, low income = - 1.421, low savings = - 2.679;
For simplicity the regressions with no FEs have been used</td></tr>
</table>

Table 33: Overview addition of coefficients for interpretability Triple DiD with cosigner (C); 2019-2021

<table>
<tr><th>Intercept</th><th>+ Treated</th><th>+ C
+ CxTreated</th><th>+ Post + PostxTreated
+ PostxC + PostxTreatedxC</th><th>Resulting
LTVabove90</th></tr>
<tr><td rowspan="8">Intercept
0.303</td><td rowspan="4">Treated
+ 0.346</td><td rowspan="2">cosigner
+ 0</td><td>Pre
+ 0</td><td>0.649</td></tr>
<tr><td>Post
- 0.154 - 0.060 + 0.061</td><td>0.496</td></tr>
<tr><td rowspan="2">no-cosigner
+ 0</td><td>Pre
+ 0</td><td>0.649</td></tr>
<tr><td>Post
- 0.154 - 0.060</td><td>0.435</td></tr>
<tr><td rowspan="4">Control
+ 0</td><td rowspan="2">cosigner
+ 0</td><td>Pre
+ 0</td><td>0.303</td></tr>
<tr><td>Post
- 0.154</td><td>0.149</td></tr>
<tr><td rowspan="2">no-cosigner
+ 0</td><td>Pre
+ 0</td><td>0.303</td></tr>
<tr><td>Post
- 0.154</td><td>0.149</td></tr>
<tr><td colspan="5">Note: FTB = + 0.125, Young = + 0.019, low income = - 0.040, low savings = - 0.0;
For simplicity the regressions with no FEs have been used</td></tr>
</table>

Table 34: Overview addition of coefficients for interpretability Triple DiD with low savings (LS); 2019-2021

Intercept	**+ Treated**	**+ LS + LSxTreated**	**+ Post + PostxTreated + PostxLS + PostxTreatedxLS**	**Resulting LTV**
Intercept 73.738	**Treated** + 11.039	**LS** + 0.954	**Pre** + 0	85.731
			Post + 1.772 - 1.426 - 5.985 - 2.312	77.780
		non-LS + 0	**Pre** + 0	84.777
			Post + 1.772 - 1.426	80.564
	Control + 0	**LS** + 0.954	**Pre** + 0	74.692
			Post + 1.772 - 5.985	70.479
		non-LS + 0	**Pre** + 0	73.738
			Post + 1.772	75.510
Note: FTB = + 4.346, Young = + 4.975, low income = - 1.401, cosigner = + 1.168; For simplicity the regressions with no FEs have been used				

Table 35: Overview addition of coefficients for interpretability Triple DiD with low savings (LS); 2019-2021

Intercept	**+ Treated**	**+ LS + LSxTreated**	**+ Post + PostxTreated + PostxLS + PostxTreatedxLS**	**Resulting LTVabove90**
Intercept 0.242	**Treated** + 0.357	**LS** + 0.085	**Pre** + 0	0.684
			Post - 0.089 - 0.110	0.485
		non-LS + 0	**Pre** + 0	0.599
			Post - 0.089	0.510
	Control + 0	**LS** + 0.085	**Pre** + 0	0.327
			Post - 0.089 - 0.110	0.128
		non-LS + 0	**Pre** + 0	0.242
			Post - 0.089	0.153
Note: FTB = + 0.127, Young = + 0.020, low income = - 0.040, cosigner = + 0.020; For simplicity the regressions with no FEs have been used				

Table 36: DiD regression; dependent variable: *LTV* and *LTVabove90*; 2019-2021

	LTV						LTVabove90					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treated	9.502***	8.881***	10.570***	10.277***	7.831***	7.344***	0.299***	0.291***	0.322***	0.319***	0.251***	0.245***
	(0.392)	(0.404)	(0.305)	(0.301)	(0.350)	(0.355)	(0.011)	(0.010)	(0.009)	(0.009)	(0.010)	(0.010)
Post × Treated	-2.228***	-2.210***	-3.416***	-3.379***	-3.177***	-3.148***	-0.042***	-0.045***	-0.073***	-0.074***	-0.070***	-0.072***
	(0.414)	(0.404)	(0.375)	(0.366)	(0.363)	(0.355)	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)
cosigner	1.401***	1.138***			0.184	-0.006	0.031***	0.024***			-0.010	-0.013
	(0.338)	(0.336)			(0.321)	(0.327)	(0.008)	(0.009)			(0.008)	(0.008)
FTB	5.524***	5.647***			4.952***	5.049***	0.143***	0.143***			0.147***	0.146***
	(0.337)	(0.339)			(0.314)	(0.314)	(0.009)	(0.008)			(0.008)	(0.008)
Age	1.025***	1.018***			0.090	0.111	0.009***	0.008***			-0.006***	-0.007***
	(0.108)	(0.108)			(0.106)	(0.105)	(0.002)	(0.002)			(0.002)	(0.002)
$(\text{Age})^2$	-0.019***	-0.019***			-0.004***	-0.005***	-0.000***	-0.000***			0.000*	0.000*
	(0.001)	(0.001)			(0.001)	(0.001)	(0.000)	(0.000)			(0.000)	(0.000)
Inc/1000	2.442***	3.143***			4.230***	4.891***	0.066***	0.078***			0.119***	0.129***
	(0.428)	(0.427)			(0.419)	(0.416)	(0.010)	(0.010)			(0.010)	(0.010)
$(\text{Inc}/1000)^2$	-0.215***	-0.255***			-0.275***	-0.313***	-0.006***	-0.007***			-0.009***	-0.009***
	(0.044)	(0.045)			(0.044)	(0.044)	(0.001)	(0.001)			(0.001)	(0.001)
Sav/10000	0.039	0.027			0.159***	0.143**	-0.011***	-0.011***			-0.007***	-0.007***
	(0.063)	(0.064)			(0.056)	(0.057)	(0.001)	(0.001)			(0.001)	(0.001)
$(\text{Sav}/10000)^2$	-0.006***	-0.006***			-0.006***	-0.006***	0.000***	0.000***			0.000***	0.000***
	(0.001)	(0.001)			(0.001)	(0.001)	(0.000)	(0.000)			(0.000)	(0.000)
Interest			7.028***	6.515***	9.823***	9.342***			0.296***	0.290***	0.339***	0.332***
			(0.412)	(0.409)	(0.429)	(0.420)			(0.010)	(0.010)	(0.011)	(0.011)
Fixed			-3.890***	-3.851***	-4.284***	-4.228***			-0.101***	-0.104***	-0.106***	-0.109***
			(0.838)	(0.822)	(0.847)	(0.831)			(0.025)	(0.026)	(0.026)	(0.027)
Maturity			0.136***	0.138***	0.118***	0.121***			0.002***	0.002***	0.001***	0.001***
			(0.003)	(0.003)	(0.004)	(0.004)			(0.000)	(0.000)	(0.000)	(0.000)
House			-1.266***	-0.958***	-2.823***	-2.885***			0.014*	0.013*	-0.015*	-0.022***
			(0.277)	(0.281)	(0.305)	(0.311)			(0.008)	(0.008)	(0.008)	(0.008)
Time FE	x	x	x	x	x	x	x	x	x	x	x	x
Region FE	x	-	x	-	x	-	x	-	x	-	x	-
Municipality FE	-	x	-	x	-	x	-	x	-	x	-	x
Observations	22,681	22,678	22,681	22,678	22,681	22,678	22,681	22,678	22,681	22,678	22,681	22,678
R^2	0.197	0.234	0.293	0.321	0.326	0.358	0.203	0.234	0.262	0.288	0.284	0.309
R^2 Within	0.186	0.184	0.283	0.277	0.316	0.316	0.148	0.144	0.211	0.204	0.234	0.228

* p<0.1 ; ** p<0.05; *** p<0.01; Standard errors clustered at municipality level.

13.3.2 Appendix C.2: Additional regressions: different threshold for "Excluded" vs "Control" and for "Control" vs "Treated" (robustness check)

Table 37: DiD regression; dependent variable: *LTV* and *LTVabove90*; 2019-2021; treatment threshold 0.50 (instead of 0.5-0.575)

	LTV				LTVabove90			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	13.149***	12.815***	8.600***	8.114***	0.346***	0.340***	0.257***	0.249***
	(0.337)	(0.340)	(0.373)	(0.382)	(0.009)	(0.008)	(0.009)	(0.009)
Post × Treated	-2.337***	-2.401***	-1.961***	-1.984***	-0.045***	-0.048***	-0.042***	-0.044***
	(0.437)	(0.427)	(0.412)	(0.400)	(0.011)	(0.011)	(0.011)	(0.011)
Borrower Controls	-	-	x	x	-	-	x	x
Time FE	x	x	x	x	x	x	x	x
Region FE	x	-	x	-	x	-	x	-
Municipality FE	-	x	-	x	-	x	-	x
Observations	26,302	26,299	26,302	26,299	26,302	26,299	26,302	26,299
R^2	0.116	0.149	0.183	0.217	0.157	0.186	0.178	0.207
R^2 Within	0.105	0.100	0.173	0.171	0.103	0.100	0.126	0.123

* p<0.1 ; ** p<0.05; *** p<0.01; Standard errors clustered at municipality level.

Table 38: DiD regression; dependent variable: *LTV* and *LTVabove90*; 2019-2021; treatment threshold 0.575 (instead of 0.5-0.575)

	LTV				LTVabove90			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	12.246***	11.853***	7.417***	6.915***	0.337***	0.331***	0.241***	0.233***
	(0.337)	(0.339)	(0.336)	(0.345)	(0.011)	(0.010)	(0.011)	(0.011)
Post × Treated	-1.822***	-1.807***	-1.738***	-1.718***	-0.025**	-0.027**	-0.030**	-0.031***
	(0.395)	(0.390)	(0.380)	(0.376)	(0.012)	(0.012)	(0.012)	(0.012)
Borrower Controls	-	-	x	x	-	-	x	x
Time FE	x	x	x	x	x	x	x	x
Region FE	x	-	x	-	x	-	x	-
Municipality FE	-	x	-	x	-	x	-	x
Observations	26,302	26,299	26,302	26,299	26,302	26,299	26,302	26,299
R^2	0.093	0.128	0.175	0.210	0.145	0.175	0.175	0.204
R^2 Within	0.083	0.078	0.165	0.165	0.091	0.087	0.123	0.119

* p<0.1 ; ** p<0.05; *** p<0.01; Standard errors clustered at municipality level.

Table 39: DiD regression; dependent variable: *LTV* and *LTVabove90*; 2019-2021; exclusion threshold 0.15 (instead of 0.2)

	LTV				LTVabove90			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	16.648***	16.153***	9.453***	8.800***	0.431***	0.424***	0.316***	0.308***
	(0.348)	(0.351)	(0.383)	(0.392)	(0.009)	(0.009)	(0.010)	(0.010)
Post × Treated	-2.685***	-2.678***	-2.480***	-2.413***	-0.051***	-0.053***	-0.053***	-0.054***
	(0.426)	(0.423)	(0.397)	(0.393)	(0.012)	(0.012)	(0.011)	(0.011)
Borrower Controls	-	-	x	x	-	-	x	x
Time FE	x	x	x	x	x	x	x	x
Region FE	x	-	x	-	x	-	x	-
Municipality FE	-	x	-	x	-	x	-	x
Observations	25,756	25,754	25,756	25,754	25,756	25,754	25,756	25,754
R^2	0.140	0.172	0.236	0.267	0.202	0.229	0.227	0.253
R^2 Within	0.128	0.121	0.226	0.222	0.147	0.141	0.174	0.168

* p<0.1 ; ** p<0.05; *** p<0.01; Standard errors clustered at municipality level.

Table 40: DiD regression; dependent variable: *LTV* and *LTVabove90*; 2019-2021; exclusion threshold 0.25 (instead of 0.2)

	LTV				LTVabove90			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	12.886***	12.503***	9.404***	8.829***	0.363***	0.358***	0.288***	0.280***
	(0.350)	(0.354)	(0.400)	(0.412)	(0.010)	(0.010)	(0.012)	(0.011)
Post × Treated	-2.463***	-2.456***	-2.061***	-2.022***	-0.042***	-0.045***	-0.039***	-0.041***
	(0.443)	(0.437)	(0.427)	(0.421)	(0.013)	(0.013)	(0.013)	(0.013)
Borrower Controls	-	-	x	x	-	-	x	x
Time FE	x	x	x	x	x	x	x	x
Region FE	x	-	x	-	x	-	x	-
Municipality FE	-	x	-	x	-	x	-	x
Observations	20,229	20,225	20,229	20,225	20,229	20,225	20,229	20,225
R^2	0.116	0.155	0.168	0.208	0.166	0.201	0.183	0.217
R^2 Within	0.102	0.097	0.155	0.153	0.110	0.105	0.127	0.124

* p<0.1 ; ** p<0.05; *** p<0.01; Standard errors clustered at municipality level.

13.3.3 Appendix C.3: Additional regressions: different combinations of control variables (robustness check)

Table 41: DiD regression; dependent variable: LTV; 2019-2021; variety of borrower controls

	LTV								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated	14.636***	12.528***	11.226***	14.213***	15.523***	16.173***	14.411***	9.502***	9.515***
	(0.347)	(0.360)	(0.342)	(0.355)	(0.369)	(0.380)	(0.346)	(0.392)	(0.399)
Post × Treated	-2.594***	-2.433***	-2.191***	-2.707***	-2.549***	-3.051***	-2.586***	-2.228***	-2.501***
	(0.438)	(0.430)	(0.420)	(0.442)	(0.441)	(0.439)	(0.438)	(0.414)	(0.413)
FTB		6.065***						5.524***	5.312***
		(0.312)						(0.337)	(0.337)
cosigner		2.555***						1.401***	1.411***
		(0.263)						(0.338)	(0.335)
Age			0.778***					1.025***	0.919***
			(0.109)					(0.108)	(0.110)
$(\text{Age})^2$			-0.016***					-0.019***	-0.018***
			(0.001)					(0.001)	(0.002)
Inc/1000				2.989***				2.442***	2.131***
				(0.333)				(0.428)	(0.431)
$(\text{Inc}/1000)^2$				-0.374***				-0.215***	-0.159***
				(0.038)				(0.044)	(0.045)
Sav/1000					0.655***			0.039	-0.084
					(0.055)			(0.063)	(0.067)
$(\text{Sav}/1000)^2$					-0.019***			-0.006***	-0.002
					(0.001)			(0.001)	(0.001)
SEG High-Sav-retail						5.272***			2.004***
						(0.298)			(0.321)
SEG Privilege						2.173***			-1.278***
						(0.429)			(0.456)
Private-Wealth						-3.812***			-4.894***
						(0.829)			(0.891)
PROF Indépendant							-0.674		-0.920**
							(0.456)		(0.442)
PROF libérale							-0.903		-2.021**
							(0.874)		(0.824)
PROF Gérant							-3.305***		0.093
							(0.459)		(0.486)
Time FE	x	x	x	x	x	x	x	x	x
Region FE	x	x	x	x	x	x	x	x	x
Observations	22,681	22,681	22,681	22,681	22,681	22,681	22,681	22,681	22,681
R^2	0.127	0.147	0.177	0.132	0.136	0.145	0.130	0.197	0.203
R^2 Within	0.115	0.135	0.165	0.120	0.124	0.133	0.118	0.186	0.192

* p<0.1 ; ** p<0.05; *** p<0.01; Standard errors clustered at municipality level.

Table 42: DiD regression; dependent variable: *LTVabove90*; 2019-2021; variety of borrower controls

	LTVabove90								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated	0.394***	0.349***	0.367***	0.386***	0.388***	0.404***	0.389***	0.299***	0.308***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.011)	(0.011)
Post × Treated	-0.043***	-0.040***	-0.039***	-0.045***	-0.046***	-0.051***	-0.043***	-0.042***	-0.048***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
FTB		0.131***						0.143***	0.136***
		(0.008)						(0.009)	(0.009)
cosigner		0.053***						0.031***	0.034***
		(0.007)						(0.008)	(0.008)
Age			0.002					0.009***	0.008***
			(0.002)					(0.002)	(0.002)
$(\text{Age})^2$			-0.000**					-0.000***	-0.000***
			(0.000)					(0.000)	(0.000)
Inc/1000				0.066***				0.066***	0.057***
				(0.009)				(0.010)	(0.010)
$(\text{Inc}/1000)^2$				-0.008***				-0.006***	-0.005***
				(0.001)				(0.001)	(0.001)
Sav/10000					-0.002			-0.011***	-0.015***
					(0.001)			(0.001)	(0.002)
$(\text{Sav}/10000)^2$					-0.000**			0.000***	0.000***
					(0.000)			(0.000)	(0.000)
SEG High-Sav-retail						0.062***			0.058***
						(0.008)			(0.009)
SEG Privilege						-0.012			-0.004
						(0.010)			(0.012)
SEG Private-Wealth						-0.071***			-0.046**
						(0.016)			(0.019)
PROF Indépendant							-0.054***		-0.050***
							(0.012)		(0.012)
PROF libérale							-0.029		-0.029
							(0.023)		(0.022)
PROF Gérant							-0.076***		-0.025*
							(0.012)		(0.013)
Time FE	x	x	x	x	x	x	x	x	x
Region FE	x	x	x	x	x	x	x	x	x
Observations	22,681	22,681	22,681	22,681	22,681	22,681	22,681	22,681	22,681
R^2	0.184	0.196	0.188	0.187	0.185	0.189	0.186	0.203	0.207
R^2 Within	0.127	0.140	0.131	0.130	0.128	0.132	0.130	0.148	0.152

* p<0.1 ; ** p<0.05; *** p<0.01; Standard errors clustered at municipality level.

13.3.4 Appendix C.4: Additional regressions: different fixed effects (robustness check)

Table 43: DiD regression; dependent variable: *LTV* and *LTVabove90*; 2019-2021; Time x Region FE & Time x Municipality FE

	LTV				LTVabove90			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	14.580***	14.157***	9.434***	8.925***	0.396***	0.392***	0.301***	0.290***
	(0.363)	(0.391)	(0.403)	(0.466)	(0.010)	(0.010)	(0.011)	(0.012)
Post × Treated	-2.472***	-2.840***	-2.137***	-2.482***	-0.050***	-0.056***	-0.050***	-0.053***
	(0.468)	(0.524)	(0.441)	(0.478)	(0.012)	(0.014)	(0.012)	(0.014)
Borrower Controls	-	-	x	x	-	-	x	x
Time × Muni. FE	-	x	-	x	-	x	-	x
Time × Region FE	x	-	x	-	x	-	x	-
Observations	22,681	21,143	22,681	21,143	22,681	21,143	22,681	21,143
R^2	0.129	0.311	0.198	0.367	0.186	0.350	0.206	0.367
R^2 Within	0.115	0.109	0.186	0.182	0.127	0.123	0.148	0.146

* p<0.1 ; ** p<0.05; *** p<0.01; Standard errors clustered at municipality level.

13.3.5 Appendix C.5: Additional regressions: leaving out observations from quarters closest to the policy implementation (robustness check)

Table 44: DiD regression; dependent variable: *LTV* and *LTVabove90*; excluding 2019Q3-2020Q2

	LTV				LTVabove90			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	15.283***	14.792***	9.556***	8.897***	0.397***	0.388***	0.297***	0.287***
	(0.566)	(0.566)	(0.639)	(0.639)	(0.015)	(0.015)	(0.017)	(0.016)
Post × Treated	-3.391***	-3.300***	-2.577***	-2.501***	-0.057***	-0.057***	-0.052***	-0.051***
	(0.685)	(0.678)	(0.658)	(0.644)	(0.018)	(0.018)	(0.017)	(0.017)
Borrower Controls	-	-	x	x	-	-	x	x
Time FE	x	x	x	x	x	x	x	x
Region FE	x	-	x	-	x	-	x	-
Municipality FE	-	x	-	x	-	x	-	x
Observations	14,007	14,003	14,007	14,003	14,007	14,003	14,007	14,003
R^2	0.112	0.165	0.196	0.248	0.172	0.214	0.196	0.237
R^2 Within	0.103	0.098	0.189	0.188	0.117	0.112	0.142	0.138

* p<0.1 ; ** p<0.05; *** p<0.01; Standard errors clustered at municipality level.

13.3.6 Appendix C.6: Regressions for alternative predictions (robustness check)

Table 45: DiD regression; dependent variable: *LTV* and *LTVabove90*; 2019-2021; XGBoost model (instead of LightGBM)

	LTV				LTVabove90			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	15.034***	14.620***	9.740***	9.179***	0.404***	0.398***	0.307***	0.301***
	(0.358)	(0.357)	(0.374)	(0.384)	(0.009)	(0.009)	(0.010)	(0.010)
Post × Treated	-2.379***	-2.386***	-2.226***	-2.197***	-0.049***	-0.051***	-0.049***	-0.051***
	(0.436)	(0.422)	(0.415)	(0.403)	(0.012)	(0.012)	(0.012)	(0.012)
Borrower Controls	-	-	x	x	-	-	x	x
Time FE	x	x	x	x	x	x	x	x
Region FE	x	-	x	-	x	-	x	-
Municipality FE	-	x	-	x	-	x	-	x
Observations	23,240	23,237	23,240	23,237	23,240	23,237	23,240	23,237
R^2	0.139	0.172	0.210	0.243	0.192	0.223	0.211	0.241
R^2 Within	0.126	0.118	0.198	0.193	0.135	0.130	0.155	0.150

* p<0.1 ; ** p<0.05; *** p<0.01; Standard errors clustered at municipality level.

13.3.7 Appendix C.7: Placebo regression (robustness check)

Table 46: DiD regression; dependent variable: *LTV* and *LTVabove90*; 2019 with 2019Q1-Q2 as ”pre” period and 2019Q3-Q4 as ”post” period

	LTV				LTVabove90			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	15.266***	15.044***	10.503***	10.082***	0.402***	0.397***	0.315***	0.305***
	(0.571)	(0.578)	(0.662)	(0.677)	(0.015)	(0.015)	(0.018)	(0.019)
Post × Treated	-1.089	-1.160	-0.700	-0.767	-0.008	-0.010	-0.003	-0.004
	(0.713)	(0.710)	(0.707)	(0.702)	(0.018)	(0.019)	(0.018)	(0.019)
Borrower Controls	-	-	x	x	-	-	x	x
Time FE	x	x	x	x	x	x	x	x
Region FE	x	-	x	-	x	-	x	-
Municipality FE	-	x	-	x	-	x	-	x
Observations	9,476	9,457	9,476	9,457	9,476	9,457	9,476	9,457
R^2	0.150	0.211	0.186	0.249	0.160	0.221	0.172	0.232
R^2 Within	0.144	0.140	0.181	0.181	0.153	0.149	0.164	0.162

* p<0.1 ; ** p<0.05; *** p<0.01; Standard errors clustered at municipality level.