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Abstract

We study whether and how EU banks comply with tighter macroprudential policy (MPP). Observing contractual details for more than one million securitized loans, we document an elusive risk-shifting response by EU banks in reaction to tighter loan-to-value (LTV) restrictions between 2009 and 2022. Our staggered difference-in-differences reveals that banks respond to these MPP measures at the *portfolio level* by issuing *new loans* after LTV shocks that are smaller, have shorter maturities, and show a higher collateral valuation while holding constant interest rates. Instead of contracting aggregate lending as intended by tighter MPP, banks increase the number and total volume of newly issued loans. Importantly, new loans finance especially properties in less liquid markets identified by a new European Real Estate Index (EREI), which we interpret as a novel, elusive form of risk-shifting.

Keywords: European Real Estate Index, LTV, macroprudential policy, risk shifting

JEL classification: H30, R00, R31

1 Introduction

Historically, excessive mortgage credit growth often preceded price bubbles in real estate markets (Case and Shiller, 2003; Reinhart and Rogoff, 2008; Arce and López-Salido, 2011; Favara and Imbs, 2015; Mian and Sufi, 2018). Sudden corrections in house prices paired with highly indebted households can trigger financial crises (Laeven and Valencia, 2020), distorting housing choices, wealth distributions, and welfare even in the long run (Duca et al., 2016; Cloyne et al., 2019; Hacamo, 2024). Mian and Sufi (2009, 2010, 2011) identify imbalances in real estate lending as one of the main drivers of the Great Financial Crisis of 2007/2008, which led to the development of macroprudential policy (MPP) tools, such as loan-to-value (LTV) caps, to contain excessive (mortgage) borrowing (see, for example, van den Bussche et al., 2015; Claessens, 2015; Cerutti et al., 2017; Peydró et al., 2024).

In this paper, we study not only whether banks comply with MPP shocks collected by Alam et al. (2025), but we provide in particular evidence on the mechanisms, how banks comply with LTV caps set at the portfolio level. Although many studies on LTV regulation consider aggregate credit and average real estate prices (see, for example, Grodecka, 2020; Duca et al., 2021; Cokayne et al., 2024), we leverage granular information on more than 1 million loan contracts that finance residential properties observed at the ZIP code level in 10 European countries between 2009 and 2022. We combine these data with the IWH European Real Estate Index (EREI, Koetter et al., 2024), which uses machine-based methods to collect pricing and offering patterns from real estate platforms at a monthly frequency.

Using a dynamic difference-in-differences setting at the loan level, we estimate that loans issued after a tightening MPP shock exhibit a LTV that is on average 11.5 percentage points lower compared to the four months prior to the policy launch. Hence, EU banks are compliant with macroprudential policy measures at the *portfolio level*, which confirms prior findings that MPP is effective in reducing borrower indebtedness. However, our empirical analysis reveals a number of statistically significant changes in lending terms at the *loan level*, raising concerns about potentially elusive risk shifting by banks.

We document that the likelihood of remortgaging increases after a LTV policy shock by 7.5 percentage points. Mortgages originated after an activation of LTV caps are in comparison to a pre-event period on average 6 percentage points smaller in volume, exhibit a maturity that is 11 percentage points shorter, but which are priced identically. Most importantly, the residential properties serving as collateral in these newly originated mortgages are valued almost 12 percentage points higher relative to the control group. This finding suggests that affected banks might adjust portfolios by replacing maturing loans with mortgages that facilitate what might be an illusive compliance with new MPP: a more benevolent appraisal of identical real estate that is remortgaged at more LTV-friendly terms.

Analyses at the county level support this interpretation of our empirical findings since both the aggregate number as well as the volume of mortgages hike after tightening LTV shocks on the order of 15 and 9 percentage points, respectively. Put differently, while banks comply with reduced LTV caps on the portfolio level, we find neither evidence of contracting loan supply nor residential property prices, as also the average valuation of real estate per county hikes significantly. The EREI also provides granular information on average listing spells per locality as a proxy for the liquidity of local real estate markets. According to differential effects, it shows that the lending expansion after LTV shocks is significantly more pronounced in illiquid regional markets, which indicates an elusive form of risk-shifting while complying de jure with LTV caps at the portfolio level.

Our results complement the few available studies of LTV responses at the loan level. Tzur-Ilan (2023) shows that imposing LTV limits – Israel's main macroprudential tool – does indeed reduce household leverage, but at the cost of pushing many affected borrowers toward less expensive homes that are located farther from the central business district and in lower socioeconomic neighborhoods. Acharya et al. (2022) find that in response to loan-to-income and loan-to-value limits, mortgage credit is reallocated from low- to high-income borrowers and from urban to rural counties. This reallocation of portfolios is possibly consistent with higher remortgaging found in our sample for less liquid regional real estate markets, which in fact tend to be rural regions, too. They conclude that the observed portfolio reallocation weakens the feedback between credit and house prices and slows house price growth in "hot" housing markets, an effect that we cannot confirm for this sample in which the valuation of financed residential property actually increases. Our results of more lending in less liquid markets relate also to loan- and borrower-level data from Brazil employed in De Araujo et al. (2020). Their analyses reveal that constrained borrowers respond to stricter LTV limits by making larger down payments and opting for more affordable homes, rather than by reducing housing market participation entirely.[1] Similar to our findings, DeFusco et al. (2020) document that a cap on household leverage imposed in US mortgage markets had only limited effects on the pricing of loans. In contrast to our findings, they find, however, a substantial contraction of aggregate lending, especially to low-quality borrowers.

Our paper relates more generally to studies assessing the use and effectiveness of MPP in mitigating systemic risks in financial markets, especially in mortgage lending. Some studies, such as Cokayne et al. (2024) conclude enthusiastically that MPP curbed risky lending, mitigated bank capital crunches, and curtailed systemic risks by debilitating the house price-leverage spiral. As a consequence, they argue also that MPP caused milder macroeconomic contractions and enhanced household welfare. Claessens (2015) remarks, however, that MPP may interfere with monetary and microprudential policies, poten-

[1] Ono et al. (2021) also study LTV ratios the loan-level, but observe actual LTV for business loans and lack any changes in MPP for a Japanese sample.

tially adversely affecting resource allocation and incentives for circumvention. Related, Hartmann (2015); Duca et al. (2019); Galati and Moessner (2013, 2018) also acknowledge the potentially important role of MPP in preventing real estate market turmoil. These studies emphasize at the same time that geographically differentiated markets require tailored policies and caution that causal evidence on the effects of MPP remains sparse. Our paper complements this strand of the literature on the effectiveness of macroprudential policies with a more nuanced assessment on causal LTV cap responses that are compliant, yet entail elusive risk-shifting bank behavior.

As such, our empirical evidence speaks to a theoretical literature on MPP. For example Bianchi and Mendoza (2018) suggest that constraints on borrowing against collateral can prevent credit booms and busts, thereby reducing volatility and cyclicality. Benetton (2021) show that a 1% increase in risk-weighted capital requirements increases lenders' marginal costs of borrowing by 11% on average. Using a heterogeneous agent model, Gatt (2024) shows that a greater share of households is affected when wealth is distributed more equally before the LTV tightening, leading to a stronger fall in house prices and a larger rise in the share of constrained homeowners and housing wealth inequality. These predictions are supported by empirical evidence, which shows stricter LTV ratios indeed moderate credit growth (Lim et al., 2011; Akinci and Olmstead-Rumsey, 2018; Cerutti et al., 2017). Research by Mokas and Giuliodori (2023) indicates that even the mere announcement of LTV adjustments influences banking practices, particularly evident in the immediate reduction in household credit following LTV tightening announcements. We complement these important studies that focus more on aggregate credit market outcomes with an analysis of loan-level responses to actually observed LTV shocks in EU banking markets. Our empirical findings add to these studies the insight that banks' efforts to comply with LTV caps at the portfolio level result in part from actively replacing loans in illiquid, rural regions.

As such, we shed light on a potential economic cost beyond those documented by, for example, Richter et al. (2019). They discuss the costs associated with implementing macroprudential policies, including those aimed at controlling real estate exposures. Their work highlights that while potentially necessary, such policies can have significant economic trade-offs, impacting lending behaviors and potentially leading to tighter credit conditions. Duca et al. (2016) show that such shifts in credit supply are a key driver of house prices. We show that another trade-off is that tighter LTV may entail less liquid collateral underlying compliant mortgage credit portfolios of EU banks.

Han et al. (2021) examine a targeted macroprudential rule in Canada that imposes higher down payment requirements for homes priced at or above one million dollars. They find that this policy did not achieve the specific goal of cooling the housing boom but instead heated a narrow segment just below the threshold. The authors attribute the lack of policy effectiveness to local market characteristics as well as the strategic responses of market participants, stressing that MPP hinges on designing rules that account for such factors. We add to this line of research by focusing on the strategic response of lenders in response to MPP tightenings.

The remainder of the paper is structured as follows. In Section 2, we present the institutional and empirical setting alongside our data. Section 3 presents our main findings. Section 4 concludes.

2 Setting and Data

2.1 Macroprudential policy shocks

Empirically, we draw on the harmonized and comprehensive macroprudential policy dataset by Alam et al. (2025), which systematically documents policy measures across 134 countries. Within our focal period (2009–2022), four European countries – the Netherlands, Ireland, Portugal, and Belgium – executed new or more stringent loan-to-value (LTV) caps on residential mortgages. These changes in LTV regulation provide a unique setting to assess whether and how banks respond to tighter portfolio constraints on mortgage lending. We treat each of the eleven policy implementations as a distinct event and restrict our analysis to short time windows of ± 4 months around the respective enactment dates, thereby ensuring that observed changes in mortgage origination can be more cleanly attributed to a specific LTV policy shock. Table 1 details the timing, location, and institutional features of each LTV policy measure considered in our analysis.

2.2 Sample Selection

Our laboratory to analyze whether and how a macroprudential policy tool of stricter LTV regulation is effective are mortgage loans issued in ten European countries between 2009 and 2022. Our sample selection is determined by the two major datasets we use.

First, we employ loan-level data from the European Data Warehouse (EDW), established in 2012, after the subprime crisis, to improve transparency regarding collateral pools of asset-backed securities (ABS). To this end, the EDW is a central data repository where ABS issuers must deposit comprehensive loan-level data. These data comprise various types of ABS, such as residential mortgages, commercial mortgages, and auto loans, and is used increasingly for academic purposes recently.[2] EDW data was initially collected by the ECB from 2013 to 2024, with ESMA taking over as of 2021. We collect

[2]For example, Latino et al. (2024) surveys the evidence on securitized auto loans using EDW data. Hibbeln and Osterkamp (2024) use residential real estate loans to study the effect of risk retention on bank behavior. Bittucci et al. (2021) investigate the securitization of SME loans by 12 Italian banks to test whether government guarantees mitigate or spur risk-taking and risk-shifting.

Table 1: Overiew of LTV events

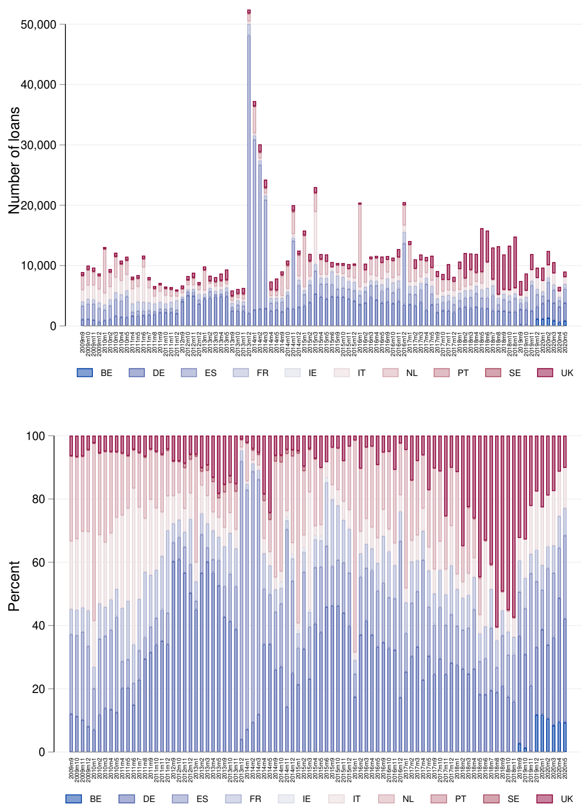
Country	Date of Implementation	Description
Netherlands	M1:2010	Authorities set a maximum loan-to-foreclosure-value (LTFV) ratio of 112%, requiring amounts above 100% to be repaid within seven years.
	M8:2011	The revised Code of Conduct tightened mortgage lending by limiting interest-only mortgages to 50% of a home's market value, down from 100%. Household borrowing was capped at 104% of market value plus transfer tax, reduced from 110%. With the transfer tax lowered to 2%, the new limit effectively stood at 106%.
	M1:2013	The LTV limit for new mortgages decreased by 1 percentage point annually, from 106% in 2012 to 100% in 2018. As of January 1, 2013, the maximum LTV was 105%.
	M1:2014	The LTV limit for new residential mortgages was reduced from 105% to 104%.
	M1:2015	The LTV limit for new residential mortgages was reduced from 104% to 103%.
	M1:2016	The LTV limit for all new residential mortgages was lowered to 102% from 103%.
	M1:2017	The LTV limit for all new residential mortgages was lowered to 101% from 102%.
	M1:2018	The LTV limit for all new residential mortgages was lowered to 100% from 101%.
Ireland	M2:2015	Effective February 9, 2015, the Central Bank of Ireland (CBI) introduced LTV caps and volume limits. LTV caps: First-time buyers (FTBs) could borrow up to 90% on the first €220,000 of a property's value and 80% beyond that. The LTV limit was 80% for second-time buyers and 70% for buy-to-let borrowers. LTV-based volume limits: For primary dwellings, only 15% of new lending could exceed the 90% LTV limit for FTBs and the 80% limit for non-FTBs.
Portugal	M7:2018	The following LTV limits were introduced on a comply-or-explain basis: 90% for loans on primary residences, 80% for other purposes, and 100% for purchasing properties held by credit institutions or for property financial leasing agreements.
Belgium	M1:2020	The National Bank of Belgium sets clear expectations for the internal management of mortgage credit standards for banks and insurance companies in the Belgian residential property market.

Notes: This tables provides an overview of the the LTV event we use in our analysis. We take the information from Alam et al. (2025).

data originally reported to the ECB as well as that filed with the ESMA. The resulting data items and identifiers for both reporting formats are depicted in Table A1. We consider two types of loan information: Static information as of origination (e.g., location or original loan amount) and current information (e.g., performance/default or remaining loan amount). Once a loan is securitized, both types of information must be reported. Hence, original loan data become available even if the loan is securitized years after origination. We exploit this feature because we primarily consider loans at origination, that is, at the point in time and information under which banks (originators) decide to issue a loan. We then use 'current' information for an ex-post analysis of loan performance (e.g., defaults). Although our data set is limited to loans that are securitized, our study is independent of the point in time where the said securitization takes place.

Second, we use data from the European Real Estate Index (EREI) (Koetter et al., 2024), which provides novel, machine-collected information on the offers of houses and apartments for sale in Nuts-3 regions in Europe. The frequency of these data is on the monthly level, which allows us to track offerings tightly from their appearance on a platform until they are taken down.

Figure 1: Across Country Distribution of Loans over Time



Notes: The left subfigure shows the number of new loans per month and year across the countries in our sample. The subfigure on the right displays the share (in percent) of loans across countries and time.

Combining both data sources yields a sample of 1,041,351 mortgage loans from ten European countries. Out of these, the Netherlands, Ireland, Portugal, and Belgium set stricter limits for national LTV regulations between 2009 and 2022. We take the exact dates when each policy came into effect from the database provided by Alam et al. (2025) and provide detailed information in Table 1. The remaining part of the sample consists of loans issued in six countries that did not alter LTV regulation: Germany, Spain, France, Italy, Sweden, and the United Kingdom. The last sample selection step is to limit the data to periods of ± 4 months around each regulatory event. In detail, we take the loans for these periods from the countries that underwent stricter LTV regulation and augment the sample with loans from all other countries that did not change their regulation in that period. We ignore loans from countries that previously changed their LTV regulation in our sample period for later events. For example, we did not consider loans from the Netherlands (first change in 2010:m1) when sampling the loans for the regulatory event of, for example, Portugal, which took place in July of 2018. Figure 1 shows the number of loans originated per month and country (left panel) as well as the share of loans across the ten countries per month (right panel) in our sample.

2.3 Baseline Analysis

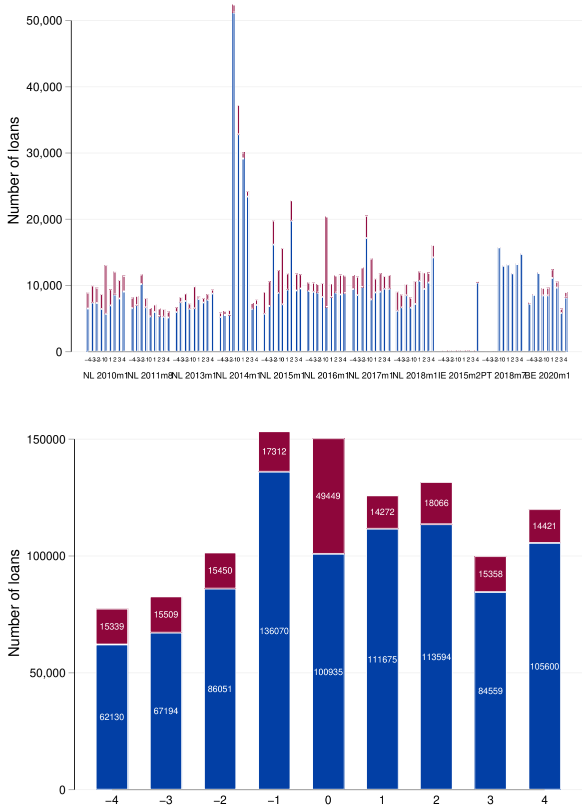
We exploit the policies by running event study analyses and standard 2x2 difference-in-difference regressions. We derive period-specific and average point estimates by running regressions based on

$$Y_{i,s,o,c,r,t} = \sum_{\substack{\tau=-4, \\ \tau \neq -1}}^{\tau=4} \gamma_\tau \text{LTV Regulation}_{c,t} + \vartheta_\tau + \vartheta_t + \vartheta_o + \vartheta_e + \vartheta_r + \vartheta_o + \epsilon_{i,s,o,c,r,t}. \qquad (1)$$

Using Equation (1), we estimate differential effects on mortgage loans' LTVs and other loan characteristics Y for nine periods τ for a symmetric window of ± 4 months (including the event month) around each LTV regulation. The reference period is the month before the policy came into effect. Our sample comprises each loan i only at its origination month t. We further know to which security s the loans belong and which institutions o originated the loan. Importantly, we also know the country c and Nuts-3 region r, where the loan originated. Since our sample is not a panel but comprises many cross-sections of newly originated loans, we are limited in our choice of fixed effects. We chose ϑ_τ, ϑ_t, ϑ_o, ϑ_r, and ϑ_e and thereby adjust for event-month, year-month, originator, Nuts-3, and regulation-event fixed effects. In all our regressions, we cluster standard errors at the originator level to capture potential heterogeneity in the standard errors due to regulatory events.

Figure 2 details the number of loans originating around every regulatory event (top bar chart). In detail, the bars in red show the number of loans from a country that underwent stricter LTV regulation. At the same time, the bars in blue count the number of loans from countries without an LTV change. The bottom chart of Figure 2 provides the cumulative sum of loans from LTV-regulating and non-regulating countries across the nine periods we exploit for each event.

Figure 2: Distribution of Loans around the regulatory Events



Notes: The left subfigure shows the number of loans for the months (±4) around each deregulation event in event time. The subfigure on the right displays the accumulated number of loans in event time across all events.

Our dependent variables Y are loan characteristics at the origination of a loan. In particular, we employ six loan traits. First, we use the LTV ratio of the loan at origination (LTV). Second and third, we employ the natural logarithm of the loan balance (Ln Balance) and the collateral valuation (Ln Valuation) at origination. Fourth, we analyze the contracted-for life span of the loan (Loan Term) measured in months. We employ the interest rate (Interest Rate) set out at origination as the fifth dependent variable. Last, we use information about the purpose of a loan and generate a dummy variable of whether the loan was issued to replace an existing mortgage loan (Re Mortgage). Table 2 provides descriptive statistics for each dependent variable we employ, while Table A2 compares means across loans in regulated and unregulated countries before and after the regulation events and indicates normalized differences following Imbens and Wooldridge (2009) to check the significance of the mean difference.

Table 2: Descriptive Statistics

	Mean	Sd	Percentiles 1%	50%	90%	N
LTV	74.2	29.9	0.7	77.5	146.0	1,041,351
Ln Balance	11.6	0.9	9.2	11.7	13.3	1,041,351
Ln Valuation	12.0	1.0	9.5	12.0	15.1	1,041,351
Loan Term	291.1	124.3	60.0	300.0	582.0	1,041,351
Interest Rate	2.5	11.4	0.0	2.0	5.8	1,041,351
Re Mortgage	0.2	0.4	0.0	0.0	1.0	1,041,351
Stickiness	3.5	1.5	1.7	3.5	7.7	1,041,351
Balance	201.5	1298.6	10.0	115.6	617.0	1,041,351
Valuation	393.9	21096.8	13.2	170.0	3560.0	1,041,351

Notes: This table presents mean values, standard deviations, and the 1st and 99th percentiles for the variables we use in this paper.

2.4 Stickiness of local Real Estate Markets

With our baseline analysis that we lay out in Equation (1), we check typical mortgage loan characteristics around LTV events. In extension, we will further check whether the liquidity of local real estate markets interferes with the banks' decision to allocate loans regionally under stricter LTV regulations. For that, we use the European Real Estate Index (EREI) (Koetter et al., 2024) and calculate for each listing the number of months it spent on the platform. For our analysis, this serves as a proxy for the stickiness of local real estate markets. If listings stay longer on the platforms, this may indicate that banks, in the event they call the collateral, find it harder to sell them. Using the EREI, we use all available months we have for each Nuts-3 region until March 2024 and calculate the average number of months per Nuts-3 region a listing remains on a platform.

Table 3: Stickiness per Country

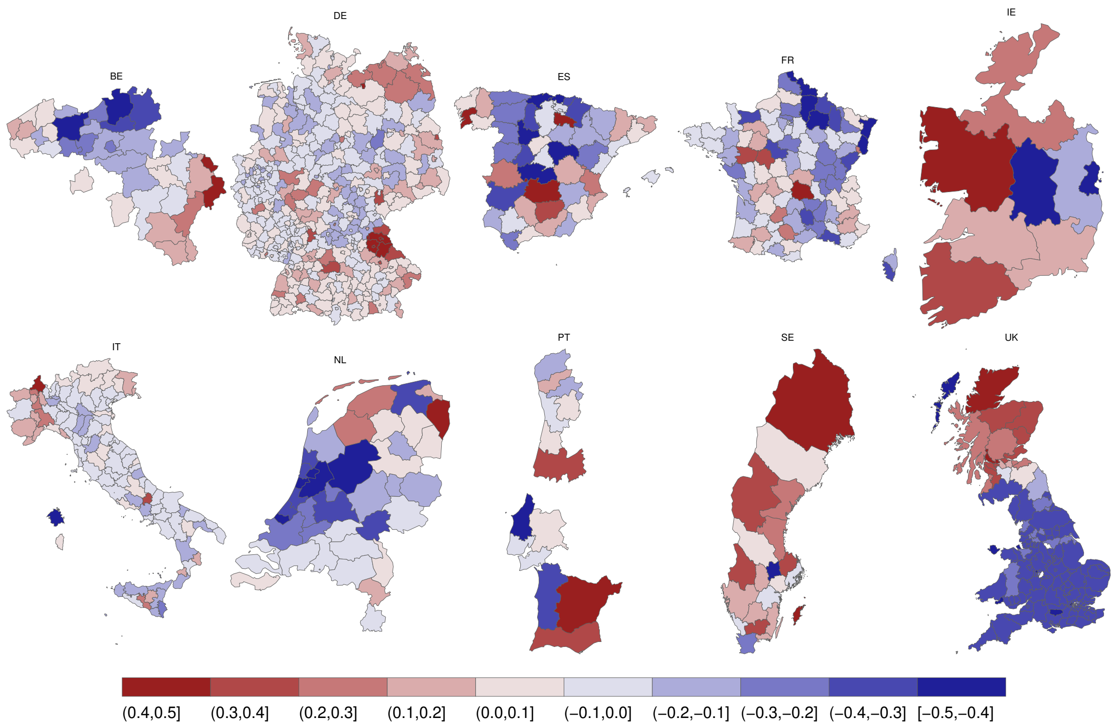
Country	Mean	Sd	Percentiles 1%	50%	90%	Nuts-3 Regions
BE	2.7	0.2	2.3	2.7	3.3	36
DE	2.0	0.2	1.6	1.9	2.7	401
ES	3.7	0.6	2.6	3.6	5.1	52
FR	3.9	1.1	1.5	4.0	7.2	96
IE	4.9	0.4	4.3	5.0	5.3	8
IT	3.6	0.9	1.4	3.4	6.0	101
NL	5.3	0.9	3.8	5.4	8.0	40
PT	3.8	0.8	2.6	3.6	5.1	9
SE	1.8	0.1	1.5	1.8	2.0	21
UK	4.8	3.4	1.0	3.6	16.7	157

Notes: This table presents mean values, standard deviations, and the 1st and 99th percentiles for the variable Stickiness measured in months. The last column lists the number of Nuts3-regions per country.

Figure 3 provides descriptive statistics for Stickiness across the ten countries in our dataset. A comparison of mean stickiness between countries reveals a rather remarkable heterogeneity. Listings in Germany appear to stay on average relatively shortly on the platforms, while listings in Ireland, the United Kingdom, and the Netherlands remain on the platforms for more than 4.8 months. Especially for the United Kingdom, the divide between London and the rest of the country may be critical here, as the standard deviation and the median value of 3.6 seem to suggest.

In later analyses, we use a normalized version of Stickiness. The Normalized Stickiness is the Nuts-3-specific average stickiness level from which we subtract the country-specific mean and divide this value by the difference in the country-specific maximin and minimum for Stickiness. Figure 3 shows the Normalized Stickiness across the countries we use, which allows a deeper look into within-country heterogeneity of the Stickiness of local real estate markets. Counties colored in blue are those with lower Stickiness than the average county, while red indicates above-mean levels of Stickiness.

Figure 3: Within Country Variation in Stickiness



Notes: Each map displays the normalized stickiness per Nuts-3 region across countries. For each country, we normalized Stickiness by subtracting the country-specific mean and dividing by the distance between the maximum and minimum value per country. Thereby, a value of, for example, 0.1 indicates a 10% higher Stickiness for Nuts-3 regions compared to the average Nuts-3 regions in the country.

3 Results

This section discusses the empirical results by running regressions based on Equations (1).

3.1 Compliance with regulatory LTV changes

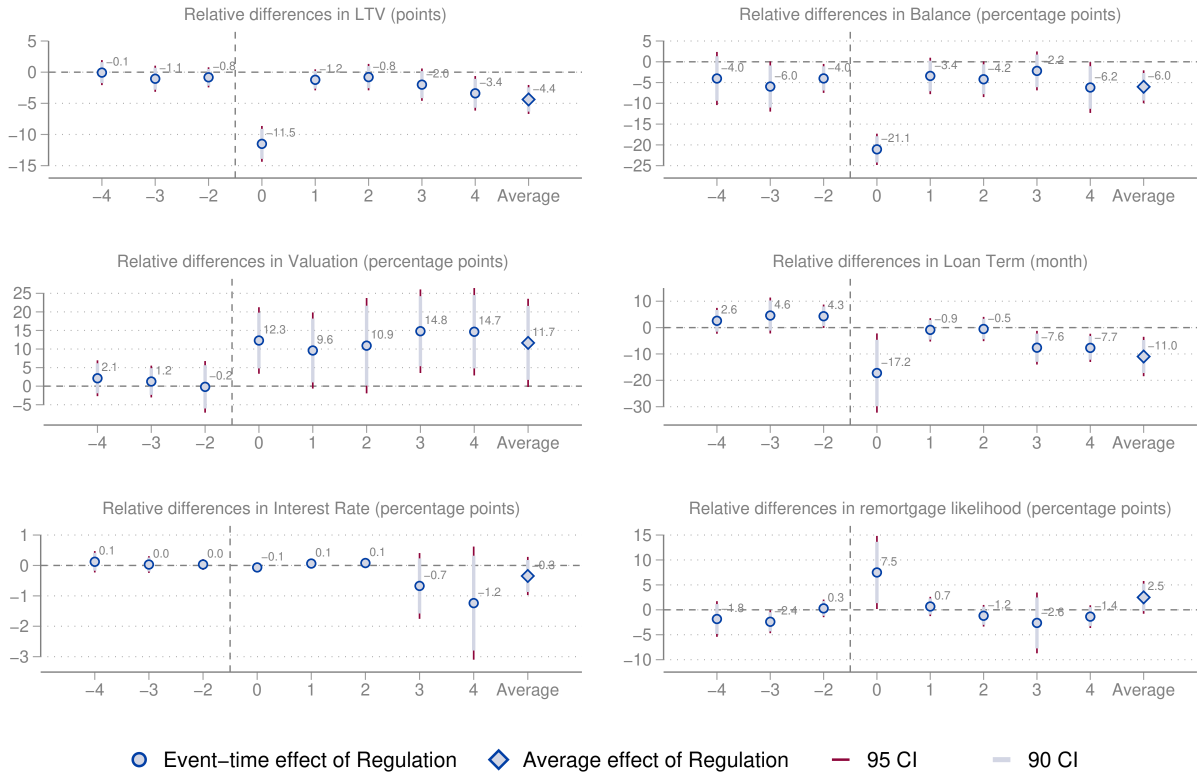
Figure 4 presents our empirical results investigating the developments of mortgage loans after LTV regulation by running regressions based on Equation (1). In the top left, we focus on the LTV ratios of newly originated loans. Since we focus on regulation with the purpose of stricken LTVs, our results are intuitive. We find that loans issued in the month when a stricter LTV regulation comes into force ($\tau = 0$) have a significant (5% level) lower LTV ratio. The relative reduction compared to the last pre-event month concerning loans not subject to stricter LTV regulation is 11.5 points. Given a mean LTV of around 74 in our sample, this effect is sizeable and amounts to more than half of one standard deviation of LTV in our sample. By checking the other post-event coefficients, we find that the significant reduction of LTV is primarily limited to the immediate event period, only showing up in the fourth post-event period but at a much smaller extent (-3.4 points). However, the average effect that we provide on the very right of the graph shows that when we compare LTVs from all four pre-periods with the five periods from the time the stricter regulation was in place, LTVs for loans in LTV-regulated countries went down relative by a significant 4.4 points.

Our results for LTV ratios are intuitive since they show that stricter LTV regulation lowers LTVs for newly issued loans. The more critical question is how banks comply with the more stringent rules. For that, we check the two components of the LTV ratio separately. We start by investigating the size of the new mortgage loans in the top right of Figure 4. We find that immediately after the stricter regulation was enacted, the loan balance significantly decreased by around 21 percentage points. Again, our results indicate that the significance of the impact is limited to the first period. However, the average effect is significant and indicates a relative reduction in the size of loans of about 6.0 percentage points.

When we turn to the value of the collateral on the left of the middle panel, we find that right after the regulation comes into effect, the point estimates increase immensely and turn significant for all post-periods. The effect is again sizeable, indicating an increase in valuation of about 12 percentage points post-regulation. Combined with the results of the reduction in loan size, banks comply with the stricter LTV regulation by originating smaller loans and adjusting the valuation of the underlying real estate.

Two other potential dimensions in which banks can adjust the riskiness of mortgage loans are the time the loan must be repaid and the price of the loan. We investigate loan terms on the right of the middle panel of Figure 4. There, we find that loan terms are

Figure 4: Baseline Effects



Notes: Each subfigure shows event-month specific and average effects from Equation (1) for six outcomes variables: LTV (top left), Ln Balance (top right), Ln Valuation (middle left), Loan Term (middle right), Interest Rate (bottom left), and Re Mortgage (bottom right). The lines around each point estimate indicate the 95 and 90% confidence intervals. Each of the ±4 event-month effects (blue points) is evaluated against the month just before the event (-1). The average effects (blue triangles) compare the differential effect from all four pre- to all five post-event periods. We present the underlying raw coefficients and regression diagnostics in Table A3.

consistently lower for mortgage loans issued in countries with stricter LTV regulations after these regulations came into effect. Again, especially the immediate period after the regulation stands out. The loans issued there are significantly shorter by about 17 months. The average comparison reveals a significant relative reduction of eleven months.

Our results suggest that the mortgage loans issued after the stricter LTV regulation should turn out less expensive for borrowers since they come with lower risk for the banks. However, when we turn to the graph at the bottom left, we find no significant reduction in interest rates for mortgage loans past stricter LTV regulation.

Last, our results in the bottom right of Figure 4 reveal a significant increase in mortgage loans being refinanced when the LTV regulation kicked in. In numbers, we find that the loans issued in the month after an LTV regulation are 7.5 percentage points more likely to replace mortgage loans for the same collateral than mortgage loans in countries without an LTV restriction. This immediate effect also turns the average effect significant at the 90% level. Comparing mortgage loans before and after LTV regulation, the mortgage loans after the regulation are 2.5 percentage points more likely to be remortgaged.

3.2 County-level reallocation of loans

Baseline Effects In this section, we focus on how banks' responses regarding mortgage lending after stricter LTV regulation affect loan allocation on the Nuts-3 level. We, therefore, employ the following regression.

$$Y_{c,r,t} = \sum_{\substack{\tau=-4,\\ \tau\neq-1}}^{\tau=4} \gamma_\tau \text{LTV Regulation}_{c,t} + \vartheta_\tau + \vartheta_t + \vartheta_c + \vartheta_e + \epsilon_{c,r,t}. \quad (2)$$

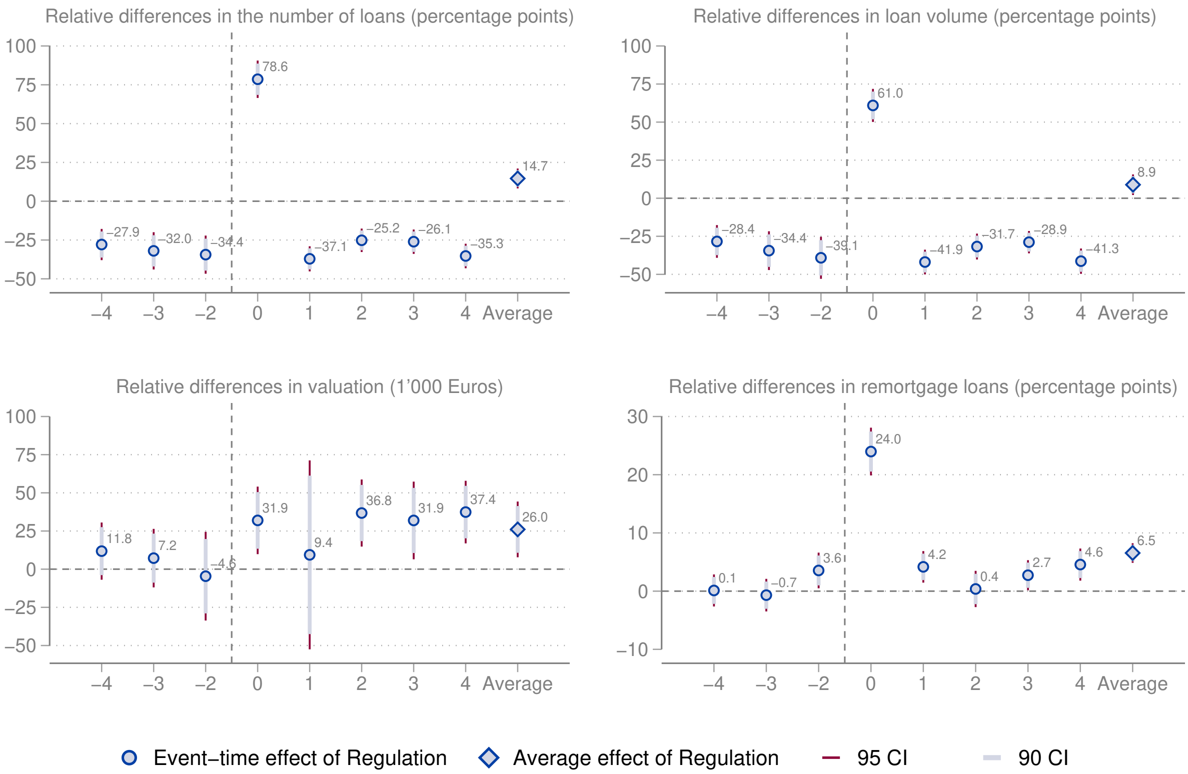
Using Equation (2), we estimate differential effects on Y on the Nuts-3 level for nine periods τ for a symmetric window of ±4 months (including the event month) around each LTV regulation. Again, the reference period is the month before the policy occurred. Y stands for four variables: the natural logarithm of the total number of loans per Nuts-3 region; the natural logarithm of the total loan volume per Nuts-3 region; the ratio of the total amount of mortgage valuation and the total number of loans; the total number of remortgaged loans. We employ ϑ_τ, ϑ_t, ϑ_r, and ϑ_e as fixed effects and thereby adjust for event-month, year-month, Nuts-3, and regulation-event effects. We cluster standard errors on the Nuts-3 level to capture potential heterogeneity in the standard errors due to the regulatory events.

Figure 5 presents estimate coefficients and standard errors from regression of Equation (2). In the top right panel, we show point estimates using aggregate loans as the dependent variables. We find that aggregated loan volume increases significantly in the month when the regulation comes into effect. The effect amounts to 78 percentage points in Nuts-3 regions in regulated countries relative to the control Nuts-3 regions without a regulatory event relative to the last pre-event month. This immediate effect also drives the average impact to a significant 14.7 percentage points relative increase.

The top left graph shows the total loan volume. Again, we find a significant relative increase of 61 percentage points, which lifts the average effect to an 8.9 percentage point differential effect.

The bottom right graph shows regression results for the average evaluation per Nuts-3 region. We find that on impact, evaluation per real estate in counties experiencing stricter LTV regulation relatively increases by around 31,000 Euros. This effect stays positive and remains significant for almost all post-event periods. The average effect is about 26,000 Euros and is significant.

Figure 5: County-level Estimates



Notes: Each subfigure shows event-month specific and average effects from Equation (2) for four outcomes variables: The natural logarithm of the total number of loans per Nuts-3 region (top left), the natural logarithm of the total loan volume per Nuts-3 region (top right), the natural logarithm of the average valuation per Nuts-3 region (bottom left), and the natural logarithm of the total number of remortgaged loans per Nuts-3 region (bottom right). The lines around each point estimate indicate the 95 and 90% confidence intervals. Each of the ± 4 event-month effects (blue points) is evaluated against the month just before the event (-1). The average effects (blue triangles) compare the differential effect from all four pre- to all five post-event periods. We present the underlying raw coefficients and regression diagnostics in Table A4.

The bottom right graph shows the aggregated number of loans that are remortgaged. We find that the number of remortgaged loans in counties under stricter LTV regulation relatively increases by 24 percentage points on impact, leading to a significant average increase of about 6.5 percentage points.

Moderating effects of mortgage market liquidity In this section, we investigate the role of Nuts-3-level mortgage market liquidity on the allocation of mortgage loans after stricter LTV regulation. We thereby augment Equation 5 by a triple difference-in-difference effect of LTV Regulation and Normalized Stickiness, resulting in the following regression:

$$Y_{c,r,t} = \sum_{\substack{\tau=-4,\\ \tau\neq-1}}^{\tau=4} \gamma_\tau \text{LTV Regulation}_{c,t} + \sum_{\substack{\tau=-4,\\ \tau\neq-1}}^{\tau=4} \lambda_\tau \left(\text{LTV Regulation}_{c,t} \times \text{Normalized Stickiness}_r\right)$$
$$+ \vartheta_\tau + \vartheta_t + \vartheta_c + \vartheta_e + \epsilon_{c,r,t} \quad (3)$$

Figure 6 presents point estimates for γ_τ and λ_τ. Given the calculation of Normalized Stickiness, the γ_τs are the differential effects from the LTV regulation for the average county in terms of Stickiness. We modified the point estimates in Figure 6 such that the λ_τs present the modifying effects of Stickiness for a county that has a level of Stickiness that is one standard deviation (0.2) higher.

The top left graph of Figure 6 shows that the significant increase in the number of loans at the month of the LTV regulation is further boosted for counties with stickier mortgage markets. A Nuts-3 region with a value of normalized Stickiness of about 0.2 on top of the mean of Stickiness experiences an additional significant loan growth of 12 percentage points. The moderating effect of Stickiness also lifts the average effect by 5.1 percentage points, which is an additional 33% increase on top of the average effect at the mean Nuts-3 region.

The effects on aggregate loan volume in the top right graph are similar. Again, the relative increase in loan volume is significantly boosted in stickier Nuts-3 regions by an additional 9.8 percentage points.

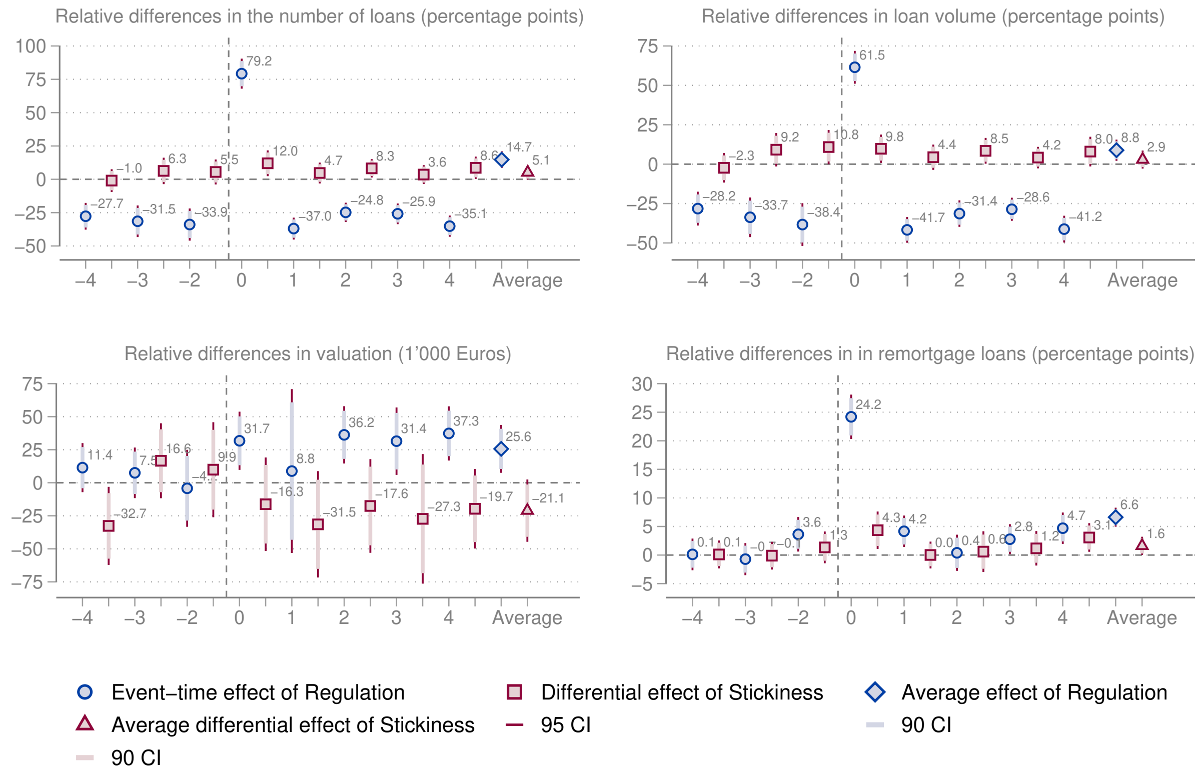
Regarding valuation, the point estimates in the bottom left graph of Figure 6 indicate the absence of significant moderating effects of Stickiness on real estate valuation. Point estimates in all post-event periods are negative, indicating that real estate in stickier Nuts-3 regions increases by only half concerning the mean Nuts-3 regions. Inspecting average effects, we find that the positive and significant differential effect for the average Nuts-3 region of 25,600 Euros is almost null in stickier Nuts-3 regions (-21,700 Euros).

Last, the bottom right graph shows that the significant differential increase of remortgaged loans in average Nuts-3 regions is toppled by an additional 4.3 percentage points in stickier Nuts-3 regions in the month when stricter LTV regulation comes into effect.

Cross Nuts-3 allocation: The Showcase of the Netherlands So far, our results indicate that banks respond to stricter LTV regulation by issuing smaller loans but evaluate the newly issued mortgage loans significantly higher. Furthermore, aggregated effects suggest that those two levers drive loans differently across Nuts-3 regions. To investigate this mechanism more intensively, we in detail investigate Nuts-3 regions in the Netherlands and present our results in Figure 7.

Figure 7 separates Nuts-3 regions in the Netherlands into 2x2 matrices along two dimensions. On the y-axis, we differentiate Nuts-3 regions into below and above-zero values

Figure 6: County-level Estimates

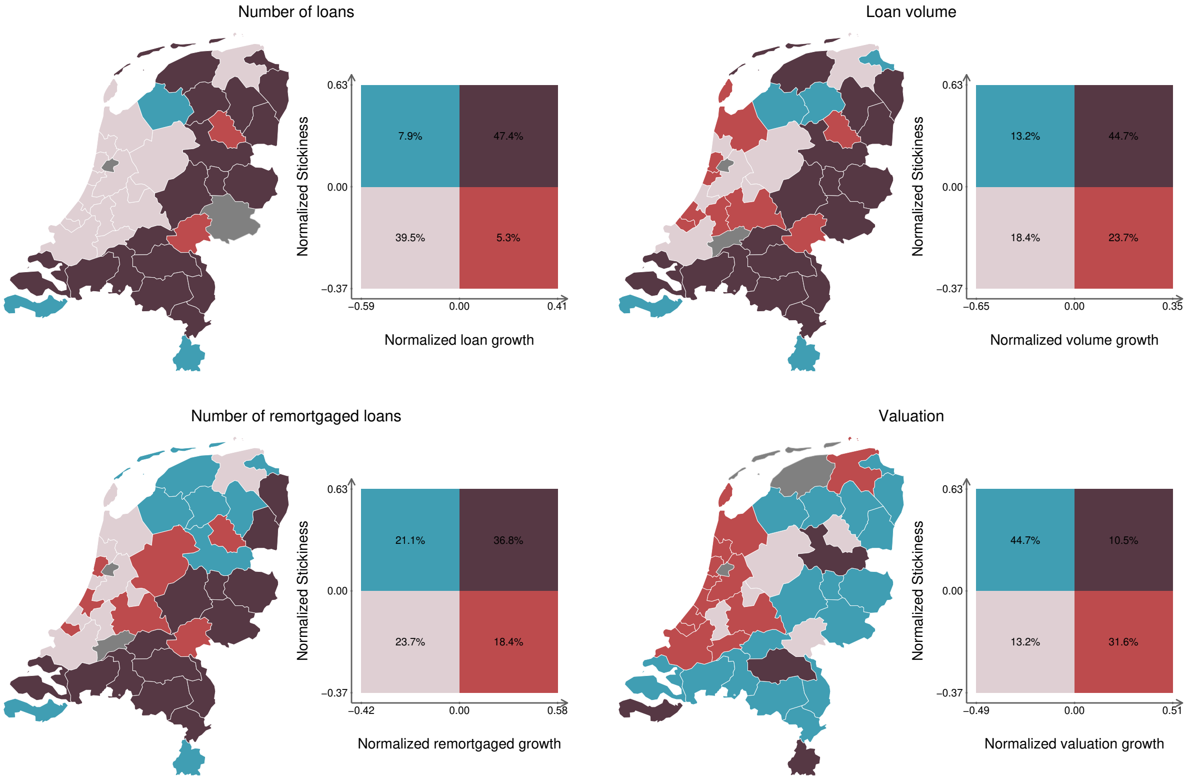


Notes: Each subfigure shows event-month specific and average effects from Equation (3) for four outcomes variables: The natural logarithm of the total number of loans per Nuts-3 region (top left), the natural logarithm of the total loan volume per Nuts-3 region (top right), the natural logarithm of the average valuation per Nuts-3 region (bottom left), and the natural logarithm of the total number of remortgaged loans per Nuts-3 region (bottom right). The lines around each point estimate indicate the 95 and 90% confidence intervals. We provide estimates for γ_τ as blue points, which is the differential effect of the regulatory event for a loan handed out in a Nuts-3 region at the mean of the Nuts-3-specific normalized stickiness. Red squares indicate the triple differential effect for loans in Nuts-3 regions at a value for the normalized stickiness that is 0.2 (one standard deviation) higher. We indicate the average effects as blue diamonds and red triangles, respectively. We present the underlying raw coefficients and regression diagnostics in Table A5.

for normalized stickiness. Thereby, the two top fields in each matrix report numbers for Nuts-3 regions with above-average sticky real estate markets. On the x-axis, we normalized growth rates for four measures: total loans, total loan volume, total remortgaged loans, and valuation. We measure the growth of each of these measures on the Nuts-3 level, comparing the change of the aggregate of all four pre-event months to all five post-event months. We then normalized each growth rate by subtracting the country-specific mean and dividing by the difference between the maximum and minimum values across all Nuts-3 regions. For example, the two fields on the left of each matrix indicate Nuts-3 regions with below-average growth rates.

The top left map of Figure 7 shows the separation of Nuts-3 regions in the Netherlands using the growth rate of the total number of loans. We find that around 40% of the Nuts-

Figure 7: Loan allocation across counties in the Netherlands



Notes: Each subfigure shows the percentage of Nuts-3 regions in the Netherlands with respect to two dimensions. The first dimension (y-axis) indicates whether a Nuts-3 has a value for the normalized stickiness below or above zero, indicating whether the stickiness of the particular mortgaged markets is above or below the average. The second dimension (x-axis) relates to four characteristics and their growth rate comparing mortgaged loans from all post-regulation relative to all pre-regulation periods. We normalized the growth rate by subtracting the country-specific mean and dividing it by the distance between the minimum and maximum values. The top left map shows the growth rate in the number of loans. On the top right, we show the growth rate of the total loan volume. The bottom left map shows the number of remortgaged loans, while the bottom right map shows the growth rate of total valuation divided by the total number of loans per Nuts-3 region.

3 regions are in the lower-right of the matrix, indicating that those Nuts-3 regions have below-average sticky markets and below-average growth rates for the number of loans after stricter LTV regulation. At the same time, 47% of NUTS-3 regions have above-average sticky markets and above-average growth rates. Our findings suggest that almost all loan growth occurs in above-average sticky markets (48 v 5%).

The picture for loan volume growth in the top right map is less pronounced. If we compare Nuts-3 regions with above-average loan volume growth, we still find that most of the loan volume went to stickier regions (45 v 24%).

The bottom left map shows that remortgage loan allocation is equal across NUTS-3 regions.

The bottom right map investigates the growth in the valuation of real estate. Opposed to the two maps on the top, comparing Nuts-3 regions experiencing above-average growth rates, we find most of the valuation growth is happening in below-average sticky markets. At the same time, comparing Nuts-3 regions with below-average valuation growth, we see the majority of observations for stickier Nuts-3 regions.

4 Conclusion

Our analysis provides insights into how banks respond to stricter LTV regulations and highlights the importance of real estate market liquidity in shaping these responses. By exploiting a unique combination of loan-level data from the European Data Warehouse and local real estate liquidity data from the European Real Estate Index (EREI), we document that banks comply with more restrictive LTV limits primarily by reducing loan sizes and by inflating collateral valuations. On balance, mortgage loans that arise in the wake of tighter regulations feature smaller principal amounts, shorter maturities, higher valuations, and – contrary to expectations – interest rates that do not significantly decline.

A key finding is that, despite a tighter cap on leverage, overall lending volumes do not necessarily contract. Instead, we observe a marked upsurge in mortgage refinancing activity shortly after LTV rules become stricter. Banks appear to respond to new regulation by actively encouraging or facilitating remortgaging, which effectively reallocates credit. Consequently, total mortgage lending even increases in the immediate aftermath of regulatory tightenings. Furthermore, county-level analyses show that this expansion is particularly pronounced for mortgages secured by residential properties in less liquid markets, as identified through the European Real Estate Index (EREI). This suggests that while the new LTV measures foster compliance *de jure*, banks respond *de facto* by shifting newly issued mortgage credit into collateral of potentially lower liquidity and quality, which may complicate foreclosure in the event of default.

These findings carry important implications for policymakers. Although stricter LTV caps do indeed reduce leverage on newly originated loans, their broader objective of containing credit growth – and thereby moderating house price inflation – may be less fully realized than intended. In part, this is due to banks' strategic behavior concerning collateral valuations and remortgaging activity. In particular, the surge in remortgaging highlights a more nuanced regulatory transmission mechanism: while average risk-taking diminishes, overall credit conditions may remain looser than policymakers anticipate.

In summary, LTV caps appear to accomplish the narrow objective of lowering credit risk on the margin, but the aggregate outcome in terms of total credit volume is shaped by significant shifts in refinancing behavior. Further research could delve deeper into the drivers behind such shifts, including a segregation of supply and demand side effects, and assess whether complementary regulatory measures – perhaps targeting collateral quality

– might be needed to better align macroprudential goals with observed lending patterns.

This behavior raises questions about policy effectiveness in moderating credit growth.

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Appendix

Table A1: Data Description

Name	ECB	ESMA	Description
account__status	AR166	RREL69	Current status of the underlying exposure (e.g. performing, defaulted, arrears)
allocated__losses	AR180	RREL73	Allocated losses to date, net of fees, accrued interest etc.
arrears__balance	AR169	RREL67	Current balance of arrears
collateral__nuts3	AR128	RREC6	Geographic region (NUTS3 classification)
collateral__postcode	AR129		Zipcode
collateral__type	AR131	RREC9	Property type (e.g. house, flat, bungalow)
current__balance	AR67	RREL30	Amount of underlying exposure outstanding
current__interest__rate	AR109	RREL43	Gross rate per annum
current__interest__rate__index	AR108	RREL44	Reference rate (e.g. LIBOR, Euribor)
current__interest__rate__margin	AR110	RREL46	Margin of the floating-rate underlying exposure over/under the reference rate
current__ltv	AR141	RREC12	Most recent loan to value ratio (LTV)
current__ltv__date	AR145	RREC15	The date of the most recent valuation
current__valuation__amount	AR143	RREC13	Most recent valuation of the collateral
days__arrears	AR170	RREL68	Number of days in arrears
default__amount	AR177	RREL71	Gross default amount before the application of sale proceeds and recoveries
default__date	AR178	RREL72	The date of default
edcode	EDCODE	ED__Code	Unique identifier for public ABS transactions
foreclosure__date	AR151	RREC20	The date of sale of the foreclosed collateral
foreclosure__price	AR179	RREC21	Price achieved on sale of foreclosed collateral
interest__rate__reset__interval	AR111	RREL47	Number of months between interest rate resets
interest__rate__type	AR107	RREL42	Interest rate type (e.g. fixed, floating)
last__arrears__date	AR168	RREL66	Date underlying exposure was last in arrears
lien	AR84	RREC8	Highest lien position held by the originator in relation to the collateral
loan__addition__date	AR57	RREL7	The date that the underlying exposure was transferred to the SSPE
loan__maturity__date	AR56	RREL24	The date of maturity of the underlying
loan__origination__date	AR55	RREL23	Date of original underlying exposure advance
loan__term	AR61	RREL25	Original term (months) at origination date
loanid	AR3	RREL2	Unique underlying exposure identifier
occupancy__type	AR130	RREC7	Type of occupancy (e.g. owner occupied)
originator	AR5	RREL82	Name of the underlying exposure originator
originator__channel	AR58	RREL26	Origination channel (e.g. branch, broker)
original__balance	AR66	RREL29	Original underlying exposure balance (inclusive of fees)
original__ltv	AR135	RREC16	Original underwritten loan To Value ratio
original__ltv__date	AR138	RREC19	Date of original valuation of the collateral
original__valuation	AR136	RREC17	Original valuation of the collateral
original__valuation__method	AR137	RREC18	Valuation method (e.g. full-internal/external, drive-by, automated)
pool__cutoff__date	AR1	RREL6	Data cut-off date for this data submission
property__type	AR131	RREC9	Property type (e.g. House, Flat, Bungalow)
purpose	AR59	RREL27	Reason for loan (e.g. purchase, remortgage, renovation, construction)

Notes: Items from the European Data Warehouse (EDW) are listed and described here. EDW data was originally collected in the ECB reporting format (2013 until 2024), which has since been replaced by the ESMA reporting format (2021 until present). Our dataset draws from both reporting formats, with the corresponding identifiers laid out above.

Table A2: Mean differences

	Pre-LTV Regulation Mean		ND	Post-LTV Regulation Mean		ND
	Regulated	Control		Regulated	Control	
LTV	81.6	73.8	0.20	73.6	73.7	-0.00
Ln Balance	11.3	11.7	-0.31	11.1	11.6	-0.39
Ln Valuation	11.6	12.2	-0.36	11.6	12.1	-0.33
Loan Term	359.9	279.7	0.43	350.1	277.5	0.39
Interest Rate	3.1	2.0	0.32	3.0	2.6	0.02
Re Mortgage	0.2	0.2	-0.00	0.4	0.2	0.28
Normalized Stickiness	-0.1	-0.0	-0.22	-0.0	-0.0	-0.15
Balance	111.5	228.0	-0.08	100.9	216.0	-0.08
Valuation	161.8	683.2	-0.01	163.1	274.9	-0.05

Notes: This table presents the mean value for the variables we use for four groups of loans. On the left, we separate loans in the pre-regulation periods between loan issues in countries that eventually got regulated and loans from countries without a regulation event. We do the same in the right for the post-event period. ND indicates the normalized differences according to Imbens and Wooldridge (2009). A value between ± 0.25 indicates no significant differences.

Table A3: Loan-level Baseline Results

	LTV	Balance	Valuation	Loan Term	Interest Rate	Re Mortgage
	(1)	(2)	(3)	(4)	(5)	(6)
Baseline(-3)	-0.0705	-0.0403	0.0213	2.5702	0.1206	-0.0184
	(1.0214)	(0.0325)	(0.0245)	(2.4922)	(0.1786)	(0.0182)
Baseline(-2)	-1.0544	-0.0596*	0.0125	4.5788	0.0301	-0.0240**
	(1.0664)	(0.0307)	(0.0218)	(3.4605)	(0.1396)	(0.0116)
Baseline(-1)	-0.8289	-0.0401**	-0.0019	4.3032*	0.0321	0.0029
	(0.8190)	(0.0177)	(0.0353)	(2.2497)	(0.0915)	(0.0090)
Baseline(1)	-11.5021***	-0.2108***	0.1230***	-17.2306**	-0.0651	0.0749**
	(1.4600)	(0.0192)	(0.0455)	(7.6349)	(0.0656)	(0.0373)
Baseline(2)	-1.2276	-0.0341	0.0960*	-0.8588	0.0617	0.0067
	(0.8576)	(0.0223)	(0.0521)	(2.2549)	(0.0773)	(0.0098)
Baseline(3)	-0.7939	-0.0420*	0.1090*	-0.5370	0.0791	-0.0119
	(1.0773)	(0.0218)	(0.0653)	(2.3408)	(0.0846)	(0.0110)
Baseline(4)	-2.0014	-0.0220	0.1480**	-7.6240**	-0.6742	-0.0263
	(1.3174)	(0.0238)	(0.0572)	(3.2450)	(0.5496)	(0.0310)
Baseline(5)	-3.3925**	-0.0619**	0.1466**	-7.6876***	-1.2389	-0.0137
	(1.4093)	(0.0311)	(0.0599)	(2.7224)	(0.9452)	(0.0116)
No. of Loans	1,042,984	1,042,984	1,042,984	1,042,984	1,042,984	1,042,984
No. of Originators	323	323	323	323	323	323
Adj. within R2 (%)	0.287	0.088	0.046	0.080	0.016	0.135
	(1)	(2)	(3)	(4)	(5)	(6)
Baseline Average	-4.3798***	-0.0602***	0.1165*	-10.9656***	-0.3470	0.0250
	(1.1828)	(0.0202)	(0.0604)	(3.7959)	(0.3189)	(0.0167)
No. of Loans	1,042,984	1,042,984	1,042,984	1,042,984	1,042,984	1,042,984
No. of Originators	323	323	323	323	323	323
Adj. within R2 (%)	0.075	0.015	0.044	0.040	0.003	0.016

Notes: This table presents raw coefficients, standard errors, and regression diagnostics for regression using Equation (1). The top panel shows event-month differential effects for three pre-months and five post-months around the LTV regulation events. The bottom panel indicates the average differential effect comparing the four pre-event periods to the five post-event periods.

Table A4: County-level Baseline Results

	No. of loans	**Loan Volume**	**Valuation**	**Remortgaged**
	(1)	(2)	(3)	(4)
Baseline(-3)	-0.2791***	-0.2839***	11.8211	0.0012
	(0.0512)	(0.0547)	(9.5498)	(0.0140)
Baseline(-2)	-0.3198***	-0.3443***	7.2314	-0.0069
	(0.0612)	(0.0649)	(9.7664)	(0.0142)
Baseline(-1)	-0.3442***	-0.3913***	-4.5990	0.0355**
	(0.0626)	(0.0704)	(14.8041)	(0.0156)
Baseline(1)	0.7858***	0.6096***	31.9490***	0.2398***
	(0.0614)	(0.0554)	(11.2708)	(0.0209)
Baseline(2)	-0.3713***	-0.4192***	9.3697	0.0417***
	(0.0412)	(0.0414)	(31.5325)	(0.0138)
Baseline(3)	-0.2518***	-0.3175***	36.7728***	0.0037
	(0.0385)	(0.0433)	(11.1933)	(0.0159)
Baseline(4)	-0.2608***	-0.2886***	31.9067**	0.0273**
	(0.0400)	(0.0374)	(12.9677)	(0.0132)
Baseline(5)	-0.3529***	-0.4134***	37.3893***	0.0456***
	(0.0402)	(0.0424)	(10.4466)	(0.0141)
No. of Observations	58,858	58,858	58,858	58,858
No. of Counties	943	943	943	943
Adj. within R2 (%)	0.962	0.517	-0.012	0.395
	(1)	(2)	(3)	(4)
Baseline Average	0.1466***	0.0887**	26.0451***	0.0655***
	(0.0326)	(0.0345)	(9.2981)	(0.0086)
No. of Observations	58,858	58,858	58,858	58,858
No. of Counties	943	943	943	943
Adj. within R2 (%)	0.039	0.008	-0.001	0.078

Notes: This table presents raw coefficients, standard errors, and regression diagnostics for regression using Equation (2). The top panel shows event-month differential effects for three pre-months and five post-months around the LTV regulation events. The bottom panel indicates the average differential effect comparing the four pre-event periods to the five post-event periods.

Table A5: County-level Triple Results

	No. of loans	Loan Volume	Valuation	Remortgaged
	(1)	(2)	(3)	(4)
Baseline(-3)	-0.2772***	-0.2822***	11.4200	0.0012
	(0.0511)	(0.0547)	(9.4578)	(0.0141)
Baseline(-2)	-0.3147***	-0.3372***	7.4997	-0.0071
	(0.0604)	(0.0641)	(9.7621)	(0.0143)
Baseline(-1)	-0.3392***	-0.3835***	-4.2181	0.0363**
	(0.0615)	(0.0692)	(14.8456)	(0.0154)
Baseline(1)	0.7924***	0.6148***	31.7369***	0.2420***
	(0.0574)	(0.0531)	(11.2264)	(0.0198)
Baseline(2)	-0.3696***	-0.4173***	8.8248	0.0416***
	(0.0418)	(0.0416)	(31.5961)	(0.0140)
Baseline(3)	-0.2479***	-0.3138***	36.2489***	0.0040
	(0.0369)	(0.0430)	(11.0459)	(0.0161)
Baseline(4)	-0.2588***	-0.2863***	31.4488**	0.0277**
	(0.0395)	(0.0373)	(13.0080)	(0.0134)
Baseline(5)	-0.3514***	-0.4123***	37.3223***	0.0470***
	(0.0407)	(0.0433)	(10.4479)	(0.0140)
Triple(-3)	-0.0491	-0.1159	-163.3851**	0.0057
	(0.2177)	(0.2387)	(75.2908)	(0.0623)
Triple(-2)	0.3147	0.4576*	83.2186	-0.0054
	(0.2524)	(0.2703)	(72.2764)	(0.0617)
Triple(-1)	0.2753	0.5422*	49.2727	0.0673
	(0.2388)	(0.2769)	(91.6190)	(0.0709)
Triple(1)	0.6018**	0.4887**	-81.4046	0.2172***
	(0.2465)	(0.2289)	(90.2368)	(0.0837)
Triple(2)	0.2360	0.2200	-157.3339	0.0006
	(0.1996)	(0.2043)	(102.4694)	(0.0598)
Triple(3)	0.4126**	0.4230**	-87.9910	0.0298
	(0.1761)	(0.2087)	(90.3748)	(0.0910)
Triple(4)	0.1779	0.2078	-136.6873	0.0590
	(0.1807)	(0.1737)	(124.8850)	(0.0768)
Triple(5)	0.4305**	0.3986*	-98.5654	0.1536**
	(0.2142)	(0.2400)	(76.5654)	(0.0639)
No. of Observations	58,858	58,858	58,858	58,858
No. of Counties	943	943	943	943
Adj. within R2 (%)	1.024	0.550	-0.020	0.401
	(1)	(2)	(3)	(4)
Baseline Average	0.1475***	0.0884**	25.6264***	0.0663***
	(0.0322)	(0.0343)	(9.2359)	(0.0085)
Triple Average	0.2554*	0.1455	-105.5593*	0.0806**
	(0.1393)	(0.1458)	(60.1182)	(0.0405)
No. of Observations	58,858	58,858	58,858	58,858
No. of Counties	943	943	943	943
Adj. within R2 (%)	0.081	0.031	0.002	0.082

Notes: This table presents raw coefficients, standard errors, and regression diagnostics for regression using Equation (3). The top panel shows event-month differential effects for three pre-months and five post-months around the LTV regulation events. The bottom panel indicates the average differential effect comparing the four pre-event periods to the five post-event periods.

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