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**Working Paper**

## Contractionary macroprudential policy, collateral valuation, and risk-shifting in EU banking

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## Discussion Papers

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# Contractionary Macroprudential Policy, Collateral Valuation, and Risk-shifting in EU Banking

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# Contractionary Macroprudential Policy, Collateral Valuation, and Risk-shifting in EU Banking\*

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## Abstract

We study real estate lending responses to tighter macroprudential policy (MPP) in the form of lower required loan-to-value (LTV) ratios. Contract details of 2.4 million mortgage loans originated between 2008 and 2020 reveal significantly fewer new loan issuances in response to contractionary MPP, commensurate with an average reduction in aggregate lending of 21 percent. Loan-level analyses reveal, however, that banks comply with lower LTVs by systematically more benevolent valuations of residential real estate pledged as collateral instead of reducing loan size. Exploiting earthquakes as plausible exogenous shocks to property values corroborates these risk-shifting patterns by banks in the form of inflated property valuations after LTV shocks.

*Keywords: collateral valuation, loan-to-value caps, macroprudential policy, risk shifting*

*JEL classification: G01, G21, G28, R31*

\* We thank seminar participants at IWH.

# 1 Introduction

Excessive growth in mortgage credit preceded many price bubbles and imbalances in real estate markets (Case and Shiller, 2003; Reinhart and Rogoff, 2008; Favara and Imbs, 2015; Mian and Sufi, 2018; Laeven and Valencia, 2020). Sudden corrections paired with highly indebted households and poor governance in financial systems also triggered the Great Financial Crisis of 2007/2008 (Mian and Sufi, 2009, 2010, 2011), which distorted housing choices, wealth distributions, and welfare even in the long run (Duca et al., 2016; Cloyne et al., 2019; Hacamo, 2024). Against this backdrop, macroprudential policy (MPP) tools like loan-to-value (LTV) caps were developed to limit the amount that households can borrow against pledged residential real estate as collateral (Claessens, 2015). LTV caps should induce banks to reduce lending volumes, either by granting smaller loan amounts or by rationing credit to selected households altogether (Acharya et al., 2022; Peydró et al., 2024).

Whereas a growing literature indicates that LTV regulation can contain aggregate credit growth and housing price dynamics (see van den Bussche et al., 2015; Cerutti et al., 2017; Grodecka, 2020; Duca et al., 2021; Cokayne et al., 2024), we isolate a new channel *how* banks adjust their behavior at granular levels: inflated collateral valuation. We challenge the notion that banks reduce lending after contractionary MPP shocks to comply with LTV caps as intended. Instead, banks seem to systematically increase the valuation of real estate pledged as collateral to comply with LTV caps, thereby undermining the long-term policy objective of financial stability (see Griffin, 2021).

Our headline results confirm that restrictive MPP reduces aggregate lending volume as well as the number of loans per region, on average, by more than 20% after a contractionary LTV shock. We also estimate, at the loan level, that banks comply with macroprudential policy measures at the portfolio level. Reported LTV ratios decline, on average, by 1 point during the five months following a contractionary LTV policy shock. But tests of the mechanism by which banks comply reveal that the size of the average loan – henceforth loan balance – is not significantly reduced. In contrast, we find that residential properties that

collateralize these new mortgages are valued around 6% higher relative to the control group. This finding suggests that affected banks practice a more benevolent appraisal of real estate. Given that Griffin (2021) shows how overoptimistic appraisals caused the largest imbalances in regional U.S. housing markets prior to 2007, this result provides circumstantial evidence of risk-shifting by EU banks in response to contractionary MPP.

To corroborate this mechanism to inflate collateral valuation, we exploit earthquakes in the Netherlands as plausible exogenous shocks to property values. These events affect geographically sharply delineated areas, which permit a natural experiment contrasting mortgage originations in affected (NUTS-3) regions with the rest of the country. *Ex ante*, seismic risk should depress collateral valuations; instead, descriptive evidence reveals that valuations reported at origination following LTV tightenings, increased faster inside compared to outside earthquake areas. The evidence from fixed effects regressions confirms the absence of significantly different valuation responses to LTV shocks in earthquake regions. This pattern supports the interpretation that compliance with tighter MPP requirements is achieved through higher collateral valuations that contradict fundamental factors that unequivocally depress real estate values.

Our empirical approach to testing whether and how banks comply with LTV caps set at the loan level relies on so far untapped granular information pertaining to 2.4 million mortgage contracts that finance residential properties observed at the ZIP code level in seven European countries between 2008 and 2020. We combine these granular residential mortgage and property data with recently collected MPP measures (Alam et al., 2025) and focus on contractionary MPP shocks in the Netherlands. In this mature, mortgage-intensive housing market, LTV adjustments are a primary MPP instrument, and macroprudential policy was tightened repeatedly over our sample period. This sequence provides a clean, event-time setting to study banks' compliance at origination, because the policy is both salient and precisely timed. In addition, the Netherlands offer plausibly exogenous variation in collateral values: man-made earthquakes linked to the Groningen gas field – most promi-

nently the 2012 Huizinge event – are geographically concentrated, allowing us to contrast affected NUTS-3 regions with the rest of the country. Taken together, this combination of frequent, well-documented LTV tightenings and a localized, negative valuation shock makes the Netherlands an ideal laboratory for identifying the mechanisms through which banks meet tighter portfolio constraints.

Our empirical 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. We complement these insights on the effect of LTV regulation on borrowers with evidence on how banks adjust their behavior. In fact, our aggregate result that banks reduce the number of loans after contractionary macroprudential policy paired with a more benevolent appraisal of real estate value for newly originated loans complement their conclusions that LTV caps have distributional implications in terms of which households are more rationed. De Araujo et al. (2020) study loan- and borrower-level data from Brazil and show 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.<sup>1</sup> 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 and entail a substantial contraction of aggregate lending, especially to low-quality borrowers.

More generally, we relate 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

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

bank capital crunches, and curtailed systemic risks by debilitating the house price-leverage spiral. As a consequence, they also argue that MPP caused milder macroeconomic contractions and enhanced household welfare.<sup>2</sup> Mokas and Giuliadori (2023) reports that already the announcement of tightening LTV ratios influences banking practices in the form of contracting household credit. Claessens (2015) remarks, however, that MPP may interfere with monetary and microprudential policies, potentially adversely affecting resource allocation and incentives for circumvention. Hartmann (2015), Galati and Moessner (2013, 2018), and Duca et al. (2019) also acknowledge the role of MPP in preventing real estate market turmoil, but emphasize that geographically differentiated markets require tailored policies and that causal evidence on the effects of MPP remains sparse. We heed the call articulated by this strand of research with a spatially more granular assessment on causal responses by banks that comply with LTV caps in a manner that entails elusive risk-shifting though.

The empirical evidence obtained at the loan-level in this paper also speaks to a theoretical literature on MPP, which focuses on aggregate outcomes. For example, Bianchi and Mendoza (2018) suggest that constraints on borrowing against collateral can prevent credit booms and busts, thereby reducing volatility and cyclicity. Using a structural model, Benetton (2021) demonstrates 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. We complement these theoretical predictions pertaining to aggregate credit market outcomes with an analysis of loan-level responses to actually observed LTV shocks in EU banking markets. Our empirical findings add the insight that banks' efforts to comply with LTV caps at the portfolio level result in part from systematically more benevolent appraisals of collateral value.

As such, we shed light on a potential economic cost beyond those documented by, for

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<sup>2</sup>Further empirical evidence that stricter LTV ratios moderate aggregate credit growth includes, for example, Lim et al. (2011), Cerutti et al. (2017), and Akinici and Olmstead-Rumsey (2018).



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. 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. In our approach, we actually exploit man-made earthquakes as a specific and spatially delineated shock to identify a new mechanism how lenders respond to MPP tightenings: the collateral valuation channel.

## 2 LTV and loan data

We describe in this section the macroprudential policy shocks and the loan data sampled to estimate banks' mortgage lending responses to contractionary MPP events.

### 2.1 Policy shocks

Macroprudential policy (MPP) has become a critical component of the regulatory toolkit used by national competent authorities to safeguard financial system stability. MPP interventions target systemic risks that can accumulate during credit expansions and asset price booms. A key instrument in this regard is the loan-to-value (LTV) ratio, which restricts the amount of credit extended relative to the underlying collateral value. By tightening or loosening LTV limits, policymakers seek to influence lending behavior, temper leverage cycles and thereby affect property prices. LTV regulation is a salient policy instrument in

selected European real estate markets. For example, the central bank of the Netherlands has executed eight such LTV tightenings during our sample period “[...] for targeting asset bubbles in the residential property sector” (De Nederlandsche Bank (DNB), 2022).

Empirically, we draw on the harmonized and comprehensive macroprudential policy dataset by Alam et al. (2025), which systematically documents policy measures across 134 countries. Our laboratory is the Netherlands which saw a stream of new or more stringent loan-to-value (LTV) caps on residential mortgages between 2010 and 2018. We compare mortgage lending practices in the Netherlands to that in six major real estate lending markets in Europe that did not alter LTV regulation: Germany, Spain, France, Sweden, and the United Kingdom. We obtain mortgage and collateral data from the European Data Warehouse repository, which we describe in greater detail below.<sup>3</sup> We exclude, in turn, three European countries that also tightened LTV regulation, but which did so only substantially later and command only a relatively small share in the residential lending market that we observe and describe below.<sup>4</sup>

[Table 1](#) details the timing, intensity, and further institutional features of each LTV policy measure in the Netherlands. We focus on MPP shocks in the Netherlands since this residential real estate markets is an ideal empirical laboratory to study the impact of contractionary LTV events on mortgage lending dynamics for three reasons. First, the Netherlands were not only the first EU country to activate LTV, but did so in a sequence of events over eight years. Second, home ownership in the Netherlands of approximately 68% represents the EU well, (see [Statista](#)) And finally, quasi-exogenous variation in mortgage valuation due to man-made earthquakes allows us to isolate causal mechanisms, how banks comply with contractionary LTV regulation.

Therefore, we argue that these Dutch changes in LTV regulation provide a unique set-

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<sup>3</sup>We cover the universe of mortgages reported to EDW except Austria, which only securitized automobile loans.

<sup>4</sup>These markets are Ireland (February, 2015), Portugal (July, 2018), and Belgium (January, 2020). Results are insensitive to including loans from these markets into the analysis and we provide comprehensive robustness tests below in [Table A4](#).

ting to assess whether and how banks respond to tighter portfolio constraints on mortgage lending. We treat each of the seven policy implementations as a distinct event and restrict our analysis to short time windows of  $\pm 4$  months around the respective enactment dates, thereby ensuring that observed changes in mortgage origination can be directly attributed to a specific LTV policy shock.

**Table 1:** Overview of LTV events in the Netherlands

Date of Implementation	Description
M1:2010	Authorities set a maximum loan-to-foreclosure-value (LTFV) ratio of 112%, requiring amounts above 100% to be repaid within seven years.
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%.

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

All policy changes become effective in January. As a result, these events may coincide with seasonal factors that influence lending behavior, such as fiscal and tax year endings for banks and clients, as well as typical intervals for regulatory supervision.<sup>5</sup> To address these potential confounders, we extend the overall sample period and explicitly control for a possible “January effect” in our baseline regressions. The resulting sample period ranges from 2008 to 2020, of which seven years experience an LTV shock in January (see [Table 1](#)). This structure allows us to distinguish the effect of regulatory changes from general seasonality in lending activity.

<sup>5</sup>See Expertisecentrum Woningwaarde (2025) for a comprehensive overview on factors that affect prices and lending on the Dutch housing market.

## 2.2 Mortgage and residential property data

A comprehensive credit register exists in European financial markets only as of 2018 in the form of Anacredit, which is collected by the ECB but does not contain information on residential real estate lending (Israël et al., 2017). To circumvent this severe data limitation, we resort to loan-level data collected originally for different purposes in a repository: the European Data Warehouse (EDW). The EDW was established in 2012 to improve transparency regarding collateral pools of asset-backed securities (ABS) after the pervasive securitization of subprime debt in the run-up to the Great Financial Crisis. All ABS issuers must deposit comprehensive loan-level and other collateral data if the underlying assets originate from a EU member state. Similar to the EU framework, the UK Securitisation Regulation requires public securitisations to be reported to a UK-registered securitisation repository. European DataWarehouse Ltd is one of two such repositories authorised by the FCA, so EDW data also cover UK securitisations even though the UK is no longer an EU member state. Consequently, the EDW repository contains detailed information about the individual loans contained in various types of ABS, such as residential mortgages, commercial mortgages, or auto loans.<sup>6</sup> Importantly, ABS issuers had to backfill data on the credit instruments underlying the structured products. Therefore, we can use mortgage loans issued in seven European countries between September 2007 and April 2020.<sup>7</sup>

EDW data were initially collected by the ECB as of 2013, with ESMA taking over as of 2021. We collect 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

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<sup>6</sup>Since the UK is no longer an EU member state, the first three columns of [Table A6](#) show that our results remain unchanged when we exclude the UK from the analysis.

<sup>7</sup>Only recently EDW data are used for academic purposes. 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.

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. 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.

The raw EDW data includes 188,597,520 mortgage loans between 2007 and 2024. Each mortgage loan is recorded multiple times since its origination and we process the data as follows. First, we exclude all loans with missing information for the LTV, loan balance, or valuation at origination, which results in 105,768,712 mortgage loans. Second, we disregard remortgaged loans, which yields 89,885,232 remaining loans, because they benefit from grandfathering rules and are exempted from LTV shocks. Third, we keep only information about the mortgage loans at the origination date, which further reduces the sample to 7,131,597 observations. Fourth, we disregard loans originated in Ireland, Portugal, and Belgium since those countries also set stricter limits for national LTV regulations between 2009 and 2022. The sample comprises after these steps 5,708,534 mortgage loans.

We take the exact dates when each policy in the Netherlands came into effect from the database provided by Alam et al. (2025) and detail the nature of the policy action in [Table 1](#). We sample control group loans that were issued in six countries that did not alter LTV regulation: Germany, Spain, France, Italy, Sweden, and the United Kingdom. As a next step, we limit the sample to periods of  $\pm 4$  months around every January between 2008 and 2020 we are left with 2,597,327 mortgage loans.<sup>8</sup>

We further adjust LTV ratios for French loans by dividing them by 100<sup>9</sup> and drop ultra high and LTV ratios of lower than 10 and higher than 500, which leaves 2,401,393 mortgage

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<sup>8</sup>Since the sample is unequally balanced between 641,529 Dutch loans and 1,756,108 loans from the remaining countries, we randomly draw from the set of non-Dutch countries to equalize sample sizes on a calendar-date basis. [Table A3](#) shows that the main results remain unchanged. In Columns (1) to (3), we maintain the original country-level sample proportions. In Columns (4) to (6), we preserve the original cross-regional distribution across non-Dutch NUTS-3 regions.

<sup>9</sup>We came across this issue after discussions with the EDW team.

loans. Finally, we disregard zero balances and valuations and very few remaining singletons, resulting in an analysis sample of 2,397,637 mortgage loans from seven European countries.

The three panels in [Figure 1](#) describe these mortgage lending data from different perspectives. The top panel illustrates the dynamics of relative market shares over the entire sample period. With the exception of Sweden, the Netherlands are the smallest economy in terms of nominal GDP in this comparison. Yet, the top panel illustrates the economically important role that Dutch mortgage lending markets play in Europe, accounting for up to 40% share in lending in this comparison. This descriptive evidence also alludes to possible effects of LTV regulation though. At the latest with the second LTV tightening in January 2013, the relative share of mortgage loans in the Netherlands starts to decline substantially, possibly due to MPP measures taken by the Dutch Central Bank to counteract overheating residential real estate markets (De Nederlandsche Bank (DNB), 2022).

The middle panel details the number of loans originating around every regulatory event. The segment of the bars that are in red show the number of loans from the Netherlands. The bars' areas shaded in blue count the number of loans from the countries without an LTV change. Whereas the share of mortgages originated in the Netherlands steadily declines over the sample period, no obvious pre-treatment spikes are systematically observable. Whether the increasingly lower number of treated mortgage loans compared to those issued in the six control countries remains an empirical question that we address below.

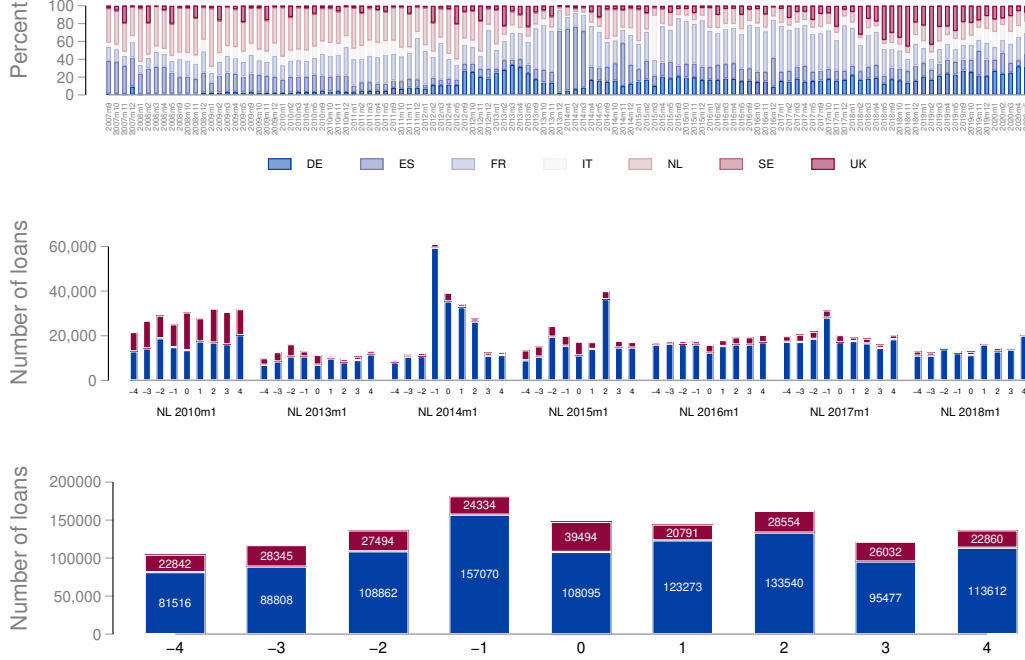
To that end, the bottom chart of [Figure 1](#) provides the cumulative sum of loans from LTV-regulating and non-regulating countries across the seven periods that we exploit for each event. Overall, no systematically different pattern emerges regarding the Dutch mortgage market vis-à-vis the control group countries.<sup>10</sup>

[Table 2](#) provides descriptive statistics for each dependent variable in the estimation sample comprising 2,397,637 mortgage loans. The average loan-to-value ratio among mortgage loans originated in the seven sampled countries amounted to 79.7% between January 2008

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<sup>10</sup>The constant relative share of treated loans in the bottom panel of [Figure 1](#) is driven by the 2010 LTV event. Our results hold when we look at this event in isolation, see the last three columns of [Table A6](#).

**Figure 1: Mortgage loan dynamics across countries, over time, and around LTV shocks**



**Notes:** The top panel displays the share of loans across countries and time in percent. The middle panel shows the number of loans for the months ( $\pm 4$ ) around each LTV tightening in the Netherlands in event time. Red segments per bar indicate loans originated in the Netherlands whereas blue segments indicate the cumulative number in the six control countries. The bottom panel on the bottom displays the accumulated number of loans in event time across all events using the same color scheme.

and January 2020. Hence, the average borrower financed her residential home with a mere 20% of own (equity) funds. Such relatively high levels of household leverage corroborate the importance of MPP to contain excessive fragility if housing markets are overheating.

We specify below dynamic difference-in-differences models to test for responses in loan balance (i.e., loan size) and the valuation of the property serving as collateral to LTV events in [Table 1](#). These outcome variables are specified in natural logarithms and [Table 2](#) illustrates that these variables' distributions are well-behaved and suited for straightforward OLS tests.<sup>11</sup> The last two entries in [Table 2](#) show that the average residential property was valued

<sup>11</sup>We also tabulate unconditional difference-in-differences in [Table A2](#) to compare averages across loans in

**Table 2:** Descriptive Statistics

	Mean	Sd	Percentiles		
			1%	50%	90%
LTV	79.7	24.8	16.4	81.9	129.0
Ln Balance	11.4	0.9	9.0	11.6	13.2
Ln Valuation	12.0	0.8	9.3	12.1	13.7
Balance	204.3	1468.0	8.0	104.5	567.0
Valuation	272.3	2054.0	10.6	172.7	929.0

**Notes:** This table presents mean values, standard deviations, and the 1st and 99th percentiles for the variables employed in subsequent regression analyses. The sample comprises 2,397,637 loans that were originated in the Netherlands, Germany, Spain, France, Italy, Sweden, and the United Kingdom during the 13 time windows (September to May) between 2008 and 2020. LTV is the ratio of a mortgage loan's balance to its valuation (in percent). Balance and Valuation are measured in thousands of Euros. Ln indicate the natural logarithm of variables in Euros.

by lenders with 272 thousand euros, while the average mortgage amounted to 204 thousand euros.

### 3 LTV caps and aggregate lending responses

In this section, we provide aggregate mortgage lending market responses to LTV shocks as a first test of whether macroprudential policy achieves its tactical target, namely to dampen real estate lending.

#### 3.1 Specification

Since we are first concerned about aggregate market responses to LTV shocks, we generate two outcome variables within each NUTS-3 region and per calendar date. First, we gauge aggregate mortgage lending responses by summing up the loan balances observed in the EDW repository.<sup>12</sup> Second, we count the number of loans issued per month and region as LTV regulation may not only reduce the average loan size as shown by Tzur-Ilan (2023),

regulated and unregulated countries before and after the regulation events. Normalized differences (Imbens and Wooldridge, 2009) indicate that average log loan balances and property valuations are not significantly different from another.

<sup>12</sup>The Dutch statistical office does neither provide real estate price indices nor aggregate mortgage lending data broken down by regions. Fortunately, virtually all mortgage loans are securitized in the Netherlands and thus contained in EDW.



but could also entail complete rationing of selected borrower groups, such as low-income households as documented by Acharya et al. (2022) or Hacamo (2024).

We then use LTV tightening events by running event study analyses and standard 2x2 difference-in-difference regressions. Regarding the former, we employ nine event-time periods  $\tau$  for a symmetric window of  $\pm 4$  months (including the event month) around each January between 2008 and 2020, such that the reference period is December ( $\tau = 0$ ). We derive period-specific point estimates by estimating regressions in event time that are based on:

$$\begin{aligned}
Y_{r,\tau,t} = & \sum_{\substack{\tau=-4, \\ \tau \neq -1}}^4 \delta_{\tau} \left( \mathbb{1}_r^{\text{LTV}} \cdot \mathbb{1}_{\tau}^{\text{Post}} \cdot \mathbb{1}_{r,t}^{\text{Treat}} \right) \\
& + \sum_{\substack{\tau=-4, \\ \tau \neq -1}}^4 \gamma_{\tau} \left( \mathbb{1}_r^{\text{LTV}} \cdot \mathbb{1}_{\tau}^{\text{Post}} \right) + \sum_{\substack{\tau=-4, \\ \tau \neq -1}}^4 \mu_{\tau} \left( \mathbb{1}_{r,t}^{\text{Treat}} \cdot \mathbb{1}_{\tau}^{\text{Post}} \right) + \lambda \left( \mathbb{1}_r^{\text{LTV}} \cdot \mathbb{1}_{r,t}^{\text{Treat}} \right) \\
& + \vartheta_t + \vartheta_r + \vartheta_k + \vartheta_{\tau} + \epsilon_{r,\tau,t}.
\end{aligned} \tag{1}$$

We use two different dependent variables  $Y$ : the total number of newly issued mortgage loans and the total sum of these loans on the NUTS-3 level  $r$  across all the NUTS-3 regions in our sample. We observe these aggregates in calendar time  $t$  for each of the  $\pm 4$ -month windows between January 2008 and January 2020.

We explain the variation of the aggregate number and volume of mortgage loans with three indicator variable and a set of four fixed effects. The first indicator variable is  $\mathbb{1}_r^{\text{LTV}}$  and equals one in all event periods  $\tau$  per LTV event cohort  $k$  shown in [Table 1](#) if the NUTS-3 region  $r$  is located in the Netherlands, and thus subject to LTV policy shocks at different points in time. If the regional credit market in question is located in one of the remaining countries in the sample,  $\mathbb{1}_r^{\text{LTV}}$  is equal to zero. Second,  $\mathbb{1}_{\tau}^{\text{Post}}$  varies on the event-time level and indicates each event-month September to November ( $\mathbb{1}_{\tau}^{\text{Post}}=0$ ) and January to May ( $\mathbb{1}_{\tau}^{\text{Post}}=1$ ) in reference to December. Last,  $\mathbb{1}_{r,t}^{\text{Treat}}$  varies on the NUTS-3 and calendar time level. This

indicator is equal to 1 if an actual LTV event according to the LTV cohorts  $k$  in [Table 1](#) occurs in any of the  $\pm 4$ -month windows between January 2008 and January 2020. It equals zero if the event window captures a time period without an LTV event around any given January.

Additionally, we specify four fixed effects  $\vartheta$  to absorb systematic variation due to unobservable factors along the following dimensions. First, we gauge unobservable business and credit cycles specifying calendar-time fixed effects  $\vartheta_t$ . Second, we account for structural differences between regions nested in the seven sampled countries by means of fixed effects per NUTS-3  $\vartheta_r$ . Third, differently timed LTV regulation might unfold systematically different impact on regional mortgage lending markets. For example, borrowers and lenders may adapt to LTV shocks, thus reducing the responsiveness to LTV shocks in later years. Therefore, we also include fixed effects for each policy shock cohort  $\vartheta_k$ . Last, we include event-time fixed effects  $\vartheta_\tau$  to absorb month-specific effects that may arise around the turn of each calendar year.

The main parameters of interest are the triple interaction terms of indicator variables,  $\delta_\tau$ , in the first line of [Equation 1](#). These point estimates reflect the differential effects on  $Y$  between Dutch and non-Dutch NUTS-3 regions and between time windows with and without LTV shock. The specification in event time implies also that  $\delta_\tau$  gauge average effects for each event-month relative to the reference outcome, which is the average December  $Y$ . The second line of [Equation 1](#) exhibits all consecutive interaction terms between the three indicator variables that remain identified given the specified fixed effects.

In addition to the dynamic point estimates obtained from [Equation 1](#) for the average event-month (in reference to the average December), we also derive basic before-after comparisons of outcome variables between regions  $r$  in the Netherlands versus those in the other six countries. We obtain so-called 2x2 estimates of the average triple interaction parameter  $\delta$  by estimating:

$$\begin{aligned}
Y_{r,\tau,t} = & \delta \left( \mathbb{1}_r^{\text{LTV}} \cdot \mathbb{1}_\tau^{\text{Post}} \cdot \mathbb{1}_{r,t}^{\text{Treat}} \right) \\
& + \gamma \left( \mathbb{1}_r^{\text{LTV}} \cdot \mathbb{1}_\tau^{\text{Post}} \right) + \mu \left( \mathbb{1}_{r,t}^{\text{Treat}} \cdot \mathbb{1}_\tau^{\text{Post}} \right) + \lambda \left( \mathbb{1}_r^{\text{LTV}} \cdot \mathbb{1}_{r,t}^{\text{Treat}} \right) \\
& + \vartheta_t + \vartheta_r + \vartheta_k + \vartheta_\tau + \epsilon_{r,\tau,t}.
\end{aligned} \tag{2}$$

In comparison to [Equation 1](#) which estimates differences in outcome variables between Dutch and the remaining regions  $r$  for every  $\pm 4$  months in reference to the respective December, [Equation 2](#) provides average differential effects comparing outcomes averaged from January until April with those between September to December.

### 3.2 Aggregate lending responses per region

To conserve on space, [Figure 3](#) shows coefficient plots for [Equation 1](#) and [Equation 2](#). Tables of the corresponding regression results are available upon request. Blue squares denote the parameters when specifying the count of originated loans per NUTS3 region. Red diamonds relate to the specification of regional sums of loan balances instead.

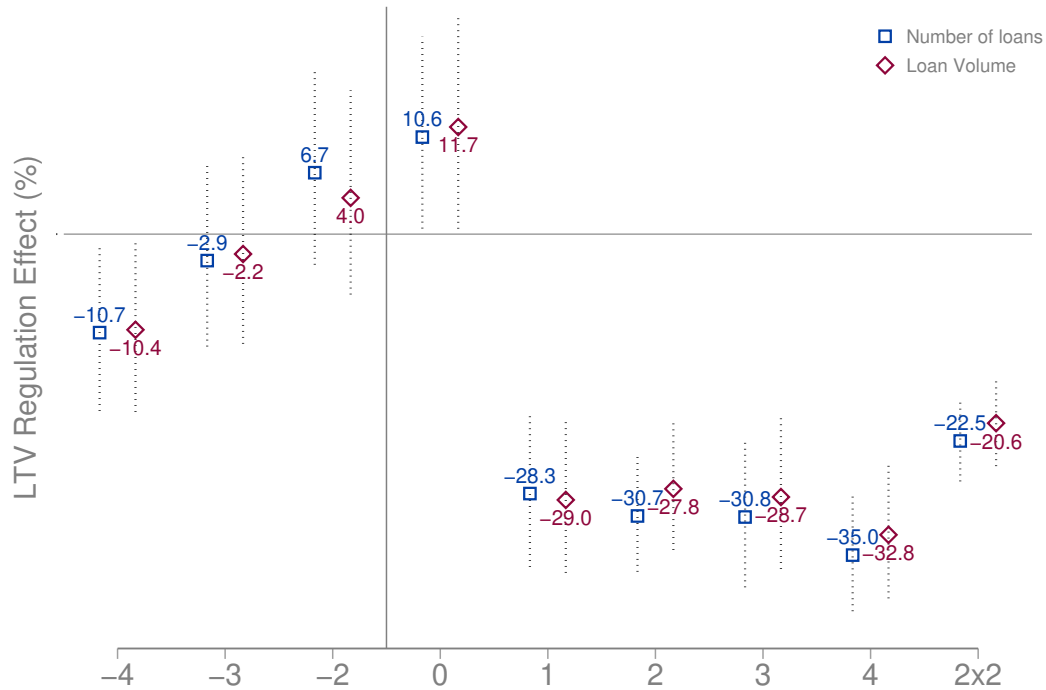
The point estimates indicate a significant contraction of mortgage lending in the four months after a tightening of LTV ratios. These responses are statistically significant at a confidence level of 95% and economically meaningful.

The extensive margin, that is, the number of loans contracts by 28.3% immediately after the policy shock, an effect that increases to 35% in the last event month, April. The average differential effect is denoted as 2x2 on the horizontal axis and indicates also a significant contraction on the order of 22.5%.

The empirical evidence pertaining to the intensive margin confirms these results. The aggregate loan volumes originated after an LTV shock in the Netherlands contract by very similar magnitudes, yielding an average 2x2 contraction of 20.6%.

This first set of results bodes well for the effectiveness of contractionary macroprudential

**Figure 2: Aggregate Effects on Number of Loans and Volume**



**Notes:** This figure shows the average differences in the number of loans (blue squares) and loan volumes (red diamonds) around all LTV events in the sample. The coefficients come from regression of Equation 1. The  $x$ -axis shows event months ( $\pm 4$ ) around each January between 2008 and 2020. The term 2x2 on the  $x$ -axis denotes the average difference in the outcome variable between all pre- and post-LTV months. Dotted black lines indicate 95% confidence intervals. Coefficients are scaled by 100 for log specifications to represent percentage changes.

policy to dampen real estate lending. The economically large magnitude of fairly mild LTV steps of just 1 point (see Table 1) apparently were helpful to achieve the declared objective of the Dutch competent authority. The joint reduction of both aggregate lending quantities and the number of loans implies, however, that most likely not all potential borrowers received smaller loans. Instead, the reduced number of loans suggests that certain applicants no longer might have received credit altogether. Whereas we do not observe loan application (and rejection) data, this heterogeneous response to LTV shocks across presumably different borrower groups would be in line with prior evidence that focuses more explicitly on the distributional consequences of macroprudential policies for households, such as Acharya et al. (2022), Tzur-Ilan (2023), or Hacamo (2024).

## 4 Loan-level responses to LTV tightening

To complement this evidence on household effects of LTV policies, we focus in this paper next on the question, how banks' adjusted their lending behavior in response to LTV tightening. For that reason, we turn to loan level analyses to unveil these adjustment mechanisms in more detail.

### 4.1 Specification

The empirical main approach remains to exploit the Dutch LTV policies by running event study analyses and standard 2x2 difference-in-difference regressions. We obtain period-specific response parameters by estimating regressions resembling [Equation 1](#) that are now specified on the loan-level  $i$ :

$$\begin{aligned}
Y_{i,\tau,t} = & \sum_{\substack{\tau=-4, \\ \tau \neq -1}}^4 \delta_{\tau} \left( \mathbb{1}_i^{\text{LTV}} \cdot \mathbb{1}_{\tau}^{\text{Post}} \cdot \mathbb{1}_{i,t}^{\text{Treat}} \right) \\
& + \sum_{\substack{\tau=-4, \\ \tau \neq -1}}^4 \gamma_{\tau} \left( \mathbb{1}_i^{\text{LTV}} \cdot \mathbb{1}_{\tau}^{\text{Post}} \right) + \sum_{\substack{\tau=-4, \\ \tau \neq -1}}^4 \mu_{\tau} \left( \mathbb{1}_{i,t}^{\text{Treat}} \cdot \mathbb{1}_{\tau}^{\text{Post}} \right) + \lambda \left( \mathbb{1}_i^{\text{LTV}} \cdot \mathbb{1}_{i,t}^{\text{Treat}} \right) \\
& + \vartheta_t + \vartheta_r + \vartheta_k + \vartheta_{\tau} + \vartheta_o + \epsilon_{i,\tau,t}.
\end{aligned} \tag{3}$$

We estimate differential effects on three different loan traits specified as outcome variables,  $Y_i$ : the LTV ratio, loan balance, and collateral valuation. All outcomes are measured at the time of loan origination and we use the natural logarithm of variables measured in monetary terms. We continue to employ nine periods  $\tau$  for a symmetric window of  $\pm 4$  months (including the event month January) around each January between 2008 and 2020. The reference period is December. The sample comprises each loan  $i$  only at its origination month  $t$ . Thus, we sample repeated cross-sections of loans at origination. We specify the identical set of fixed effects from [Equation 1](#) for the same reasons mentioned there. In

addition, we include originator  $o$  fixed effects to absorb additional variation between loans depending on which institution grants the loan.<sup>13</sup>

The main parameters of interest remain  $\delta_\tau$  in the first line of [Equation 3](#). These coefficients estimate the differential effects on  $Y$  between mortgage loans originated in the Netherlands vis-à-vis mortgaged loans originated in the control group countries between time windows with and without an actual LTV event, both on average for each event-month in reference to the average December outcome. The second line of [Equation 3](#) depicts again the consecutive interaction terms that remain identified given the saturated fixed effect structure. Resembling estimations of aggregate responses in [section 3](#), we also estimate 2x2 before-after comparisons of outcome variables between mortgage loans originated in the Netherlands and the other six countries.

## 4.2 Do banks comply with contractionary MPP?

Whereas aggregate responses along the extensive and intensive margin after contractionary LTV events suggest that banks adjusted their lending behavior as intended by the policy, it remains an empirical question if originators complied also at the loan level when supplying loans after regulatory tightening. Consider to that end the first main result based on [Equation 3](#) when we specify the LTV reported at origination at time  $t$  for each loan  $i$ .

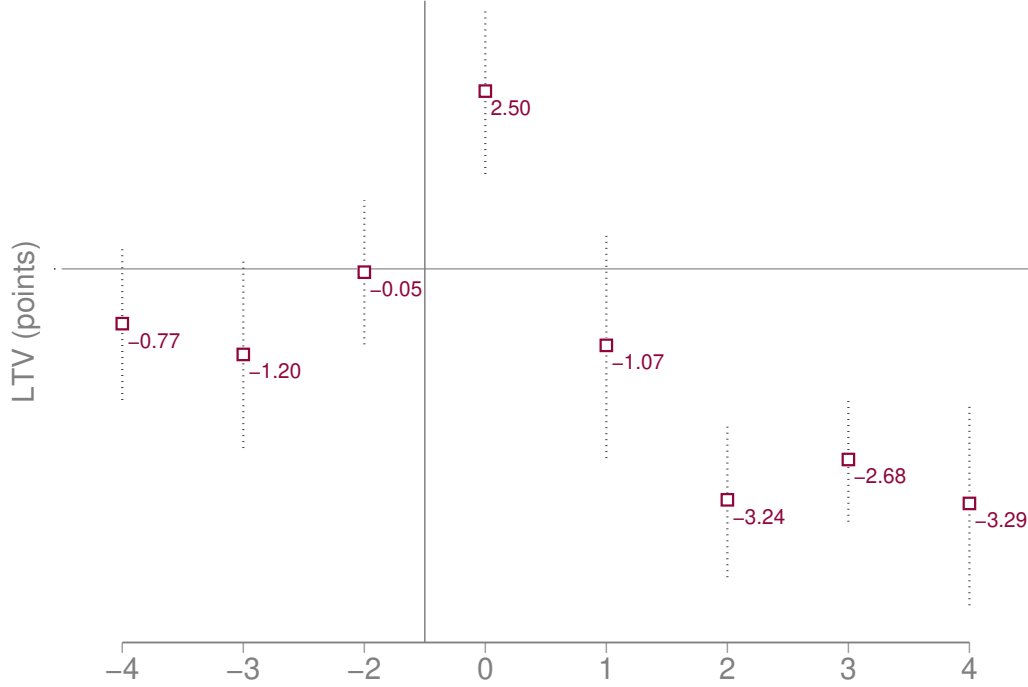
The coefficient plot in [Figure 3](#) illustrates the developments of mortgage loans' LTV ratios around each January between 2008 and 2020 and for seven LTV regulations during that period. For each estimate, we provide 95% confidence intervals by dashed gray vertical lines. The following main insights emerge.

First, we find no evidence of statistically significant responses of reported LTV ratios

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<sup>13</sup>We challenge our choice of fixed effects in [Table A5](#). In the first three columns, we omit the originator fixed effects because the originator field in the data exhibits substantial recording inconsistencies. The same originator frequently appears under different spellings or as numerical placeholders, which prevents the reliable construction of originator-level identifiers. We reviewed the entries and cleaned the obvious cases, but we provide this robustness check to demonstrate that any remaining inconsistencies do not affect our point estimates. In the last three columns, we introduce country–calendar-date fixed effects, which account for any remaining country-specific cyclical patterns in the time windows we examine. Our results remain stable across both fixed-effects specifications.

**Figure 3: Event-study Effects on LTV**



**Notes:** This figure shows the marginal effects estimated from Equation 3. The outcome variable is the LTV at origination. Red squares indicate the point estimate for the triple interaction  $\mathbb{1}_i^{LTV} \cdot \mathbb{1}_{\tau}^{\text{Post}} \cdot \mathbb{1}_{i,t}^{\text{Treat}}$ . Dotted black lines represent 95% confidence intervals. The  $x$ -axis indicates event months ( $\pm 4$ ) around each January between 2008 and 2020. Each of the  $\pm 4$  event-month effects is evaluated relative to the month immediately preceding the event (December,  $-1$ ). The estimates are based on 2,397,637 mortgage loans. We cluster standard errors at the NUTS-3 level to capture potential heterogeneity in the standard errors due to regulatory events. Coefficients are scaled by 100 for log specifications to represent percentage changes.

at origination in the four months preceeding LTV shocks. The coefficients plotted for the pre-event months  $\tau = -4, \dots, -2$  are insignificant at the 95%-level of confidence, which bodes well regarding (the absence of) pre-trending. Second, consider the coefficient in event period 0 and recall that it corresponds to January. The statistically significant hike in LTV of 2.5 points represents the differential effect—relative to loans in Decembers—of reported ratios pertaining to mortgages that were originated in a January during which an LTV event became effective (see Table 1) in comparison to the general January effect, i.e. changes in reported LTV ratios also pertaining to loans that were originated during event windows that were not subjected to contractionary MPP. The raw data reveals that hardly any individual loan violated active LTV caps. Thus, the estimated positive LTV response in Januaries may

indicate that banks catered first to loan customers closest to the threshold, maximizing the remaining scope to lend at LTV up and until the new cap, thereby aiming to avoid rationing in case of further tightening.<sup>14</sup>

Third, and more importantly, the differential LTV effects turn negative from February and remain significantly negative with a magnitude of around 3 points until 4 months after an actual LTV regulation. These LTV dynamics are thus in line with the aggregate result that contractionary MPP induced banks to lend more restrictively.

### 4.3 Bank’s adjustment to LTV caps: volume versus valuation

To shed light on the mechanism, how banks comply with tighter LTV ratios, consider next [Table 3](#). It features 2x2 effects to compare responses of three outcomes—LTV ratio, loan size, and collateral valuation—from post-event months to the complete pre-event period.

The first column depicts average estimates using the LTV as the outcome variable. Commensurate with the event-month effects in [Figure 3](#), we find that Dutch LTVs are significantly lower in the first five months in years with stricter LTV regulation. Hence, the initial hike in LTV is overcompensated in subsequent months. The average effect amounts to around 1 point. This result therefore confirms that banks comply on average with stricter LTV regulation to the extent that newly issued loans exhibit systematically lower LTV after a MPP tightening.

As to the question how banks comply with the more stringent rules, we check the two components of the LTV ratio separately: loan balance and collateral value. Specifying the size of the new mortgage loans results in point estimates depicted in the second column of [Table 3](#). Whereas we obtain a negative response parameter, we cannot reject the null that

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<sup>14</sup>Boxplots per LTV event in [Figure A1](#) show the distribution of actual LTV ratios at origination, confirming that hardly any loans violated active LTV caps between 2008 and 2020. The majority of loans already comply before the events, thus leaving indeed scope for more “aggressive” January lending.



**Table 3:** Average Effects for LTV, Balances, and Valuations

	LTV	Ln Balance	Ln Valuation
	(1)	(2)	(3)
$\mathbb{1}_r^{\text{LTV}} \cdot \mathbb{1}_\tau^{\text{Post}} \cdot \mathbb{1}_{i,t}^{\text{Treat}}$	-1.07*** (0.30)	-3.30 (2.09)	6.21*** (1.71)
No. of Loans	2,397,637	2,397,637	2,397,637
No. of NUTS-3 (Cluster for SE)	1,121	1,121	1,121
<b>Fixed Effects:</b>			
Calendar-time	Yes	Yes	Yes
Event-time	Yes	Yes	Yes
Originator	Yes	Yes	Yes
LTV Regulation Cohort	Yes	Yes	Yes
NUTS-3	Yes	Yes	Yes
Consecutive terms of $\mathbb{1}_r^{\text{LTV}} \cdot \mathbb{1}_\tau^{\text{Post}} \cdot \mathbb{1}_{i,t}^{\text{Treat}}$	Yes	Yes	Yes

**Notes:** This table presents coefficients and standard errors clustered at NUTS-3 level for average 2x2 variants of [Equation 3](#):

$$Y_{i,\tau,t} = \delta \left( \mathbb{1}_r^{\text{LTV}} \cdot \mathbb{1}_\tau^{\text{Post}} \cdot \mathbb{1}_{i,t}^{\text{Treat}} \right) + \gamma \left( \mathbb{1}_i^{\text{LTV}} \cdot \mathbb{1}_\tau^{\text{Post}} \right) + \mu \left( \mathbb{1}_{i,t}^{\text{Treat}} \cdot \mathbb{1}_\tau^{\text{Post}} \right) + \lambda \left( \mathbb{1}_i^{\text{LTV}} \cdot \mathbb{1}_{i,t}^{\text{Treat}} \right) + \vartheta_t + \vartheta_r + \vartheta_k + \vartheta_\tau + \vartheta_o + \epsilon_{i,\tau,t}.$$

The dependent variables from left to right are: the loan-to-value ratio at origination; the natural logarithm of the loan balance at origination; and the natural logarithm of the valuation at origination. We include fixed effects at the event-month, calender-month, originator, and NUTS-3 level. Each regression comprises 2,397,637 mortgage loans. Coefficients are scaled by 100 for log specifications to represent percentage changes.

this point estimate equals zero. Therefore, the evidence does not support the notion that banks supply smaller loans holding everything constant loan size is not the driver for the LTV effect.

The last column of [Table 3](#) reveals, in turn, a statistically significant and positive differential effect of LTV shocks on the valuation of collateral at origination. The point estimate is about 6.2%, which corresponds to an increase by approximately € 14,200 how banks value the average residential real estate to collateralize mortgages prior to regulation (see [Table A2](#)). The implication of this empirical result pours water into the wine of regulators, who content themselves with aggregate lending contractions after tightening LTV ratios. As it is the declared intention of policy makers to dampen excessive house price cycles, systematically inflated appraisal values may thus offset any contraction in aggregate lending that is driven primarily by a reduced number of loans originated.

## 4.4 Timing of LTV events

As illustrated in [Table 1](#), the nature of LTV policy events is different. Arguably, the first policy action that became effective as of January 2010 might have had more of a surprise character to market participants. The policy change in 2013, in turn, provided substantial forward guidance on the future path of maximum LTV ratios whereas all subsequent tightenings merely changed the numerical value of the LTV cap. Thus, it remains unclear whether and how would update their expectations and respond differently to differently timed LTV shocks.

Therefore, we augment the 2x2 estimation at the loan level with an interaction term for each separate LTV event and show the according results for the identical three outcome variables in [Table 4](#). The upshot of these specifications is threefold.

First, the positive loading on the LTV reported in [Figure 3](#) for the initial post-event period appears to be driven by the very first LTV tightening in 2010 and towards the end of the announced termination of the LTV reduction in 2018, namely in the 2016 event. This pattern supports the interpretation that banks learning for the first time about the new, hawkish MPP stance of the DNB tried to fill first of all demand for loans that were relatively close to the announced threshold, thereby increasing the average LTV after the initial policy shock by 73 basis points. Likewise, with the end of the policy trajectory in sight in 2016, banks might have loaded on all remaining loans compliant with existing LTV caps just before the ultimate tightening would occur.

Second, whereas the average differential 2x2 response of loan balances remained statistically indiscernible from zero (see Column (2) in [Table 3](#)), the separation of LTV policy events over time clearly shows that this result reflects the exceptional response to the 2016 event. With the additional exception of the initializing 2010 policy shock, all other LTV tightenings ignited statistically and economically significantly lower average loan balances

**Table 4:** Average Effects across LTV Events

	LTV	Ln Balance	Ln Valuation
	(1)	(2)	(3)
$\mathbb{1}^{\text{LTV}} \cdot \mathbb{1}^{\text{Post}} \cdot \mathbb{1}^{\text{Treat 2010}}$	0.73** (0.31)	0.53 (2.06)	6.66*** (1.70)
$\mathbb{1}^{\text{LTV}} \cdot \mathbb{1}^{\text{Post}} \cdot \mathbb{1}^{\text{Treat 2013}}$	-5.88** (2.84)	-7.68** (3.52)	8.88*** (2.49)
$\mathbb{1}^{\text{LTV}} \cdot \mathbb{1}^{\text{Post}} \cdot \mathbb{1}^{\text{Treat 2014}}$	-4.64*** (0.85)	-1.70 (2.59)	-9.90 (10.19)
$\mathbb{1}^{\text{LTV}} \cdot \mathbb{1}^{\text{Post}} \cdot \mathbb{1}^{\text{Treat 2015}}$	-3.42*** (0.63)	-7.25*** (2.83)	22.54*** (1.93)
$\mathbb{1}^{\text{LTV}} \cdot \mathbb{1}^{\text{Post}} \cdot \mathbb{1}^{\text{Treat 2016}}$	1.59** (0.76)	13.25*** (3.82)	17.70*** (2.67)
$\mathbb{1}^{\text{LTV}} \cdot \mathbb{1}^{\text{Post}} \cdot \mathbb{1}^{\text{Treat 2017}}$	0.44 (0.62)	-15.83*** (3.18)	-11.04*** (2.38)
$\mathbb{1}^{\text{LTV}} \cdot \mathbb{1}^{\text{Post}} \cdot \mathbb{1}^{\text{Treat 2018}}$	-0.45 (0.78)	-6.79** (3.01)	-5.37*** (2.10)
No. of Loans	2,397,637	2,397,637	2,397,637
No. of NUTS-3 (Cluster for SE)	1,121	1,121	1,121
<b>Fixed Effects:</b>			
Calendar-time	Yes	Yes	Yes
Event-time	Yes	Yes	Yes
Originator	Yes	Yes	Yes
LTV Regulation Cohort	Yes	Yes	Yes
NUTS-3	Yes	Yes	Yes
Consecutive terms of $\sum_{l=1}^7 \left( \mathbb{1}_r^{\text{LTV}} \cdot \mathbb{1}_\tau^{\text{Post}} \cdot \mathbb{1}_{i,l}^{\text{Treat Event}} \right)$	Yes	Yes	Yes

**Notes:** This table presents coefficients and standard errors clustered at NUTS-3 level for an adjusted regression variant of Equation 3:

$$Y_{i,\tau,t} = \delta_l \sum_{l=1}^7 \left( \mathbb{1}_r^{\text{LTV}} \cdot \mathbb{1}_\tau^{\text{Post}} \cdot \mathbb{1}_{i,l}^{\text{Treat Event}} \right) + \gamma \left( \mathbb{1}_i^{\text{LTV}} \cdot \mathbb{1}_\tau^{\text{Post}} \right) + \mu_l \sum_{l=1}^7 \left( \mathbb{1}_{i,l}^{\text{Treat Event}} \cdot \mathbb{1}_\tau^{\text{Post}} \right) + \lambda_l \sum_{l=1}^7 \left( \mathbb{1}_i^{\text{LTV}} \cdot \mathbb{1}_{i,l}^{\text{Treat Event}} \right) + \vartheta_t + \vartheta_r + \vartheta_k + \vartheta_\tau + \vartheta_o + \epsilon_{i,\tau,t}.$$

We additionally introduce dummy variables  $\mathbb{1}^{\text{Treat Event}}$  which identify each LTV in the Netherlands separately. Above, we use  $\mathbb{1}^{\text{LTV}} \cdot \mathbb{1}^{\text{Post}} \cdot \mathbb{1}^{\text{Treat 2010}}$  to  $\mathbb{1}^{\text{LTV}} \cdot \mathbb{1}^{\text{Post}} \cdot \mathbb{1}^{\text{Treat 2018}}$  as names for the coefficients  $\delta_1$  to  $\delta_7$ . The dependent variables from left to right are: the loan-to-value ratio at origination; the natural logarithm of the loan balance at origination; and the natural logarithm of the valuation at origination. We include fixed effects at the event-month, calendar-month, originator, and NUTS-3 level. Each regression comprises 2,397,637 mortgage loans. Coefficients are scaled by 100 for log specifications to represent percentage changes.

at origination. Magnitudes range from 6.8% to 15.8% differential reductions in mean loan size, which implies an effective impetus on households to reduce their leverage in residential real estate acquisitions. At the same time, the hike by 13.2% in loan size in 2016 apparently

reflects a stark contradictory trend, which is hard to reconcile neither with any blatantly obvious differences in the regulatory details nor business or credit cycle peculiarities in the Netherlands, which are largely absorbed by the saturated fixed effect structure anyways.<sup>15</sup>

Third, the most striking result is a statistically and economically significant inflation of appraisal values in response to all but the two last LTV events. Estimated differential effects range in magnitude from 6.6% to as much as 22.5% for collateral pledged after the 2015 LTV tightening. Whereas the LTV changes in the last two years towards the end of the announced policy path are significantly negative, the overall pattern illustrates vividly that banks apparently responded to contractionary MPP measures by an, at times, very large hike in appraisal values of residential housing pledged as collateral.

## 5 Isolating a mechanism

The reduced-form regressions employed until here yield rather unequivocal evidence. Yet, all inference drawn so far is based on associations, which cannot be interpreted in a causal fashion. Therefore, we scrutinize in this section whether document collateral inflation after LTV shocks may simply reflect spurious correlations with structural drivers of residential real estate prices.

### 5.1 A unidirectional valuation shock: Earthquakes

To scrutinize our interpretation that banks embark on systematically more benevolent collateral appraisal after contractionary LTV shocks, we mobilize a quasi-natural experimental setting, which clearly depresses the value of residential real estate under all circumstances: earthquakes.

The event that generates exogenous variation in actual real estate values is related to the

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<sup>15</sup>We also investigate other loan traits at origination that may interact with supplied quantities, most notably pricing and probabilities of default. Specifying these alternative outcomes yields mostly insignificant and at most incoherent empirical evidence. Results are available upon request.

Groningen gas field, the largest natural gas resource in the European Union and one of the largest worldwide. Intensive exploitation of this field led to a destabilization and subsequent series of earthquakes in the surrounding region. On 08/16/2012 the Huizinge earthquake scored an unprecedented seismic strength of 3.6M, prompting a fundamental reassessment of the safety of continued gas extraction. In response, Dutch authorities ultimately decided to cease all gas extraction activities in Groningen by 2025. The Huizinge earthquake received extensive media coverage in the Netherlands and marked a tipping point in the public and policy debate that led to this decision (voor Veiligheid, 2015).

We use the Huizinge earthquake as an exogenous shock to property values in the affected regions surrounding the city of Groningen. Shortly after the earthquake, the Dutch State Supervision of Mines (SodM) and the Royal Netherlands Meteorological Institute (KNMI) issued updated risk assessments, stating that earthquakes of similar or greater magnitude had become increasingly likely (Muntendam-Bos and de Waal, 2013). These developments plausibly reduced property prices, with the effect contained to the affected region, for at least three main reasons. First, the Huizinge earthquake caused actual damage to various properties. The relatively low seismic strength implied that physical damage remained relatively small. Yet the incidence caused a substantial upward correction of the expected odds of further quakes in the future (voor Veiligheid, 2015). Second, it remained unsettled until February 2023, whether insurances and / or the government would compensate homeowners for incurred personal and physical damages.<sup>16</sup> Thus, not only did the likelihood of future damages increase, the uncertainty around insurance coverage and government compensation further depressed prices. Third, the constant monitoring of seismic activity and the more general public debate on gas extraction from the Groningen field reduced the significant information frictions about potential hazards associated with gas extraction that were in place prior to the Huizinge quake. As such, one could consider the incident and subsequent monitoring measures as an information shock that further depressed prices.

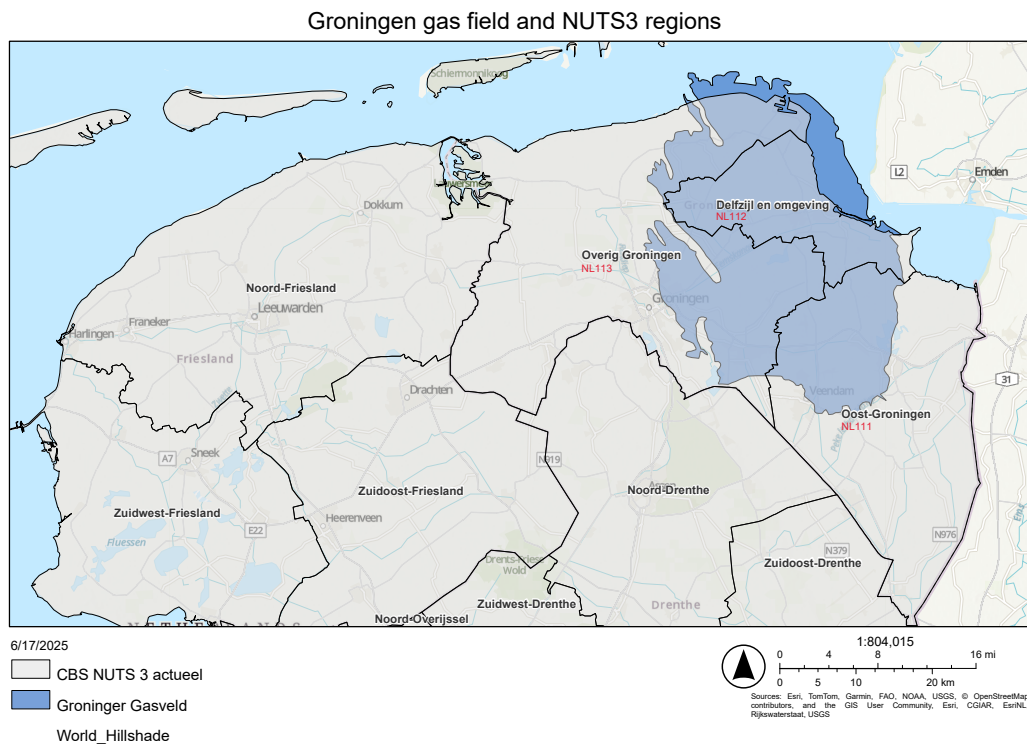
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<sup>16</sup>See [The Holland Times from June 4, 2023](#).

## 5.2 Empirical specification of the earthquake shock

Figure 4 shows the geographical location of the Groningen gas field, overlaid with NUTS3 regions, which serve as our regional unit of observation. The regions immediately impacted by the earthquake were NL111, NL112, and NL113. We define these regions as the treatment group and use the remaining 37 Dutch NUTS3 regions as the control group. The Huizinge earthquake, as well as numerous smaller earthquakes, all occurred within the area of the gas field and therefore within these specific NUTS3 regions.

**Figure 4: The Groningen gas field**



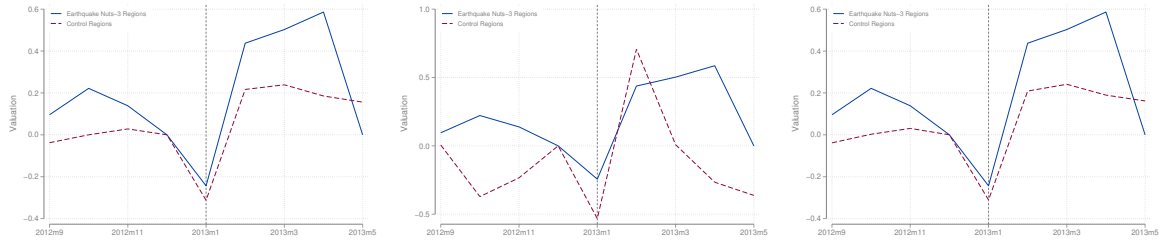
**Notes:** The Groningen gas field (dark blue shading) and the surrounding NUTS3 regions. The affected (treated) regions were NL111, NL112 and NL113.

To identify whether banks inflate collateral values after LTV shocks, we argue that any real estate asset pledged as collateral after the Huizinge earthquake should exhibit significantly lower appraisal values compared to properties in neighboring regions after an LTV shock. Figure 5 provides first descriptive evidence that this intuition is not supported by

the average valuation dynamics.

The left panel of [Figure 5](#) depicts the log-transformed average valuation of pledged properties in the three treated regions (solid blue line) in comparison to that of all other NUTS3 regions in the Netherlands (dashed red line). We show the convolution of these data four months before and after the LTV shock of January 2013. Average prices in the Groningen area contract significantly faster than those in the control group between September and December 2012, i.e. the period right after the earthquake and just before the LTV event. Whereas revealed higher geological risks paired with increased uncertainty regarding compensation policies and government support for physical and mental damages should entail a more pronounced reaction to contractionary MPP, the opposite is the case. Between January and May 2013, average house price growth in the three treated regions outpaced that in the rest of the Netherlands.

**Figure 5: Valuation around January 2013**



**Notes:** The figures show the time-series developments for NUTS-3 regions in the Netherlands over the  $\pm 4$  months surrounding the 2013 LTV regulation. In each figure, the solid blue line displays the development of the average valuation (log-transformed) of mortgage loans in the three NUTS-3 regions directly affected by the Huizinge earthquake. The dashed red lines illustrate the development of the average valuation for different comparison groups of NUTS-3 regions: all remaining NUTS-3 regions in the Netherlands (left); the NUTS-3 regions neighboring the earthquake-affected areas (middle); and the remaining NUTS-3 regions excluding these neighboring regions (right).

The middle and the right panel in [Figure 5](#) conduct the same comparison for two alternative control groups, namely directly neighboring ones and all other Dutch regions excluding directly neighboring ones. The descriptive evidence resembles very similar patterns, casting further doubts beyond the reduced form evidence presented in [subsection 4.3](#). Also in comparison to their direct spatial neighbors as well as only those farther away, earthquake regions' mean appraisal values grew faster.

We estimate the effect of an LTV shock in regions affected by the Huizinge earthquake before and after this regulatory shock more formally as follows

$$\text{Ln Valuation}_{i,t} = \delta \left( \mathbb{1}_i^{\text{LTV } 2013} \cdot \mathbb{1}_t^{\text{Post}} \right) + \vartheta_t + \vartheta_o + \vartheta_r + \epsilon_{i,t}. \quad (4)$$

Equation 4 is a 2x2 difference-in-difference regression explaining variation in mortgage loans' valuation around the 2013 LTV event.  $\mathbb{1}_{i,t}^{\text{LTV } 2013}$  separates loans  $i$  in the three NUTS-3 regions that experienced the Huizinge earthquake in 2012 and  $\mathbb{1}_t^{\text{Post}}$  is zero for September to December 2012 and one for January to May 2013. We include calendar-date  $t$ , originator  $o$ , and NUTS-3  $r$  fixed effects for the reasons explained in subsection 4.1.

**Table 5:** Average Effects for Valuations after the Huizinge earthquake

Control Group	Ln Valuation		
	Remaining NL	Neighboring NUTS-3	Remaining NL w/o Neigh.
$\mathbb{1}_i^{\text{LTV } 2013} \cdot \mathbb{1}_t^{\text{Post}}$	1.71 (4.44)	-1.62 (7.56)	1.81 (4.48)
No. of Loans	24,821	811	24,574
No. of NUTS-3 (Cluster for SE)	49	6	46
<b>Fixed Effects:</b>			
Calendar-time	Yes	Yes	Yes
Originator	Yes	Yes	Yes
NUTS-3	Yes	Yes	Yes

**Notes:** This table presents coefficients and standard errors clustered at the NUTS-3 level for regressions based on Equation 4. The dependent variable is the natural logarithm of the valuation at origination. The regressions use mortgage loans from the Netherlands only. The treatment group in each column consists of mortgage loans from the NUTS-3 regions directly affected by the Huizinge earthquake. Each column uses a different set of NUTS-3 regions as the control group: all remaining NUTS-3 regions in the Netherlands (1); the NUTS-3 regions neighboring the earthquake-affected areas (2); and the remaining NUTS-3 regions excluding these neighboring regions (3). Standard errors are clustered at the NUTS-3 level to capture potential heterogeneity arising from regulatory events. Coefficients are scaled by 100 for log specifications to represent percentage changes.

Recall that we use only treated loans in this specification, since the sample is restricted to the Netherlands. In this setting, the coefficient on  $\mathbb{1}_i^{\text{LTV } 2013} \cdot \mathbb{1}_t^{\text{Post}}$  captures the differential



change in valuation for loans inside versus outside the Huizinge regions before and after the 2013 LTV event. If banks finance residential real estate purchases in the Huizinge and price the increased risk relative to other Dutch regions, we should obtain a negative and significant coefficient.

Instead, we find no evidence of significantly different responses to LTV contractions in regions that were affected by the unidirectional, negative asset value shock for any of the three control groups. This absence of a more careful assessment of collateral that has been arguably subjected to higher risk is in line with the previous reduced-form evidence that valuations increase following the introduction of the LTV regulation. This interpretation also aligns with the time-series evidence in [Figure 5](#), which shows that valuations increase across all Dutch regions, with the largest increase observed in the Huizinge areas.

## 6 Conclusion

Our analysis provides insights into how banks respond to stricter LTV regulations and sheds light on the mechanism through which compliance at origination is achieved. Using loan-level data covering around 2.4 million mortgage originations around LTV tightenings, we show that banks comply with tighter LTVs caps by inflating collateral valuations, with only limited adjustment along loan size.

We corroborate this valuation-based compliance margin using earthquakes in the Netherlands as plausibly exogenous shocks to property values. Despite an *ex ante* prior that a sudden realization of seismic risk should depress collateral valuations, reported valuations at origination increase following LTV tightenings both inside and outside earthquake areas. Regression results further confirm that collateral values in earthquake regions are not significantly adjusted in response to LTV shocks when compared to unaffected regions. This absence of significant differences supports the interpretation of collateral value inflation as an important mechanism to comply with contractionary MPP.

At the NUTS-3 aggregate, tighter LTV caps reduce new mortgage lending: both the number and the total volume of originated loans decline after tightenings, in line with the policy objective. This contraction is uneven: where reported collateral valuations rise more strongly, the reduction is significantly smaller. In these areas, banks meet LTV limits by lowering reported LTVs via higher valuations rather than by cutting loan size, thereby diluting the intended restraint on credit growth. Thus, while LTV caps achieve *de jure* tightening at origination, inflated valuations weaken *de facto* macroprudential effectiveness.

These findings bear important ramifications for policymakers. Although stricter LTV caps reduce observed leverage on newly originated loans, their broader objective of containing credit growth – and thereby moderating house price inflation – may be less effective than intended. This is partly due to banks’ strategic behavior concerning collateral valuations, which highlights a more nuanced regulatory transmission mechanism: While average risk-taking diminishes, overall credit conditions may remain looser than policymakers anticipate.

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# Appendix

**Table A1:** Data Description

Name	ECB	ESMA	Description
originator	AR5	RREL82	Name of the underlying exposure originator
original_balance	AR66	RREL29	Original underlying exposure balance (inclusive of fees)
original_ltv	AR135	RREC16	Original underwritten loan To Value ratio
original_valuation	AR136	RREC17	Original valuation of the collateral

**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 Regulated	Mean Control	ND	Post-LTV Regulation Regulated	Mean Control	ND
LTV	86.3	78.1	0.25	82.9	77.8	0.15
Ln Balance	11.3	11.5	-0.18	11.2	11.5	-0.21
Ln Valuation	12.1	11.9	0.15	12.1	11.9	0.14
Balance	108.0	291.1	-0.09	105.2	199.5	-0.07
Valuation	237.1	296.8	-0.02	238.7	275.3	-0.02

**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  $\pm 0.25$  indicates no significant differences.



**Table A3:** Random Control Sample

	LTV	Ln Balance	Ln Valuation	LTV	Ln Balance	Ln Valuation
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}_r^{\text{LTV}} \cdot \mathbb{1}_\tau^{\text{Post}} \cdot \mathbb{1}_{i,t}^{\text{Treat}}$	-1.41*** (0.35)	-3.81* (2.15)	5.09** (1.95)	-1.41*** (0.36)	-3.78* (2.12)	5.18*** (1.96)
No. of Loans	1,256,168	1,256,168	1,256,168	1,252,257	1,252,257	1,252,257
No. of NUTS-3 (Cluster for SE)	1,116	1,116	1,116	1,114	1,114	1,114
<b>Fixed Effects:</b>						
Calendar-time	Yes	Yes	Yes	Yes	Yes	Yes
Event-time	Yes	Yes	Yes	Yes	Yes	Yes
Originator	Yes	Yes	Yes	Yes	Yes	Yes
LTV Regulation Cohort	Yes	Yes	Yes	Yes	Yes	Yes
NUTS-3	Yes	Yes	Yes	Yes	Yes	Yes
Consecutive terms of $\mathbb{1}_r^{\text{LTV}} \cdot \mathbb{1}_\tau^{\text{Post}} \cdot \mathbb{1}_{i,t}^{\text{Treat}}$	Yes	Yes	Yes	Yes	Yes	Yes

**Notes:** This table presents coefficients and standard errors clustered at NUTS-3 level for average 2x2 variants of [Equation 3](#):

$$Y_{i,\tau,t} = \delta \left( \mathbb{1}_r^{\text{LTV}} \cdot \mathbb{1}_\tau^{\text{Post}} \cdot \mathbb{1}_{i,t}^{\text{Treat}} \right) + \gamma \left( \mathbb{1}_i^{\text{LTV}} \cdot \mathbb{1}_\tau^{\text{Post}} \right) + \mu \left( \mathbb{1}_{i,t}^{\text{Treat}} \cdot \mathbb{1}_\tau^{\text{Post}} \right) + \lambda \left( \mathbb{1}_i^{\text{LTV}} \cdot \mathbb{1}_{i,t}^{\text{Treat}} \right) + \vartheta_t + \vartheta_r + \vartheta_k + \vartheta_\tau + \vartheta_o + \epsilon_{i,\tau,t}.$$

The dependent variables are the loan-to-value ratio at origination, the natural logarithm of the loan balance at origination, and the natural logarithm of the valuation at origination. In contrast to [Table 3](#), we randomly sample mortgage loans such that, for each calendar date, the total number of sampled loans equals the number of loans originated in the Netherlands. In Columns (1) to (3), we maintain the original country-level sample proportions. In Columns (4) to (6), we instead preserve the original cross-regional distribution across non-Dutch NUTS-3 regions. Coefficients are scaled by 100 for log specifications to represent percentage changes.

**Table A4:** Including LTV event in IE, PT, and BE

	LTV	Ln Balance	Ln Valuation	LTV	Ln Balance	Ln Valuation
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}_r^{LTV} \cdot \mathbb{1}_\tau^{Post} \cdot \mathbb{1}_{i,t}^{Treat}$	-0.49*	-2.94	7.36***			
$\mathbb{1}_r^{LTV} \cdot \mathbb{1}_\tau^{Post}$	(0.23)	(2.39)	(0.87)	-2.25***	-1.35*	5.54***
	(0.28)	(2.34)	(1.82)	(0.29)	(0.70)	(1.29)
No. of Loans	2,470,065	2,470,065	2,470,065	1,448,397	1,448,397	1,448,397
No. of NUTS-3 (Cluster for SE)	1,121	1,121	1,121	1,117	1,117	1,117
<b>Fixed Effects:</b>						
Calendar-time	Yes	Yes	Yes	Yes	Yes	Yes
Event-time	Yes	Yes	Yes	Yes	Yes	Yes
Originator	Yes	Yes	Yes	Yes	Yes	Yes
LTV Regulation Cohort	Yes	Yes	Yes	No	No	No
NUTS-3	Yes	Yes	Yes	Yes	Yes	Yes
Consecutive terms of $\mathbb{1}_r^{LTV} \cdot \mathbb{1}_\tau^{Post} \cdot \mathbb{1}_{i,t}^{Treat}$	Yes	Yes	Yes	na	na	na

**Notes:** This table presents regression results when we include Ireland, Portugal, and Belgium into the LTV-treatment group ( $\mathbb{1}_r^{LTV}$ ). According to Alam et al. (2025), those countries introduced stricter LTV regulation in our sample period too: Ireland in 2015, Portugal in 2018, and Belgium in 2020. The first three columns of this table presents coefficients and standard errors clustered at NUTS-3 level for average 2x2 variants of Equation 3:

$$Y_{i,\tau,t} = \delta \left( \mathbb{1}_r^{LTV} \cdot \mathbb{1}_\tau^{Post} \cdot \mathbb{1}_{i,t}^{Treat} \right) + \gamma \left( \mathbb{1}_i^{LTV} \cdot \mathbb{1}_\tau^{Post} \right) + \mu \left( \mathbb{1}_{i,t}^{Treat} \cdot \mathbb{1}_\tau^{Post} \right) + \lambda \left( \mathbb{1}_i^{LTV} \cdot \mathbb{1}_{i,t}^{Treat} \right) + \vartheta_t + \vartheta_r + \vartheta_k + \vartheta_\tau + \vartheta_o + \epsilon_{i,\tau,t}.$$

The last three columns are based on a different sampling. Here we only consider the calendar-time windows with an actual LTV tightening event and estimate:

$$Y_{i,\tau,t} = \delta \left( \mathbb{1}_r^{LTV} \cdot \mathbb{1}_\tau^{Post} \right) + \vartheta_t + \vartheta_r + \vartheta_\tau + \vartheta_o + \epsilon_{i,\tau,t}.$$

Across all columns, the dependent variables are the loan-to-value ratio at origination, the natural logarithm of the loan balance at origination, and the natural logarithm of the valuation at origination. Coefficients are scaled by 100 for log specifications to represent percentage changes.

**Table A5:** Fixed Effects Robustness

	LTV	Ln Balance	Ln Valuation	LTV	Ln Balance	Ln Valuation
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}_r^{\text{LTV}} \cdot \mathbb{1}_\tau^{\text{Post}} \cdot \mathbb{1}_{i,t}^{\text{Treat}}$	-0.84*** (0.30)	-2.23 (2.16)	6.92*** (1.77)	-1.65*** (0.34)	-1.00 (2.19)	6.90*** (2.04)
No. of Loans	2,397,637	2,397,637	2,397,637	2,397,637	2,397,637	2,397,637
No. of NUTS-3 (Cluster for SE)	1,121	1,121	1,121	1,121	1,121	1,121
<b>Fixed Effects:</b>						
Originator	No	No	No	Yes	Yes	Yes
Country×Date	No	No	No	Yes	Yes	Yes
Calendar-time	Yes	Yes	Yes	Yes	Yes	Yes
Event-time	Yes	Yes	Yes	Yes	Yes	Yes
LTV Regulation Cohort	Yes	Yes	Yes	Yes	Yes	Yes
NUTS-3	Yes	Yes	Yes	Yes	Yes	Yes
Consecutive terms of $\mathbb{1}_r^{\text{LTV}} \cdot \mathbb{1}_\tau^{\text{Post}} \cdot \mathbb{1}_{i,t}^{\text{Treat}}$	Yes	Yes	Yes	Yes	Yes	Yes

**Notes:** This table presents coefficients and standard errors clustered at NUTS-3 level for average 2x2 variants of [Equation 3](#):

$$Y_{i,\tau,t} = \delta \left( \mathbb{1}_r^{\text{LTV}} \cdot \mathbb{1}_\tau^{\text{Post}} \cdot \mathbb{1}_{i,t}^{\text{Treat}} \right) + \gamma \left( \mathbb{1}_i^{\text{LTV}} \cdot \mathbb{1}_\tau^{\text{Post}} \right) + \mu \left( \mathbb{1}_{i,t}^{\text{Treat}} \cdot \mathbb{1}_\tau^{\text{Post}} \right) + \lambda \left( \mathbb{1}_i^{\text{LTV}} \cdot \mathbb{1}_{i,t}^{\text{Treat}} \right) + \vartheta_t + \vartheta_r + \vartheta_k + \vartheta_\tau + \vartheta_o + \epsilon_{i,\tau,t}.$$

The dependent variables are the loan-to-value ratio at origination, the natural logarithm of the loan balance at origination, and the natural logarithm of the valuation at origination. In contrast to [Table 3](#), we vary the set of fixed effects. In Columns (1) to (3), we drop the originator fixed effects. In Columns (4) to (6), we augment the fixed-effects structure by including event-month–country fixed effects to directly account for any country-specific seasonality during the event periods. Coefficients are scaled by 100 for log specifications to represent percentage changes.

**Table A6:** Excluding UK and Using Only First Event in 2010

	LTV	Ln Balance	Ln Valuation	LTV	Ln Balance	Ln Valuation
	(1)	(2)	(3)	(4)	(5)	(6)
$\mathbb{1}_r^{\text{LTV}} \cdot \mathbb{1}_\tau^{\text{Post}} \cdot \mathbb{1}_{i,t}^{\text{Treat}}$	-1.05*** (0.29)	-2.32 (2.27)	8.08*** (1.71)			
$\mathbb{1}_r^{\text{LTV}} \cdot \mathbb{1}_\tau^{\text{Post}}$				-0.91** (0.38)	-1.92** (0.93)	1.49* (0.83)
No. of Loans	2,192,397	2,192,397	2,192,397	255,730	255,730	255,730
No. of NUTS-3 (Cluster for SE)	895	895	895	963	963	963
<b>Fixed Effects:</b>						
Calendar-time	Yes	Yes	Yes	Yes	Yes	Yes
Event-time	Yes	Yes	Yes	Yes	Yes	Yes
Originator	Yes	Yes	Yes	Yes	Yes	Yes
LTV Regulation Cohort	Yes	Yes	Yes	No	No	No
NUTS-3	Yes	Yes	Yes	Yes	Yes	Yes
Consecutive terms of $\mathbb{1}_r^{\text{LTV}} \cdot \mathbb{1}_\tau^{\text{Post}} \cdot \mathbb{1}_{i,t}^{\text{Treat}}$	Yes	Yes	Yes	na	na	na

**Notes:** The first three columns of this table presents coefficients and standard errors clustered at NUTS-3 level for average 2x2 variants of Equation 3 when we exclude loans originated in UK:

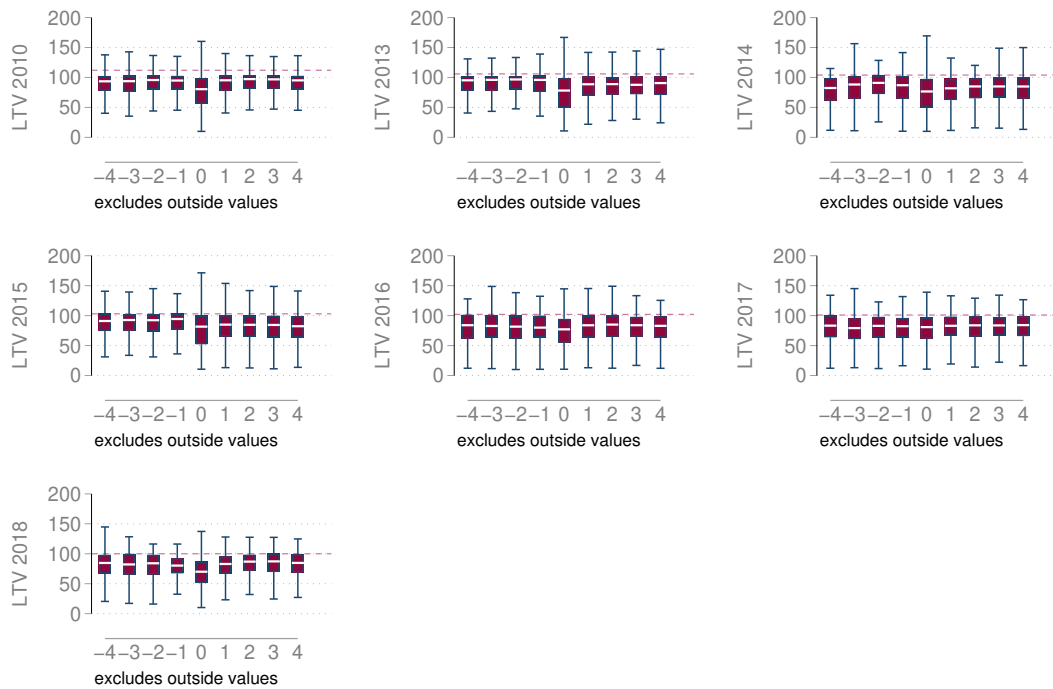
$$Y_{i,\tau,t} = \delta \left( \mathbb{1}_r^{\text{LTV}} \cdot \mathbb{1}_\tau^{\text{Post}} \cdot \mathbb{1}_{i,t}^{\text{Treat}} \right) + \gamma \left( \mathbb{1}_i^{\text{LTV}} \cdot \mathbb{1}_\tau^{\text{Post}} \right) + \mu \left( \mathbb{1}_{i,t}^{\text{Treat}} \cdot \mathbb{1}_\tau^{\text{Post}} \right) + \lambda \left( \mathbb{1}_i^{\text{LTV}} \cdot \mathbb{1}_{i,t}^{\text{Treat}} \right) + \vartheta_t + \vartheta_r + \vartheta_k + \vartheta_\tau + \vartheta_o + \epsilon_{i,\tau,t}.$$

The last three columns report estimates when we sample observations only for the  $pm$  months around the first LTV in NL in 2010:

$$Y_{i,\tau,t} = \delta \left( \mathbb{1}_r^{\text{LTV}} \cdot \mathbb{1}_\tau^{\text{Post}} \right) + \vartheta_t + \vartheta_r + \vartheta_\tau + \vartheta_o + \epsilon_{i,\tau,t}.$$

Across all columns, the dependent variables are the loan-to-value ratio at origination, the natural logarithm of the loan balance at origination, and the natural logarithm of the valuation at origination. Coefficients are scaled by 100 for log specifications to represent percentage changes.

**Figure A1: LTV Distribution in NL around Events**



**Notes:** This figure shows the distribution of mortgage loan LTVs in the Netherlands for each event month for each LTV event window. The dashed horizontal lines indicate the applicable LTV limit at each event.



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