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Effects of LTV announcements in EU economies [☆,☆☆]

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ABSTRACT

Earlier empirical studies on the effects of macroprudential policies focus on implementation dates and, in most cases, ignore potential anticipation effects. In this paper, we collect monthly data on announcements of loan-to-value (LTV) ratio restrictions covering 18 EU economies during the period 2000–2019. We show that announcements of LTV tightening policies can have a sizeable negative impact on household credit, house prices, and household durable goods consumption. We find that the estimated contractionary effects on household credit are driven by announcements with delayed implementation, whereas the effects on house prices are driven by announcements with quick implementation. We also show that the above contractionary effects are stronger following announcements of binding actions and actions without speed limits, suggesting that the design of macroprudential policies matters for their effectiveness.

1. Introduction

In response to the Global Financial Crisis (GFC), countries worldwide designed and put macroprudential policy frameworks in place to provide a macroeconomic and prudential approach to the supervision of the financial system as a whole. The EU, mainly because of its unique characteristics as an economic union, has been a front-runner in designing and implementing macroprudential policies. Not surprisingly, a proliferating literature on the effects of macroprudential policies is emerging, but the results are far from conclusive.

The previous literature for the aggregate effects of macroprudential policies has mostly relied on the use of cross-country panel data (Lim et al., 2011; Kuttner and Shim, 2016) by introducing macroprudential policies on the right-hand side of panel regressions either as a cumulative index (Akinci and Olmstead-Rumsey, 2018) or as a count of actions (Cerutti et al., 2017). Several recent papers focus on improving identification by paying particular attention to the exogeneity of macroprudential policy actions. By using the narrative identification approach (Romer and Romer, 2004; Romer and Romer, 2010), Richter

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et al. (2019) classify loan-to-value (LTV) measures under real and financial objectives. Similarly, Eickmeier et al. (2018) use the narrative approach to isolate exogenous changes in bank capital requirements.

The above empirical literature identifies macroprudential policies by using their implementation dates. However, authorities often implement macroprudential policies in a staggered way or with substantial implementation lags between the moment of their official announcement and their actual implementation. Empirical research on the effects of fiscal policy has shown that implementation lags can create strong anticipation effects, which in turn could invalidate inference (Ramey, 2011; Mertens and Ravn, 2012). Analogously, we argue that anticipation effects can be relevant for the identification of macroprudential policies, as authorities often announce measures and then commit to a plan for their implementation.

In this paper, we contribute to the existing literature by studying the effects of announcements of macroprudential policies. Specifically, we focus on one particular borrower-based measure, namely LTV ratio restrictions. There are two main reasons for this choice. First, LTV measures are the most used type of macroprudential policy targeting credit growth and imbalances in the housing market (Akinci and Olmstead-Rumsey, 2018; Cerutti et al., 2017). Secondly, these measures link directly with the theoretical literature on macroprudential policies that relies on collateral constraints (Mendoza, 2010; Bianchi and Mendoza, 2018). As such, we strive to provide empirical evidence for policy-making and the further development of theoretical models for macroprudential policy. Further, by focusing on LTV measures, we abstract from the interaction between various types of macroprudential policies.T hus, get clean results for the effects and the transmission mechanism of LTV measures following their announcement.

To improve identification, we investigate the effects of LTV announcements for a sample of 18 EU countries over the period 2000–2019 on household credit, house prices, and household consumption. We collect information from existing datasets on macroprudential policies and original sources, including information on the design of measures and their underlying motivation. At the same time, we extend the dataset to track announcements of other borrower-based macroprudential measures. To keep the amount of information manageable and maintain the cross-sectional consistency of our study, we focus on measures announced in EU economies. Thus, we keep a sample of comparable economies and get results that could have direct policy implications. Our dataset aims to complement the existing datasets on macroprudential policies and provide extensive information on their exact announcement dates, implementation schedule, and design.

Further, we go beyond the effects of LTV announcements on quantities and provide preliminary evidence for their impact on mortgage lending rates. Retail interest rates can contain information for the underlying risk of the portfolio (Morgan and Ashcraft, 2003), while they consist an important channel through which monetary policy operates. Using a sample of 32 advanced and emerging market economies, Kim and Mehrotra (2022) show that the reaction of lending rates differs by type of macroprudential policy actions. Differently from Kim and Mehrotra (2022), we use *new* mortgage interest rates rather than the outstanding lending rate to the private sector. Thus, we can better capture the immediate effects of these policies in the mortgage market. To the best of our knowledge, this is the first empirical study on the dynamic effects of macroprudential policies on the cost of credit for households.

Estimating local projections specifications (Jordà, 2005), we find that announcements of LTV ratio restrictions have strong and persistent effects on household credit and durable goods consumption. Even though our study relies on a cross-country dataset, the estimated effect on household consumption corroborates recent evidence on the effects of borrower-based measures on household balance sheets and behaviour (Aastveit et al., 2022; Tracey and Van Horen, 2021; Van Bekkum et al., 2019) suggesting that these policies may also induce broader macroeconomic effects. The impact of LTV announcements on house prices is more muted and only manifested with a time lag. Further, we find that new mortgage interest rates increase following a restrictive LTV announcement. This increase in interest rates, which in most cases is found to be statistically insignificant, could signal a shift towards riskier (among the permissible) borrowers. Recent literature (Acharya et al., 2022; Laufer and Tzur-Ilan, 2021) shows that borrower-based measures are associated with significant reallocation effects across markets and borrowers. Our results are robust to several checks, including using narrower measures of household credit, controlling for the announcement of other borrower-based measures,[1] and re-positioning the date of announcements to control for information leaks.

We proceed to study how the design of policy measures could alter their effects on the housing market and households' behaviour. First, we classify actions based on the timing of their implementation relative to the announcement date. We find that LTV announcements with delayed implementation drive the results for household credit, suggesting that banks might pre-adjust their lending to meet future requirements. In contrast, announcements with quick implementation drive our main results for house prices. This finding is consistent with a stronger signal of a change in the housing cycle provided by these announcements. Moreover, we investigate how other design features of actions, particularly their legal character and possible embedded exemptions, such as speed limits, can affect their effectiveness. We find that announcements of legally-binding measures and measures without speed limits are the most effective. Thus, the overall tightening intensity depends not only on the directional change in the maximum LTV limit but also on the overall LTV framework strictness.

Our results have several policy implications. First, they call for careful consideration of the timing between announcement and implementation, as anticipation effects can be significant. LTV ratio announcements can decrease real household credit and real house prices and induce households to limit their consumption of durable goods. Moreover, we show that not

[1] LTV measures are often implemented together with other macroprudential borrower-based measures. Our main specification controls for the implementation of other borrower-based measures. Moreover, in the robustness section we provide evidence that announcements of other borrower-based measures are unlikely to drive our results on the effects of LTV actions.

only the type of measures matters but also their design. Adding features to achieve secondary objectives, such as easing access to financing for certain borrower groups via speed limits, might undermine their ability to meet their primary aim. Similarly, measures based on soft law might not meet their objectives.

The remainder of the paper is organized as follows. Section 2 reviews the related literature. Section 3 provides details on our dataset of LTV announcements and on additional variables used in the paper. Section 4 describes our empirical methodology and Section 5 presents the main empirical results, which are followed by several robustness checks (Section 6). In Section 7 we show the reactions to different types of LTV announcements based on their timing and design. Finally, Section 8 concludes. Supplementary material containing details on the dataset and additional empirical results is included in Appendix A and the Online Appendix.

2. Literature review

Our work is closely related to the growing empirical literature on the effects of macroprudential policies on macroeconomic and financial aggregates (Galati and Moessner, 2018). This literature relies mostly on cross-country datasets and panel regressions.[2] Lim et al. (2011) conducted one of the first comprehensive studies on the impact of macroprudential policies, among them LTV policies, on measures of systemic risk using a panel of 49 advanced and emerging market economies. The authors find that LTV policies reduce the correlation between the growth rate of credit to the private sector and the growth rate of real GDP by 80%. Akinci and Olmstead-Rumsey (2018) constructed an aggregate monthly index for macroprudential policy stance for 57 economies. Using panel regressions, the authors show that LTV measures have significant negative effects on bank and housing credit and on house prices. Cerutti et al. (2017) document that borrower-based measures are more often employed in advanced economies and estimate that LTV ratio limits are more effective when credit growth is high. The authors find evidence that the effectiveness of the measures depends on the financial structure and the openness of the economy.[3]

Similarly to this paper, several studies focus on the effects of macroprudential policies on the housing market. Kuttner and Shim (2016) find that targeted policies, such as limits to LTV and debt-service-to-income ratio, can slow housing credit, but the evidence of the effectiveness of these policies on house prices is mixed. The authors show that only tightening LTV actions can curb house price appreciation. In a study focusing on southeastern European economies, Vandenbussche et al. (2015) provide little evidence for the effectiveness of LTV ratio measures in limiting credit and house price growth. Using a sample of Asian economies, Zhang and Zoli (2016) estimate that macroprudential policies were effective in curbing credit growth and house price inflation. Similarly, Morgan et al. (2019) use country-bank level panel regressions and show that LTV ratio actions can moderate mortgage credit growth.

A recent strand of the literature estimates the dynamic effects of macroprudential policies on financial and macroeconomic aggregates. Kim and Mehrotra (2018) employ a panel VAR with both endogenous and exogenous variables. The vector of endogenous variables includes real GDP, credit to the private sector, the consumer price index, the prudential policies index of Shim et al. (2013), and the monetary policy rate ordered as last. Identification of macroprudential policy shocks relies on the assumption that macroeconomic variables are contemporaneously exogenous to the monetary policy rate and the index of prudential policies. In their model, macroprudential policy is allowed to react not only to credit, but also to real GDP and the price level. The estimated impulse response functions show that macroprudential policies can have persistent effects on private sector credit, while they can affect output and the price level. Tillmann (2015) introduce the binary index of macroprudential policy actions, which contains tightening LTV and debt-to-income (DTI) actions, in a VAR model augmented with qualitative variables (Qual-VAR) together with real credit growth and real house price growth. Using the Qual-VAR the authors are able to estimate a continuous "latent probability" of macroprudential tightening from the binary index of tightening actions. Shocks are recovered by assuming that house prices and credit do not react contemporaneously to the latent macroprudential stance. Instead, Greenwood-Nimmo and Tarassow (2016) rely on a sign-restricted VAR to identify the effect of macroprudential policy shocks on the credit to GDP ratio. The authors assume that a macroprudential policy shock does not contemporaneously increase credit and asset prices and does not reduce banks' non-borrowed reserves.

A few recent papers have proposed using the narrative identification method (Romer and Romer, 1989; Romer and Romer, 2004; Romer and Romer, 2010) to tackle exogeneity issues relating to macroprudential policies. Richter et al. (2019) study LTV measures and classify them according to their stated objective and then estimate the effects of macroprudential policies on output and inflation. The authors define exogenous LTV measures as actions that are unpredictable with respect to current and lagged real variables. Klingelhöfer and Sun (2019) use the narrative approach to disentangle macro-

[2] Another strand of the literature has resorted to the use of microeconomic data and microeconometric techniques, and has studied, among others, the effects of macroprudential policies on credit growth and house prices (Ayyagari et al., 2017; Laufer and Tzur-Ilan, 2021; Johnson, 2020), credit allocation (Jiménez et al., 2017; Acharya et al., 2022), their cross-border effects (Aiyar et al., 2014; Buch et al., 2017), and their interaction with monetary policy (Aiyar et al., 2016; Altavilla et al., 2504). Further, researchers have investigated the effects of macroprudential policies for both developed and emerging economies, as well as their interaction with the business cycle (Claessens et al., 2013).

[3] Apart from credit, the literature also studies cross-border and substitution effects. Dell'Ariccia et al. (2016) show that macroprudential policies, and not only LTV measures, can reduce the probability of credit booms, while Fendoglu (2017) find that borrower-based measures could contain credit cycles in emerging market economies. Beirne and Friedrich (2017) and Bruno and Shin (2015) find that macroprudential policies can reduce cross-border bank credit flows, while Cizel et al. (2019) show that after macroprudential measures non-bank credit substitutes the reduction in bank credit. Signs of regulatory arbitrage have been found by Reinhardt and Sowerbutts (2015).

prudential and monetary policy actions in China. They identify macroprudential policies as state-varying actions and target the credit cycle and the financial system's overall resilience. The set of identified macroprudential policy actions goes beyond LTV measures and includes reserve requirements, window guidance, supervisory pressure, and housing related policies. Eickmeier et al. (2018), reading of legislative documents, identify changes in the aggregate US bank capital ratio, which are unrelated to the real and financial cycles.

The above empirical literature focuses on the effects of policies at implementation and, in most cases, ignores potential anticipation effects, with some exceptions. Addressing anticipation, Richter et al. (2019) find that, when using announcement dates instead of implementation dates for LTV ratio measures, the estimated effects on output and inflation do not differ. However, in their dataset of advanced and emerging market economies, which covers the period 1990–2012, only a handful of actions are announced and implemented in different quarters. Specifically, two actions were taken in Canada and one in Hong Kong, while the lag between announcement and implementation for these actions was only one quarter. In contrast, Eickmeier et al. (2018) find that anticipation effects stemming from lengthy consultation processes seem to matter not only for the reaction of banks but also for the reactions of non-financial corporations and central banks alike. Unlike previous studies, we aim to precisely track each measure's exact announcement date and implementation schedule to address anticipation effects explicitly.

Our work is also inspired by the empirical identification methods used in fiscal policies. Like several macroprudential policies in our dataset, fiscal measures are announced and implemented with significant time lags and in a multiyear fashion. In contrast to the literature on macroprudential policies, the literature on fiscal policies has extensively studied anticipation effects and their implications for structural identification. Leeper et al. (2013) show that foresight about future macroeconomic fundamentals can pose severe challenges for structural identification. Ramey (2011) argues that anticipation effects are essential for correctly identifying government spending shocks, while Mertens and Ravn (2012) find significant anticipation effects on real activity.

With our study, we revisit the empirical evidence of borrower-based macroprudential policies on households and the housing market by focusing on the announcement of LTV measures. In order to improve the empirical identification and explicitly account for anticipation effects, we collect detailed information on the exact timing of LTV announcements. Further, we add to the above literature by estimating the effects of LTV policies not only on quantities but also on the cost of mortgage credit. Retail interest rates can contain information for the underlying risk of the portfolio, while they are an important channel through which monetary policy operates.

3. Data

3.1. Dataset on LTV ratio announcements

This section describes the dataset on LTV ratio announcements and presents some of its properties. Implementations of restrictions on LTV ratios are often staggered and the announcement date does not correspond to the start of the implementation plan. In what follows we provide some particularly revealing examples to clarify how our identification strategy differs from most of the existing empirical literature on the effects of macroprudential policies.

The first example concerns actions with an implementation gap, such as those taken by the Central Bank of Estonia in 2014 and the Finnish Financial Supervisory Authority in 2018. The Estonian policy measure was announced on the 12th of December 2014 via a Governor's Decree stating that as of the 1$^{\text{th}}$ of March 2015, banks operating in Estonia had to comply with an LTV ratio limit of 85% when issuing new mortgage loans. Another example of a policy with some implementation lag is the Finnish measure which, announced on the 19th of March 2018, only entered into force 15 weeks later.

The second example concerns an action with a staggered implementation plan. The measure was announced by the Dutch authorities in the official government gazette on 20th of December 2012. The decision stated the following: "*The maximum amount of the mortgage loan in relation to the value of the home is: a. 105 percent from January 1, 2013; b. 104 percent from January 1, 2014; c. 103 percent from January 1, 2015; d. 102 percent from January 1, 2016; e. 101 percent from January 1, 2017. [...] The maximum amount of the mortgage loan in relation to the value of the home is one hundred percent from 1 January 2018.*" The above LTV measure is typically coded as six different actions in existing studies on macroprudential policies. However, all six actions were announced in December 2012, and, as a result, the changes in the maximum LTV ratio limit were fully anticipated at the date of their implementation.

To deal with the above identification issues, we recover announcement dates for each of the LTV ratio restrictions implemented in 18 EU countries from 2000–2019, as well as the exact implementation plan of each action. Apart from the timing of announcements and implementation, we also recover information on the design of actions, their legal status, the deciding authority, and their underlying motivation. Further, we provide information on the numerical level of maximum LTV ratio limits applicable after each action.[4]

We focus on a European sample for several reasons. So far, studies on the dynamic effects of borrower-based measures have focused either on individual countries or panels of countries containing actions mostly for emerging market economies,

[4] The detailed description of actions, together with additional classifications, announcement and implementation dates, as well as underlying motivation, is provided in the Online Appendix.

limiting thus the ability to generalize the research findings to the European context. On the other hand, EU economies are a relatively uniform sample of countries. Over the last few years, these countries have established common systemic risk oversight bodies (both formal and informal) and have agreed and implemented institutional frameworks to exercise macroprudential policies.[5] Another important reason is that the information availability of sources regarding the details of policy actions is relatively rich, while data definitions are also more uniform.

As a starting point, we consolidate information on implemented actions found in the datasets of Cerutti et al. (2017) and Cerutti et al. (2016), the IMF Annual Macroprudential Policies database, Kuttner and Shim (2016), and the ESRB Macroprudential Policies Database. The reason is that a few differences are present across the above datasets and the full information regarding the announcement, the design, and the process of decision (e.g.. consultation, reasoning) is not present in a single dataset for macroprudential policies. We cross-check the completeness of our sample of actions with the newly published dataset of Alam et al. (2019).

In the second stage, we collect detailed information on LTV ratio actions by recovering the underlying documentation, mainly official decisions and/or legislation acts. Our goal is to pinpoint the exact announcement date of actions and their envisaged implementation schedule. We chose to set the announcement date to the date on which the final rule was made known to the public. In the case of official decisions, we set the announcement date to the date of the press release. In the case of legislation acts, we set the announcement date to the date in which the act was passed or published in the official government gazette.

Details regarding the measures were possibly made known to the public or the industry well ahead of the final decision date of the measure. To ensure that the identified announcement dates are accurate, we search for the existence of a consultation process launched ahead of the measures. We find that a consultation process preceded only three measures.[6] The consultation documents notified the proposals of authorities to take action and provided details about the exact limits and designs of the measures. The proposals presented to the public were very close to the final measures decided by the authorities. In one case, the consultation documents included instructions about supervisory expectations for the behavior of lenders. The Irish Central Bank, in its consultation paper, instructed *"lenders to take into account now the likely introduction of such regime (e.g.. LTV and LTI limits) and begin to adapt their lending practices already in anticipation of its introduction.*" For Finland, the Finnish Financial Supervisory Authority (FSA) did not launch a public consultation process but instead asked directly for the involved financial institutions' opinions.

Nevertheless, only a few authorities launched consultation processes. For the Netherlands, the intention of the government to adopt LTV measures and other instruments to limit risks in the mortgage market was included in the fiscal consolidation plans. Even though the measures were only decided in December 2012, they were already included in the Fiscal Consolidation Plans as early as April 2012, which were discussed in Parliament. Thus, it is possible that proposals presented via a consultation process or other institutional channels constituted a de facto announcement of the measures. To tackle this issue, we perform robustness checks in which we set the decision dates to the date of public consultation or in the case of the Netherlands to the date the final Stability Programme (Annual Budgetary Framework) was sent to the Parliament and the European Commission. We find that our baseline reactions are qualitatively and quantitatively robust to these changes.

Moreover, it is possible that details of measures leaked well ahead of their final announcement via other channels, such as the press. However, given the lack of extensive media and information coverage on macroprudential policy measures and the relatively large number of countries we aim to cover, it is difficult to ensure that there are no information leaks ahead of our identified announcement dates. If the latter is true, our index of announced LTV actions will not track unanticipated LTV actions creating potential concerns about our identification strategy. In our empirical study, we transform the monthly index into the quarterly frequency. Thus, for actions announced in the second and third month of a quarter, we capture information leaks up to one and two months before the announcement. However, we might miss a potential earlier announcement for actions announced during the first month of the quarter. For example, the National Bank of Slovakia issued its "*Recommendation in the area of macroprudential policy on risks related to market developments in retail lending*" on October 7, 2014. If there had been an information release not captured by our sources, it is possible that it would have happened in the previous quarter. To account for these potential information leakages, we set the announcement date of measures announced during the first month of a quarter to the previous quarter.[7]

To study the effects of announcements, we create a quarterly dummy index that indicates whether an LTV measure was announced within a quarter. More specifically, we combine in the same dummy unanticipated actions announced and implemented in the same quarter and actions announced in the current quarter, but implemented in subsequent quarters. If a measure is announced in the fourth quarter of a certain year and implemented gradually in the second, third, and fourth quarter of the next year, we only assign the value "1" at the fourth quarter of the year of the announcement. Thus, the dummy indicates whether an *action plan* was announced irrespective of its implementation schedule. Our measure of announced actions is denoted as $ltv_{i,t}^{ann} = \mathbf{1}\{\Delta LTV_{i,t,s} < 0\}_{t \leqslant s}$, where $\Delta LTV_{i,t,s}$ is the decrease in the maximum LTV ratio limit announced in quarter t and implemented in quarter s by country i. Additionally, we construct a dummy variable for

[5] For more details on the institutional framework surrounding the design of macroprudential frameworks in the EU see European Systemic Risk Board (2014).

[6] The three affected measures were taken in Ireland in 2015, Finland in 2018, and Denmark 2014. The measures taken in the Netherlands were not subject to a consultation process, but they were included in the fiscal consolidation plans and were discussed in the Parliament. Details are provided in the Online Appendix.

[7] This strategy, which is also used in the fiscal policy literature (Ramey, 2011), leads to a reassignment of five announced LTV ratio measures.

pre-announced or anticipated LTV measures. This variable takes the value "1" if an LTV measure was implemented within a quarter, but it was announced in previous quarters. Analogously, our measure of implemented and anticipated actions for country i is denoted as $ltv_{i,t}^{ant} = \mathbf{1}\{\Delta LTV_{i,t,s} < 0\}_{t>s}$, where now t denotes the implementation quarter and s the announcement quarter. Finally, we define a dummy for all implementations of LTV tightening measures regardless of their timing of announcement as $ltv_{i,t}^{impl} = \mathbf{1}\{\Delta LTV_{i,t} < 0\}$ where t is the implementation quarter.

We use a dummy variable approach rather than a quantified measure of the LTV announcements because of the relatively high degree of subjectivity needed to quantify changes in the maximum LTV ratio. Quantifying the changes in LTV limits could introduce significant measurement errors and make our empirical results sensitive to the chosen quantification approach. Alam et al. (2019) construct time series of average LTV ratios per country as the average of known LTV limits to different types of loans in a given quarter. However, it is difficult to ensure that the calculated average limit is the actual limit applying to a particular country in a given quarter and to ensure that the quantitative measure captures all products or financing sources that might be used as substitutes for housing borrowing. Further, we would also need to make assumptions to quantify these actions that introduced LTV limits for the first time. Alam et al. (2019) assume that the implicit LTV limit before the introduction of the actual limits was 100%, while Richter et al. (2019) opt to discard these actions. With the first approach, we would introduce potential measurement errors, while with the second approach, we would lose potentially valuable observations. Both approaches could have implications for our empirical results due to the relatively small number of LTV announcements in hand.[8]

3.2. Properties of the dataset on announcements

The dataset contains 32 announcements for tightening measures of LTV ratio limits associated with 46 implemented tightening actions LTV limits. Even though we collected information for all EU economies, only 18 countries in our sample took at least one tightening LTV-related measure between 2000 and the second quarter of 2019. Among these actions, we observe significant variation in the timing between announcement and implementation. To illustrate this difference, Fig. 1 shows the aggregated indices of announced and implemented LTV ratio actions. We identify three groups of countries for the timing between announcement and implementation dates (see Fig. 2): countries that announced and implemented at the same quarter, countries which opted to delay the implementation of the announced actions, and countries which opted for a staggered implementation schedule (phase-in). The fact that some measures are announced and implemented in a later date introduces foresight from the side of borrowers and lenders. Further, in the case of a phase-in implementation plan, future shifts in LTV limits are not independent of past or future shifts implemented as part of the same LTV measure. Therefore, macroprudential policy actions are fully anticipated in the latter two cases and cannot be used for valid identification. With our identification strategy and our focus on announced changes in LTV limits, we aim to purge our LTV variable from anticipation effects.

We recognize that several LTV ratio actions were implemented together with actions related to other borrower-based measures forming a package of actions. Additionally, some LTV ratio actions were preceded or followed by announcements of other borrower-based measures (see Fig. 3). The dataset contains 37 borrower-based actions (both tightening and loosening) implemented along LTV measures. These additional measures correspond to 32 announcements, of which 16 coincide with the announcement of LTV measures, and 16 are announcements of other stand-alone borrower-based measures. The omission of other borrower-based measures could pose problems for our identification strategy. To alleviate such concerns, our baseline specification controls for the implementation of other borrower-based measures. In addition, in the robustness section we show that the baseline results do not change significantly when we control for the announcement of other borrower-based measures.

Regarding the design of LTV measures, we focus on their legal character and an option called *speed limit* (see Fig. 4). The majority of the announced LTV measures (23 actions from a total of 32 actions) had a binding character.[9] The so-called speed limit allows a certain percentage of new mortgage loan production to exceed the maximum LTV ratio limit. This feature is designed to reduce the effects of LTV ratio restrictions on certain groups of credit-constrained households, such as first-time borrowers. In total, we identify 9 LTV announcements of measures with a speed limit.

3.3. Data on dependent variables and controls

We collect data on total credit and loans to households from the BIS and the Quarterly Sectoral Accounts available on the ECB SDW. We obtain real house prices from the BIS and collect data on household consumption from Eurostat. We deflate the credit and consumption time series using the Harmonized Consumer Price Index (HCPI) downloaded from Eurostat. These time-series are available at a quarterly frequency. We enrich the dataset with data on real GDP growth rates from Eurostat, long-term rates from the ECB Long-term Interest Rate Statistics (IRS), 3-month interbank rates, and monetary policy rates from Refinitv Eikon. Descriptive statistics are shown in Table 1.

[8] With these caveats in mind, we construct a numerical counterpart of our dummy index and show the results in the Online Appendix.

[9] We classify as non-binding actions based on soft law, such as Recommendations or Guidelines.

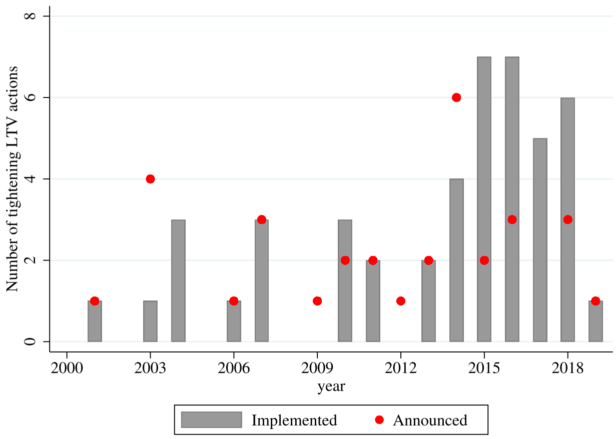

Fig. 1. Yearly number of implemented and announced LTV actions.

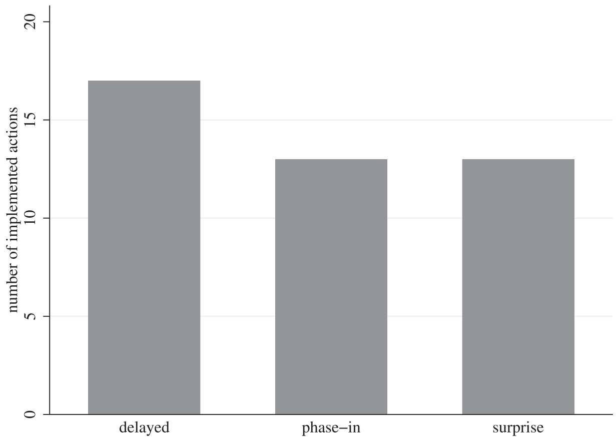

Fig. 2. Implementation schedule of LTV actions.

We also investigate LTV measures' effects on the cost of mortgage credit. We use *new* lending rates since these are the most likely to be affected by borrower-based measures. We create these series by combining data from the ECB's Monetary Interest Rates (MIR) dataset and national central banks' data. Consistently with the credit, housing, and macroeconomic variables, we aggregate these time series at a quarterly frequency.[10]

4. Methodology

We consider the following local projection (LP) (Jordà, 2005) specification at the quarterly frequency for each horizon $h = 0, 1, 2, \ldots, 16$,

$$\begin{aligned} \Delta_h y_{i,t} = \alpha_i^h \quad & + \tau_t^h + \sum_{p=0}^{4} \beta_p^h ltv_{i,t-p}^{ann} + \sum_{p=1}^{4} \gamma_p^h \Delta y_{i,t-p} \\ & + \sum_{p=0}^{h} \Theta_p^{1,h} Z_{i,t+h-p} + \sum_{p=0}^{4} \Theta_p^{2,h} X_{i,t-p} + \epsilon_{i,t+h} \end{aligned} \tag{1}$$

where $\Delta_h y_{i,t} = y_{i,t+h} - y_{i,t-1}$ is the cumulative change in the dependent variable from time $t-1$ to time $t+h$. $ltv_{i,t}^{ann}$ is the dummy index for announced LTV tightening measures described in Section 3.[11] We include country-fixed effects (α_i^h) to control for unobserved country-specific time-invariant characteristics, and time-fixed effects (τ_t^h) to account for common shocks

[10] We provide a detailed list of data sources in the Online Appendix.
[11] All dependent variables except for the interest rates are expressed in logs.

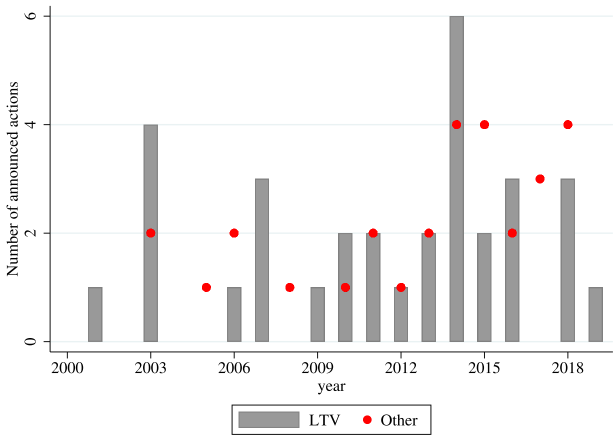

Fig. 3. Yearly number of announcements of tightening LTV actions and other borrower-based measures.

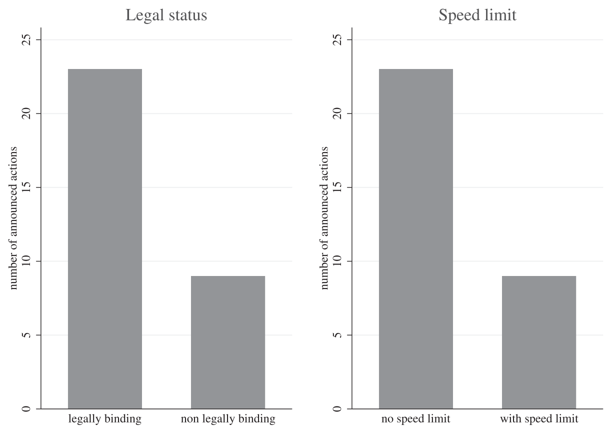

Fig. 4. Classification of announced LTV actions by legal status and design.

(like the GFC and the European sovereign debt crisis) affecting all countries in our sample. $\epsilon_{i,t+h}$ is the LP residual at horizon h. The impulse response function of y_t with respect to an announced change in the maximum LTV limit is given by the sequence of coefficients $\{\beta^h_{p=0}\}_{h\geqslant 0}$. To account for the relatively small number of countries for the estimation of clustered robust standard errors, we use the wild cluster bootstrap-t method proposed by Cameron et al. (2008), and derive the 90% confidence interval using the inverted statistic of the bootstrap-t procedure.

The vector of controls $X_{i,t}$ includes $ltv^{ant}_{i,t}$ described in Section 3, quarterly changes in the monetary policy rate, a dummy index for implementation of another borrower-based measures, and macro-financial variables specific to the dependent variable.[12] The vector of controls $Z_{i,t}$ includes $ltv^{impl}_{i,t}$, to control for implementation effects between time t and $t+h$, as well as implementations of other borrower-based measures between time t and $t+h$. Hence, we capture any effects from contemporaneous and subsequent implementations of other borrower-based actions which might have been activated along with the LTV restrictions.

Our dataset contains information on the underlying motivation of actions. We classify actions into those motivated by cyclical and structural aims. Under cyclical considerations, we observe that actions mainly were taken to target credit growth, borrower and lender resilience, systemic risk, asset prices, and credit standards or capital flows. Under structural considerations, we find that actions were taken for market regulation and consumer protection.[13] We observe that most actions were motivated by cyclical considerations related to the financial cycle. Thus, by using the narrative approach to isolate changes in the maximum LTV limits that are potentially exogenous to our main dependent variables, we would have to discard

[12] Consistently with the previous literature, we control for the quarterly inflation rate and the quarterly real GDP growth rate when modelling real credit to households, real house price growth, and real household consumption (or its components). For new mortgage lending rates, we control for the quarterly changes in the 3-month country-specific interbank rate, the country-specific long-term rate (10-year government bond yield), and a linear trend.

[13] We provide information on the motivation of actions in the Online Appendix.

Table 1
Descriptive statistics.

	Count	Mean	Std. Deviation	Minimum	Maximum
Real credit to households, qoq	908	0.86	2.47	−12.64	14.80
Real loans to households, qoq	908	0.82	2.97	−33.68	16.35
Real mortgage credit, qoq	638	1.29	3.14	−20.21	17.31
Real house prices, qoq	927	0.32	3.23	−25.11	22.63
New mortgage rate	923	3.83	1.72	0.81	12.87
Real household consumption, qoq	956	0.44	1.68	−14.27	6.54
Real household durable goods consumption, qoq	956	0.23	5.11	−54.48	31.06
Real household non-durable goods consumption, qoq	956	0.46	1.56	−13.16	6.54
New mortgage rate, fixed below 1 year	912	3.71	1.63	0.80	12.32
New mortgage rate, fixed above 1 year	830	4.36	1.95	1.00	12.91
Monetary policy rate	956	1.11	1.71	−0.75	10.33
3-month interbank rate	956	1.72	2.19	−0.56	15.70
Long-term interest rate	903	3.57	2.28	−0.03	14.50
Real GDP, qoq	956	0.55	1.59	−12.70	23.20
Inflation rate	956	0.47	1.08	−3.53	5.67

most of the LTV actions in our sample. To tackle these potential endogeneity concerns, we resort to the inverse propensity score weighted (IPW) estimator as in Jordà and Taylor (2016).[14] In short, the estimator consists of running the baseline LP specification by introducing regression weights derived from a first-stage regression. The idea is to assign more weight to these actions that were the least predictable at the time of their announcement based on observables and variables which might enter policy makers' reaction function.

In practice, we follow a two-step approach. In the first stage, we estimate a logit model predicting announced tightening actions with a set of controls $X_{i,t}$ which includes the annual growth rate in deflated total credit to households, the annual growth rate in real house price index, the lagged annual growth rate in real GDP, and up to four lags of past borrower-based actions, including LTV ratio actions. We include country-specific fixed effects (α_i) to account for the relatively larger propensity of some countries to use macroprudential policies. Using the predicted probabilities from the first-stage model, we construct the following regression weights:

$$\hat{weight}_{i,t} = \frac{\mathbf{1}\{ltv^{ann}_{i,t} = 1\}}{\hat{prob}^{tight}(X_{i,t}; \hat{\beta}, \hat{\alpha}_i)} - \frac{\mathbf{1}\{ltv^{ann}_{i,t} = 0\}}{1 - \hat{prob}^{tight}(X_{i,t}; \hat{\beta}, \hat{\alpha}_i)}. \tag{2}$$

These weights are then used in the second stage in which we run LP specifications.[15]

5. Empirical Results

Fig. 5 presents the cumulative responses of real household credit and real house prices after the announcement of tightening LTV actions. We find that these measures have a strong negative effect on real household credit immediately after their announcement.[16] Similarly, real house prices respond by decreasing following the announcement of a tightening LTV action, but in a more sluggish way. This result is in line with the fact that LTV policies primarily target credit, while they affect house prices by reducing lending to households with some lags. The response of house prices is becoming more pronounced after eight quarters following the announcement when real household credit has almost reached its minimum cumulative reaction. The cumulative response of real credit to households reaches a statistically significant cumulative response of approximately −5.2% ten quarters after the announcement of the LTV announcement. House prices reach the cumulative response of −8% at the end of our 16 quarters projections horizon. The response is statistically significant at the 90% confidence level after six quarters.

Although we focus on LTV announcements and use a different sample of countries, our estimated responses of credit are qualitatively and quantitatively similar to the IRFs presented by Richter et al. (2019), who find a minimum cumulative impact on household credit of around −6%. For EU economies, Poghosyan (2020) finds no effect of borrower-based measures on the total claims to the private non-financial sector. However, the results are not directly comparable, not only due to our

[14] The estimator has been used by Richter et al. (2019) and Poghosyan (2020) for the study of macroprudential policies.

[15] With the exceptions of the country-specific fixed effects, we find that the propensity to announce LTV actions is weakly related to our set of controls (see Table A.1). However, Fig. A.1 shows that the empirical distributions of the control and treated units exhibit sufficient overlap. If announcements were randomized, then the empirical distributions of the control and treated units (country-quarters with an announcement) would be uniform and overlap perfectly. If announcements were fully predictable on the basis of observables, then the distribution of the treated units would have its mass at 1 and the distribution of the control units would have its mass at 0. The overlap indicates that the two-stage approach can provide some benefits for the identification of LTV announcements. This is confirmed by Fig. A.10, which shows that when we use unweighted local projections, the results are very similar, but, at the same time, the standard errors are slightly larger. For a thorough discussion of the method see Angrist et al. (2018) and Jordà and Taylor (2016).

[16] More detailed results are presented in Table A.2,A.3, A.4.

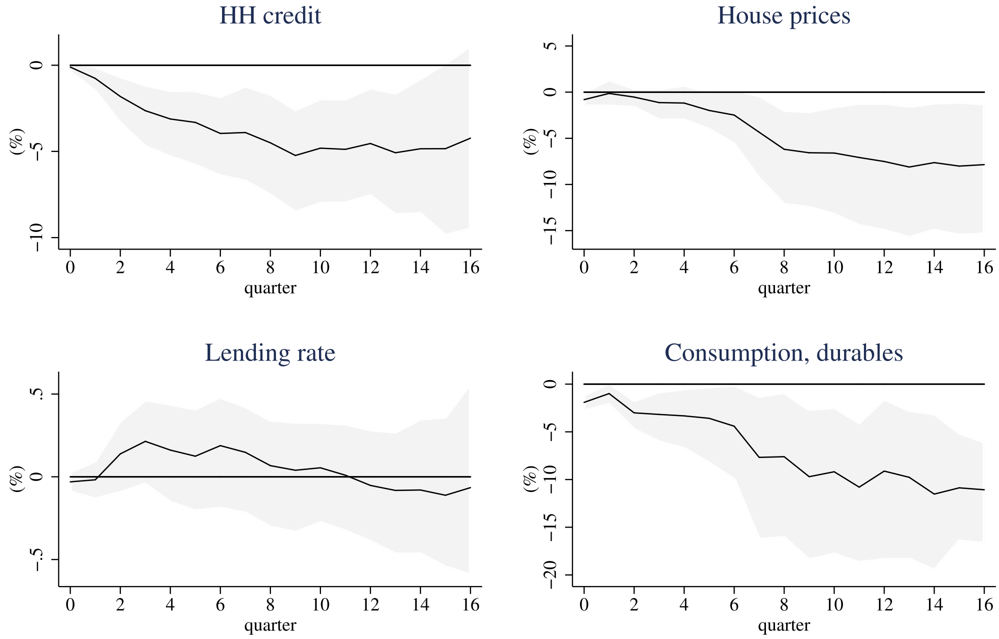

Fig. 5. Responses to an announcement of tightening in the maximum LTV ratio. *Notes:* Shaded areas correspond to the 90% confidence interval derived from inverted wild cluster bootstrap-t statistic clustered at the country level.

focus on announcement dates but also because we only use LTV measures and our dependent variable is household credit. Regarding the response of house prices, our results suggest a delayed response as in Poghosyan (2020), but the response magnitude is comparable to Richter et al. (2019). Nevertheless, the types of measures and their timing are different.

To better understand the channels via which LTV policies affect household behavior and potentially the economy, in the remainder of this section, we estimate the effects of LTV announcements on household consumption and mortgage lending rates. Regarding the impact of macroprudential policies on consumption, Kim and Mehrotra (2018) show that contractionary macroprudential policy shocks have a negative and persistent impact on real private consumption in a sample of inflation targeting Asian-Pacific economies. In a more recent study, however, Kim and Mehrotra (2022) show that contractionary macroprudential policy shocks in a broader sample of 32 advanced and emerging market economies do not affect private consumption. This result also holds when the authors use LTV limits as the macroprudential policy shock.

We focus on household durable goods consumption due to its link with housing. We show the response in the lower right panel of Fig. 5. Additionally, we plot the response of aggregated real household consumption and real household consumption excluding durable goods in Fig. 6.[17] We find that LTV policies affect aggregate real household consumption, reaching a minimum cumulative response of 2.8% while the response is statistically significant at the 90% confidence level.

We find that the impact of LTV restrictions varies widely by type of consumption, with their dynamic responses showing a different impact. Consumption of durable goods reacts immediately after the announcement of a tightening in the maximum LTV limit, while the estimated reaction is persistent. The minimum cumulative response is approximately -11.5% and reached after 14 quarters. On the other hand, household consumption excluding durable goods does not react immediately following an announcement. At the same time, its minimum cumulative response is much lower at -2.5% in line with the response of aggregate real household goods consumption. Our results corroborate recent evidence by Tracey and Van Horen (2021) who show that relaxing down-payment requirements in the UK led to an increase in household consumption in the areas most exposed to the policy.

Next, we analyse the impact of tightening LTV announcements on the cost of credit and specifically the level of *new* mortgage lending rates. Only a few studies look at macroprudential policies' effects on lending rates. Using microdata from Ireland, Acharya et al. (2022) find that LTV ratio actions affect the cost of credit differently for high and low-income households, with banks offering lower lending rates to higher-income households and higher lending rates to lower-income households which are *ex-post* more constrained by the policy. Similarly, using microdata for Israel, Laufer and Tzur-Ilan (2021) and Tzur-Ilan (2017) show that borrowers shifted to properties in lower quality areas, and paid higher interest rates for their mortgages after the introduction of LTV-dependent risk weights in Israel. The differential impact of LTV policies on borrowers and regions can help explain part of the effects on interest rates. Regarding aggregate effects, Kim and Mehrotra (2022) show that macroprudential policy actions increase lending rates to the private sector in a sample of 32 advanced and emerging

[17] Consumption excluding durable goods includes consumption expenditure for non-durable goods, semi-durable goods, and services. Consumption excluding durable goods amounts to around 90% of the total final nominal consumption expenditure in EU countries.

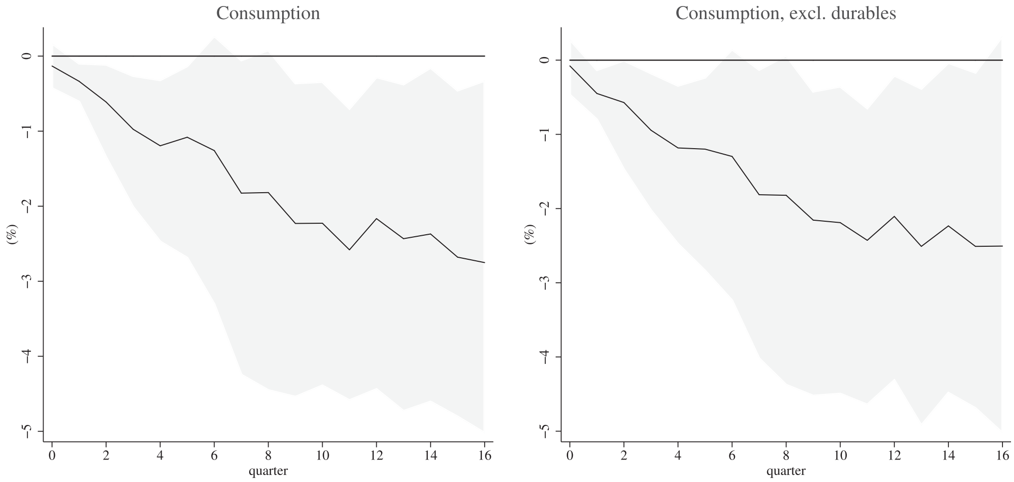

Fig. 6. Additional responses of real household consumption to an announcement of tightening in the maximum LTV ratio. *Notes*: Shaded areas correspond to the 90% confidence interval derived from inverted wild cluster bootstrap-t statistic clustered at the country level.

market economies. However, when the authors keep only actions operating via the asset side of financial institutions, such as LTV limits, they find that lending rates react in the opposite direction and decrease following a contractionary macroprudential policy. Our difference with Kim and Mehrotra (2022) is that we use *new* mortgage interest rates rather than the outstanding lending rate to the private sector. Thus, we can better capture the immediate effects of these policies in the mortgage market.

All in all, the response of lending rates will depend on several factors that affect the supply and demand for mortgage loans. It is possible that LTV ratio measures create a strong signal about the creditworthiness of borrowers and could thus change the risk perceptions of banks and consequently, their risk pricing. Ciccarelli et al. (2013) find that LTV ratio measures led to a tightening of lending standards in euro-area economies. On the other hand, if banks target a specific level of risk in their portfolio and the fact that overall gross returns might decrease after the policy, lenders might take more risks and start targeting the riskier among the pool of permissible borrowers.

We plot the reaction of *new* mortgage lending rates in the lower left panel of Fig. 5. We find that the announcement of a tightening LTV ratio action has a contemporaneous negative impact, but interest rates tend to increase in the quarters following the announcement. The cumulative response reaches a peak of around 0.2%, but it reverts to the baseline after 12 quarters. Nevertheless, the response of new mortgage lending rates is below the 90% statistical confidence level.[18]

Overall, we find sizeable effects on real household credit, real house prices, and real household consumption following the announcement of restrictive LTV ratio measures. The effects on real household consumption are particularly pronounced for the consumption of durable goods. We also find that lending rates tend to increase following the announcement of a restrictive LTV ratio policy and then decrease towards the end of the local projections horizon.

6. Robustness analysis

In this section, we perform a battery of robustness checks. First, we estimate the response of alternative measures of credit. For the baseline estimates, we use real household credit. The latter variable follows a broad definition, has a good sample coverage, and allows us to capture a wide set of credit instruments and potential substitutions among them.[19] Nevertheless, LTV actions target household loans and particularly real household mortgage credit. We present the responses of real household loans and real household mortgage credit in Fig. A.3.[20] We observe that these reactions are qualitatively very similar to the reaction of real household credit, which we use as our baseline variable. However, the responses of real household loans and real mortgage credit are stronger. Real household loans reach a minimum cumulative response of approximately -6.7% and real mortgage credit a minimum cumulative response of approximately -9%, lower than the approximately -5.2% minimum cumulative response of real household credit. These stronger cumulative responses are consistent with LTV policies targeting the narrower credit variables directly.

[18] To uncover potential asymmetries, we have additionally estimated the responses for interest rates of products with different interest rate fixation periods, above and below one year. These dynamic responses do not reveal a distinctive pattern. We show these results in Fig. A.2.

[19] For instance, authorities expressed concerns about the circumvention of the LTV restrictions via consumer credit. The Czech National Bank stated in its Recommendation that *"Providers should not circumvent the LTV limits through the concurrent provision of consumer credit relating to the residential property concerned above and beyond retail loans secured by property."* Therefore, a broader definition of household credit helps to capture substitution effects among credit instruments.

[20] Data on household mortgage credit are sourced from Richter et al. (2019) and cover the period up to 2015.

Second, we test whether our baseline results change with the choice of the underlying sample of countries. We estimate the baseline impulse response functions by discarding all observations for each country at the time. We report the reactions of our main variables in Fig. A.4 and the coefficient estimates in Table A.6, A.7, A.8, A.9. We observe that the responses are qualitatively similar to the baseline estimates and within the 90% confidence interval of our main estimates. The reaction of real household credit seems to be sensitive to the exclusion of three countries, but only after quarter 12 of the local projections horizon. On the other hand, the exclusion of Latvia appears to soften the reaction of real house prices after the announcement of a tightening LTV measure from quarter 8 of the local projections horizon and onward. Latvia announced LTV measures in 2007 and 2014, which from visual inspection had a strong negative impact on house prices. Thus, these actions appear to carry large information content in our relatively small sample of actions. Nevertheless, the reaction remains within the 90% confidence interval of our baseline estimates.

Third, we control for an alternative index of implemented LTV measures to test for the possible omission of implemented measures and whether the choice of included LTV measures affects our results. We replace our implementation index in our baseline local projections specification with the index of LTV measures obtained from the iMAPP dataset (Alam et al., 2019). This index includes 47 tightening actions between 2000 and 2019. We show these results in Fig. A.5. The estimated impulse response functions appear similar to our baseline estimates, but confidence intervals are wider than for the baseline estimates.

Fourth, we control for possible information leaks ahead of the actual announcement date of the measures. We carry out two types of checks to control for both formal and informal channels via which the public might have obtained information about authorities' intentions to take macroprudential actions. As described in Section 3, we replace the announcement dates of measures that were subject to consultation or appeared in other official documents to the date of these documents. Four actions are affected, but only for three actions the date of the consultation falls in an earlier quarter. Estimation results are presented in Fig. A.6. Responses appear to be similar to the baseline responses.

Additionally, to allow for earlier reactions of our dependent variables, we re-position the index of announced measures. It is possible that the public or credit institutions knew the details of LTV measures ahead of their finalization via informal channels. We set the announcement date of measures to the previous quarter if an action was announced in the first month of a quarter. For measures announced in the second and third month of a quarter, our specification already allows the dependent variables to react at least one month ahead. Re-positioning of announcement dates affects five announcements which correspond to around one-sixth of our sample. Results are presented in Fig. A.7. We observe that the estimated impulse response functions become noisier, while the reaction of house prices turns statistically insignificant. Nevertheless, the pattern of the reactions appears qualitatively similar to our baseline results.

Fifth, we check for another type of omitted variables bias stemming from the announcement of other borrower-based measures. Half of the announced tightening LTV measures were part of a package that included other measures targeting borrowers. At the same time, there were 16 announcements of other borrower-based measures not coinciding with announcements of LTV measures. Debt-service-to-income (DSTI) and loan-to-income (LTI) ratio restrictions were the most common policy measures beyond LTV restrictions. We perform the test by adding a dummy variable for announcements of non-LTV borrower-based measures as a control in the baseline specification. We present the results of this check in Fig. A.8. We observe that the estimated impulse response functions are qualitatively similar to our baseline estimates, with a noticeable difference. The reactions of new interest rates are sharper and more precisely estimated after we control for announcements of other borrower-based measures. In general, these results suggest that our main findings are unlikely to be driven by announcements of other borrower-based measures, which are often combined with LTV actions.

Then, we control for announcements of loosening LTV measures. In our study, we use the IPW estimator to address the potential endogeneity of LTV announcements. Due to the probability model involved in the first step of the IPW estimator, we only keep tightening actions. However, we record seven announcements of loosening actions in our dataset. To control whether the omission of these announcements can affect our results, we add a dummy for announcements of loosening LTV actions in the baseline specification. Similarly to tightening actions, we also add as controls a dummy for implementations loosening LTV actions between time t and $t + h$. We delete the IPW regression weights for this check because these correspond to the propensity to tighten LTV policies. Nevertheless, we maintain the same estimation sample for comparability with the baseline estimates. We present these results in Fig. A.10 and Fig. A.11. Deleting the IPW weights results in IRFs with wider confidence intervals. However, controlling for the announcement of loosening LTV actions does not seem to alter the estimation results suggesting that the effects of LTV announcements in our sample are driven mainly by tightening LTV actions.

Finally, we perform tests for the omission of additional country-specific variables which could drive the financial cycle but are not captured by the time-fixed effects nor our baseline time-varying control variables. We follow two approaches. First, we add crisis dummies into the right hand side of the local projections specifications. For definitions of crises we use the Laeven and Valencia (2020) dataset. The results are presented in Fig. A.12 and are very similar to the baseline results. Second, we control for aggregate country-specific credit conditions using the total credit to the private non-financial sector. The results shown in Fig. A.13 are again very close to the baseline estimates.

7. Asymmetric effects

When authorities decide on the appropriate macroprudential policy action, not only do they have to choose from a menu of available borrower-based measures, but they also have to set the key parameters of the chosen measure. In this section, we study how the design of macroprudential policy measures could alter their effects on households' behaviour and the housing market. From our reading of the announcement documents, we can identify three main design features shared across the cross-section of countries in our study. First, authorities set transition periods between the announcement and implementation. Second, measures have different legal standing, as some are taken as legally binding acts while others are taken as recommendations or guidelines. Third, authorities opt to introduce exceptions for particular groups of borrowers to relax borrowing constraints.

To estimate whether responses of the main dependent variables differ by type of LTV measures, we re-estimate the baseline LP specification and replace the index of announced measures with one index per type of measure. We repeat the analysis three times, one time for each type of classification. Thus, we rewrite the local projections specification as follows:

$$\begin{aligned}\Delta_h y_{i,t} &= \alpha_i^h + \tau_t^h + \sum_{p=0}^{4}\beta_p^{1,h} ltv_{i,t-p}^{ann,1} + \sum_{p=0}^{4}\beta_p^{2,h} ltv_{i,t-p}^{ann,2} \\ &+ \sum_{p=1}^{4}\gamma_p^h \Delta y_{i,t-p} + \sum_{p=0}^{h}\Theta_p^{1,h} Z_{i,t+h-p} + \sum_{p=0}^{4}\Theta_p^{2,h} X_{i,t-p} + \epsilon_{i,t+h}\end{aligned} \tag{3}$$

where $ltv_{i,t}^{ann,1}$ denotes the index of announced measures classified as having a delayed implementation, or binding measures, or measures without speed limits, while $ltv_{i,t}^{ann,2}$ denotes the index of announced measures with a quick implementation, or non-binding measures, or measures with speed limits. We retain the same control variables $X_{i,t}$ and $Z_{i,t}$ as in the baseline specification and the same first-stage regression weights. We produce impulse response functions by plotting the sequences of estimated coefficients $\{\beta_{p=0}^{1,h}\}_{h\geqslant 0}$ and $\{\beta_{p=0}^{2,h}\}_{h\geqslant 0}$.

First, we investigate whether the reactions differ depending on the implementation schedule. More specifically, we split the announcements index into two groups: the first group includes actions that are announced and implemented within one quarter after the announcement (e.g. announcements with quick implementation), and the second group contains announcements of measures with the first implementation in subsequent quarters and includes measures with staggered implementation plans (e.g. announcements with delayed implementation).[21] The average time gap in quarters between implemented changes to the maximum LTV limit and their announcement is 6.5 quarters. We plot the responses of our main variables of interest to announcements with quick and delayed implementation in Fig. 7.[22] Although the results of this split should be assessed with caution due to the small number of announcements in each group, the estimated impulse responses reveal interesting differences. Real household credit decreases following the announcement of measures with delayed implementation, but it does not react significantly to announcements of measures with quick implementation. At the same time, we observe that the decrease in real house prices is mainly driven by announcements of actions with quick implementation. A plausible interpretation of these findings is the following. If the signal of a change in the housing cycle is stronger for the announcements with quick implementation, this might explain immediate reactions in house prices, whereas longer-term credit effects are controlled for by the specification (as implementation happens shortly after and is controlled for). On the other hand, for announcements with delayed implementation, banks might pre-adjust their lending to meet future requirements, but the signal is less strong, making house price reactions more muted.

In the next split, we focus on the responses to announcements of legally binding measures and measures relying on soft-law. We classify measures as non-binding when they rely on soft-law, such as recommendations or supervisory guidance. On the other hand, whenever measures are announced in hard law, such as legal acts or decrees, they are classified as binding. According to our classification, 23 announcements are associated with legally binding measures and 9 with non-legally binding measures (or soft-law measures). We plot the responses of our main dependent variables in Fig. 8. We observe that responses following announcements of binding measures are close to our baseline estimates and produce strong contractionary effects. Further, binding measures appear to produce contemporaneous effects for real household credit, real house prices, and real household durable goods consumption, which are statistically significant at the 90% confidence level. On the contrary, measures based on soft-law lead to responses in the opposite direction for real household credit and real house prices. At the same time, the responses of the new lending rates remain statistically insignificant.

Finally, we focus on measures with speed limits and investigate whether announcements of such measures can lead to different reactions of our main variables. This feature allows a certain percentage of new mortgage production to exceed the maximum LTV ratio limit. The speed limit allows a degree of discretion to lenders and is often used to assist credit constraint borrowers, such as first-time buyers. In total, our sample has 9 announcements of measures with the speed limit

[21] For the first group of actions, we allow for one quarter between the announcement and the first implementation mainly because in several cases the announcement is made at the end a quarter and the implementation carried out at the beginning of the following quarter, effectively implying an almost immediate implementation. Moreover, this classification allows us to have a sufficient amount of actions in one and the other group. In our sample, 19 announcements correspond to measures implemented during or the subsequent quarter of the announcement and 13 announcements correspond to measures implemented in subsequent quarters.

[22] A few measures were implemented in a staggered way, with the first implementation falling within the current or subsequent quarter from the announcement. We have included such measures in both announcement indexes, and in effect, we remain agnostic regarding the classification of these measures. Measures announced in the Netherlands and the Slovak Republic share this feature. We do a robustness check by dropping these announcements from the two indexes. Responses remain broadly similar. We present these results in Fig. A.9.

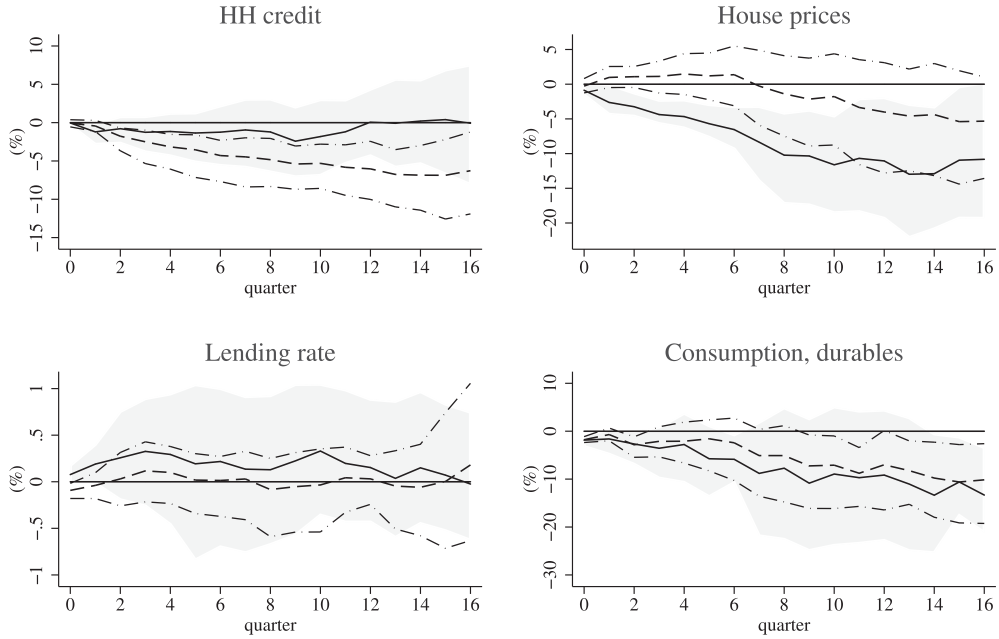

Fig. 7. Responses to an announcement of tightening in the maximum LTV ratio, measures with quick and delayed implementation. *Notes*: Solid lines correspond to responses after the announcement of measures implemented within one quarter from the announcement (quick implementation). The dashed lines correspond to responses after the announcement of measures with the first implementation beyond one quarter after the announcement (delayed implementation). Shaded area and dash-dotted lines correspond to the 90% confidence interval derived from inverted wild bootstrap-t statistic clustered at the country level.

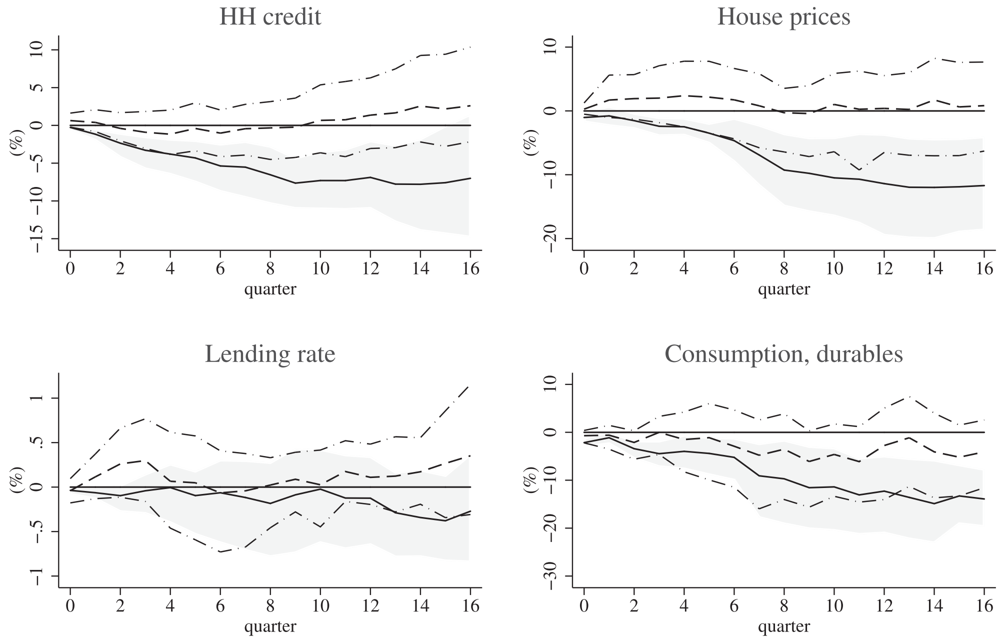

Fig. 8. Responses to an announcement of tightening in the maximum LTV ratio, binding and non-binding measures. *Notes*: Solid lines correspond to responses after the announcement of legally binding measures. The dashed lines correspond to responses after the announcement of non-binding measures (measures based on soft law). Shaded area and dash-dotted lines correspond to the 90% confidence intervals derived from inverted wild bootstrap-t statistic clustered at the country level.

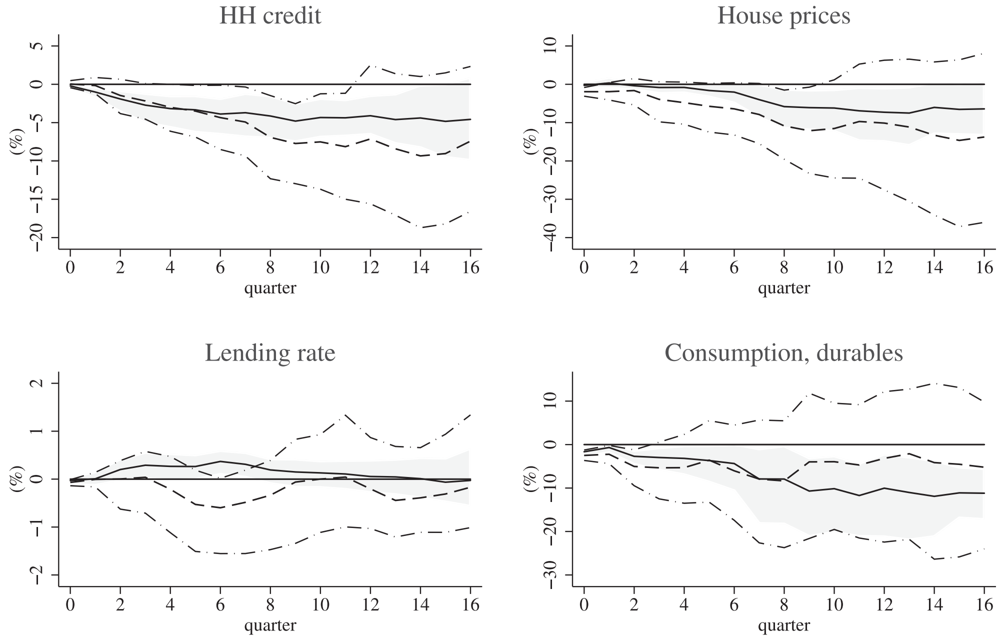

Fig. 9. Responses to an announcement of tightening in the maximum LTV ratio, measures with and without speed limit. *Notes*: Solid lines correspond to responses after the announcement of measures without a speed limit. The dashed lines correspond to responses after the announcement of measures with speed limit. Shaded area and dash-dotted lines correspond to the 90% confidence intervals derived from inverted wild bootstrap-t statistic clustered at the country level.

option. Responses after the announcement of tightening measures with and without the speed limit are presented in Fig. 9. Similarly to the findings for binding and non-binding measures, we observe noticeable differences in the reactions of our dependent variables. These results show that measures with such exemptions are less effective and that only the reactions of the measures without a speed limit are associated with contractionary effects. Moreover, we now observe a statistically significant increase in new lending rates after the announcement. The upward response of new lending rates could point that credit supply shifts to the riskier among permissible borrowers after imposing a strict LTV limit. At the same time, the flat response of new lending rates when the speed limit is present can indicate that the demand above the LTV limit is not necessarily riskier. Indeed, Kelly et al. (2015) explicitly studied carve-outs for first-time borrowers using detailed loan-level data for Ireland. The authors found that borrowers targeted by the carve-outs exhibit a lower probability of default than subsequent buyers.

Overall, our results suggest that macroprudential policy announcements can result in significant reactions which depend on the implementation schedule.[23] Further, the mandate of macroprudential authorities and, specifically, their ability to take measures enshrined in primary law or with sanctions could be of high importance for the effectiveness of their actions. At last, the balance of the different features embedded in the announced measures may not only reduce their effectiveness but potentially lead to counterproductive outcomes in contrast to the authorities' intentions.

8. Conclusions

In this paper, we provide novel evidence for the effects of announcements of LTV ratio policies for a sample of 18 EU countries over the period 2000–2019 and investigate their effects on the housing market and households' consumption.

Our main findings can be summarised as follows. First, we show that announcements of LTV ratio measures can have a sizeable impact on credit, suggesting that anticipation effects can be significant. This result is confirmed by the impulse response function estimates following announcements of actions with delayed implementation. Second, we find that house prices react negatively to LTV announcements. This is particularly the case for announcements of measures with quick implementation, which might represent a stronger signal of a change in the housing cycle. Third, we show that macroprudential policy announcements induce households to reduce their consumption of durable goods. Finally, we show that the contractionary effects after the LTV announcements are mostly driven by binding actions and actions with non-discretionary components, such the speed limits.

[23] Unfortunately, there needs to be more variation in the data to perform additional combinations of splits. Nevertheless, our results for the response of our main variables to actions with quick and delayed implementation are unlikely to be driven by legally binding or speed limit components. The share of actions with legally binding character or speed limit components is similar for actions with quick and delayed implementation.

Our results have several policy implications. First, they call for careful consideration of the timing between the announcement and implementation of macroprudential policy measures. Second, our results suggest that parameters of macroprudential measures, such as their legal basis or embedded exceptions, can be important for their effectiveness.

In future work, extending the dataset on announcements to cover a broader sample of countries and time periods would be desirable. Further, in this study, we investigate only one particular channel of borrower-based measures: the effect on the mortgage market and household behaviour. However, it would be interesting to uncover the workings of other channels through which these policies operate, such as re-allocation of credit, cross-border effects, and risk-taking in banks' portfolios. At last, we investigate how different design choices of measures alter the announcement effects. However, we abstract from the underlying reasons for such choices. It would be relevant in the future to study explicitly these design choices and the variables entering the macroprudential policy function, as policy-makers could actively change the design and timing of measures to tackle the problem at hand.

Data availability

Data will be made available on request.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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We would like to thank Maurice Bun, Adam Elbourne, Paul Hilbers, David Jan Jansen, Jan Kakes, Ashleigh Neil, Anna Samarina, and participants of the Joint ECB and Central Bank of Malta research workshop, the De Nederlandsche Bank (DNB) Research Seminar Series, the Netherlands Bureau for Economic Policy Analysis (CPB) Webex-seminar Series, and two anonymous referees for valuable comments and suggestions.

Appendix A. Additional Results

Figs. A.1,A.2,A.3,A.4,A.5,A.6,A.7,A.8,A.9,A.10,A.11,A.12,A.13 and Tables A.1,A.2,A.3,A.4,A.5,A.6,A.7,A.8,A.9.

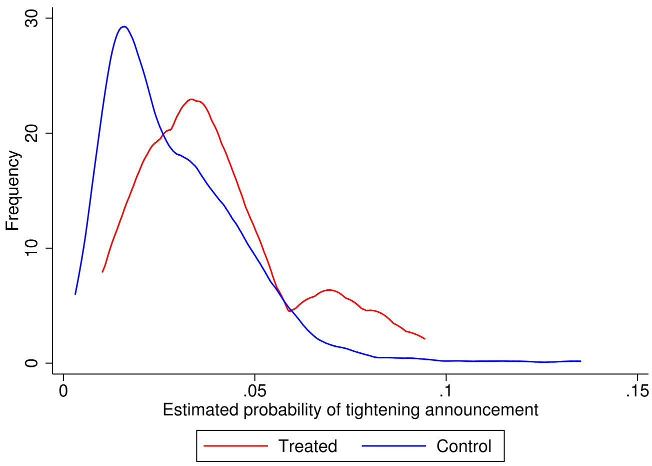

Fig. A.1. Empirical distribution of the first stage treatment propensity score.

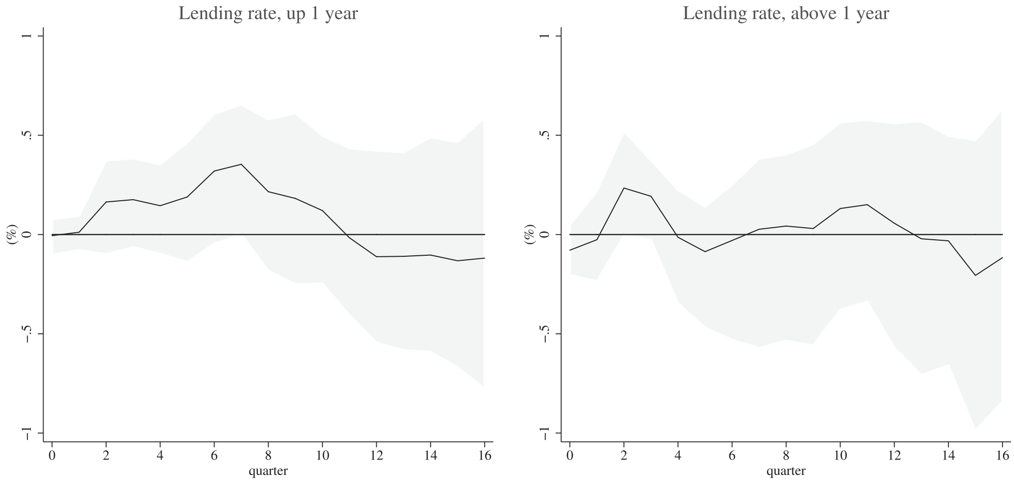

Fig. A.2. Responses of new mortgage lending rates with an interest rate fixation up to one year and new mortgage lending rates with an interest rate fixation period above one year to an announcement of tightening in the maximum LTV ratio. *Notes*: Shaded areas correspond to the 90% confidence interval derived from inverted wild cluster bootstrap-t statistic clustered at the country level.

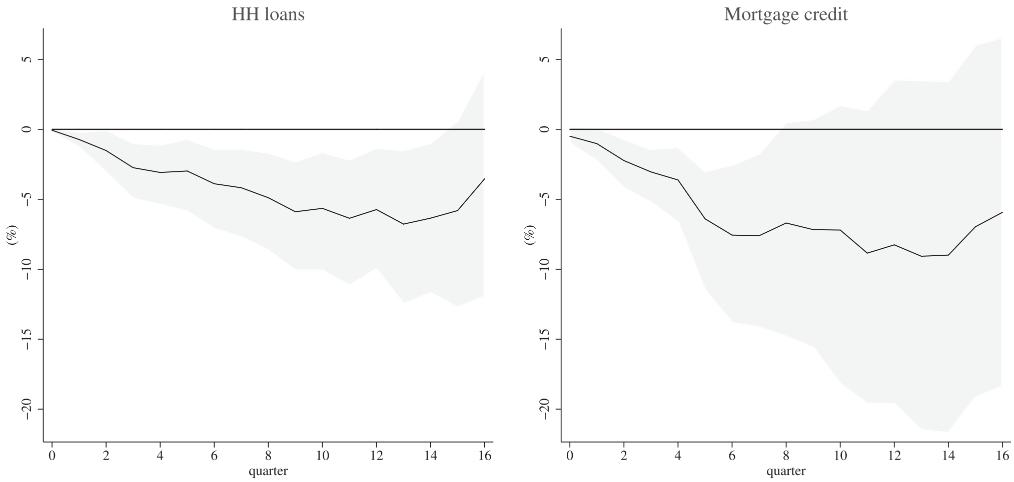

Fig. A.3. Response of real household loans and real household mortgage credit to an announcement of tightening in the maximum LTV ratio. *Notes*: Shaded area corresponds to the 90% confidence interval derived from inverted wild cluster bootstrap-t statistic clustered at the country level. Real household mortgage credit, sourced from Richter et al. (2019), is available up to 2015.

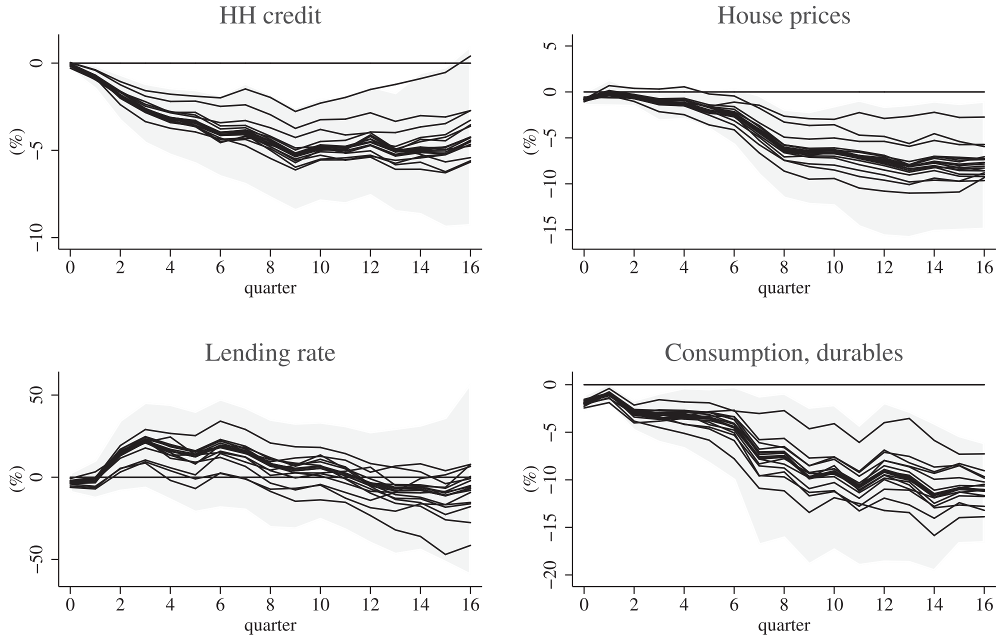

Fig. A.4. Responses of main depended variables to an announcement of a tightening in the maximum LTV ratio, dropping countries one by one. *Notes*: Shaded area corresponds to the 90% confidence interval for the baseline estimate derived from inverted wild cluster bootstrap-t statistic clustered at the country level. Solid lines correspond to the local projections coefficient estimates excluding one country at a time.

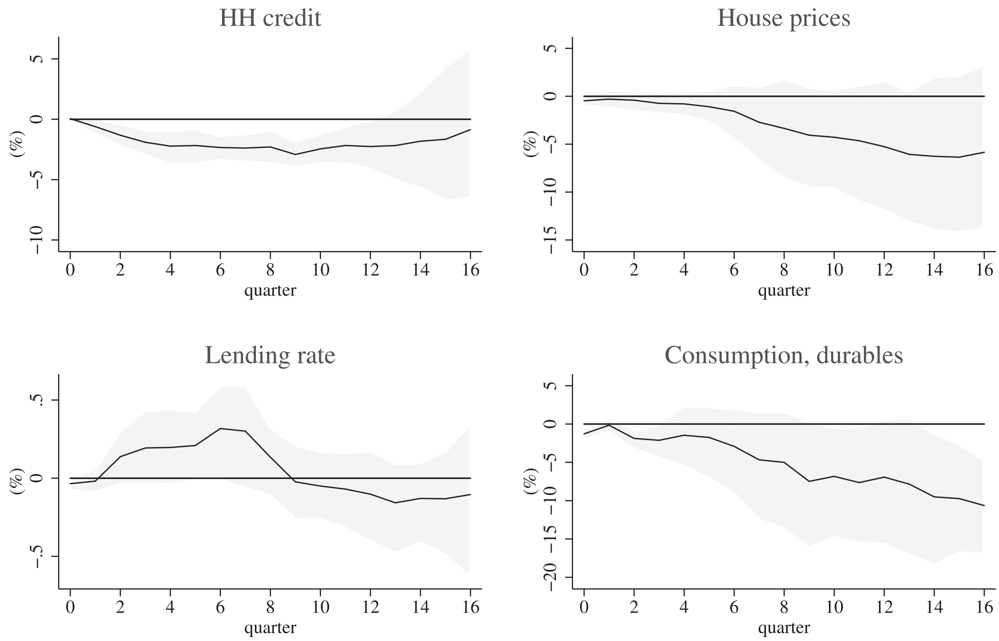

Fig. A.5. Responses of main dependent variables to an announcement of a tightening in the maximum LTV ratio, controlling for implemented actions with the iMaPP index. *Notes*: Shaded areas correspond to the 90% confidence interval derived from inverted wild cluster bootstrap-t statistic clustered at the country level.

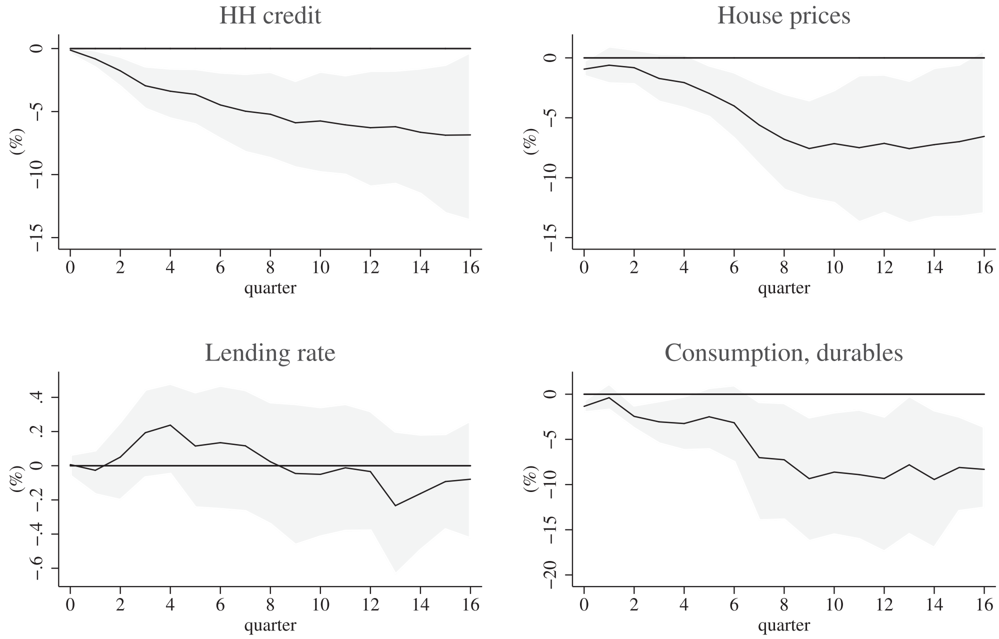

Fig. A.6. Responses of main dependent variables to an announcement of a tightening in the maximum LTV ratio, re-positioning announcement dates for actions with a consultation process. *Notes*: Shaded areas correspond to the 90% confidence interval derived from inverted wild cluster bootstrap-t statistic clustered at the country level. The dates affected are the following: Ireland 2015q1 set to 2014q4, Denmark 2014q4 set to 2014q3, Netherlands 2012q4 set to 2012q2. Finland 2018q1 remained unchanged.

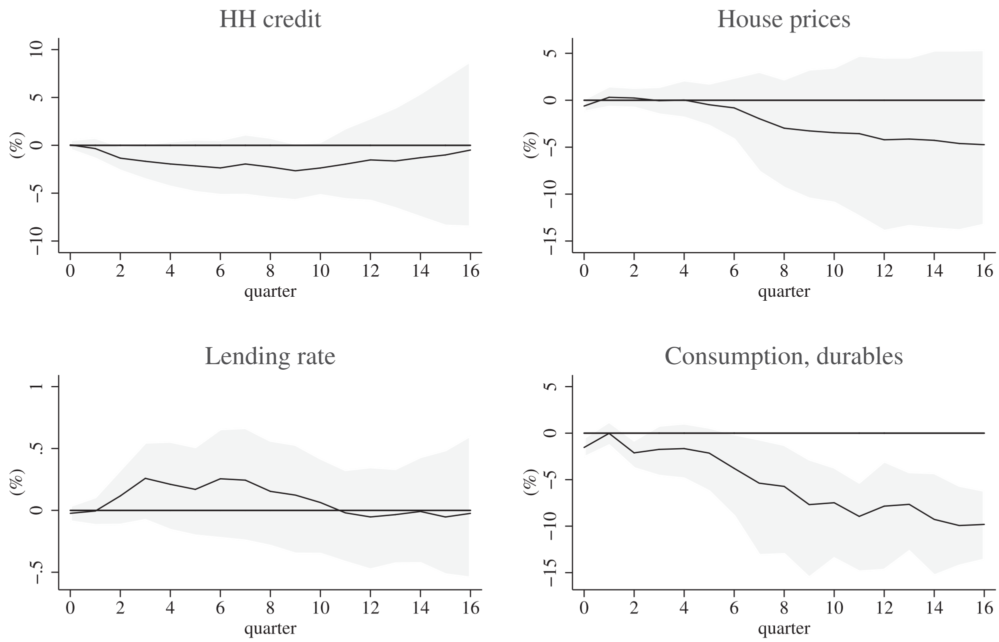

Fig. A.7. Responses of dependent variables to an announcement of a tightening in the maximum LTV ratio, re-positioning of announcement dates when announcement falls within the first month of the quarter. *Notes*: Shaded areas correspond to the 90% confidence interval derived from inverted wild cluster bootstrap-t statistic clustered at the country level. The dates affected are the following: Croatia 2006q4 set to 2006q3, Cyprus 2007q3 set to 2007q2, Denmark 2007q3 set to 2007q2, Slovakia 2014q4 set to 2014q3, Sweden 2010q3 set to 2010q2.

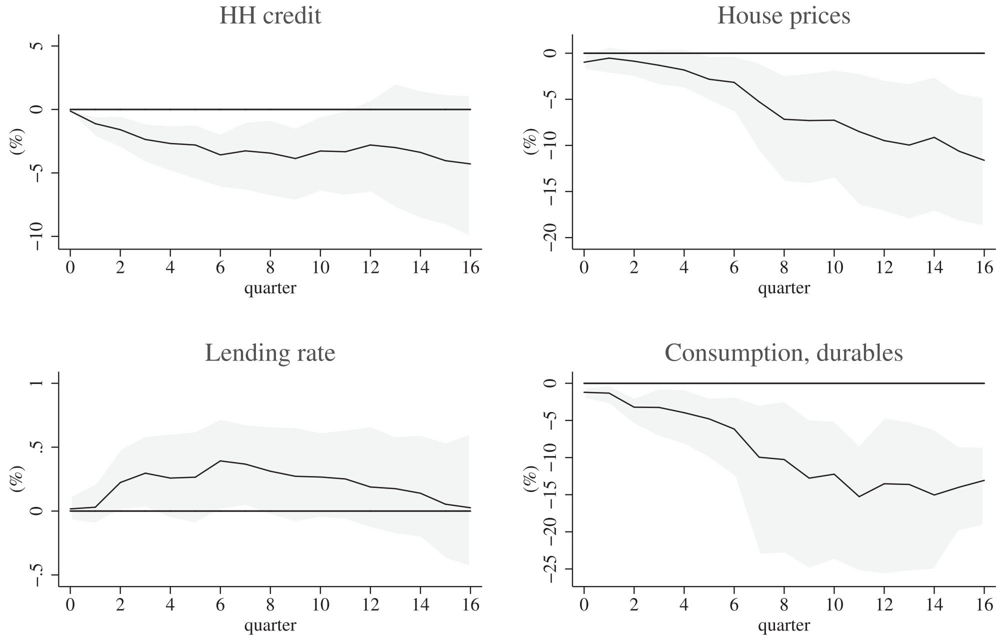

Fig. A.8. Responses of main dependent variables to an announcement of a tightening in the maximum LTV ratio, controlling for announcements of other borrower-based actions. *Notes*: Shaded areas correspond to the 90% confidence interval derived from inverted wild cluster bootstrap-t statistic clustered at the country level.

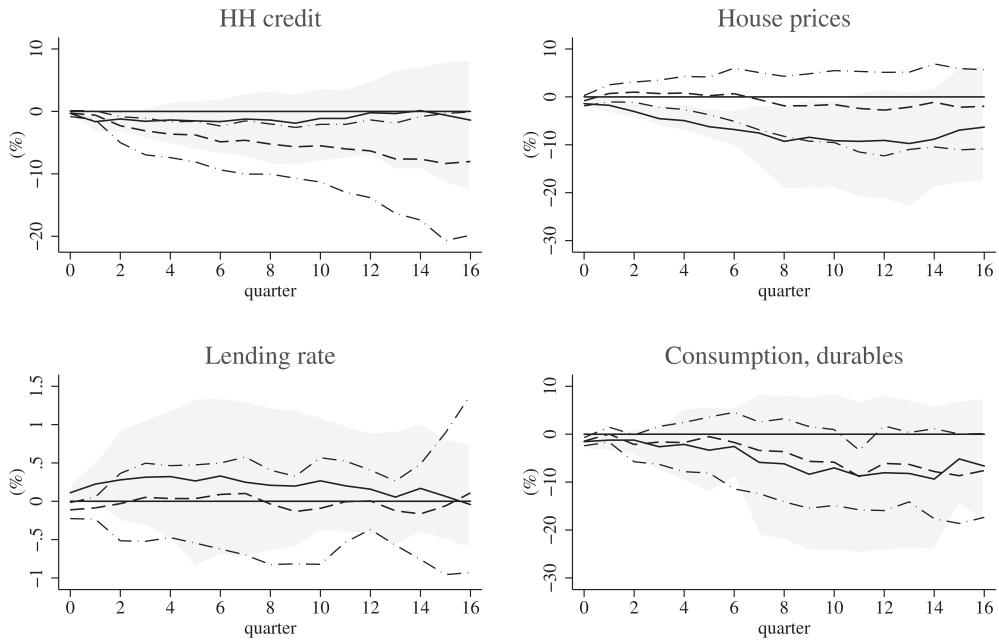

Fig. A.9. Responses to an announcement of tightening in the maximum LTV ratio, measures with quick and delayed implementation after dropping actions with contemporaneous announcement and implementation components. *Notes*: We drop the announcements of measures in the Netherlands and the Slovak Republic. Solid lines correspond to responses after the announcement of measures implemented within one quarter from the announcement (quick implementation). The dashed lines correspond to responses after the announcement of measures with first implementation one quarter after the announcement and beyond (delayed implementation). Solid area and dash-dotted lines correspond to the 90% confidence intervals derived from inverted wild cluster bootstrap-t statistic clustered at the country level.

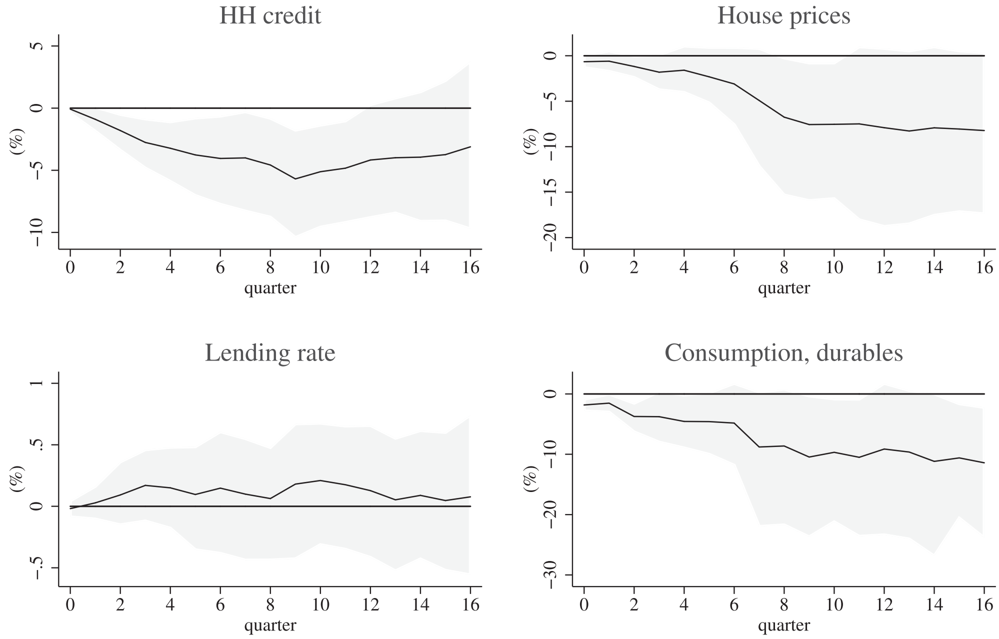

Fig. A.10. Responses of main dependent variables to an announcement of a tightening in the maximum LTV ratio, results from unweighted local projections. *Notes*: Shaded areas correspond to the 90% confidence interval derived from inverted wild cluster bootstrap-t statistic clustered at the country level.

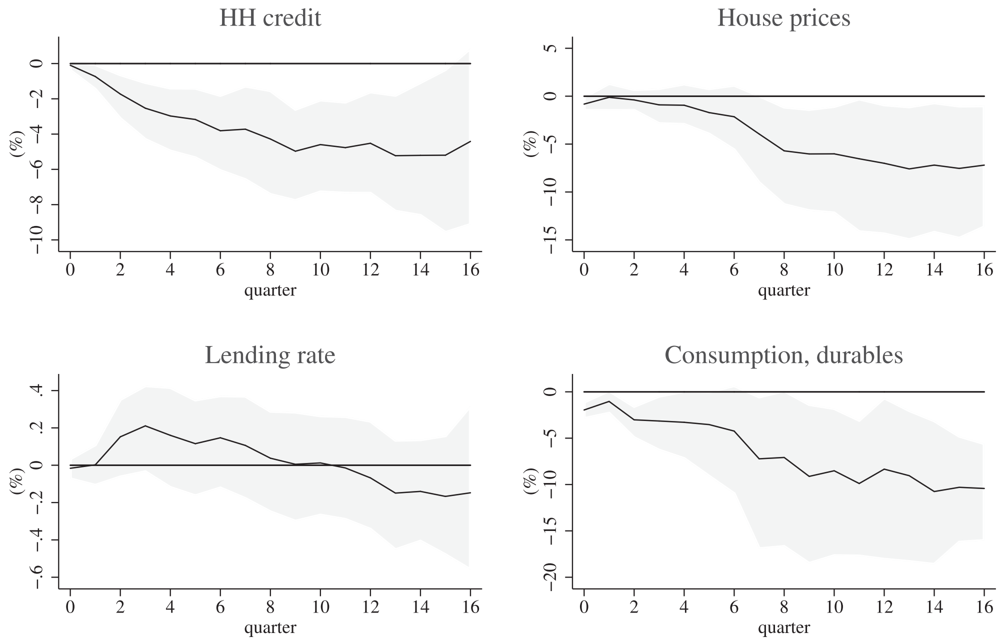

Fig. A.11. Responses of main dependent variables to an announcement of a tightening in the maximum LTV ratio, controlling for announcements of loosening LTV actions. *Notes*: Estimation results from unweighted local projections. Shaded areas correspond to the 90% confidence interval derived from inverted wild cluster bootstrap-t statistic clustered at the country level.

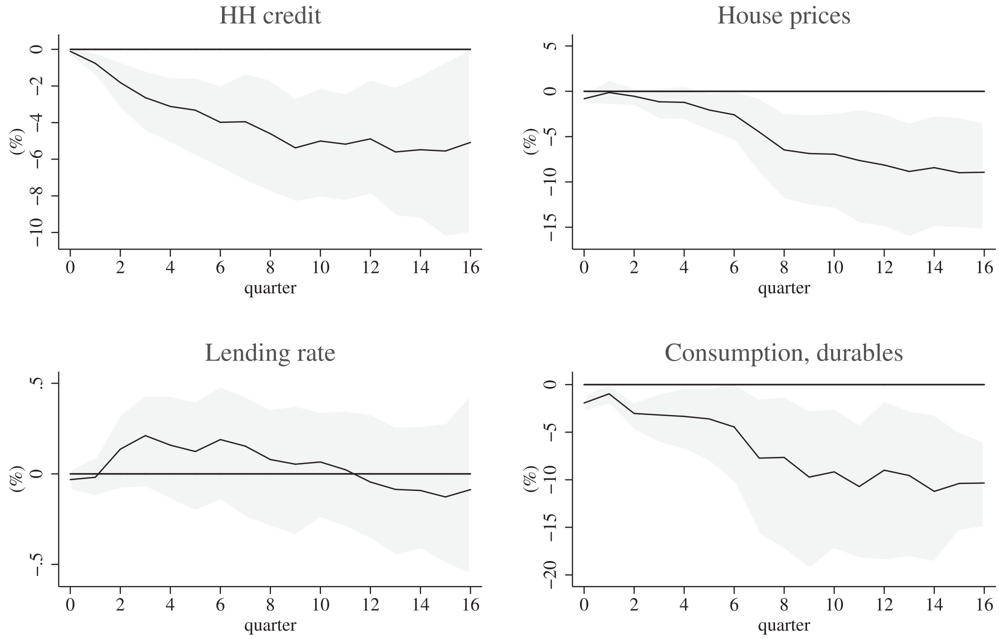

Fig. A.12. Responses of main dependent variables to an announcement of a tightening in the maximum LTV ratio, controlling for crisis episodes. *Notes*: Shaded areas correspond to the 90% confidence interval derived from inverted wild cluster bootstrap-t statistic clustered at the country level. Crisis dummy from Laeven and Valencia (2020) captures episodes of systemic banking crisis, currency, or sovereign crisis.

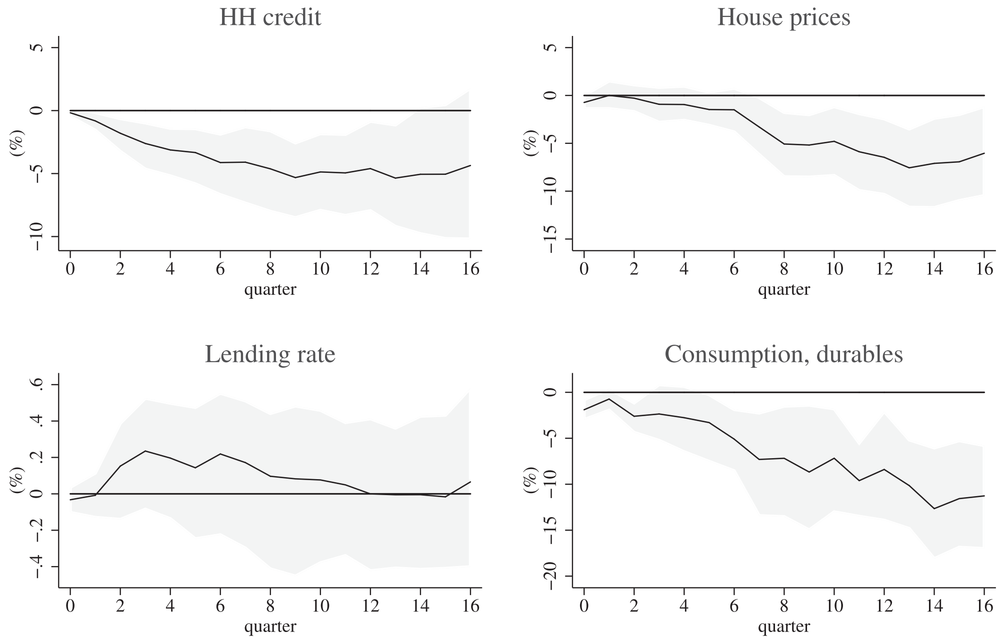

Fig. A.13. Responses of main dependent variables to an announcement of a tightening in the maximum LTV ratio, controlling for aggregate credit conditions. *Notes*: Shaded areas correspond to the 90% confidence interval derived from inverted wild cluster bootstrap-t statistic clustered at the country level. Aggregate credit conditions are proxied with the real total credit to the private non-financial sector.

Table A.1
Estimation results for first stage logistic regression.

	(1)
4-quarter real household credit growth, 1 quarter lag	−0.02
	(-0.04)
4-quarter real house price growth, 1 quarter lag	0.05
	(-0.05)
4-quarter real GDP growth, 1 quarter lag	−0.05
	(-0.11)
MaPP action, 1 quarter lag	−0.82
	(-0.85)
MaPP action, 2 quarters lag	−0.87
	(-0.96)
MaPP action, 3 quarters lag	−0.70
	(-0.87)
MaPP action, 4 quarters lag	0.22
	(-0.70)
Constant	-2.30^{***}
	(-0.66)
Observations	956
$PseudoR^2$	0.04
AUC	0.67

Notes: Dependent variable is a dummy for announcements of LTV tightening measures. *MaPP* is a dummy variable indicating the implementation of a macroprudential borrower-based measure. ***/**/* indicate statistical significance at 1%, 5% and 10% levels. Robust standard errors in parenthesis. Specification include country specific effects.

Table A.2
Estimation results for real household credit.

	Quarter 0	Quarter 4	Quarter 8	Quarter 12	Quarter 16
Announcement, LTV tightening	−0.11	-3.12^{***}	-4.50^{***}	-4.54^{**}	−4.23
	(-0.70)	(-3.56)	(-2.92)	(-2.67)	(-1.58)
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	900	834	766	698	630
$R2_{adj.}$	0.72	0.80	0.78	0.79	0.82
Number of announcements	26	25	22	20	19

Notes: Wild cluster bootstrap-t t-statistics in parenthesis clustered by country. ***/**/* indicate statistical significance at 1%, 5% and 10% levels. Specifications include additional macroeconomic and financial controls.

Table A.3
Estimation results for real house prices.

	Quarter 0	Quarter 4	Quarter 8	Quarter 12	Quarter 16
Announcement, LTV tightening	-0.81^{**}	−1.18	-6.19^{**}	-7.51^{**}	-7.85^{*}
	(-2.49)	(-1.19)	(-2.66)	(-2.28)	(-2.30)
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	919	853	785	717	649
$R^2_{adj.}$	0.48	0.65	0.65	0.66	0.72
Number of announcements	28	27	24	22	21

Notes: Wild cluster bootstrap-t t-statistics in parenthesis clustered by country. ***/**/* indicate statistical significance at 1%, 5% and 10% levels. Specifications include additional macroeconomic and financial controls.

Table A.4
Estimation results for new mortgage lending rates.

	Quarter 0	Quarter 4	Quarter 8	Quarter 12	Quarter 16
Announcement, LTV tightening	−0.03	0.16	0.07	−0.05	−0.07
	(-1.05)	(1.24)	(0.41)	(-0.31)	(-0.24)
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	845	783	719	655	591
$R^2_{adj.}$	0.66	0.64	0.66	0.65	0.68
Number of announcements	25	24	21	19	18

Notes: Wild cluster bootstrap-t t-statistics in parenthesis clustered by country. ***/**/* indicate statistical significance at 1%, 5% and 10% levels. Specifications include additional macroeconomic and financial controls.

Table A.5
Estimation results for real household durable goods consumption.

	Quarter 0	Quarter 4	Quarter 8	Quarter 12	Quarter 16
Announcement, LTV tightening	−1.91***	−3.33*	−7.61*	−9.12**	−11.07***
	(-4.58)	(-2.02)	(-2.26)	(-2.57)	(-4.11)
Country fixed effects	Yes	Yes	Yes	Yes	Yes
Time fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	956	890	822	754	686
$R^2_{adj.}$	0.41	0.62	0.66	0.65	0.71
Number of announcements	28	27	24	22	21

Notes: Wild cluster bootstrap-t t-statistics in parenthesis clustered by country. ***/**/* indicate statistical significance at 1%, 5% and 10% levels. Specifications include additional macroeconomic and financial controls.

Table A.6
Responses of real household credit to an announcement of a tightening in the maximum LTV ratio, dropping countries one by one.

	Quarter 0	Quarter 4	Quarter 8	Quarter 12	Quarter 16
baseline coefficient	−0.11	−3.12*	−4.50*	−4.54*	−4.23
CY	−0.16	−2.85*	−3.70*	−3.99*	−5.42*
CZ	−0.11	−3.11*	−4.46*	−4.52*	−4.24
DK	−0.15	−3.36*	−5.46*	−5.33*	−4.66
EE	−0.12	−3.24*	−4.78*	−5.38*	−5.67*
FI	−0.09	−3.73*	−4.37*	−4.20*	−3.54
HR	−0.11	−3.12*	−4.50*	−4.54*	−4.23
HU	−0.03	−2.83*	−4.19*	−4.35*	−4.75
IE	−0.10	−3.19*	−4.25*	−3.95*	−2.72
LT	0.02	−2.20*	−2.96*	−2.85	−2.72
LV	0.04	−1.78*	−1.97*	−1.51*	0.40
MT	−0.18	−3.13*	−4.58*	−4.69*	−4.45
NL	−0.17	−3.15*	−4.64*	−4.51*	−3.27
PL	−0.17	−3.16*	−5.00*	−5.28*	−4.42
PT	−0.05	−3.23*	−4.60*	−4.68*	−4.27
RO	−0.06	−2.78*	−4.37*	−4.36*	−4.25
SE	−0.06	−3.41*	−4.90*	−4.12	−3.61
SI	−0.11	−3.32*	−4.80*	−4.72*	−4.51
SK	−0.29	−3.29*	−4.69*	−5.03*	−5.60

Notes: * indicates statistical significance at the 10% level. Specifications include additional macroeconomic and financial controls.

Table A.7
Responses of real house prices to an announcement of a tightening in the maximum LTV ratio, dropping countries one by one.

	Quarter 0	Quarter 4	Quarter 8	Quarter 12	Quarter 16
baseline coefficient	−0.81*	−1.18	−6.19*	−7.51*	−7.85*
CY	−0.79*	−0.89	−6.01*	−7.76*	−8.98*
CZ	−0.77	−1.13	−6.17*	−7.84*	−8.28*
DK	−0.97*	−2.47*	−8.62*	−10.8*	−9.32**
EE	−0.64	−0.93	−6.03*	−7.03*	−7.37
FI	−0.69	−0.84	−5.76*	−7.33*	−7.34
HR	−0.81*	−1.18	−6.19*	−7.51*	−7.85*
HU	−0.82	−1.43	−7.07*	−8.02*	−8.58
IE	−0.65	−0.70	−4.92*	−5.38*	−5.72
LT	−0.90*	−1.50	−7.45*	−9.61*	−9.65*
LV	−0.55	−1.17	−2.64*	−2.89	−2.74
MT	−0.66	−1.36	−6.62*	−8.5*	−9.2*
NL	−1.07*	−1.17	−6.51*	−7.17*	−7.06*
PL	−0.84*	−0.88	−5.77*	−6.9	−7.26
PT	−1.02*	−1.51	−6.39*	−7.63*	−7.68*
RO	−0.84	−0.87	−5.8*	−7.52*	−8.59*
SE	−0.76	−0.93	−7.44*	−9.05*	−8.81*
SI	−0.82*	−1.22	−6.31*	−7.45*	−8.08*
SK	−0.80	0.56	−3.28	−4.85	−5.95

Notes: * indicate statistical significance at the 10% level. Specifications include additional macroeconomic and financial controls.

Table A.8
Responses of new mortgage lending rates to an announcement of a tightening in the maximum LTV ratio, dropping countries one by one.

	Quarter 0	Quarter 4	Quarter 8	Quarter 12	Quarter 16
baseline coefficient	−0.03	0.16	0.07	−0.05	−0.07
CY	−0.02	0.19	0.11	−0.10	−0.28
CZ	−0.03	0.17	0.08	−0.04	0.00
DK	−0.01	0.25	0.01	0.06	0.08
EE	−0.03	0.16	0.07	−0.05	−0.07
FI	−0.04	0.12	−0.04	−0.14	0.07
HR	−0.03	0.16	0.07	−0.05	−0.07
HU	−0.02	0.15	0.12	−0.04	−0.15
IE	−0.02	0.20	0.11	−0.01	−0.08
LT	−0.03	0.27*	0.21	0.08	−0.01
LV	−0.02	0.16	0.09	0.03	0.08
MT	−0.03	0.16	0.07	−0.04	−0.07
NL	−0.04	0.13	0.04	−0.07	−0.16
PL	−0.04	0.06	0.04	−0.06	−0.18
PT	−0.02	0.19	0.08	−0.03	−0.06
RO	−0.02	0.13	0.07	−0.09	−0.02
SE	−0.06*	0.04	−0.07	−0.18	−0.09
SI	−0.03	0.19	0.08	−0.03	−0.05
SK	−0.05	−0.02	−0.09	−0.23	−0.41

Notes: * indicates statistical significance at the 10% level. Specifications include additional macroeconomic and financial controls.

Table A.9
Responses of real household durable goods consumption to an announcement of a tightening in the maximum LTV ratio, dropping countries one by one.

	Quarter 0	Quarter 4	Quarter 8	Quarter 12	Quarter 16
baseline coefficient	−1.91*	−3.33*	−7.61*	−9.12*	−11.07*
CY	−1.95*	−4.17*	−8.76*	−9.97*	−10.48*
CZ	−1.89*	−3.1*	−7.47*	−9.10*	−11.10*
DK	−2.10*	−5.11*	−11.15*	−13.23*	−13.88*
EE	−1.89*	−2.90	−7.07	−8.52*	−10.65*
FI	−1.93*	−3.03	−7.77	−7.97	−9.83*
HR	−1.91*	−3.33*	−7.61*	−9.12*	−11.07*
HU	−1.92*	−2.94	−7.19*	−9.50*	−12.79*
IE	−1.98*	−2.80	−6.51*	−7.97*	−10.22*
LT	−1.65*	−4.13*	−9.17*	−10.98*	−11.75*
LV	−1.69*	−1.82	−2.72	−3.99	−7.27*
MT	−1.86*	−3.21*	−7.45*	−9.47*	−11.04*
NL	−1.54*	−3.48*	−8.35*	−9.82*	−11.09*

(*continued on next page*)

Table A.9 (*continued*)

	Quarter 0	Quarter 4	Quarter 8	Quarter 12	Quarter 16
PL	−1.98*	−3.32*	−5.65	−6.87*	−9.04*
PT	−1.78*	−3.61*	−7.63*	−9.07*	−11.02*
RO	−2.00*	−2.70*	−7.01*	−8.91*	−11.66*
SE	−2.27*	−3.36	−9.69*	−11.92*	−13.21*
SI	−1.91*	−3.14	−7.48	−9.20*	−11.20*
SK	−2.45*	−3.54*	−6.62*	−7.10*	−9.67*

Notes: * indicates statistical significance at the 10% level. Specifications include additional macroeconomic and financial controls.

Appendix B. Supplementary material

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.jimonfin.2023.102838.

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