DNB Working Paper
No. 704 / January 2021

Effects of LTV announcements in EU economies

Dimitris Mokas and Massimo Giuliodori

DeNederlandscheBank
EUROSYSTEEM

Effects of LTV announcements in EU economies

Dimitris Mokas and Massimo Giuliodori *

* Views expressed are those of the authors and do not necessarily reflect official positions of De Nederlandsche Bank.

Working Paper No. 704

January 2021

De Nederlandsche Bank NV
P.O. Box 98
1000 AB AMSTERDAM
The Netherlands

Effects of LTV announcements in EU economies*

Dimitris Mokas† ** and Massimo Giuliodori††

January 2021

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 28 EU economies during the period 2000-2019. We show that announcements of LTV policies can have a sizeable impact on household credit, house prices and household durable goods consumption. New mortgage lending rates appear to increase following the announcement of a LTV ratio restriction. We find that the estimated contractionary effects are driven mostly by binding actions and actions with non-discretionary components, suggesting that the design of macroprudential policies matters for their effectiveness.

JEL Codes: E58, G21, G28

Key Words: Macroprudential Policy, Loan-to-value Ratios, Cost of Credit, Local Projections

*We would like to thank Maurice Bun, Adam Elbourne, Paul Hilbers, David Jan Jansen, Jan Kakes, Anna Samarina and participants of the De Nederlandsche Bank (DNB) Research Seminar Series and the Netherlands Bureau for Economic Policy Analysis (CPB) Webex-seminar Series for valuable comments and suggestions. The views presented do not represent official views of the De Nederlandsche Bank or the Eurosystem. Any errors are ours.

†De Nederlandsche Bank and University of Amsterdam

††University of Amsterdam and Tinbergen Institute

**Corresponding author. E-mail address: d.mokas@uva.nl

1. Introduction

In response to the Global Financial Crisis (GFC), countries around the world designed and put in place macroprudential policy frameworks to provide a macroeconomic and prudential approach to the supervision of the financial system as a whole. The EU, mainly because of its special 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 has mostly relied on the use of cross-country panel data regressions (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, 2010), Richter 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 future 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 provide a direct link 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 both 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 and thus, get clean results for the effects and the transmission mechanism of LTV measures following their

announcement.

To improve identification, we collect LTV announcements for a sample of 28 EU countries over the period 2000-2019 and investigate their effects on household credit, house prices and household consumption by consolidating information from existing datasets on macroprudential policies and original sources. To answer our main research question, we additionally collect information on the design of measures and their underlying motivation, while 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 for their exact announcement dates and implementation schedule and design.

Further, we go beyond the effects of LTV announcements on quantities and provide preliminary evidence for their impact on new 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. By using a sample of 32 advanced and emerging market economies, Kim and Mehrotra (2019) show that the reaction of lending rates differs by type of macroprudential policy actions. Differently from Kim and Mehrotra (2019), we use *new* mortgage interest rates rather than the outstanding lending rate to the private sector. Thus, we are able to capture better 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 measures have strong and persistent effects on household credit and durable goods consumption. The impact of LTV announcements on house prices is more muted and only manifested with a time lag. We also document significant reactions to pre-announced LTV measures, that is measures announced, but implemented in subsequent quarters, suggesting that policies have important anticipation effects. Our results are robust to several checks, including controlling for the announcement of other borrower-based measures and re-positioning the date of announcements to control for information leaks. Further, we find that mortgage interest rates increase following an LTV announcement. However, the cumulative impact becomes negative at the end of the projections horizon. The initial increase in interest rates could signal a shift towards the riskier (among the permissible) borrowers. Finally, we investigate how the design of an action and in particular its legal character, as well as possible embedded exemptions, can affect its effectiveness. We find that our main results are driven by legally binding measures with no discretionary components. Thus, the overall level of tightening depends not only

on the direction of change in the maximum LTV limit, but also on the overall strictness of the LTV framework.

Our results have a number of policy implications. First, they call for a careful consideration of the timing between announcement and implementation. Policy makers might want to take into account potential anticipation effects when evaluating the effectiveness of various measures. Second, macroprudential policies can have unintended consequences. By affecting lending rates, LTV measures could interfere with monetary policy objectives via this channel. Further, it is possible that banks shift credit to riskier borrowers, reducing the effectiveness of LTV measures as a risk reduction mechanism. Third, we show that not only the type of measures matters, but also their design. Adding additional features in measures to achieve secondary objectives, such as easing access to financing for certain borrower groups, might undermine their ability to meet their primary aim. Finally, measures based on soft-law might not meet their objectives.

The remainder of paper is organized as follows. Section 2 reviews the related literature. Section 3 provides details on our dataset of macroprudential announcements, as well as 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 a number of robustness checks (Section 6). In Section 7 we show the reactions to different types of LTV announcements. Finally, Section 8 concludes. Supplementary material containing details on the dataset and additional empirical results is included in the Online Appendix (not for publication).

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.[1] 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 growth rate of credit to the private sector and growth rate of real GDP by 80%. Akinci and Olmstead-Rumsey (2018) construct 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

[1] 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 (Ayyagari et al., 2017) and credit allocation (Jiménez et al., 2017), their cross-border effects (Aiyar et al., 2014, Buch et al., 2017), and their interaction with monetary policy (Aiyar et al., 2016). 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).

credit, as well as 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 also find evidence that the effectiveness of the measures depends on the financial structure and the openness of the economy.[2]

Similarly to this paper, a number of studies focuses on the effects of macroprudential policies in 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 on the effectiveness of these policies on house prices is mixed. The authors find that only tightening LTV actions can curb house price appreciation. In a study focusing on southeastern European economies, Vandenbussche et al. (2015) provide limited 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) using country-bank level panel regressions 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

[2] 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).

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 the use of the narrative identification method (Romer and Romer, 1989, 2004, 2010) to tackle exogeneity issues relating to macroprudential policies. Richter et al. (2019) study LTV measures and classify them according to the their stated objective and then estimate the effects of macroprudential policies on output and inflation. The authors define exogenous LTV measures as actions which are unpredictable with respect to current and lagged real variables. Klingelhöfer and Sun (2019) use the narrative approach to disentangle macroprudential and monetary policy actions in China. They identify macroprudential policies as actions which are state-varying and target the credit cycle and the resilience of the financial system as a whole. 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) identify changes in the aggregate US bank capital ratio which are unrelated to the real and financial cycles by reading of legislative documents.

The above empirical literature focuses on the effects of policies at implementation and, in most cases, ignores potential anticipation effects, with some exceptions. Addressing this issue, 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 years 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 the exact announcement date and implementation schedule of each measure in order to explicitly address anticipation effects.

Our work is also inspired by the empirical identification methods used in fiscal policies. Like macroprudential policies, 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 studied extensively anticipation effects and their implications for structural identification. Leeper et al. (2013) show that foresight about future macroeconomic fundamentals can pose serious challenges for structural identification. Ramey (2011) argues that anticipation effects are important for the correct identification of government spending shocks, while Mertens and Ravn (2012) find

significant anticipation effects on the 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

In this section we describe the dataset on LTV ratio announcements and present some of its properties. Restrictions on LTV ratios are often implemented in a staggered way and the announcement date does not correspond to the start of the implementation. Many examples can be provided. But two of them are particularly revealing in clarifying how our identification strategy differs from most of the existing empirical literature on the effects of macroprudential policies.

The first example is the LTV ratio measure announced by the Dutch authorities in the official government gazette on the 20$^{\text{th}}$ 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.*" In existing studies on macroprudential policies, the above LTV measure is typically coded as six different actions. 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.

The second example is the LTV ratio measure taken by the Central Bank of Estonia in 2014. The policy measure was known to the public already 10 weeks before its final implementation, allowing plenty of time for borrowers and lenders to adjust ex-ante their behaviour. More specifically, on the 12$^{\text{th}}$ of December 2014 it was announced via a Governor's Decree that from the 1$^{\text{th}}$ of March 2015 banks operating in Estonia will have to comply with an LTV ratio limit of 85% when issuing new mortgage loans. The lag between announcement and implementation implies that agents could have reacted before its implementation.

To deal with the above identification issues, we recover announcement dates for each of the LTV ratio restrictions implemented in 28 EU countries during the years 2000-2019.

Apart from announcement dates, we also recover information on the design of actions, their legal status, the deciding authority, as well as their underlying motivation. This detailed description of actions together with additional classifications, announcement and implementation dates, as well as underlying motivation, is provided in the Online Appendix. Further, we provide information on the numerical level of maximum LTV ratio limits applicable after each action.

We focus on a European sample for a number of 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, limiting thus the ability to generalise 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 for the exercise of macroprudential policies.[3] Another important reason is that the information availability of sources regarding the details of policy actions is relatively rich, while definitions of data are also more uniform. Finally, our sample consists mostly of euro area countries, meaning that our results are unlikely to be driven by possible monetary policy reactions of central banks after LTV tightening episodes. Nevertheless, in our baseline specification we control for changes in the monetary policy rate.

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 that the full information regarding the announcement, the design, as well as the process of decision (e.g. consultation, reasoning) are 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). Our additional contribution is that we extend the dataset by Alam et al. (2019) to include LTV ratio actions announced in 28 EU countries up to June 2019. We opt not to include a few LTV actions which do not have a macroprudential character or documents could not be recovered.[4]

At a second stage, we collect detailed information on LTV ratio actions by recovering the underlying documentation, which consists mostly of official decisions and/or legislation acts. Our goal is to pinpoint the exact announcement date of actions and their envisaged implementation schedule. We choose to set the announcement date to the date in which the final rule was made known to the public. In the case of official decisions,

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

[4]A comparison between actions included in the dataset of Alam et al. (2019), but not in our dataset are provided in the Online Appendix.

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.

It is possible that details regarding the measures were 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 consultation process launched ahead of the measures. We find that only three measures were preceded by a consultation process.[5] The consultation documents notified the proposals of authorities to take action and provided details about the exact limits and designs of the measures. The documents 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 behaviour 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 introdution"*. For Finland, the Finnish Financial Supervisory Authority (FSA) did not launched a public consultation process but it rather asked directly the opinion of the involved financial institutions. 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 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.

Nevertheless, it is possible that details of measures leaked well ahead of their final announcement also via informal 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 would not track unanticipated LTV actions creating potential concerns about our identification strategy. To tackle this, we implement

[5]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.

different robustness checks. First, we re-position the announcement date to account for information leakages. In our empirical study we transform the monthly index to 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, respectively. However, for actions announced during the first month of the quarter we might miss potential earlier announcement. 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$^{\text{th}}$ 2014. If there was an information release not captured by sources it is possible that it would have happened in the previous quarter. To account for potential information leakages, we set the announcement date of measures announced during the first month of a quarter to the previous quarter.[6] Our baseline results are robust to this variant.

To study the effects of announcements, we create a quarterly dummy index which 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 that are 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 $ltv_{i,t}^{announced} = 1\{\Delta LTV_{i,t,s} < 0\}_{t \leq s}$, 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 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 anticipated actions for country i is denoted as $ltv_{i,t}^{anticipated} = 1\{\Delta LTV_{i,t,s} < 0\}_{t > s}$, where now t denotes the implementation quarter and s the announcement quarter. Note that we assign at time t the quarter of implementation and not the quarter of announcement.

We use a dummy variable approach, rather than a quantified measure of the LTV announcements because of the relatively high degree of subjectivity which is needed to quantify changes in 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

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

actual limit applying to a particular country in a given quarter as well as 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 in order 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 useful observations. Both approaches could have implications for our empirical results due to the relatively small number of LTV announcements in hand. With these caveats in mind, we nevertheless construct a numerical counterpart of our dummy index and show the results in the Online Appendix.

3.1.1. Properties of the dataset on announcements

The dataset contains in total 32 announcements for tightening actions of LTV ratio limits associated with 46 implemented tightening actions LTV limits. Most of the countries in our sample took at least one tightening LTV related action between 2000 and the second half of 2019. Among these actions, we observe significant variation in the timing between announcement and implementation. To illustrate this difference we plot in Fig. 1 the aggregated indices of announced and implemented LTV ratio actions. We identify three groups of countries with respect to the timing between announcement and implementation dates (shown in Fig. 2): countries which announced and implemented in 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, in the latter two cases, macroprudential policy actions are fully anticipated 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 recognise that a number of LTV ratio actions were implemented together with 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 in total 37 borrower-based actions (both tightening and loosening) that were implemented along LTV measures. These 37 additional measures correspond to 32 announcements. Out of the 32 announcements 16 coincide with the announcement of LTV measures and 16 are announcements of stand-alone other borrower-based measures. The omission of other borrower-bases measures could pose problems for our identification strategy. To alleviate such concerns, we show in the ro-

bustness checks that the baseline results do not change significantly when we control for the announcement and implementation of other borrower-based measures.

As regards to the design of LTV measures we focus on their legal character and an option called *speed limit*. The majority of the announced LTV measures (23 actions from a total of 32) had a binding character (see left panel Fig. 4 for a classification of measures according to their legal character).[7] 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, there are 9 announced LTV ratio measures with a *speed limit* (see right panel Fig. 4 for a classification of actions according to the speed limit option).

3.2. Data on dependent variables and controls

We collect data on total credit to households from the BIS and the Quarterly Sectoral Accounts available on the ECB SDW. Data on real house prices are obtained from the BIS. Data on household consumption are collected from Eurostat. We deflate the credit and consumption time series using the Harmonized Consumer Price Index (HCPI) downloaded from Eurostat. These times series are available at the 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 while we provide a detailed list of data sources in the Online Appendix.

We additionally investigate the effects of LTV measures 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 Monetary Interest Rates (MIR) dataset of the ECB and data from national central banks. Consistently with the credit, housing and macroeconomic variables, we aggregate these time series at the quarterly frequency. Further details for the construction of the new mortgage interest rate series are presented in the Online Appendix.

4. Methodology

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

$$\Delta_h y_{i,t} = \alpha_i^h + \tau_t^h + \sum_{p=1}^{4} \gamma_p^h y_{i,t-p} + \sum_{p=0}^{4} \beta_p^h ltv_{i,t-p}^{announced} + \sum_{p=0}^{4} \Theta_p^h X_{i,t-p} + \epsilon_{i,t+h} \qquad (1)$$

[7] We classify as non-binding these actions which were taken on the basis of soft-law, such as Recommendations or Guidelines.

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 $t+h$. $ltv_{i,t}^{announced}$ is the dummy index for announced LTV actions described in Section 3. 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 $\{\beta_{p=0}^{h}\}_{h\geq 0}$. $\epsilon_{i,t+h}$ is the local projection residual at horizon h. As in Jordà and Taylor (2016) and Richter et al. (2019), we use robust standard errors clustered at the country level.

We include country-fixed effects (α_i^h) to control for unobserved country-specific time-invariant characteristics. Further, we include time-fixed effects (τ_t^h) to account for common shocks affecting all countries in our sample. The vector of controls X_i includes quarterly changes in the monetary policy rate, the variable of anticipated LTV actions $ltv_{i,t}^{anticipated}$ described in Section 3, and a dummy index taking the value 1 if another borrower-based measure is implemented at time t. Additionally, X_i includes a set of macro-financial variables specific to the dependent variable.[8]

Our dataset contains information for the underlying motivation of actions. We classify actions into those motivated by cyclical and structural aims. Under cyclical considerations, we observe that actions were mostly 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.[9] We observe that the majority of actions was 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 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).[10] In short, the estimator consists of running the baseline local projections specification by introducing regression weights derived by 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 on the basis of 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 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,

[8] Consistently with the previous literature, when modelling credit to households, house price growth, and household consumption (or its components), we control for the quarterly inflation rate and the quarterly real GDP growth rate. When we model the 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.

[9] The motivation of actions included in our sample is provided in the Online Appendix.

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

including LTV ratio actions. We include country-specific fixed effects 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:

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

After using the IPW estimator, we are able to keep only tightening LTV actions. However, it is unlikely that our results and conclusions are affected by discarding loosening actions. In total, there are seven LTV loosening actions which we do not include in the final sample of announced LTV measures. In robustness checks, we replace the index of announced tightening LTV measures with an index recording tightening announcements with +1 and loosening announcements with -1. The estimated IRFs were comparable to our main results, but more imprecisely estimated, suggesting that the effects of LTV announcements in our sample are mostly driven by tightening LTV actions.[11] Notwithstanding, to avoid any concerns about endogeneity in our baseline specification, we opt to make use of the IPW estimator, and thus focus only on the effects of tightening LTV ratio announcements.

5. Empirical Results

Fig. 5 presents the cumulative responses of household credit and house prices after the announcement of tightening LTV actions. We find that these measures have a strong negative effect on household credit immediately after their announcement. Similarly, 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 target primarily credit, while they affect house prices via reducing lending to households with some lags. The response of house prices is becoming more pronounced after 4 quarters following the announcement, when credit has almost reached its maximum cumulative reaction. The cumulative response of credit to households reaches a peak response of approximately -2.5% within 8 quarters after the announcement of the LTV action.[12] The response is statistically significant at the 90% confidence level. House prices only reach the maximum cumulative impact of -5.3% at the end of our 16 quarters projections horizon. The response is statistically significant at the 68% confidence level but not at the 90% confidence level throughout the 16 quarters projections horizon.

Although we focus on LTV announcements and use a different sample of countries, our

[11] In this exercise we do not make use of the first-stage IPW weights.

[12] We additionally checked the response of loans to households and the results were qualitatively similar. These results are available upon request.

estimated responses of credit are qualitatively similar to the IRFs presented by Richter et al. (2019) who find a maximum cumulative impact on household credit of around -6%. For EU economies, a recent paper by 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 focus on announcement dates, but also because we only use LTV measures and our dependent variable is household credit. As regards to the response of house prices, our results are closer to Poghosyan (2020) suggesting a muted and delayed response. But again, the type of measures and their timing are different.

To better understand the channels via which LTV policies affect household behaviour 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 (2019) find that in a broader sample of 32 advanced and emerging market economies contractionary macroprudential policy shocks do not affect private consumption. This result holds also when the authors use LTV limits as the macroprudential policy shock.

We plot the estimated IRFs for household consumption in Fig. 6. We find that LTV policies have a limited effect on aggregate real household consumption which reaches a pick cumulative response of 1.2% but the response is statistically not significant at the 90% confidence level after six quarters following the announcement. It is possible that LTV restrictions operate "locally" and have different effects on the consumption of different goods. To investigate this, in Fig. 7 we distinguish between household consumption for durable goods and household consumption excluding durable goods.[13] We find that indeed the impact of LTV restrictions varies widely by type of consumption, with their dynamic responses showing a strikingly different pattern. Consumption of durable goods reacts immediately after the announcement of a tightening in the maximum LTV limit, while the estimated reaction is persistent. The maximum accumulated impact is approximately -10% and reached after 14 quarters. On the other hand, household consumption excluding durable goods reacts in a similar way as aggregate consumption. Its pick response is less than -1% and it becomes statistically not significant at the 90% confidence after six quarters following the announcement.

Next, we analyse the impact of tightening LTV ratio measure announcements on the

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

cost of credit and specifically the level of *new* mortgage lending rates. There are only a few studies looking at the effects of macroprudential policies on lending rates. Acharya et al. (2020) using microdata from Ireland find that LTV ratio actions affect the cost of credit differently for high and low income households. Banks offered lower lending rates to higher income households and higher lending rates for lower income households. On the other hand, Tzur-Ilan (2019) using microdata for Israel find that borrowers paid higher interest rates for their mortgages after an LTV action. The authors attribute the increase to a change in banks' risk perceptions and subsequently risk pricing behaviour following the macroprudential intervention of authorities. Another possible explanation provided by Tzur-Ilan is that credit constrained borrowers shifted their demand towards property located further away from overheated areas which was perceived as riskier by banks. In another recent paper, Kim and Mehrotra (2019) show that macroprudential policy actions increase lending rates in a sample of 32 advanced and emerging 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 to the opposite direction and actually decrease following a contractionary macroprudential policy. Our difference with Kim and Mehrotra (2019) is that we use *new* mortgage interest rates rather than the outstanding lending rate to the private sector. Thus, we are able to capture better the immediate effects of these policies in the mortgage market.

All in all, the overall result on lending rates will depend on a number of 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 actually 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, it is possible that 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 Fig. 8. We find that the announcement of a tightening LTV ratio action has a negative contemporaneous impact but interest rates tend to increase in the quarters following the announcement. The maximum accumulated increase reaches a peak of around 0.2%, which is only significant at the 68% confidence level and is relatively short-lived: it starts decreasing after it peaks and becomes statistically not significant after 8 quarters. The cumulative response becomes negative after 12 quarters and reaches a maximum decline of -0.14% at 16 quarters.

It is possible that different loan products respond differently to macroprudential measures. Thus, we estimate the reaction of interest rates with fixation period up to one year and the reaction of interest rates with interest rate fixation period above one year. The

former aims to approximate the interest rate for floating mortgages and the latter the interest rate for fixed rate mortgages. These results are presented in Fig. 9. After the announcement of an LTV measure, new rates for mortgages with interest rate fixation period below one year do not react. Even though the response becomes positive after three quarters, it is imprecisely estimated and it is statistically not significant at the 68% confidence level. On the other hand, interest rates with fixation period above one year decrease by 0.1% after the announcement. This reaction is statistically significant at the 90% confidence level. Interest rates with interest fixation period above one year reach a maximum increase at five quarters and then decline similarly to the new mortgage lending rates presented in Fig. 8.

Overall, we find sizeable effects on household credit and durable goods consumption following the announcement of restrictive LTV ratio measures, while house prices respond with a lag and their dynamic responses are less precisely estimated. Lending rates tend to decrease following the announcement of a restrictive LTV ratio policy and then increase during the first quarters.

6. Robustness Analysis

In this section we perform a battery of robustness checks to ensure that our results are not driven by our key modelling assumptions, among others the selection of actions and the setting of announcement dates. Overall, we find that the responses of credit, interest rates and consumption are robust to all our robustness tests. However, the response of house prices appears to be sensitive to the underlying sample of countries and the timing of the announcement. Thus, our findings point that announcements of LTV measures can affect the household sector, but their effects on the broader housing market can be subdued and dependent on the underlying sample.

First, 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 plot the reactions of our main variables in **??** of the Online Appendix. We observe that the responses are qualitatively similar to the baseline estimates and within the 68% confidence interval. However, for house prices, the exclusion of one country appears to change the pattern of the reaction after the announcement of a tightening LTV measure. The different reaction of house prices is obtained after we exclude Latvia. Latvia announced an LTV measure in 2014 which seemed to have a strong contemporaneous impact on house prices. Thus, this particular action appears 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.

Second, we control for an alternative index of implemented LTV measures to test for 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 covers the years up to and including 2015 and therefore, when we re-estimate the local projections we discard observations for the years 2016-2019. In total, the iMaPP index includes 21 pre-announced tightening measures. We show these results in **??**. The estimated impulse response functions appear similar to the ones obtained from our baseline specification.

Third, 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 the intentions of authorities to take macroprudential actions. First, as described in Section 3, we replace the announcement dates of measures which 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 **??**. Responses appear to be similar to the baseline responses. The response of credit however points to a more persistent impact of the announcement towards the end of the projections horizon. Second, we re-position the index of announced measures to allow for earlier reactions of our dependent variables to the announced measures. It is possible that details of LTV measures were known to the public or to credit institutions ahead of their finalisation 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 in total five announcements which correspond to around one sixth of our sample. Results are presented in **??**. We observe that the estimated impulse response functions become more noisy, while the reaction of house prices turns statistically not significant at the 68% confidence level. Nevertheless, the pattern of the reactions appears qualitatively similar to our baseline results.

Fourth, we control for another type of omitted variables bias, stemming from the announcement and implementation of other borrower-based measures. It is possible that announcements of such measures have an effect on our target variables, raising then concerns about our identification strategy. Half of the announced tightening LTV measures were part of a package which included other measures targeting borrowers, while there were 13 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. Akinci and Olmstead-Rumsey (2018) show that LTV measures decrease bank credit, housing credit and house prices, while DSTI measures have effects on housing credit and house prices.

However, the authors use implementation dates and do not control simultaneously for the effects of the two policies, even though they document that they are often used together. Cerutti et al. (2017) show that in the sample of advanced economies, DTI policies reduce household credit but not house prices, while LTV measures do not have an effect on these variables. Again, the authors use implementation dates and do not account for the simultaneous implementation of LTV and other borrower-based measures as we do in our baseline specification. As a check, we control in the baseline specification for both announced and implemented measures beyond LTV restrictions. Results are presented in **??**. We observe that the estimated impulse response functions are qualitatively similar to our baseline estimates but we note a few differences. The reactions of interest rates and house prices are more precisely estimated after we control for announcements of other borrower-based measures and the initial decline in new lending rates disappears. These results suggest that LTV measures may be more effective than other borrower-based measures for taming the credit cycle further motivating our focus on this type of macroprudential actions.

7. Asymmetric Effects

When authorities decide on the appropriate action targeting borrowers, not only do they have to make a choice among a menu of available borrower-based measures, but they also have to set the key parameters of the chosen measure. The design of macroprudential policy measures could be a deciding factor for their effectiveness.

In this section, we investigate how the design of an announced LTV measure and in particular its legal status and possible exceptions can alter its effects on households' behaviour and the housing market. First, 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 act or decrees, they are classified as binding. Second, we classify actions according to an exemption called the "speed limit". This feature allows a certain percentage of new mortgage production to exceed the specified maximum LTV ratio limit. The latter is often used to assist credit constraint borrowers, such as first-time buyers.

To estimate whether responses of the main dependent variables differ by type of LTV measures, we re-estimate the baseline local projections specification and replace the index of announced measures with one index per type of measures. Thus, we rewrite the local projections equation as:

$$\Delta_h y_{i,t} = \alpha_i^h + \tau_t^h + \sum_{p=1}^{4} \gamma_p^h y_{i,t-p} + \sum_{p=0}^{4} \beta_p^{1,h} ltv_{i,t-p}^1 + \sum_{p=0}^{4} \beta_p^{2,h} ltv_{i,t-p}^2 + \sum_{p=0}^{4} \Theta_p^h X_{i,t-p} + \epsilon_{i,t+h} \quad (3)$$

where $ltv^1_{i,t}$ denotes the index of announced binding measures or measures without the "speed limit", while $ltv^2_{i,t}$ denotes the index of announced non-binding measures or measures with "speed limit". Control variables are the same as in the baseline specification and we retain the same first-stage regression weights. Impulse response functions are produced by plotting the sequences of estimated coefficients $\{\beta^{1,h}_{p=0}\}_{h\geq 0}$ and $\{\beta^{2,h}_{p=0}\}_{h\geq 0}$.

First, we focus on responses after an announcement of legally binding measures and measures relying on soft law. According to our classification, 24 announcements are associated with legally binding measures and 8 with non-legally binding measures (or soft-law measures). We plot the responses of our main dependent variables in Fig. 10. 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 to the opposite direction for real household credit and real house prices, while they seem to lead to a negative and statistically significant reaction of new lending rates following announcement. We revisit this point later in this section.

Next, we focus on measures with "speed-limit" and investigate whether announcements of such measures can lead to different reactions of our main variables. In total, there are 9 announcements of measures with the "speed-limit" option in our sample. Responses after the announcement of tightening measures with and without the "speed-limit" are presented in Fig. 11. Similarly to the findings for binding and non-binding measures, we observe noticeable differences in the reaction of our dependent variables. Announcements of measures with the "speed-limit" do not seem to create strong contractionary effects on credit growth, real house prices or household durable goods consumption. Instead, measures that did not allow for the "speed-limit" result to contractionary effects similar to these estimated following the announcement of legally binding measures. Interestingly, interest rates react upwards after an announcement of a measure without "speed-limit" and the response is significant at the 90% confidence level at its peak.

The responses of the dependent variables following announcements of soft-law measures deserves more attention, as it can uncover details on the transmission mechanism of such policies. One would expect that announcements of measures based on soft-law do not lead to any significant responses. However, we observe responses which could point to an expansion in the housing market following the announcement. Real household credit and real house prices react positively, while lending rates negatively and real household durable goods consumption does not react. At the same time, we observe that three soft-law measures in our sample also embed the "speed-limit" option. It is likely that this overlap not only leads to non-significant responses, but even to responses to the opposite direction. The latter could be due to signalling effects for the imposition of a subsequent

"hard" LTV ratio limit in the market and thus, households are incentivized to accelerate their house purchases. To investigate this, we re-estimate the responses presented in Fig. 10 by deleting the non-binding measures which embed the "speed-limit" option. These additional results are presented in **??** of the Online Appendix. We observe that after deleting these measures, the response of credit and house prices becomes statistically not significant but the dynamic responses of the binding measures remain robust.

Overall, our results suggest that 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. Further, the balance of the difference features embedded in the announced measures may not only reduce their effectiveness, but potentially lead to counterproductive outcomes in contrast to the intentions of the authorities.

8. Conclusions

In this paper, we provide novel evidence for the effects of macroprudential policies on announcement. We collect data on announcements of LTV measures taken in 28 EU countries over the period 2000-2019 and investigate their effects on the mortgage market and households by also considering their impact on the cost of credit.

Our main findings can be summarised as follows. First, we find that announcements of LTV ratio measures that will be implemented in subsequent quarters can have sizeable impact on credit, suggesting that anticipation effects can be significant. House prices react with a time lag to LTV announcements, although this reaction is imprecisely estimated. Second, we show that macroprudential policy announcements induce households to reduce their consumption of durable goods, whereas the effects of LTV measures on overall household consumption are more muted. Third, announcements of macroprudential LTV measures can increase the cost of mortgage lending for households. Finally we show that the contractionary effects after the announcement of macroprudential policies are mostly driven by legally binding measures with no embedded exemptions in their application.

Our results have a number of policy implications. First, they call for careful consideration of the timing between announcement and implementation of macroprudential policy measures. However, the effects of these announcements are contained in the mortgage market and their impact on the broader economy via household consumption is limited. Second, LTV policies might increase the risk of new loan production as banks shift their supply to riskier borrowers. On the other hand, the higher cost of borrowing in the short-term could function as an additional risk reduction mechanism by deterring eligible borrowers to increase their leverage. Further, the increase of mortgage interest rates during the first quarter post announcement could imply interference with monetary policy, calling for potential coordination. At last, 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 it would be interesting to extend the dataset on announcements to cover a broader sample of countries and time periods. Further, in this study we investigate only one particular channel of borrower-based measures, that is the effect on mortgage market and household behaviour. However it would be desirable 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.

References

Acharya, V. V., Bergant, K., Crosignani, M., Eisert, T. and McCann, F. J. (2020), The Anatomy of the Transmission of Macroprudential Policies, NBER Working Paper 27292, National Bureau of Economic Research.

Aiyar, S., Calomiris, C. W., Hooley, J., Korniyenko, Y. and Wieladek, T. (2014), 'The International Transmission of Bank Capital Requirements: Evidence from the UK', *Journal of Financial Economics* **113**(3), 368–382.

Aiyar, S., Calomiris, C. W. and Wieladek, T. (2016), 'How Does Credit Supply Respond to Monetary Policy and Bank Minimum Capital Requirements?', *European Economic Review* **82**, 142–165.

Akinci, O. and Olmstead-Rumsey, J. (2018), 'How Effective are Macroprudential Policies? An Empirical Investigation', *Journal of Financial Intermediation* **33**, 33–57.

Alam, Z., Alter, A., Eiseman, J., Gelos, R., Kang, H., Narita, M., Nier, E. and Wang, N. (2019), Digging Deeper–Evidence on the Effects of Macroprudential Policies from a New Database, IMF Working Paper 19/66, International Monetary Fund.

Ayyagari, M., Beck, T. and Martinez Peria, M. S. (2017), Credit Growth and Macroprudential Policies: Preliminary Evidence on the Firm Level, BIS Working Paper No. 91, Bank for International Settlements.

Beirne, J. and Friedrich, C. (2017), 'Macroprudential Policies, Capital Flows, and the Structure of the Banking Sector', *Journal of International Money and Finance* **75**, 47–68.

Bianchi, J. and Mendoza, E. G. (2018), 'Optimal Time-Consistent Macroprudential Policy', *Journal of Political Economy* **126**(2), 588–634.

Bruno, V. and Shin, H. S. (2015), 'Capital Flows and the Risk-Taking Channel of Monetary Policy', *Journal of Monetary Economics* **71**, 119–132.

Buch, C. M., Bussiere, M., Goldberg, L. et al. (2017), 'International Prudential Policy Spillovers: Evidence from the International Banking Research Network', *International Journal of Central Banking* **13**(2), 1–4.

Cerutti, E., Claessens, S. and Laeven, L. (2017), 'The Use and Effectiveness of Macroprudential Policies: New Evidence', *Journal of Financial Stability* **28**, 203–224.

Cerutti, E., Correa, R., Fiorentino, E. and Segalla, E. (2016), 'Changes in Prudential Policy Instruments', *International Journal of Central Banking* **13**(16/110), 477–503.

Ciccarelli, M., Maddaloni, A. and Peydró, J.-L. (2013), 'Heterogeneous Transmission Mechanism: Monetary Policy and Financial Fragility in the Eurozone', *Economic Policy* **28**(75), 459–512.

Cizel, J., Frost, J., Houben, A. and Wierts, P. (2019), 'Effective Macroprudential Policy: Cross-Sector Substitution from Price and Quantity Measures', *Journal of Money, Credit and Banking* **51**(5), 1209–1235.

Claessens, S., Ghosh, S. R. and Mihet, R. (2013), 'Macroprudential Policies to Mitigate Financial System Vulnerabilities', *Journal of International Money and Finance* **39**, 153–185.

Dell'Ariccia, G., Igan, D., Laeven, L. and Tong, H. (2016), 'Credit Booms and Macrofinancial Stability', *Economic Policy* **31**(86), 299–355.

Eickmeier, S., Kolb, B. and Prieto, E. (2018), Macroeconomic Effects of Bank Capital Regulation, Discussion Paper 44, Deutsche Bundesbank.

European Systemic Risk Board (2014), Flagship Report on Macro-prudential Policy in the Banking Sector, ESRB Policy Report.

Fendoglu, S. (2017), 'Credit Cycles and Capital Flows: Effectiveness of the Macroprudential Policy Framework in Emerging Market Economies', *Journal of Banking and Finance* **79**, 110–128.

Galati, G. and Moessner, R. (2018), 'What Do We Know About the Effects of Macroprudential Policy?', *Economica* **85**(340), 735–770.

Greenwood-Nimmo, M. and Tarassow, A. (2016), 'Monetary Shocks, Macroprudential Shocks and Financial Stability', *Economic Modelling* **56**, 11–24.

Jiménez, G., Ongena, S., Peydró, J.-L. and Saurina, J. (2017), 'Macroprudential Policy, Countercyclical Bank Capital Buffers, and Credit Supply: Evidence from the Spanish Dynamic Provisioning Experiments', *Journal of Political Economy* **125**(6), 2126–2177.

Jordà, Ò. (2005), 'Estimation and Inference of Impulse Responses by Local Projections', *American Economic Review* **95**(1), 161–182.

Jordà, Ò. and Taylor, A. M. (2016), 'The Time for Austerity: Estimating the Average Treatment Effect of Fiscal Policy', *Economic Journal* **126**(590), 219–255.

Kim, S. and Mehrotra, A. (2018), 'Effects of Monetary and Macroprudential Policies - Evidence from Four Inflation Targeting Economies', *Journal of Money, Credit and Banking* **50**(5), 967–992.

Kim, S. and Mehrotra, A. N. (2019), Examining Macroprudential Policy and its Macroeconomic Effects – Some New Evidence, BIS Working Papers No 825, Bank for International Settlements.

Klingelhöfer, J. and Sun, R. (2019), 'Macroprudential Policy, Central Banks and Financial Stability: Evidence from China', *Journal of International Money and Finance* **93**(79033), 19–41.

Kuttner, K. N. and Shim, I. (2016), 'Can Non-Interest Rate Policies Stabilise Housing Markets? Evidence from a Panel of 57 Economies', *Journal of Financial Stability* **26**(433), 31–44.

Leeper, E. M., Walker, T. B. and Yang, S.-C. S. (2013), 'Fiscal Foresight and Information Flows', *Econometrica* **81**(3), 1115–1145.

Lim, C. H., Costa, A., Columba, F., Kongsamut, P., Otani, A., Saiyid, M., Wezel, T. and Wu, X. (2011), Macroprudential Policy: What Instruments and How to Use Them? Lessons from Country Experiences, IMF Working Papers WP/11/238, International Monetary Fund.

Mendoza, E. G. (2010), 'Sudden Stops, Financial Crises and Modelling Credit Constraints', *American Economic Review* **100**(5), 1941–1966.

Mertens, K. and Ravn, M. O. (2012), 'Empirical Evidence on the Aggregate Effects of Anticipated and Unanticipated US Tax Policy Shocks', *American Economic Journal: Economic Policy* **4**(2), 145–81.

Morgan, D. P. and Ashcraft, A. B. (2003), 'Using Loan Rates to Measure and Regulate Bank Risk: Findings and an Immodest Proposal', *Journal of Financial Services Research* **24**(2-3), 181–200.

Morgan, P. J., Regis, P. J. and Salike, N. (2019), 'LTV Policy as a Macroprudential Tool and its Effects on Residential Mortgage Loans', *Journal of Financial Intermediation* **37**, 89–103.

Poghosyan, T. (2020), 'How Effective is Macroprudential Policy? Evidence from Lending Restriction Measures in EU Countries', *Journal of Housing Economics* **49**, 101694.

Ramey, V. A. (2011), 'Identifying Government Spending Shocks: It's All in the Timing', *The Quarterly Journal of Economics* **126**(1), 1–50.

Reinhardt, D. and Sowerbutts, R. (2015), Regulatory Arbitrage in Action: Evidence from Banking Flows and Macroprudential Policy, BoE Working Paper 546, Bank of England.

Richter, B., Schularick, M. and Shim, I. (2019), 'The Costs of Macroprudential Policy', *Journal of International Economics* **118**, 263–282.

Romer, C. D. and Romer, D. H. (1989), 'Does Monetary Policy Matter? A New Test in the Spirit of Friedman and Schwartz', *NBER Macroeconomics Annual* **4**, 121–170.

Romer, C. D. and Romer, D. H. (2004), 'A New Measure of Monetary Shocks: Derivation and Implications', *American Economic Review* **94**(4), 1055–1084.

Romer, C. D. and Romer, D. H. (2010), 'The Macroeconomic Effects of Tax Changes: Estimates Based on a New Measure of Fiscal Shocks', *American Economic Review* **100**(3), 763–801.

Shim, I., Bogdanova, B., Shek, J. and Subelyte, A. (2013), 'Database for Policy Actions on Housing Markets', *BIS Quarterly Review* (September), 83–95.

Tillmann, P. (2015), 'Estimating the Effects of Macroprudential Policy Shocks: A Qual VAR Approach', *Economics Letters* **135**, 1–4.

Tzur-Ilan, N. (2019), Unintended Consequences of Credit Constraints on Housing: The Case of LTV Limits. SSRN Electronic Journal.

Vandenbussche, J., Vogel, U. and Detragiache, E. (2015), 'Macroprudential Policies and Housing Prices: A New Database and Empirical Evidence for Central, Eastern and Southeastern Europe', *Journal of Money, Credit and Banking* **47**(1), 1–35.

Zhang, L. and Zoli, E. (2016), 'Leaning Against the Wind: Macroprudential Policy in Asia', *Journal of Asian Economics* **42**, 33–52.

Figures and Tables

Table 1: Descriptive statistics

	Count	Mean	Std. Deviation	Minimum	Maximum
Credit to households, qoq	1923	1.39	3.23	-31.51	22.06
Real house prices, qoq	1796	0.44	3.02	-25.11	23.92
New mortgage rate	1904	4.27	2.19	0.81	17.41
Real household consumption, qoq	2105	0.50	1.67	-14.27	11.19
Real household durables consumption, qoq	2105	0.38	4.73	-54.48	31.78
Real household non-durables consumption, qoq	2105	0.52	1.60	-13.16	9.28
New mortgage rate, fixed below 1 year	1864	4.03	1.85	0.80	12.32
New mortgage rate, fixed above 1 year	1570	4.38	2.03	1.00	14.52
Monetary policy rate	2172	1.74	2.64	-0.75	21.25
3-month interbank rate	2164	2.73	4.11	-0.56	70.25
Long-term interest rate	2010	3.96	2.39	-0.16	25.40
Real GDP, qoq	2182	0.61	1.38	-12.70	23.20
Inflation rate	2184	0.62	1.17	-3.53	9.42

Figure 1: Yearly number of implemented and announced LTV actions in European economies.

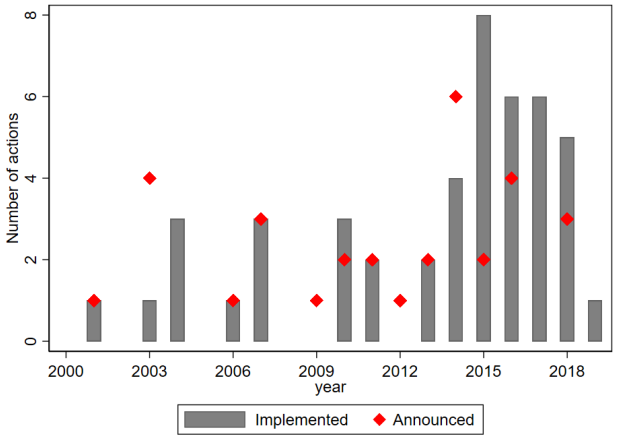


Figure 2: Implementation schedule of LTV actions.

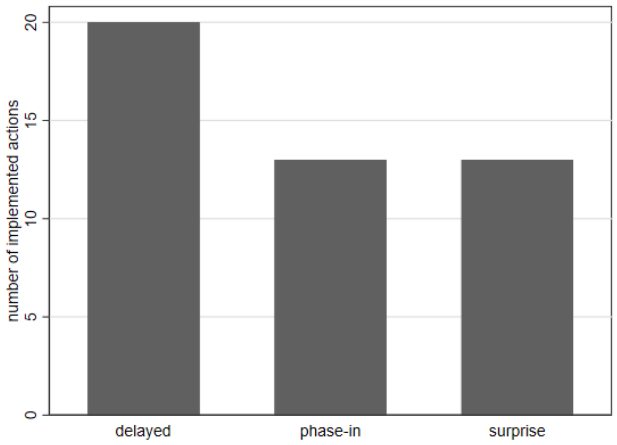


Figure 3: Yearly number of announcements of tightening LTV actions and yearly number of actions of other borrower-based measures.

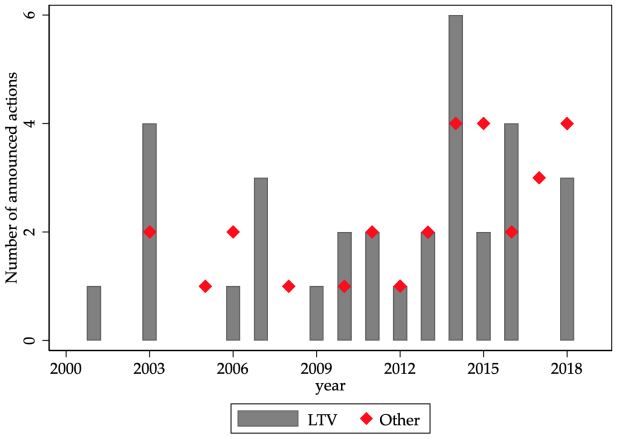
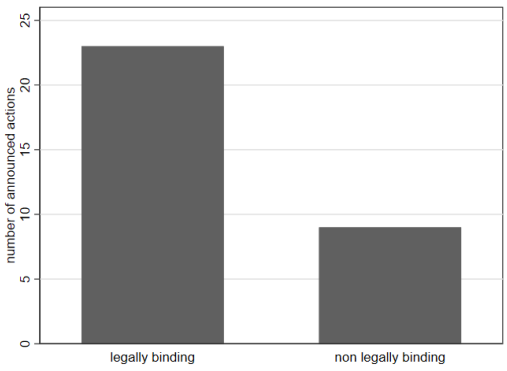
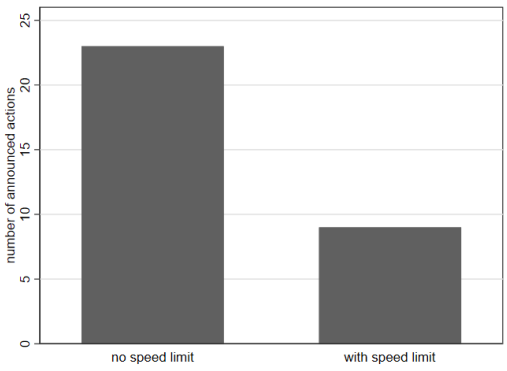


Figure 4: Classification of announced LTV actions by legal status and design.

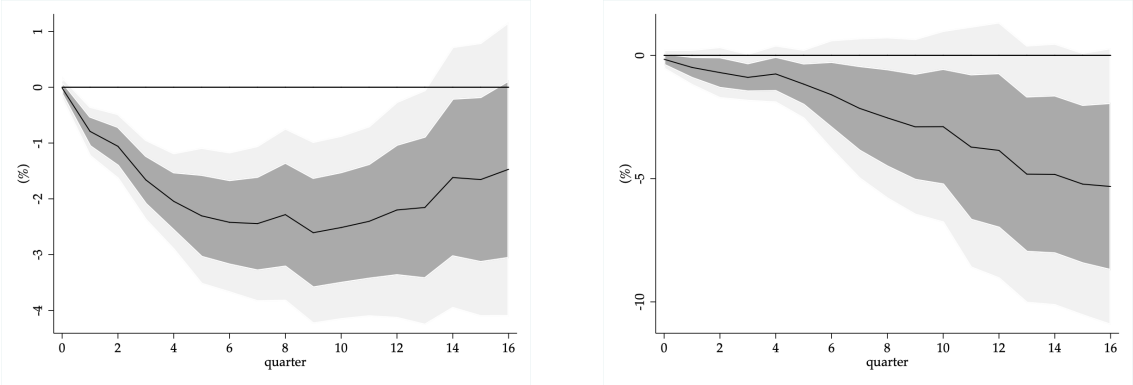


(a) legal status



(b) speed limit

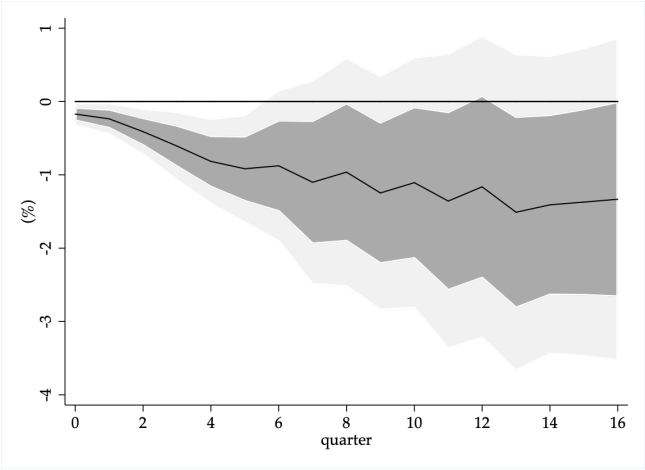
Figure 5: Responses of real household credit and real house prices to an announcement of tightening in the maximum LTV ratio.



(a) real household credit (b) real house prices

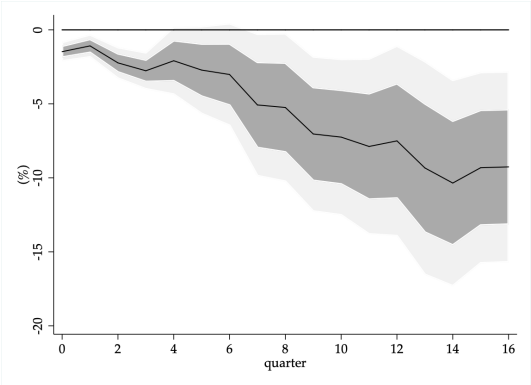
Notes: Shaded areas correspond to 68% and 90% confidence intervals.

Figure 6: Response of real household consumption to an announcement of tightening in the maximum LTV ratio.

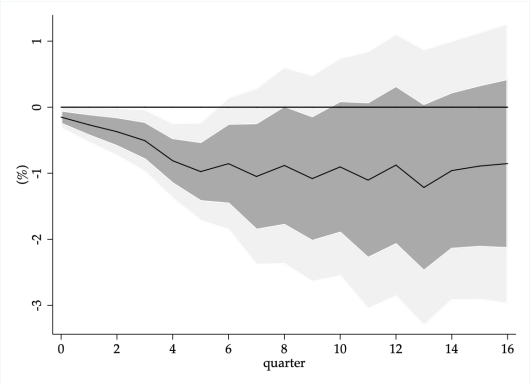


Notes: Shaded areas correspond to 68% and 90% confidence intervals.

Figure 7: Responses of real household durable goods consumption and real household consumption excluding consumption of durable goods to an announcement of tightening in the maximum LTV ratio.



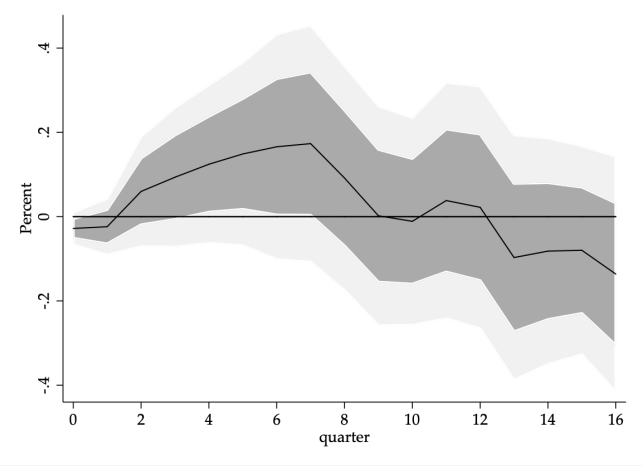
(a) real household durable goods consumption



(b) real household durable goods consumption, excl. consumption of durable goods

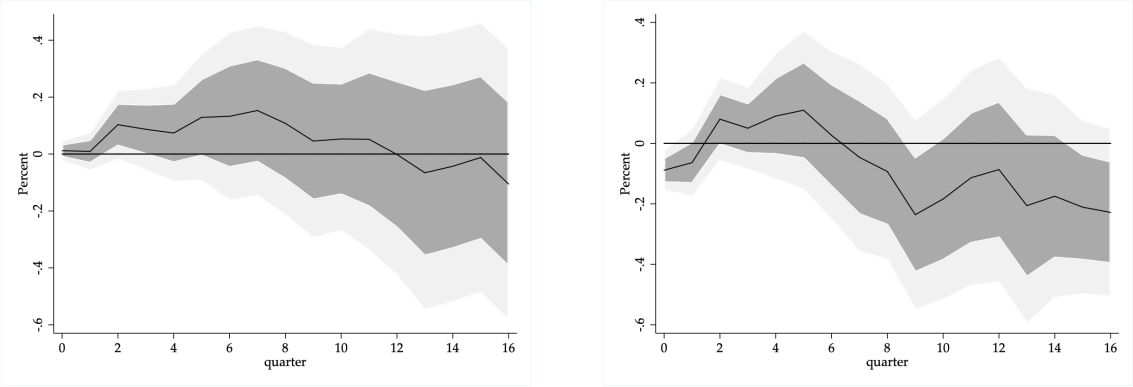
Notes: Shaded areas correspond to 68% and 90% confidence intervals.

Figure 8: Response of new mortgage lending rate to an announcement of tightening in the maximum LTV ratio.



Notes: Shaded areas correspond to 68% and 90% confidence intervals.

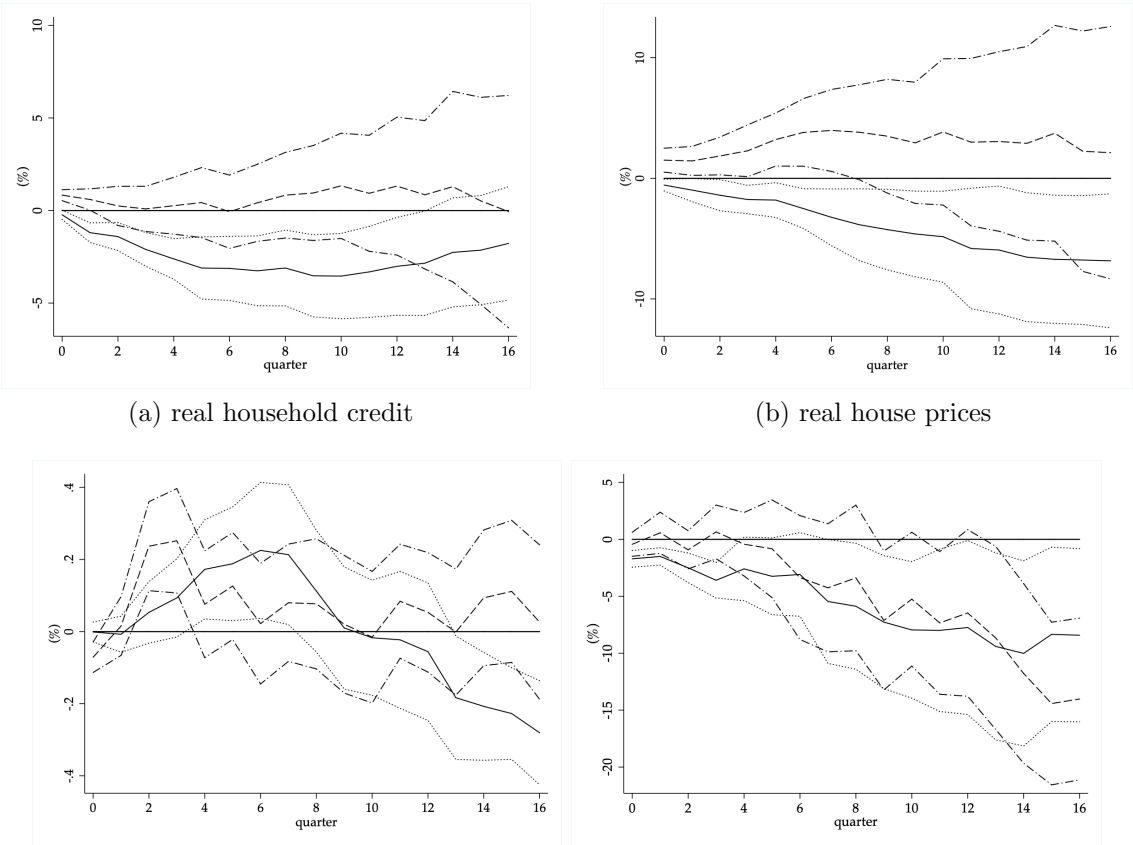
Figure 9: Responses of new mortgage lending rates with an interest rate fixation up to 1 year and new mortgage lending rates with interest rate fixation period above 1 year to an announcement of tightening in the maximum LTV ratio.



(a) fixation period up to 1 year (b) fixation period above 1 year

Notes: Shaded areas correspond to 68% and 90% confidence intervals.

Figure 10: Responses to an announcement of tightening in the maximum LTV ratio, binding and non-binding measures.

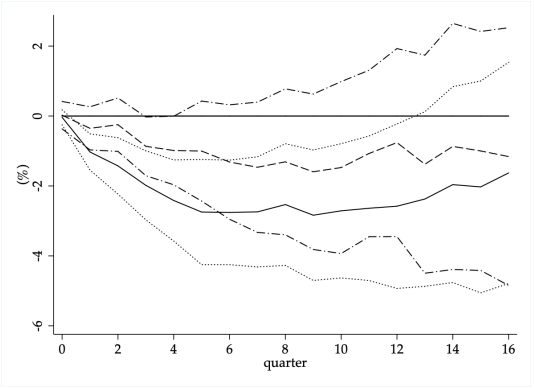


(a) real household credit (b) real house prices

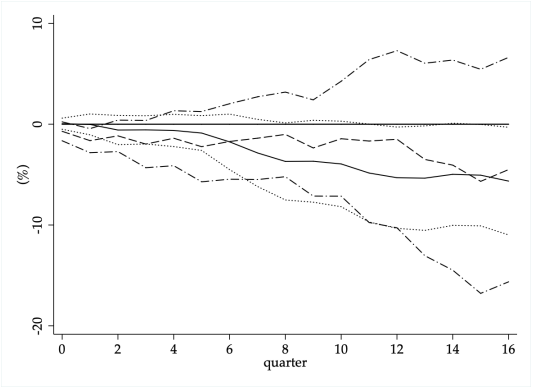
(c) new lending rate (d) real household durable goods consumption

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. Dotted and dash-dotted lines correspond to 90% confidence intervals.

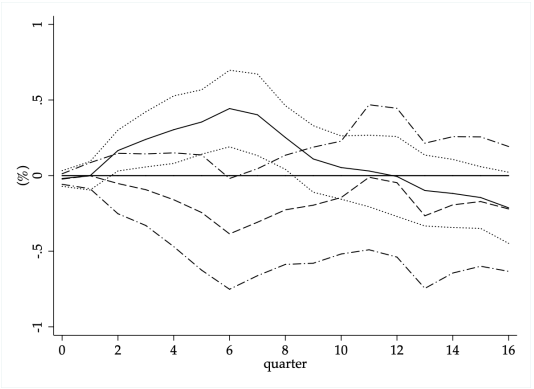
Figure 11: Responses to an announcement of tightening in the maximum LTV ratio, measures with and without speed-limit.



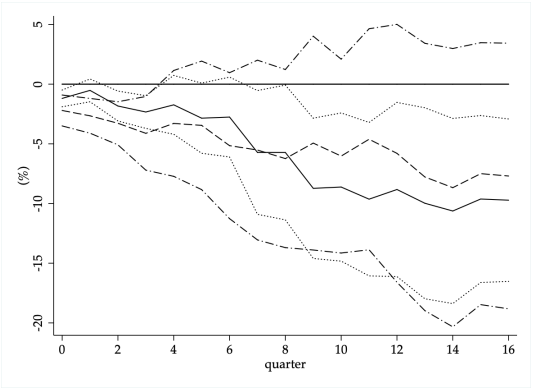
(a) real household credit



(b) real house prices



(c) new lending rate



(d) real household durable goods consumption

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. Dotted and dash-dotted lines correspond to 90% confidence intervals.

DeNederlandscheBank

EUROSYSTEEM

De Nederlandsche Bank N.V.
Postbus 98, 1000 AB Amsterdam
020 524 91 11
dnb.nl