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Abstract

Central banks increasingly rely on macroprudential measures to manage the financial cycle. However, the effects of such policies on the core objectives of monetary policy to stabilise output and inflation are largely unknown. In this paper we quantify the effects of changes in maximum loan-to-value (LTV) ratios on output and inflation. We rely on a narrative identification approach based on detailed reading of policy-makers' objectives when implementing the measures. We find that over a four year horizon, a 10 percentage point decrease in the maximum LTV ratio leads to a 1.1% reduction in output. As a rule of thumb, the impact of a 10 percentage point LTV tightening can be viewed as roughly comparable to that of a 25 basis point increase in the policy rate. However, the effects are imprecisely estimated and the effect is only present in emerging market economies. We also find that tightening LTV limits has larger economic effects than loosening them. At the same time, we show that changes in maximum LTV ratios have substantial effects on credit and house price growth. Using inverse propensity weights to rerandomise LTV actions, we show that these effects are likely causal.

Keywords: macroprudential policy, loan-to-value ratios, local projections, narrative approach.

JEL Classification: E58, G28.

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1. Introduction

How do macroprudential policies interact with the core objectives of monetary policy to stabilise prices and output? As a response to the global financial crisis, central banks and regulators across the world have increasingly relied on macroprudential policies to maintain financial stability. A recent literature has shown that policy-makers can moderate credit and asset price cycles using macroprudential instruments (e.g. Akinci and Olmstead-Rumsey (2018), Bruno *et al.* (2017) and Kuttner and Shim (2016)). This way, they can reduce negative output tail risks emanating from the link between excess credit and costly financial crises (Schularick and Taylor (2012) and Jordà *et al.* (2013)). However, there is very little empirical evidence on how the use of such instruments affects the traditional objectives of monetary policy, that is, output and inflation.

In this paper, we explore the effects of macroprudential measures on output and inflation for a large cross-country panel of 56 countries over more than two decades. Building on a new dataset, we use a narrative approach in order to address identification challenges. Our results show that macroprudential measures, specifically changes in the maximum loan-to-value (LTV) ratio, do have modest and imprecisely estimated spillover effects on output and inflation. In particular, we find that a 10 percentage point reduction in the maximum LTV ratio lowers output by about 1.1% after four years. However, this effect is rather imprecisely estimated. The near-term impact on the price level is in most specifications even smaller and close to zero. The effect is more pronounced in emerging market economies (EMEs), while the path of output is almost unaffected by LTV limit changes in the set of advanced economies (AEs). In a back-of-the-envelope calculation, we compare the magnitude of this effect to estimates of GDP responses to monetary policy in Jordà *et al.* (2017), which are also based on a broad sample of countries. We find that the two-year response of GDP to a 10 percentage point reduction in the maximum LTV ratio can be compared to that of a 26 basis points increase in interest rates.

Importantly we also test for potential asymmetries and find that the output effect is mainly driven by the negative effects of tightenings in maximum LTV ratios, and not by higher output generated through loosening actions. We also assess the treatment effects of LTV limit tightenings on financial variables using inverse propensity weighting and find that credit and house prices fall after a tightening. Overall, these results imply that central banks might be able to use macroprudential policies to dampen the financial cycle without risking major interference with their core monetary policy objectives.

Macroprudential measures are not randomly assigned. In the ideal environment to measure the effects of changes in LTV limits on output and inflation, the following three conditions are satisfied: (i) LTV policy actions are exogenous with respect to current and lagged real variables; (ii) such actions are uncorrelated with other shocks (e.g., monetary policy acting at the same time); and (iii) they are unexpected. While the unsystematic nature of macroprudential policies means they are typically unexpected, we clearly need to worry about the exogeneity of the policy action. To address the exogeneity condition, we rely in this paper on a novel hand-collected dataset detailing the intentions or stated objectives of policy-makers when they change LTV limits. This approach is in the tradition of the narrative approach pioneered in Friedman and Schwartz (1963) and Romer and Romer (1989). In a similar spirit to the narrative identification of monetary policy shocks, we argue that macroprudential actions taken without reference to current or expected trends in real output and inflation can be seen as exogenous with respect to price and output stabilisation objectives of monetary policy. This new narrative measure therefore allows us to establish the causal effects of macroprudential actions on economic activity and inflation. Using a battery of tests, we confirm that there is indeed no systematic relationship between changes in our narrative measure and real economic variables. To address the second condition, we control for monetary policy shocks in all our specifications. To trace the dynamic propagation of such exogenous policy interventions, we rely on local projections (Jordà (2005)).

Our identification approach requires detailed reading and understanding of the underlying motivations for macroprudential measures and the information set of policy-makers. To keep the required information manageable, we focused on one specific tool that is frequently used to tackle boom-bust cycles in credit and housing markets, namely, changes in maximum LTV ratios. We compiled a comprehensive new dataset consisting of quarterly observations of LTV actions in 56 economies, building on the database developed by Shim *et al.* (2013). Our quarterly dataset contains 92 changes in maximum LTV ratios and loan prohibitions.

Almost all papers in the literature on measuring the effectiveness of macroprudential actions employ dummy variables to measure macroprudential policies. Such variables however do not capture the intensity of policy actions of the same type. For instance, a decrease in the maximum LTV ratio by 10 percentage points and a decrease in the ratio by 20 percentage points are treated equally. We use instead a numerical variable quantifying the quarterly changes in the maximum LTV ratio. To our knowledge, this paper is the first that constructs an intensity-adjusted LTV change variable, which considers not only the change in the maximum LTV ratio in percentage points, but also accounts for the scope of loans to which such a change is applied. This allows us to estimate the effect of a one percentage point change in LTV limits as described above. We also assess the differences between tightenings and loosenings and find that the negative long-run effects are driven mainly by tightenings.

Do LTV limits help dampen the financial cycle? To answer this question, we turn to inverse propensity weighting to mimic random allocation. As in Jordà *et al.* (2015), we employ a two-stage procedure, where the probability of an economy being treated with a macroprudential action, here a tightening in maximum LTV ratios, is estimated in a separate first stage regression. This purges the data of observable sources of endogeneity. In the second stage regression, observations are weighted inversely to the estimated probability of receiving treatment, thus giving greater weight to an action that comes closer to the random allocation ideal. We find indeed that real household credit and real mortgage credit are reduced when LTV limits are decreased. At the same time, house prices fall. Macroprudential policies seem to achieve the desired targets. Our results on real variables indicate furthermore that they do so at a relatively small cost. Therefore, for central banks such macroprudential measures may serve as a complementary policy tool that does not interfere with other objectives in a major way.

This paper is organised as follows. Section 2 provides a literature review. Section 3 describes data and empirical strategies focusing on how we identify the stated objectives of LTV measures, how we construct the intensity-adjusted quantifiable LTV change variable, and whether LTV measures are exogenous to the real cycle. Section 4 presents the empirical results on the response of real variables to changes in LTV limits. Section 5 considers the response of financial variables to LTV actions. Finally, section 6 concludes.

2. Literature

A large amount of literature has considered the effects of monetary policy on output and price levels. For an overview of the literature on this relationship see Ramey (2016). While the global financial crisis has renewed interest in the ability of macroprudential policies to help manage the financial cycle, the responses of output and inflation to macroprudential policies have rarely been addressed. The few investigations into the effects of these policies often use historical data on credit controls that were common in many Western European countries in the decades following World War II (Kelber and Monnet (2014)).

As one of the few studies, Aikman *et al.* (2016) evaluate in a joint framework the impact of monetary and macroprudential policies in the United Kingdom from the 1950s to the early 1980s. They rely on local projection methods to estimate impulse response functions to the two policy shocks augmented by forecasts (in line with Romer and Romer (1989)) and factors (in line with Bernanke *et al.* (2005)). They find that an analogue to macroprudential measures, credit controls, were quite effective in taming the credit cycle and had a dampening effect on industrial output. Furthermore, they find little evidence for an effect of credit controls on the price level. Monnet (2014) studies the effect of quantitative controls on money and credit during France's golden age between 1948 and 1973 and finds strong effects on output and prices. Similar to our approach, identification builds on narrative evidence on the intentions associated with enacted policies extracted from archival records.

There are also a few contributions building on recently collected cross-country databases on macroprudential policy actions. Kim and Mehrotra (2017) analyse the responses of credit, output and inflation to changes in macroprudential and monetary policies based on data for four countries in the Asia-Pacific region using a VAR framework. They find a negative effect of changes in macroprudential policies on output as well as inflation. Their macroprudential policy measures are based on data from the Shim *et al.* (2013) database. Also based on this database, Boar *et al.* (2017) analyse the relationship between a country's propensity to use macroprudential measures and output outcomes. Dividing countries into two groups depending on their use of macroprudential policies, they find countries which use macroprudential policies more frequently, experience higher growth rates in the cross-section, while the use of macroprudential policies reduces output volatility. Finally, Sanchez and Röhn (2016) analyse the effects of various policies on economic growth using quantile regressions. They find that macroprudential policies reduce output growth, but also reduce the tail risk of output growth. Among other policy variables, they specifically analyse the role of LTV policies (based on a dummy variable) and find consistently negative effects of these measures on output growth. However, the significance of this result depends on the quantiles analysed.

More generally, many studies have examined the impact of macroprudential policies on the financial cycle, particularly on measures of credit and house price cycles. For example, Kuttner and Shim (2016) find that introductions or reductions of the maximum debt-service-to-income (DSTI) ratio and increases in housing-related taxes have significant negative effects on real housing credit growth for 55 economies over the period of 1980 to 2012. They find that a typical DSTI tightening action lowers the real credit growth rate by 4-6 percentage points over four quarters. Using data on total credit to households and non-profit institutions serving households (NPISHs) from the BIS total credit database, Cerutti *et al.* (2015) find that borrower-based measures such as the maximum LTV ratio and the maximum DSTI ratio are associated with lower growth in credit to households over 2001–13. Dell'Ariccia *et al.* (2012) find that macroprudential policies can contain the incidence of credit booms and limit the costs of busts associated with credit booms.

Claessens *et al.* (2013) investigate how macroprudential policies affect individual banks and find that maximum LTV and DSTI ratios reduce asset and leverage growth. Policies implemented in adverse times, however, do not help to stop declines. In a single country study, Wong *et al.* (2011) find that higher LTV caps lead to a lower level of the mortgage debt-to-GDP ratio in Hong Kong SAR in the 1990s and 2000s. Tillmann (2015) considers the impact of LTV and DSTI limits on household credit in Korea from 2000Q1 to 2012Q4. In particular, he uses a qualitative VAR method to estimate impulse response functions for macroprudential shocks. He found an unexpected tightening in LTV and DSTI limits had a significant effect on household credit growth in Korea.

Using a dynamic stochastic general equilibrium (DSGE) model with housing and household debt, Alpanda and Zubairy (2017) consider the effectiveness of monetary policy, LTV limits and housing-related tax policies on reducing household indebtedness. They find that reductions in mortgage interest payment deductions and regulatory LTV ratios are the most effective tools to limit household credit, as these measures are the most targeted. Rubio and Yao (2017) show in a DSGE model that a macroprudential authority can act as a complementary macro-financial stabiliser for both real and financial cycles when the steady-state interest rate is low and monetary policy hits the zero lower bound.

While all these papers study changes in regulatory caps on LTV ratios, Bachmann and Rueth (2017) analyse the effects of changes in average LTV ratios on output and credit in the United States. They use a structural VAR framework to identify exogenous variation in LTV ratios and find that a 25 basis points tightening in the LTV ratio reduces GDP by approximately 0.1%. At the same time, they find that the Federal Reserve responded to a tightening in LTV ratios with lower policy rates. As a result, mortgage rates fall and residential investment increases after a tightening in LTV ratios.

The narrative approach focuses on evidence derived from the historical record (Romer and Romer (1989)). More specifically, researchers conduct narrative analysis by systemically using qualitative information from contemporary primary sources to construct numerical measures often with the aim of addressing issues of causation. Narrative analysis has been used not only in the context of monetary policy (Friedman and Schwartz (1963) and Romer and Romer (1989)) but also for fiscal policy (Romer and Romer (2007) and Gillitzer (2017)) and financial distress episodes (Romer and Romer (2017)). By contrast, the narrative approach has not been used in the context of macroprudential policy. Budnik and Kleibl (2018) describe a database on policy actions of a macroprudential nature taken by the European Union member countries to affect the banking sector in 1995–2014. The information in the database is based on responses from a survey with the aim of using narrative information for the impact assessment of macroprudential policies.

3. Data and empirical strategy

We base our analysis on quarterly data for 56 economies, including both AEs and EMEs, from 1990Q1 to 2012Q2.[1] The data used in this paper rely on various sources such as the BIS Databank (national sources) and the database on housing market policy actions from Shim *et al.* (2013). As our dependent variables we use output (real GDP) and the level of the consumer price index from the BIS Databank. For credit variables, we use data from the BIS Databank on bank credit to the private non-financial sector, bank credit to households and housing credit. Explanatory variables include policy variables, other macroeconomic variables, asset prices and structural variables. For policy rates, we use actual policy rates, backdated with one-month or three-month market interest rates obtained from the BIS Databank. For the US policy rate, we use the federal funds rate obtained from Bloomberg.

A major contribution lies in the construction of additional data on macroprudential policy actions: we extend the database for policy actions on housing markets constructed by Shim *et al.* (2013).[2] While the literature has so far analysed macroprudential measures using dummy variables, we collected data to quantify policy actions. Our main focus is on changes in maximum LTV ratios. This instrument is comparable across countries and the size of a change can be identified in most instances. Because we want to measure the effects of macroprudential policies on output and inflation, we focus on changes in LTV limits that target the financial cycle without being driven by concerns about growth or inflation.

3.1. Narrative identification of macroprudential policy shocks

The greatest challenge in measuring the causal effects of macroprudential policy consists in constructing a measure of macroprudential policy shocks. The following three criteria must be fulfilled: (i) policy actions are exogenous with respect to the current and lagged real variables; (ii) actions are uncorrelated with other shocks; and (iii) they are unexpected. To address the exogeneity condition, we rely in this paper on a novel hand-collected dataset documenting the stated objectives of policy-makers when they change LTV limits.

[1] The 23 advanced economies are Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Italy, Japan, Luxembourg, Malta, Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, the United Kingdom and the United States. The 33 emerging market economies are Argentina, Brazil, Bulgaria, Chile, China, Chinese Taipei, Colombia, Croatia, the Czech Republic, Estonia, Hong Kong SAR, Hungary, India, Indonesia, Israel, Korea, Latvia, Lithuania, Malaysia, Mexico, Peru, the Philippines, Poland, Romania, Russia, Serbia, Singapore, Slovakia, Slovenia, South Africa, Thailand, Turkey and Ukraine.

[2] Actions included in the database can be sorted into the following categories: (i) total bank credit targeted measures, which capture changes in reserve requirements, liquidity requirements and credit growth limits; and (ii) housing credit targeted measures, which capture changes in LTV limits, DSTI limits, risk weights on housing loans, provisioning requirements and exposure limits on the property sector.

Table 1: *Stated objectives of 92 LTV actions, 56 economies, 1990Q1 - 2012Q2.*

Real objectives	GDP	3
	CPI	0
	Other	0
Financial objectives	House prices	36
	Total credit	1
	Housing credit	26
	Bank buffer	9
	Risk taking	34
	FX limits	6
	Other	1
Sum of objectives		116

Notes: In terms of objectives, LTV actions can be taken with reference to the real cycle, further distinguishing between GDP, inflation and other real objectives. LTV actions can also be taken with reference to the financial cycle, further broken down by their specific objectives: house price, total credit, housing or household credit, bank buffer, risk taking, FX borrowing and other financial objectives. In this table we report the number of LTV actions that were implemented referencing the respective objective. One LTV action can have more than one stated objective.

For the narrative identification, we proceeded in the following way. We first listed all 92 LTV policy actions documented in the database of Shim *et al.* (2013) from 1990Q1 to 2012Q2 for the 56 economies. Such actions consist of the introduction, tightening, loosening or abolition of the maximum LTV ratio and the prohibition of certain types of loan (that is, applying a zero LTV ratio). We then consulted official documents for each of these policy actions such as press releases announcing these actions, annual reports describing the background of specific policy actions taken and regulatory documents such as circulars to understand the reasoning behind those actions and identify objectives for the implemented LTV actions. We then classified the motivations broadly into real and financial objectives. More specifically, we classified real objectives into the following three categories: GDP, inflation and other real objectives. We also classified financial objectives into the following seven categories: house price, total credit, housing and household credit, bank buffer, risk taking, FX borrowing (including borrowing from non-residents), and other financial objectives. Table 1 provides a summary of the stated objectives of the 92 LTV actions taken among the 56 economies from 1990Q1 to 2012Q2. It should be noted that one LTV action can have more than one stated objective, hence the total number of stated objectives is greater than the total number of LTV actions.[3]

In the next step, we dropped all policy actions that were primarily motivated by real

[3] It should also be noted that for four country-quarter observations, two LTV actions were taken within one quarter (i.e., 2010Q2, 2010Q3 and 2011Q1 for China, and 1996Q2 for Singapore). For these four country-quarter observations, we need to carefully match the stated objective with each of the two actions. It turned out that all these eight LTV actions (2 actions x 4 quarters) had house prices as the only stated objective.

objectives. Yet since a policy action can have more than one stated objective, we went through all actions motivated by financial objectives and verified that at the same time the authorities did not voice concerns over real imbalances in the economy. Among the 92 LTV actions, three actions had a stated real objective, hence we excluded them from the sample. The resulting sample consists of 89 LTV actions accompanied by stated financial objectives only.

3.2. Intensity adjustment

So far, papers in the literature on measuring the effects of macroprudential policy in cross-country data use dummy variables taking the value 1 for tightening actions and zero otherwise, or those taking value 1 for tightening actions, -1 for loosening actions and zero for no change.[4] Such variables do not capture the intensity of policy actions of the same type. By definition, in such a research design a decrease in the maximum LTV ratio by 10 percentage points and a decrease in the ratio by 20 percentage points are treated equally. The coefficients on these dummy variables show the impact of a "typical" policy action in a certain type. That is because these coefficients only show the average impact of all policy actions with different magnitudes of actual changes.

To measure the economic magnitude of the impact of a certain type of macroprudential policy on target variables such as output and inflation, we need to construct a variable measuring the size of policy changes. This is especially important for policy-makers when they try to calibrate the size of the change in regulatory ratios to achieve a certain amount of slowdown in the growth rate of output, credit or asset prices. It should be noted that when we construct such a variable, we need to consider both the size of the change in the relevant ratio and the scope of such policy actions being applied, that is, the range of loans to which the change in the maximum LTV ratio applies.[5]

Specifically, the following criteria are applied to construct the intensity-adjusted LTV action variable. We denote this variable by $\Delta MaPP_{i,t}$.

- When the maximum LTV ratio is lowered by 10 percentage points, the LTV variable takes a value of 10. When the maximum LTV ratio is raised by 10 percentage points, the LTV variable takes a value of –10. This is in line with the aforementioned

[4] One exception is Vandenbussche *et al.* (2015). They quantify relative strength of different types of policy action by using conversion coefficients on changes in the regulatory capital ratio, the reserve requirement ratio and the maximum LTV ratio by central, eastern and southeastern European countries. Glocker and Towbin (2015) use intensity-adjusted reserve requirement variables for a single country, Brazil.

[5] In principle, one could also consider the initial level of the maximum LTV ratio, but we do not consider this aspect here.

Table 2: *List of 53 LTV actions with accurate information on the size of changes in LTV limits available and with only financial objectives stated.*

Economy	Timing	Scope Adjusted	Scope Unadjusted	Economy	Timing	Scope Adjusted	Scope Unadjusted	Economy	Timing	Scope Adjusted	Scope Unadjusted	Economy	Timing	Scope Adjusted	Scope Unadjusted
CN	2007Q3	1	10	HK	2010Q3	6	8.33	PH	2002Q3	−15	−20	IS	2004Q4	−5	−5
CN	2010Q1	2	20	HK	2010Q4	9.5	10	SG	1996Q2	18	90	IS	2006Q3	10	10
CN	2010Q2	11.5	35	HK	2011Q2	3	10	SG	2001Q4	−8	−80	IS	2007Q1	−10	−10
CN	2010Q3	10.5	43.33	KR	2002Q3	3.75	15	SG	2005Q3	−10	−10	IS	2008Q2	−8.53	−8.53
CN	2011Q1	6.5	40	KR	2002Q4	11.25	15	SG	2009Q3	9	90	IT	1995Q2	−27.5	−17.5
CN	2011Q3	5.5	55	KR	2003Q2	5	10	SG	2010Q1	10	10	LU	2008Q4	−20	−20
HK	1991Q4	20	20	KR	2003Q4	2.25	10	SG	2010Q3	2	10	NL	2011Q3	8.6	27
HK	1997Q1	5	10	KR	2004Q1	−1	−10	SG	2011Q1	5	20	NO	2011Q4	5	5
HK	1999Q1	−7.5	−15	KR	2005Q3	2.25	20	TH	2009Q1	−5	−10	CA	2008Q4	5	5
HK	2000Q3	−2.5	−5	KR	2006Q4	1.5	20	HU	2010Q3	26.25	52.5	CA	2010Q2	6.25	10
HK	2001Q4	−8	−20	KR	2008Q4	−3	−15	CL	2009Q3	−2.5	−25	CA	2011Q1	2.5	5
HK	2004Q3	−2.5	−5	KR	2009Q3	3.8	10	DK	2009Q3	−2.5	−5				
HK	2007Q4	−3.75	−15	KR	2009Q4	1	10	ES	2009Q2	−7.5	−15				
HK	2009Q4	8.75	12.5	KR	2012Q2	−0.5	−10	IS	2001Q3	−15	−15				

Notes: CN = China; HK = Hong Kong SAR; KR = Korea; PH = the Philippines; SG = Singapore; TH = Thailand; HU = Hungary; CL = Chile; DK = Denmark; ES = Spain; IS = Iceland; IT=Italy; LU = Luxembourg; NL = Netherlands; NO = Norway; CA = Canada. The values in the ”Timing” column are the quarter in which each LTV action was implemented. The figures in the ”Scope Adjusted” and ”Scope Unadjusted” columns are in percentage points. Positive values indicate tightening, and negative values loosening.

convention of assigning 1 to tightening actions and -1 to loosening actions for dummy variables.

- When a specific type of loan is prohibited, it is regarded as applying a zero maximum LTV ratio. If the previously applied maximum LTV ratio is known, then the level of the previous maximum LTV ratio is equal to the size of the change in the maximum LTV ratio. If there is no previous maximum LTV ratio applied to this type of loan, then it is regarded as the introduction of a new zero maximum LTV ratio. In this case, we cannot determine the actual size of the change in the maximum LTV ratio.

- When the prohibition of a specific type of loan is lifted, it is regarded as no longer applying a zero maximum LTV ratio. If a newly applied maximum LTV ratio exists, then the negative of the level of the new maximum LTV ratio is the value of the change in the maximum LTV ratio. If there is no new maximum LTV ratio applied to this type of loan, then it is regarded as the abolition of the maximum LTV ratio. In this case, we cannot determine the actual size of the change in the maximum LTV ratio.

- When a new maximum LTV ratio is introduced, we cannot determine the actual size of the change in the maximum LTV ratio. When the existing maximum LTV ratio is abolished, again we cannot determine the actual size of the change in the maximum LTV ratio.

- When the maximum LTV ratio changes more than once in a quarter, we sum up all the changes (i.e., calculate cumulative changes) in a quarter and treat them as one LTV change.

- If a specific non-standard type of housing loan becomes subject to a LTV change, we assign 10% weight to the loan type. For example, if housing loans extended to first-time home buyers only become subject to the maximum LTV ratio of 70% rather than 60%, then the LTV variable takes the value of –1, which is calculated as –10 multiplied by 10%. Also, if a change in the maximum LTV ratio is applied only to foreign currency loans, we assign 50% weight to this type of loan.

Among the 89 LTV actions without stated real objectives, two LTV actions were taken in one quarter by the same country in the case of four country-quarter observations (i.e., 2010Q2, 2010Q3 and 2011Q1 for China, and 1996Q2 for Singapore). When we use the intensity-adjusted quantifiable LTV change variable, we sum up all LTV actions taken in a quarter. As a result, we have 85 distinct LTV actions in the sample. Among the 85 LTV

Table 3: *Summary statistics of 53 LTV actions.*

		Total	Average	Median	Min	Max
Scope-adjusted	Tightening action	32	7.11	5.25	1	26.25
	Loosening action	21	−7.87	−7.5	−0.5	−27.5
Scope-unadjusted	Tightening action	32	22.46	11.25	5	90
	Loosening action	21	−16.00	−15.00	−5	−80

actions, 32 actions involve either introductions or abolitions of the maximum LTV ratio or loan prohibition for which we do not have information about the actual size of the change in the maximum LTV ratio. For our baseline regressions, we do not consider these 32 actions with insufficient information, but only consider the remaining 53 quantified actions. Table 2 provides the list of the 53 actions that were not taken with reference to the real cycle and for which we have accurate information on the size of the change.

For robustness check, we also construct a quantified LTV action variable for which we do not adjust for the scope of loans to which the changes in LTV limits apply. Table 3 shows the summary statistics of the 53 LTV variables with and without scope adjustment. The average size of the scope-adjusted LTV change variable is 7.1 percentage points for the tightening actions and –7.9 percentage points for the loosening actions. When we do not adjust for the scope of LTV changes applied, the average size of the change in the maximum LTV ratio is 22.5 percentage points for the tightening actions and –16.0 percentage points for the loosening actions. When we compare the scope-adjusted and scope-unadjusted LTV change variables, scope adjustment reduces the average size of the LTV change variable into one third to one half of the scope-unadjusted LTV change variable. The large difference between the two types of the quantifiable LTV variable is also due to loan prohibition actions which tend to take very large values of the scope-unadjusted numerical LTV change variable (such as +90 or –80).

As a robustness check based on the intensity-adjusted measure $\Delta MaPP_{i,t}$, we also define an additional measure. $\Delta MaPP_{i,t}^{Index}$ is an index variable based on our intensity adjusted policy action variable:

$$\Delta MaPP_{i,t}^{Index} = \begin{cases} 1 & \text{if } \Delta MaPP_{i,t} > 0, \\ 0 & \text{if } \Delta MaPP_{i,t} = 0, \\ -1 & \text{if } \Delta MaPP_{i,t} < 0. \end{cases}$$

Similarly, $\Delta^{Tight}MaPP_{i,t}$ and $\Delta^{Loose}MaPP_{i,t}$ are dummy variables for tightenings and loosenings, respectively:

$$\Delta^{Tight}MaPP_{i,t} = \begin{cases} 1 & \text{if } \Delta MaPP_{i,t} > 0 \\ 0 & \text{if } \Delta MaPP_{i,t} \leq 0 \end{cases}$$

$$\Delta^{Loose}MaPP_{i,t} = \begin{cases} -1 & \text{if } \Delta MaPP_{i,t} < 0 \\ 0 & \text{if } \Delta MaPP_{i,t} \geq 0 \end{cases}$$

3.3. Are LTV changes exogenous to the real cycle?

In the previous section, we presented the new narratively identified data on the stated objectives and size of LTV changes. In our empirical analysis we combine both to estimate the responses of real variables to a one percentage point change in LTV limits. As described before, focusing on actions referencing only the financial objectives allows a causal interpretation of the results.

A relevant concern in this setting is that policy-makers may target real objectives, without stating them explicitly when implementing macroprudential actions. In this section, we test this important prerequisite for a causal interpretation of our results.

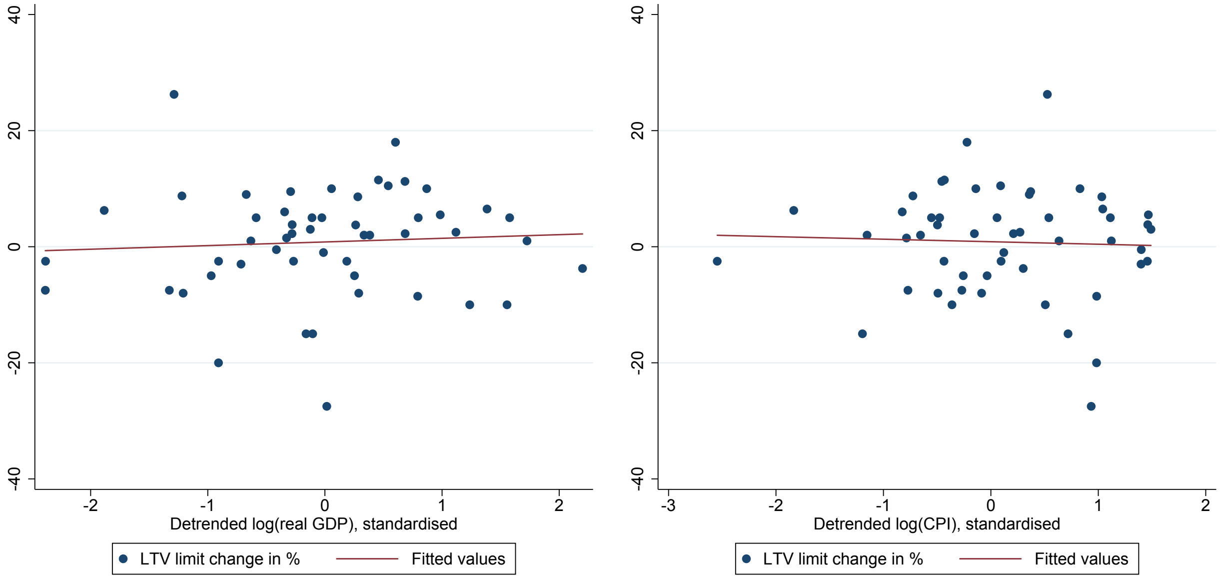
We first determine cyclical deviations of the real variables. To do so, we calculate the deviations of real GDP and prices from trend. To detrend the data we follow Hamilton (2017). The procedure is based on the idea that the trend component of a variable at time $t+h$ is the value we could have predicted based on historical data. The cyclical component will be the difference between the realised value and this trend.

Let h denote the horizon for which we build such a prediction. Then the cyclical (detrended) component is the difference between the realised value at time $t+h$ and the expectation about this value formed at time t. To build this expectation, Hamilton (2017) proposes a regression of the value y at time $t+h$ on the four most recent values of y at time t, i.e. y_t, y_{t-1}, y_{t-2} and y_{t-3}. Formally, this regression can be written as

$$y_{t+h} = \beta_0 + \beta_1 y_t + \beta_2 y_{t-1} + \beta_3 y_{t-2} + \beta_4 y_{t-3} + v_{t+h}.$$

The choice of h depends on the horizon we attribute to the cyclical component. As suggested, we choose a horizon of eight quarters, so the residual is the deviation of the realised value y_{t+8} from the expectation formed at time t based on information on y_t, y_{t-1}, y_{t-2} and y_{t-3}. We normalise this variable by its country-specific standard deviation.

Figure 1: *Scatterplot: Relationship between intensity-adjusted LTV changes and detrended real variables.*



Notes: The graphs show the relationship between changes in LTV limits and detrended real variables for the 53 LTV actions listed in Table 2. See text for details.

Figure 1 shows scatterplots for the size of our LTV changes and detrended real GDP as well as the detrended price level. The data show no clear pattern that could be interpreted as an indication that LTV changes are a reaction to the output gap or price level gap. In particular, changes in the maximum LTV ratio implemented when the output gap (positive and negative) was larger than two standard deviations, do not stand out as large LTV changes. We obtain similar results when we use standard H-P filters and when we use longer lags of detrended real variables.

In Table 4 and Table 5 we turn to formal procedures to test the relationship between the treatment (that is, the implementation of macroprudential actions involving LTV limits) and real economic variables. Note that in an ideal randomised allocation of treatment and control, there would be no difference between treatment and control sub-populations.

In Table 4, we differentiate between two treatments, a tightening and a loosening, and the non-treated control group of observations. We then compare the means of those sub-populations and test for their equality. In the upper panel we compare the group of tightening observations to the control group. We compare real GDP and the price level in treated and non-treated observations based on two measures. First, we compute the smoothed growth rate of these variables over the previous year, which is over four lags, and demean this measure at the country level to account for the fact that fast growing EMEs have historically been more active in using macroprudential policies. The results show no significant difference between the two sub-populations. We also compare the lag of the

Table 4: *Check for balance of treated and control sub-populations*

	Difference: Tightening-Control	
Smoothed 4-quarter GDP growth, demeaned	-0.26	(0.22)
Smoothed 4-quarter CPI growth, demeaned	-0.82	(0.94)
Detrended log real GDP	-0.04	(0.18)
Detrended log price level	-0.09	(0.18)
Observations	3721	
	Difference: Loosening-Control	
Smoothed 4-quarter GDP growth, demeaned	0.44	(0.27)
Smoothed 4-quarter CPI growth, demeaned	-0.60	(1.16)
Detrended log real GDP	0.21	(0.22)
Detrended log price level	-0.24	(0.21)
Observations	3710	

Notes: Test for the equality of means in the subpopulations of tightenings and loosenings compared to the no-action subpopulation respectively. Real variables are first lags of either the smoothed growth rates over four quarters demeaned at the country level, or the normalised deviation from a Hamilton (2017) trend. Standard errors in parentheses.

output gap and the detrended price level between the two sub-populations, but do not find a significant difference.

In the bottom panel of Table 4, we present the results of tests for the equality of means between loosening actions and the control group. Again, we do not find a statistically significant difference between the two sub-populations.

In Table 5 we test whether we can predict either $\Delta MaPP_{i,t}$ or $\Delta MaPP_{i,t}^{Index}$ based on the one-period lagged detrended GDP and the price level. Column (1) shows the results of a regression of the LTV change on the lagged output gap, the detrended price level and a constant. In column (2) we additionally include country-fixed effects. Columns (3) and (4) use $\Delta MaPP_{i,t}^{Index}$ as the dependent variable instead. Reassuringly, the coefficients are across all specifications insignificant and the variables have little explanatory power. Based on the results in this section, we conclude that LTV changes are not predicted by real economic variables and that their implementation can be seen as orthogonal to the real cycle.

Another concern may be that macroprudential actions are anticipated by market participants. We therefore conducted additional tests whether bank equity indices display abnormal returns before the announcement of an LTV action. When we analyse monthly and quarterly returns, we find no evidence that actions were anticipated by the market. Looking at a higher frequency of daily returns, there seems to be some information leakage about the policy change around 10 days before the announcement. For our exercise, however, it matters that actions were not anticipated in the previous quarter.

Table 5: *Prediction of LTV changes.*

	(1) $\Delta MaPP_{i,t}$	(2) $\Delta MaPP_{i,t}$	(3) $\Delta MaPP^{Index}_{i,t}$	(4) $\Delta MaPP^{Index}_{i,t}$
Detrended log real GDP	-0.88	-1.00	0.15	0.13
	(1.72)	(1.73)	(0.21)	(0.20)
Detrended price level	-1.39	-0.41	-0.06	0.03
	(1.54)	(1.51)	(0.17)	(0.19)
FE	No	Yes	No	Yes
R^2	0.000	0.022	0.000	0.029
Observations	3373	3373	3373	3373

Notes: The table shows OLS regression results. The dependent variable is the intensity-adjusted LTV change in columns (1) and (2) and the LTV index in columns (3) and (4). Clustered standard errors in parentheses. $^{*}p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4. The output and price effects of changes in LTV limits

For the main part of the empirical analysis, this paper uses local projection methods. Jordà (2005) introduces local projections as a way to compute impulse responses without specification and estimation of the underlying multivariate dynamic system. Local projections are estimated at each period of interest rather than extrapolating into increasingly distant horizons from a given model as it is done with VAR models. He discusses the advantages of local projections such as being more robust to mis-specification, being simple in joint or point-wise analytic inference, and being able to easily accommodate experimentation with highly nonlinear and flexible specifications.

Jordà *et al.* (2013) use local projection methods to condition on a broad set of macroeconomic controls when studying how past credit accumulation affects key macroeconomic variables. Here we study the path of output and prices conditional on a change in LTV limits and macroeconomic controls. We denote the dependent variables, real output and the price level of country i at time t, by $y_{i,t}$. $\Delta_h y_{i,t} = y_{i,t+h} - y_{i,t}$ denotes our response variable of interest, the change in real output or in the price level between base quarter t and quarter $t+h$ over varying prediction horizons $h = 1, 2, ..., H$, where H is 16 in our specifications. We are interested in the response of this variable to a perturbation in our measure of macroprudential policy $\Delta MaPP_{i,t}$. Specifically, we estimate

$$\Delta_h y_{i,t} = \alpha_i^h + \gamma_t^h + \beta^h \Delta MaPP_{i,t} + \sum_{k=0}^{4} \phi_k^h \Delta X_{i,t-k} + \epsilon_{i,t+h}\,, \tag{1}$$

for $h = 1, ..., 16$. $\Delta MaPP_{i,t}$ denotes changes in macroprudential policy, here the regulatory LTV limit, implemented in country i and quarter t.

In various robustness tests we vary this treatment variable $\Delta MaPP_{i,t}$ in a number of ways. We first present the results based on the intensity-adjusted variable described in the data section ($\Delta MaPP_{i,t}$) to assess the impact of a one percentage point change in LTV limits. In the following specification we replace $\Delta MaPP_{i,t}$ with an index to connect to the existing literature, assigning a value of 1 to a tightening action in LTV limits, a value of -1 to a loosening action and zero if there is no action. We denote this variable by $\Delta MaPP_{i,t}^{Index}$. We also show results for tightenings (positive values of $\Delta MaPP_{i,t}$) and loosenings (negative values) separately. We include a rich set of covariates in each specification. These include country dummies to control for country-specific growth rates α_i^h as well as time-fixed effects γ_t^h to control for global trends. $X_{i,t}$ is a vector that contains the GDP growth, inflation and policy rate changes.

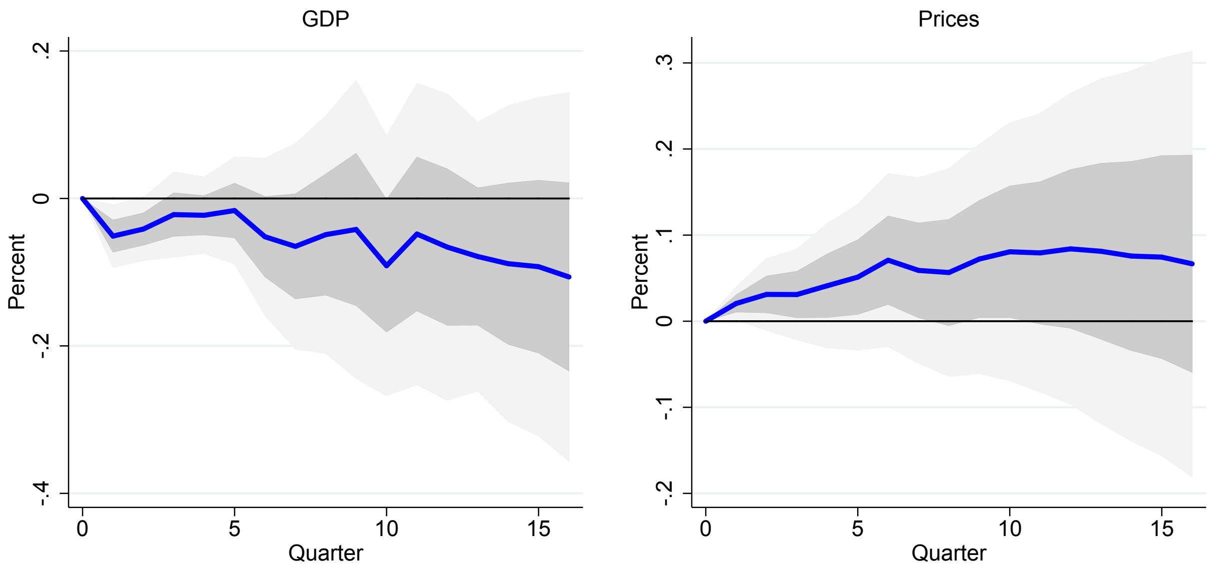
4.1. Main results

We start with our baseline specification, and include the intensity-adjusted LTV change variable $\Delta MaPP_{i,t}$, which refers to the percentage point change in regulatory maximum LTV ratios between $t-1$ and t constructed as described earlier in the data section. The results of estimating Equation 1 using the numerical measure of $\Delta MaPP_{i,t}$ are visualised in Figure 2. The two panels display the cumulative responses of output and of the price level to a one percentage point change in the maximum LTV ratio over the following 16 quarters. Note that we define $\Delta MaPP_{i,t}$ such that a positive value refers to a tightening in macroprudential policies. A value of $\Delta MaPP_{i,t} = 10$ refers to a 10 percentage point decrease in the regulatory maximum LTV ratio, for example a tightening from 80% to 70%.

Figure 2 shows that the response to a one percentage point change in LTV limits is a 0.05% lower real GDP after two years, which increases to a 0.11% loss after four years. These results are rather imprecisely estimated: the coefficient is only significant for very short horizons immediately after the LTV action is taken. As we will see later this effect immediately after the implementation of a policy can be attributed to loosening actions only. Consider again the tightening in the maximum LTV ratio by 10 percentage points from 80% to 70%: our estimates correspond to 0.5% lower real GDP after 24 months and 1.1% lower real GDP after 48 months for this scenario.

The price response is slightly positive, but also insignificant at most times. The coefficient rises from 0.02% after one quarter to 0.08% after 12 quarters, before it again declines to 0.07% after four years at the end of our horizon. This estimate is also very imprecise and the coefficient is only significant after the first quarter. The coefficients are reported in Table 6. We conclude that the effects of LTV limits on the real economic cycle seem to be rather small. Figure A.3 in the appendix shows that the response of real consumption is similar to the response of GDP. The coefficient is also estimated rather imprecisely, fluctuating around –0.1, in line with our estimate for the path of real GDP. In Figure A.8 in the appendix we

Figure 2: *Local projection: Responses of real GDP and the price level to a change in maximum loan-to-value ratios, quantified measure.*



Notes: The blue lines display the coefficients of cumulative responses of real GDP and the price level over the 16 quarters following a 1 percentage point change in maximum LTV ratios. Shaded areas refer to 1 standard deviation (dark) and 1.96 standard deviations (light).

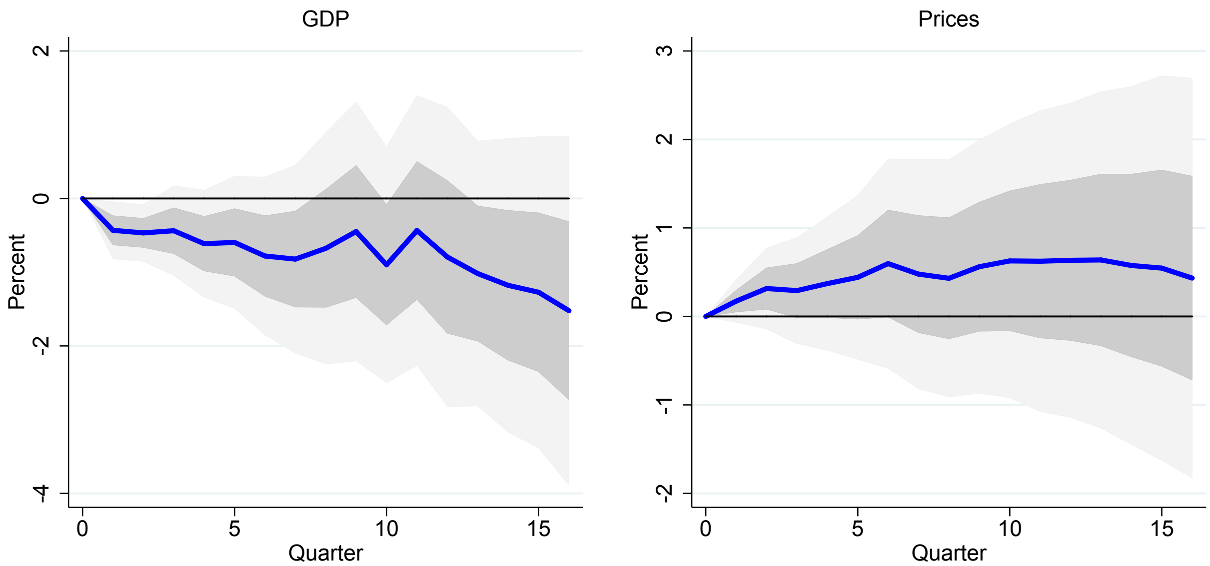
Table 6: *Local projection: Responses of real GDP and the price level to a change in maximum loan-to-value ratios, quantified measure.*

	$h=1$	$h=4$	$h=8$	$h=12$	$h=16$
Dep. Var.: 100 × log real GDP					
LTV change	-0.05**	-0.02	-0.05	-0.07	-0.11
	(0.02)	(0.03)	(0.08)	(0.11)	(0.13)
Observations	3171	3171	3171	3171	3171
Dep. Var.: 100 × log CPI					
LTV change	0.02**	0.04	0.06	0.08	0.07
	(0.01)	(0.04)	(0.06)	(0.09)	(0.13)
Observations	3171	3171	3171	3171	3171

Notes: Clustered (by country) standard errors in parentheses. Control vector includes the current value and four lags of GDP growth, CPI inflation, and policy rate changes. Specifications include country-fixed effects and quarter-fixed effects. See text.

add four lags of changes in the credit-to-GDP ratio and in the household-credit-to-GDP ratio as well as four lags of the LTV change. We do not include these variables in our baseline specification as the number of observations is reduced by 20%. The results show that the price response after eight quarters remains stable, while the output response in

Figure 3: *Local projection: Responses of real GDP and the price level to a change in maximum loan-to-value ratios, index variable.*



Notes: The blue lines display the coefficients of cumulative responses of real GDP and the price level over the 16 quarters following a change in maximum LTV ratios. Shaded areas refer to 1 standard deviation (dark) and 1.96 standard deviations (light). See text.

the first eight quarters is a bit stronger than in the baseline specification. In the following subsections we will vary the treatment variable in the baseline specification in a number of ways.

4.2. Index variable specification

We now assess the average effects of changes in LTV limits on real economic output and the price level. To connect to the existing literature on macroprudential policies that has focused on changes in macroprudential policy expressed as binary or index variables, we estimate our baseline specification, including the index variable and the set of control variables described earlier. This boils down to the following expression

$$\Delta_h y_{i,t} = \alpha_i^h + \gamma_t^h + \beta^h \Delta MaPP_{i,t}^{Index} + \sum_{k=0}^{4} \phi_k^h \Delta X_{i,t-k} + \epsilon_{i,t+h},$$

for $h = 1, ..., 16$. The results are displayed in Figure 3 and show that this typical LTV action has insignificant contractionary effects on real GDP, while there is almost no effect on the price level. The coefficients for various horizons are displayed in Table 7. Increasing our index variable by 1, which corresponds to varying it from no action to a tightening action, lowers real GDP by 1.52% after 16 quarters. This response is again rather imprecisely

Table 7: *Local projection: Responses of real GDP and the price level to a change in maximum loan-to-value ratios, index variable.*

	h =1	h =4	h =8	h =12	h =16
Dep. Var.: 100 × log real GDP					
LTV Index	-0.432**	-0.614	-0.677	-0.793	-1.522
	(0.198)	(0.370)	(0.800)	(1.036)	(1.208)
R^2	0.257	0.362	0.360	0.358	0.368
Observations	3171	3171	3171	3171	3171
Dep. Var.: 100 × log CPI					
LTV Index	0.173	0.371	0.431	0.634	0.433
	(0.123)	(0.385)	(0.684)	(0.906)	(1.152)
R^2	0.523	0.497	0.526	0.525	0.532
Observations	3171	3171	3171	3171	3171

Notes: Clustered (by country) standard errors in parentheses. Control vector includes the current value and four lags of GDP growth, CPI inflation, and policy rate changes. Specifications include country-fixed effects, quarter-fixed effects and a crisis dummy. See text.

estimated and not significant as indicated by the shaded area in light grey referring to 1.96 standard deviations. Comparing this result to the coefficient using the numerical value shows that the index specification overestimates the strength of the output effects: the average size of the change in the quantified LTV variable in this sample is around 7.5 percentage points and hence the effect in the index specification is almost twice as high as in the baseline results. The response of the price level is closer to zero over all horizons h.

4.3. The effects of tightening and loosening actions

The negative coefficient for $\Delta MaPP_{i,t}$ suggests that there are small negative spillovers to the real economy from tightening macroprudential policies. Importantly, this applies to policies specifically targeting the financial cycle. The result could also be interpreted such that loose macroprudential policies may be used to stimulate output. To distinguish between the two domains of policy-making, we analyse whether there is a systematic difference between the responses to the quantified LTV tightening and loosening actions. To do so, we include the quantified LTV change in tightening and loosening actions separately. Let us define two variables as follows:

$$\Delta^{>0} MaPP_{i,t} = \begin{cases} \Delta MaPP_{i,t} & \text{if } \Delta MaPP_{i,t} > 0 \\ 0 & \text{if } \Delta MaPP_{i,t} \leq 0. \end{cases}$$

Hence, $\Delta^{>0}MaPP_{i,t}$ measures the size of a tightening action only. $\Delta^{<0}MaPP_{i,t}$ is the loosening analogue:

$$\Delta^{<0}MaPP_{i,t} = \begin{cases} \Delta MaPP_{i,t} & \text{if } \Delta MaPP_{i,t} < 0 \\ 0 & \text{if } \Delta MaPP_{i,t} \geq 0. \end{cases}$$

We can add these two variables to the baseline specification:

$$\Delta_h y_{i,t} = \alpha_i^h + \gamma_t^h + \beta^h \Delta^{>0} MaPP_{i,t} + \kappa^h \Delta^{<0} MaPP_{i,t} + \sum_{k=0}^{4} \phi_k^h \Delta X_{i,t-k} + \epsilon_{i,t+h}, \qquad (2)$$

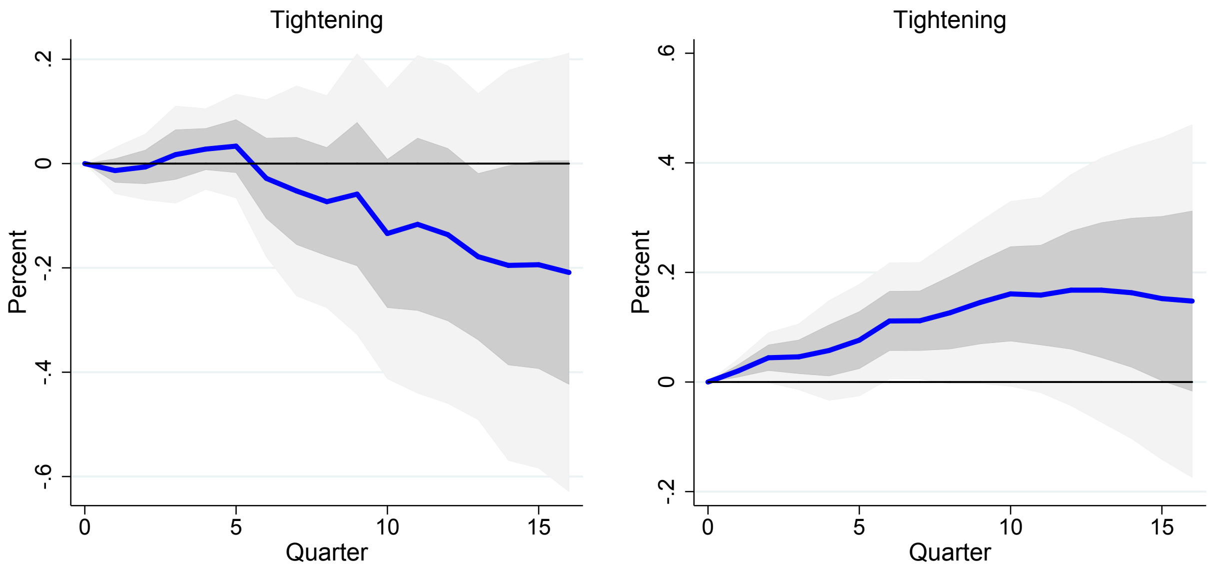
for $h = 1, ..., 16$. In this setup, β^h denotes the coefficient on changes in maximum LTV ratios in the positive domain and κ^h in the negative domain. Remember that our $MaPP$ variable is defined such that tightenings in policy are associated with a positive value of $\Delta MaPP_{i,t}$. Hence, the β^h coefficient refers to the effect of a one percentage point lower maximum LTV ratio when a tightening action is implemented, while κ^h refers to the effect of a one percentage point higher maximum LTV ratio when a loosening action is implemented. Hence, the baseline estimates are a weighted average of these two estimates.

The results are displayed separately in Figure 4 and Figure 5. The responses of output displayed on the left-hand panels in these figures show a difference between a one percentage point tightening and loosening LTV actions. In particular, the average slightly negative response displayed in the first seven quarters is mainly driven by loosening actions and the response after eight quarters is mainly driven by tightening actions. The price level increases slightly as a reaction to a tightening action, while it shows almost no response to a loosening action. These findings from the visual inspection are confirmed in Table 8.

4.4. Robustness

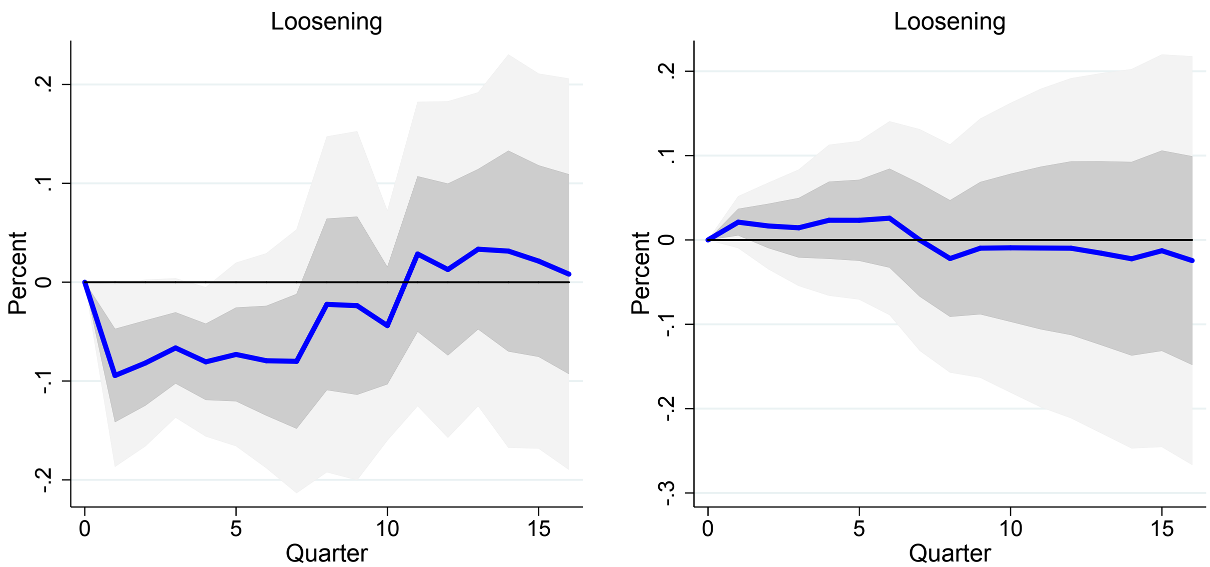
In this section we check the robustness of our results in a number of subsamples. In particular, we investigate whether the results differ across subsamples of our data. As described earlier, macroprudential policies increasingly received attention after the global financial crisis and this is also true for policies implementing LTV limits. To rule out that results are driven by some characteristics associated with the post-2007 period, we test whether our results also hold in a pre-2007 subsample. We therefore run our baseline local projection using the quantified LTV changes only for policies implemented until 2006. As shown in Figure 6, in this subsample the responses of GDP and prices to a change in the maximum LTV ratio look very similar to the full sample results. The negative response of output is slightly stronger at longer horizons in this sample than in the full sample.

Figure 4: *Local projection: Responses of real GDP and the price level to a change in maximum loan-to-value ratios, quantified measure, tightenings.*



Notes: The blue lines display the coefficients of cumulative responses of real GDP and the price level over the 16 quarters following a 1 percentage point decrease (i.e. tightening) in maximum LTV ratios. Shaded areas refer to 1 standard deviation (dark) and 1.96 standard deviations (light).

Figure 5: *Local projection: Responses of real GDP and the price level to a change in maximum loan-to-value ratios, quantified measure, loosenings.*



Notes: The blue lines display the coefficients of cumulative responses of real GDP and price level over the 16 quarters following a 1 percentage point increase (i.e. loosening) in maximum LTV ratios. Shaded areas refer to 1 standard deviation (dark) and 1.96 standard deviations (light).

Table 8: *Local projection: Responses of real GDP and the price level to a change in maximum loan-to-value ratios using a quantified measure, tightenings and loosenings separately.*

	h =1	h =4	h =8	h =12	h =16
Dep. Var.: 100 × log real GDP					
Quantified Tightenings	-0.01	0.03	-0.07	-0.14	-0.21
	(0.02)	(0.04)	(0.10)	(0.17)	(0.21)
Quantified Loosenings	-0.09*	-0.08**	-0.02	0.01	0.01
	(0.05)	(0.04)	(0.09)	(0.09)	(0.10)
Observations	3171	3171	3171	3171	3171
Dep. Var.: 100 × log CPI					
Quantified Tightenings	0.02*	0.06	0.13*	0.17	0.15
	(0.01)	(0.05)	(0.07)	(0.11)	(0.16)
Quantified Loosenings	0.02	0.02	-0.02	-0.01	-0.03
	(0.02)	(0.05)	(0.07)	(0.10)	(0.12)
Observations	3171	3171	3171	3171	3171

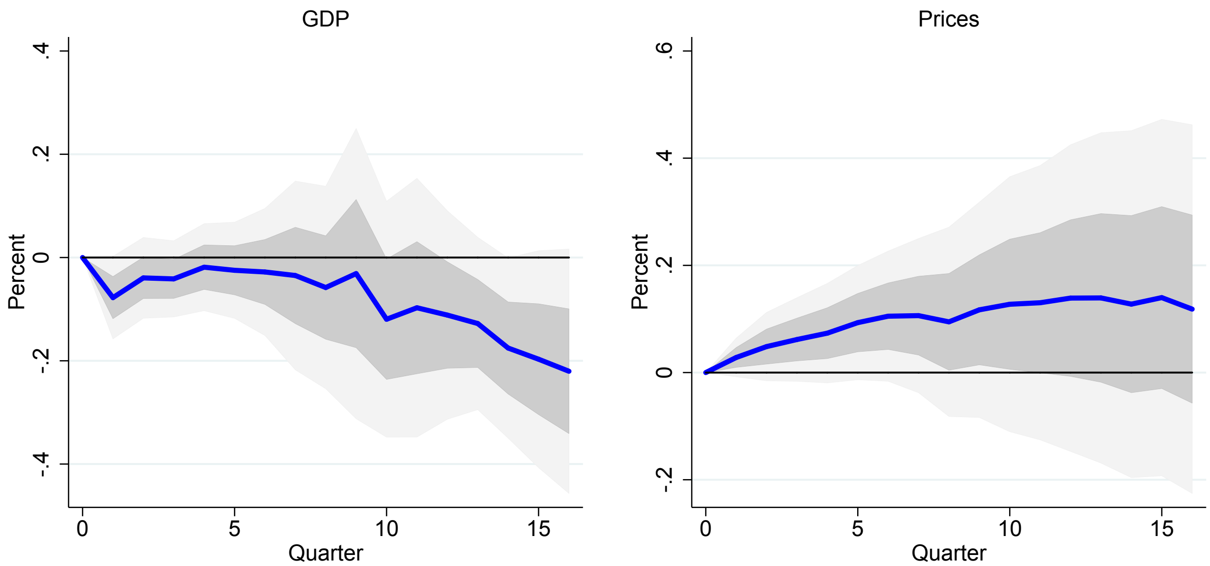
Notes: Clustered (by country) standard errors in parentheses. Control vector includes the current value and four lags of GDP growth, CPI inflation, and policy rate changes. Specifications include country-fixed effects and quarter-fixed effects. See text.

The efficacy of macroprudential policies implemented might depend on the general economic environment and real responses might differ between countries at various development stages. We address this possibility in our next test and run our baseline specification for subsamples of EMEs and AEs separately. Our broad sample contains 23 AEs and 33 EMEs, whose classification we use here is based on the BIS definition.

Figure 7 and Figure 8 show that the responses of output and prices indeed depend on the development stage. The negative output response is entirely driven by EMEs (see Figure 8). In these countries the response is negative at all horizons and this result is statistically significant in the first two years after the policy is implemented. In the sample of AEs (see Figure 7) the response of GDP is very close to zero at all horizons. The price level also displays heterogeneous responses between the two groups. While the response of the price level is small and mostly insignificant in both cases, the pattern differs: the price level first increases slightly in EMEs and returns to zero, while it decreases in AEs first and becomes slightly positive after two years. These results indicate that the costs of using LTV limits are smaller for AEs. One possible channel for these differences could be that policies are better calibrated to desired targets in AEs and hence do not trigger a misallocation of credit.

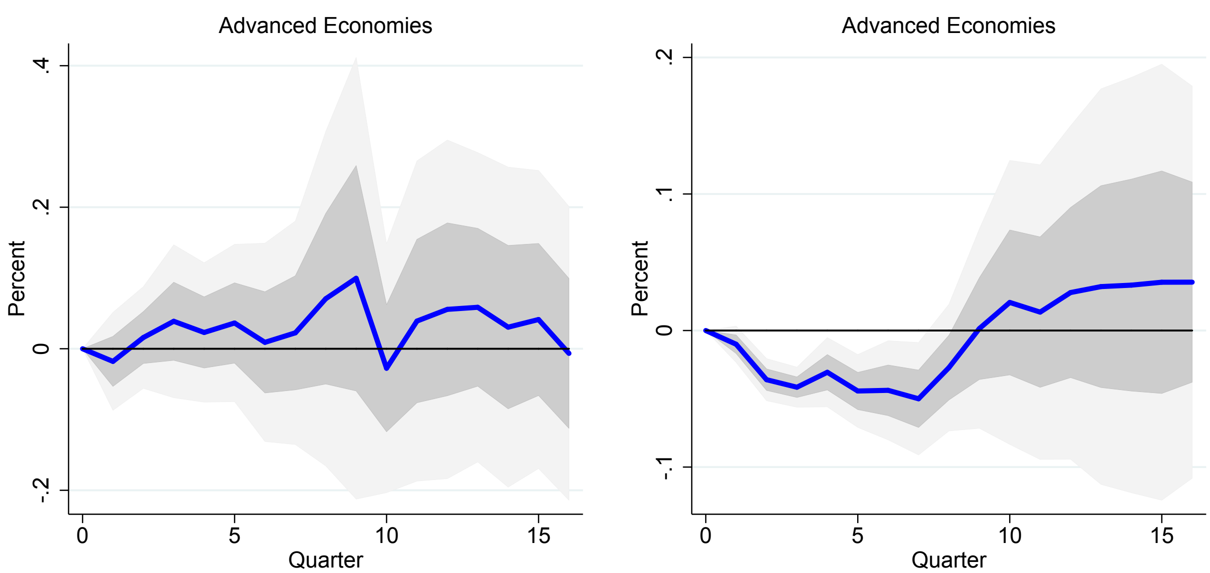
In addition, we report here the results of using the scope-unadjusted measure for changes in LTV limits. As explained in the data section, we adjusted the size of LTV limit

Figure 6: *Local projection: Responses of real GDP and the price level to a change in maximum loan-to-value ratios, quantified measure, pre-2007 sample.*



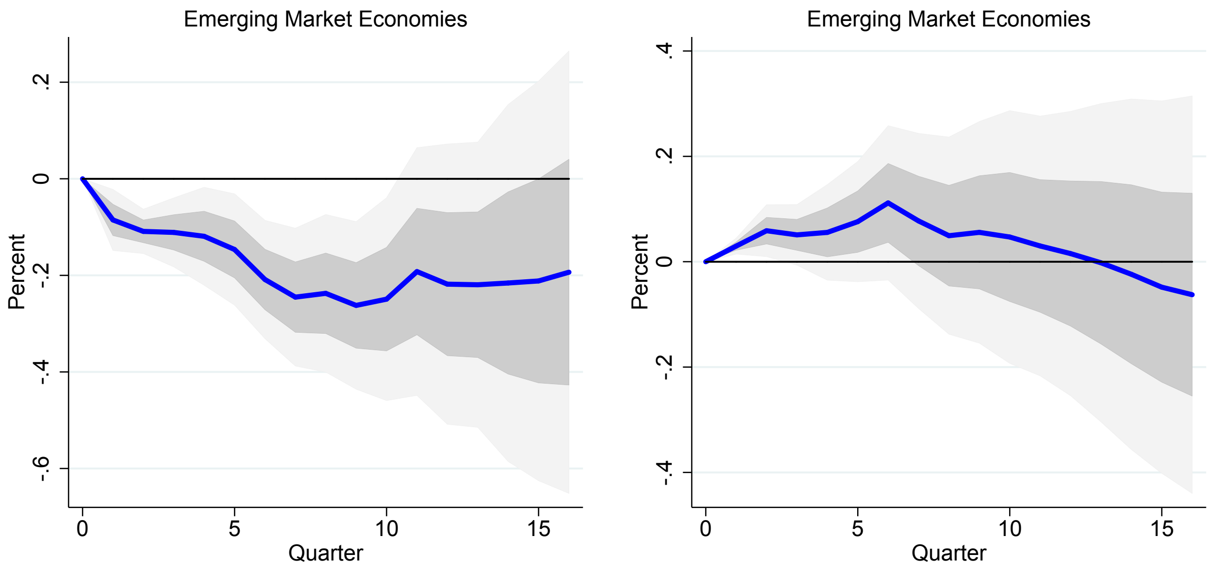
Notes: The blue lines display the coefficients of cumulative responses of real GDP and the price level over the 16 quarters following a 1 percentage point change in maximum LTV ratios. Shaded areas refer to 1 standard deviation (dark) and 1.96 standard deviations (light).

Figure 7: *Local projection: Responses of real GDP and the price level to a change in maximum loan-to-value ratios, quantified measure, subsample of advanced economies.*



Notes: The blue lines display the coefficients of cumulative responses of real GDP and the price level over the 16 quarters following a 1 percentage point change in maximum LTV ratios. Shaded areas refer to 1 standard deviation (dark) and 1.96 standard deviations (light).

Figure 8: *Local projection: Responses of real GDP and the price level to a change in maximum loan-to-value ratios, quantified measure, subsample of emerging market economies.*

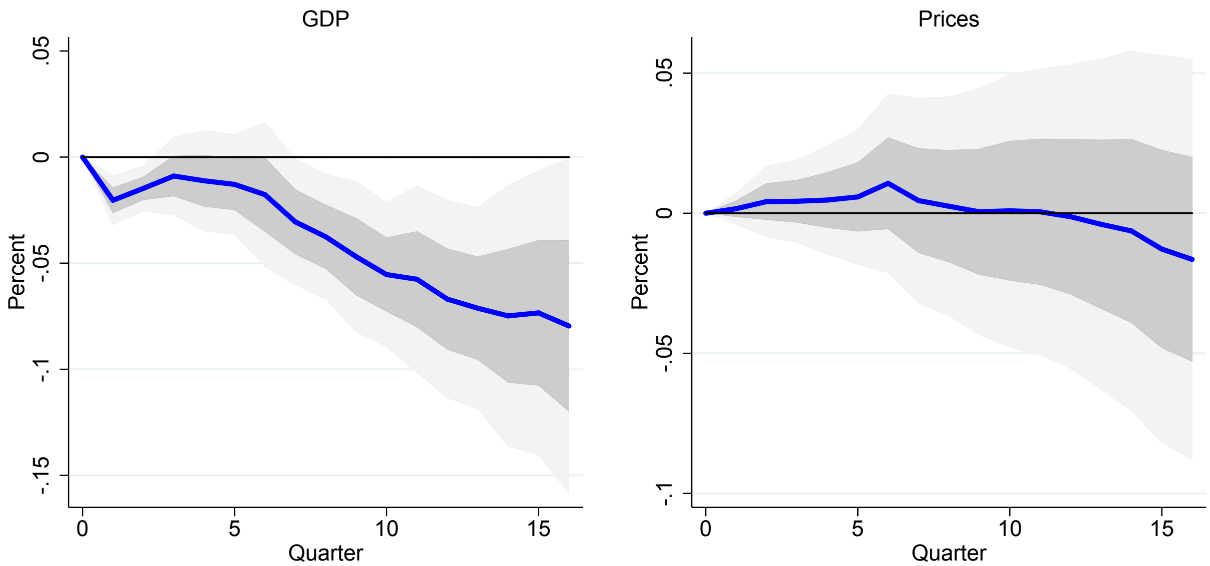


Notes: The blue lines display the coefficients of cumulative responses of real GDP and the price level over the 16 quarters following a 1 percentage point change in maximum LTV ratios. Shaded areas refer to 1 standard deviation (dark) and 1.96 standard deviations (light).

changes for the scope of policies implemented. In this test we assume all changes in LTV limits affect all types of credit and hence no adjustment for scope is necessary. Figure 9 shows the results for our baseline specification using this approach: the coefficients are even smaller, although more precisely estimated.

The appendix shows results of additional tests: responses over a 30– quarter horizon (Figure A.7), the response of consumption (Figure A.3), and responses in boom and slump subsamples (Figure A.5 and Figure A.6). In all our specifications we use the implementation dates of changed policies. Usually, the announcement of a policy and its implementation fall into the same quarter. We did, however, check whether our results change if we employ announcement dates rather than implementation dates in the analysis. In total, six actions were affected. In two cases the announcement was in the last week of a quarter. Therefore we assume there was no time for economic actors to adjust their behaviour in this quarter. We then use the announcement quarter rather than the implementation quarter for the other four changes. The results for the baseline specification are also shown in the appendix (Figure A.4), and there is no visible change compared to the results based on the implementation quarter.

Figure 9: *Local projection: Response of real GDP and the price level to a change in maximum loan-to-value ratios, quantified measure unadjusted for scope.*



Notes: The blue lines display the coefficients of cumulative responses of real GDP and the price level over the 16 quarters following a 1 percentage point change in maximum LTV ratios. Shaded areas refer to 1 standard deviation (dark) and 1.96 standard deviations (light).

4.5. Comparing LTV changes with monetary policy

Our data on the size of LTV changes also allow us to compare the effects of quantified LTV changes to those of monetary policy. Table 9 displays estimates of the response of real output to a 100 basis point increase in policy rates found in several studies. In particular, we present estimates based on a narrative identification approach (Romer and Romer (2004)), estimates based on high-frequency identification (Gertler and Karadi (2015)) and estimates that are obtained by exploiting the open-economy trilemma for identification (Jordà *et al.* (2017)). Furthermore, we report the state-dependent effects analysed in Tenreyro and Thwaites (2016).[6]

We compare our estimates of the effects of LTV limit changes with those of monetary policy. We do this based on the following question: what is the monetary policy adjustment that yields the same output response as a 10 percentage point LTV limit change? In our baseline regression, the estimated response to this change after two years is –0.5%.

This response would correspond to a change of 12 basis points in the policy rate based on the results reported in Romer and Romer (2004). Based on a high frequency identification procedure, Gertler and Karadi (2015) find smaller effects and our estimate of a 10 percentage

[6]See Ramey (2016) for a more comprehensive overview of estimates.

Table 9: *Output effects of monetary policy.*

Paper	2 year response	Peak response	Significance
Romer and Romer (2004)	-4.3%	-4.3% at 24months	Yes
Gertler and Karadi (2015)	-1.2%	-1.6% at 18 months	Yes (peak)
Jordà *et al.* (2017)	-1.9%	-2.9% at 4 years	Yes
Tenreyro and Thwaites (2016)	Expansion: -4.5% Recession: -1.3%	Expansion: -7.5% at 36 months Recession: -3.9% at 48 months	Yes

Notes: the table reports estimates of the output effects of a 100 basis point increase in policy rates/short term interest rates.

point LTV limit tightening would correspond to a change of around 42 basis points in the federal funds rate. In another study, Jordà *et al.* (2017) estimate the response of real GDP to short-term interest rates and find an effect of –1.9% after two years. This estimate is most likely the best comparison for our purpose, as it is also based on local projections using an international panel dataset. Here, our estimated response of real GDP corresponds to a 26 basis point interest rate change, falling in the middle of the range between the other results.

What do we learn from this exercise? A monetary policy shock of 26 basis points is certainly not negligible, but our estimates are rather imprecise compared to those identified in the literature on monetary policy shocks. Tenreyro and Thwaites (2016) and Jordà *et al.* (2017) show that the output costs of monetary policy are much higher in the boom than in the slump. We show in the appendix that these effects are very similar for LTV changes. There seems to be little room for policy-makers to exploit state dependence of the effects when choosing between monetary and macroprudential policies. The good news for policy-makers is that the output response to a change in LTV limits is attenuated in the sample of AEs. Here the output costs seem rather low, while the costs of interest rate policies discussed above applied almost exclusively to the United States or a sample of AEs.

5. The effects on financial variables

What are the benefits of macroprudential tightenings? The previous section has shown that tightenings in LTV limits are associated with modest output costs, but the cost-benefit tradeoff of implementing macroprudential policies also depends on the efficacy of macroprudential policies to dampen the financial cycle. In this section we therefore analyse the responses of the ratios of household and mortgage credit to GDP and of asset prices to a tightening in the maximum LTV ratio. To answer the question, we cannot rely on the same identification strategy we used to assess the output costs. As we previously have argued, the objectives of LTV limit changes are mostly related to the financial cycle and policies are often implemented to affect credit variables. We therefore need to approach this question using a different identification strategy.

To estimate the response to a tightening action in LTV limits, which is the treatment, we employ an inverse probability weighted (IPW) estimator. The IPW estimator gives more weight to those treatments that are difficult to predict based on observables and less weight to those instances that are endogenous due to the other factors. Jordà *et al.* (2016) use the local projections weighted by the inverse propensity score for the probability of observing a financial crisis driven recession rather than a normal recession. Other applications study the response to sovereign defaults (Kuvshinov and Zimmermann (2016)) and austerity (Jordà and Taylor (2016)).

Building on a large literature in biostatistics and more recently in econometrics, Angrist *et al.* (2016) propose the inverse probability weighted (IPW) estimator. The estimation procedure consists of two stages. In the first stage, a model is constructed to determine the probability that a policy measure, here a tightening, is taken: $p(d_{i,t} = 1|X_{i,t-1})$. $d_{i,t}$ refers to the dummy $\Delta^{Tight} MaPP_{i,t}$ that takes one if $\Delta MaPP_{i,t} > 0$ and zero otherwise. $X_{i,t-1}$ denotes a vector of observable macroeconomic controls at time $t-1$, where we include smoothed four-quarter changes in real and financial variables. The probability of treatment will be called the *propensity score* and we denote its estimate by $\hat{p}_{i,t}$. The second stage consists of running the local projections with the tightening dummy using weights given by the inverse of the estimated propensity score. Weighting by the inverse of the propensity score puts more weight on those observations that were difficult to predict. These observations come closest to the random allocation ideal and receive more weight than those instances in which the observation was endogenous due to the observable factors. Because it compensates for unknown non-linearities, the inverse probability weighting can be seen as a more flexible mechanism to control for the role of observables compared to controlling only through the conditional mean.

In our application, this implies putting more weight on regulatory LTV ratio tightenings that were taken as a surprise based on observables, and putting less weight on those tightenings that could be predicted. Let $\Delta_h y_{i,t}$ now denote the change in our financial variables between time t and $t+h$ and n the number of observations falling into the respective bin (n_{Tight} vs. $n_{NoTight}$). The second-stage regression is given by:

$$\Delta_h y_{i,t} = \alpha_i^h + \gamma_t^h + \beta^h d_{i,t} + \sum_{k=0}^{4} \phi_k^h \Delta X_{i,t-k} + \epsilon_{i,t+h}\,. \tag{3}$$

The average treatment effect of a tightening for horizon h is then calculated as:

$$\Lambda^h = \frac{1}{n_{Tight}} \sum_i \sum_t \frac{\Delta_h \hat{y}_{i,t} d_{i,t}}{\hat{p}_{i,t}} - \frac{1}{n_{NoTight}} \sum_i \sum_t \frac{\Delta_h \hat{y}_{i,t}(1-d_{i,t})}{1-\hat{p}_{i,t}}. \tag{4}$$

To implement the estimator in expression (4) all that is needed is to estimate expression (3) using weighted least squares (WLS) with weights defined by $w_{i,t} = d_{i,t}/\hat{p}_{i,t} + (1 - d_{i,t})/(1 - \hat{p}_{i,t})$, where we truncate $w_{i,t}$ at 10. The WLS estimation of this extended regression is an example of a "doubly robust" method (e.g. Lunceford and Davidian (2004), Wooldridge (2010) and Glynn and Quinn (2010)). It is called doubly robust because we control for observables via two channels: first, directly in the regression; and second, indirectly through the propensity score. Only one of these two channels needs to be properly specified to produce consistent estimates. Further details can be found in Jordà *et al.* (2016).

5.1. Credit responses

We first apply this procedure to estimate the response of credit variables targeted by LTV limits. These targeted variables are normally household credit and mortgage credit. Table 10 presents the results of our first stage. We run logit classification models for the tightening dummy $\Delta^{Tight} MaPP_{i,t}$ as we want to account for increases in financial variables presumably targeted by tightening actions. Hence, we include in this regression the smoothed growth rates over the previous four quarters of detrended GDP and the detrended price level. Furthermore, we include the growth rate of real credit variables and the real stock price index over the previous four quarters in the regression as well as country-fixed effects. Credit variables, and here particularly the growth rate of total real private credit, emerge as the best predictors of a tightening action in LTV limits. The number of observations is reduced to 455, which is due to country fixed effects, because not all countries in our sample introduced an LTV tightening during our sample period. Furthermore, housing and mortgage credit data are available only for a subset of our observations. We report the AUC statistics which stands for area under the receiver operating curve. The statistic measures the ability of a model to correctly sort observations into the "tightening" and "no tightening" bins as combinations of true positive and false positive rates that result from changing the threshold variable for classification. In other words, it yields a summary measure of predictive ability that is independent of individual cut-off values chosen. The AUC takes on the value of 1 for perfect classification ability and 0.5 for an uninformed classifier or the results of a 'coin toss'. Here the AUC is 0.77 which is a significant improvement over the coin toss.

Figure A.1 in the appendix plots the estimated probabilities of treatment based on the first stage, differentiating between treated units (red) and control units (blue). Our procedure in the second-stage regression now assigns a higher weight to the treated observations that were less likely to be treated based on this analysis, i.e. those observations with very low probabilities. Figure 10 presents the results of these inverse propensity

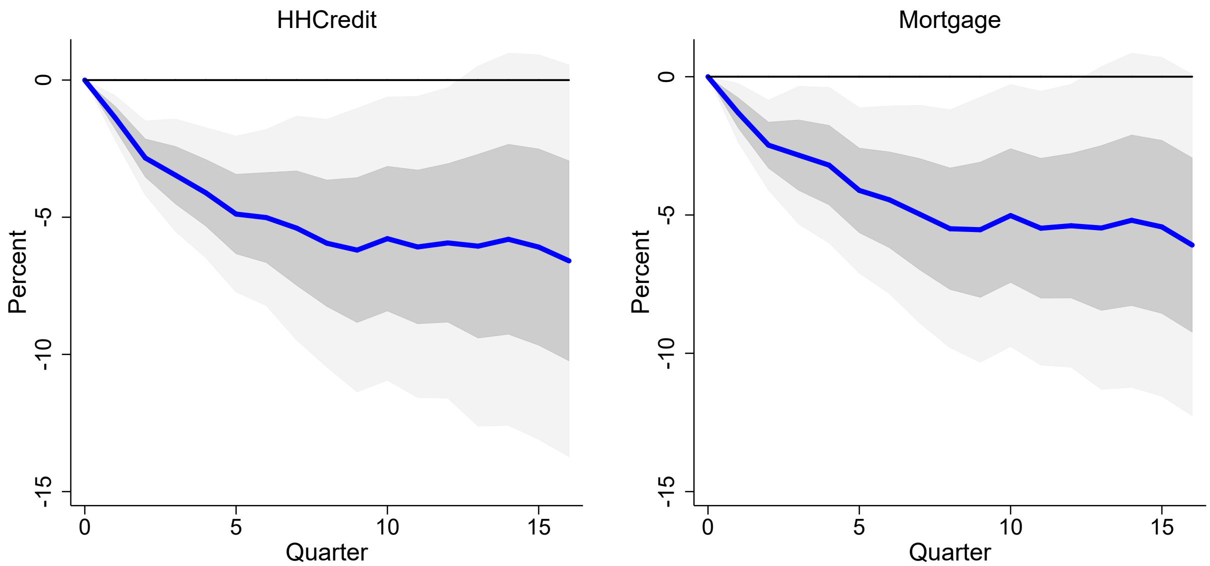
Table 10: *First stage results for credit responses: Prediction of tightening actions.*

	Tightening Dummy
Δ_4 Output gap	0.72
	(1.20)
Δ_4 CPI gap	-0.89
	(1.02)
Δ_4 Real private credit	0.28*
	(0.16)
Δ_4 Real mortgage credit	0.11
	(0.21)
Δ_4 Real household credit	-0.16
	(0.18)
Δ_4 Real stock price index	2.40
	(2.68)
Pseudo R^2	0.133
AUC	0.77
	(0.05)
Observations	455

Notes: The table shows logit classification models where the dependent variable is the tightening dummy. Δ_4 denotes the change over the previous four quarters (the growth rate in the case of credit variables and asset prices). The model includes country-fixed effects. Clustered standard errors in parentheses. See text. $^{*}p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

weighted local projections. We find that a tightening action in maximum LTV ratios decreases credit variables as intended. Real household credit is reduced by almost 6% after two years and mortgage credit by more than 5%. Both coefficients are statistically significant. The coefficients remain stable for longer time horizons, while confidence intervals widen and, as a result, the effects are no longer significant after four years. These results are in line with a number of recent studies that find negative effects of macroprudential tightenings on credit. Remember that the number of observations is reduced in this set-up, as we use a large set of fixed effects and covariates in the first stage logit regression to predict the treatment probabilities. Figure A.9 shows that conditional estimates, which do not employ an IPW estimator, display a stronger decline in credit.

Figure 10: *Inverse propensity weighted local projection: Responses of household and mortgage credit to a change in maximum loan-to-value ratios, tightening dummy.*



Notes: The solid blue lines display the coefficients of cumulative responses of household credit/GDP and mortgage credit/GDP over the 16 quarters following a tightening in maximum LTV ratios. The grey areas display one (dark) and 1.96 (light) standard deviation intervals.

Table 11: *Inverse propensity weighted local projection: Responses of credit variables to a change in maximum loan-to-value ratios, tightening dummy.*

	$h=1$	$h=4$	$h=8$	$h=12$	$h=16$
Dep. Var.: 100 × log(household credit/CPI)					
Tightening	-1.368**	-4.108**	-5.946**	-5.936*	-6.588
	(0.411)	(1.210)	(2.302)	(2.887)	(3.643)
R^2	0.768	0.802	0.802	0.808	0.802
Observations	437	437	437	437	437
Dep. Var.: 100 × log(mortgage credit/CPI)					
Tightening	-1.306**	-3.196*	-5.496**	-5.388*	-6.085*
	(0.541)	(1.434)	(2.194)	(2.610)	(3.149)
R^2	0.761	0.770	0.741	0.751	0.757
Observations	437	437	437	437	437

Notes: Clustered (by country) standard errors in parentheses. Control vector includes the current value and four lags of GDP growth, CPI inflation and policy rate changes. Specifications include country-fixed effects and quarter-fixed effects. Regression weights derived from first stage logit.

5.2. Asset prices

In addition to the effect of LTV tightenings on credit variables, we assess the effect of these measures on asset prices; here we focus on stock prices and house prices. The first stage is again a logit classification model for the tightening dummy. We use a similar set of predictors as in the previous set-up, but replace household and mortgage credit by real house price growth, which is also only available for a subset of observations. Figure A.2 in the appendix plots the estimated probabilities of treatment between treated units (red) and control units (blue). Table 12 presents the results of this approach. In line with the first stage for credit variables, we find that real growth of private credit is a driver of the probability of an LTV limit tightening being enacted.

Table 12: *First stage results for asset price responses: Prediction of tightening actions.*

	Tightening Dummy
Δ_4 Output gap	0.79
	(1.51)
Δ_4 CPI gap	-0.90
	(1.03)
Δ_4 Real stock price index	1.10
	(1.92)
Δ_4 Real house price index	10.11
	(14.45)
Δ_4 Real private credit	0.22*
	(0.12)
Pseudo R^2	0.129
AUC	0.76
	(0.04)
Observations	369

Notes: The table shows logit classification models where the dependent variable is the tightening dummy. Δ_4 denotes the change over the previous four quarters (the growth rate in the case of credit variables and asset prices). The model includes country fixed effects. Clustered standard errors in parentheses. See text. $^{*}p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

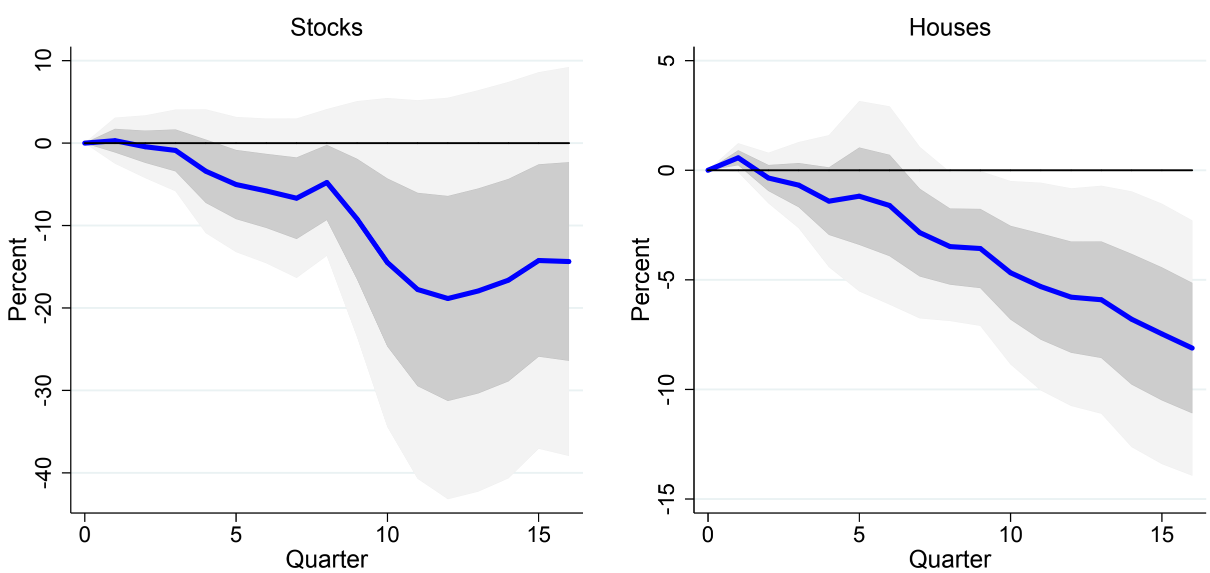
Table 13, Figure 11 and Figure A.10 present the results of this exercise. We see that both, real stock and house prices, initially fall after a tightening action has been implemented. There is strong heterogeneity in the response of stock prices as displayed in the left-hand panel. While the coefficient is negative and large, the confidence intervals are wide and, as a result, this negative effect is insignificant. The response of real house prices is quite different. Here, the confidence intervals are rather small and the negative effect becomes significant after two years. The coefficient further declines the longer the horizon, reaching

Table 13: *Inverse propensity weighted local projection: Responses of real stock and house prices to a change in maximum loan-to-value ratios, tightening dummy.*

	$h=1$	$h=4$	$h=8$	$h=12$	$h=16$
Dep. Var.: 100 × log real stock prices					
Tightening	0.298	-3.410	-4.774	-18.849	-14.364
	(1.411)	(3.817)	(4.528)	(12.411)	(12.024)
R^2	0.723	0.687	0.620	0.681	0.696
Observations	369	369	369	369	369
Dep. Var.: 100 × log real house prices					
Tightening	0.570	-1.410	-3.483*	-5.789*	-8.112**
	(0.333)	(1.533)	(1.728)	(2.528)	(2.967)
R^2	0.544	0.608	0.609	0.635	0.655
Observations	369	369	369	369	369

Notes: Clustered (by country) standard errors in parentheses. Control vector includes the current value and four lags of GDP growth, CPI inflation, policy rate changes and four lags of the change in the loan-to-value variable. Specifications include country-fixed effects and quarter-fixed effects. Regression weights derived from first stage logit.

Figure 11: *Inverse propensity weighted local projection: Responses of real stock and house prices to a change in maximum loan-to-value ratios, tightening dummy.*



Notes: The solid blue lines display the coefficients of cumulative responses of real stock and house price indices over the 16 quarters following a tightening in maximum LTV ratios. The grey areas display one (dark) and 1.96 (light) standard deviation intervals.

a highly significant 8% decline in real house prices after four years. This result is in line with many findings in the literature as well as the targeted nature of LTV limits. As we have seen from analysing the response of credit variables, LTV actions seem to be effective in reducing mortgage credit and they also dampen house prices.

6. Conclusion

This paper uses a narrative identification approach to determine the causal effect of changes in maximum LTV ratios, an important element of the macroprudential toolkit, on output and inflation. Our main objective is to understand to what extent, if at all, macroprudential policy interferes with the main objectives of monetary policy to stabilise output and inflation. For this purpose, we introduced a dataset that codes exogenous and intensity-adjusted changes in maximum LTV ratios for a sample of advanced and emerging market economies.

Our main result is that changes in maximum LTV ratios appear to have relatively modest effects on output and inflation. The output effects tend to be imprecisely estimated and small in advanced economies. After employing a number of different specifications and a battery of robustness tests, we show that our results point to more sizeable effects of LTV tightening than loosening, as well as to consistently larger effects in emerging market economies. The effects of LTV changes on inflation tend to be negligible. As a rule of thumb, over a two-year horizon the mean output effect of a 10 percentage point change in maximum LTV ratios corresponds roughly to that of a 25 basis point change in policy rates, but the standard errors are large. Using inverse propensity weighting methods to mimic random allocation of the LTV treatment, we also provide evidence that LTV changes have substantial effects on credit growth and house prices.

This paper contains several potentially important implications for policy makers. First, our results suggest that central banks in advanced economies could be in a position to use macroprudential instruments to manage financial booms without interfering with the monetary policy objectives in a major way. Second, the use of a scope-adjusted quantified LTV change variable in this paper makes first inroads towards calibrating macroprudential tools. Finally, the evidence in this paper demonstrates that changes in maximum LTV ratios introduced under financial objectives tend to have rather substantial effects on activity in credit markets and house prices as intended.

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Figure A.1: *Treatment propensity score: First stage results for credit responses.*

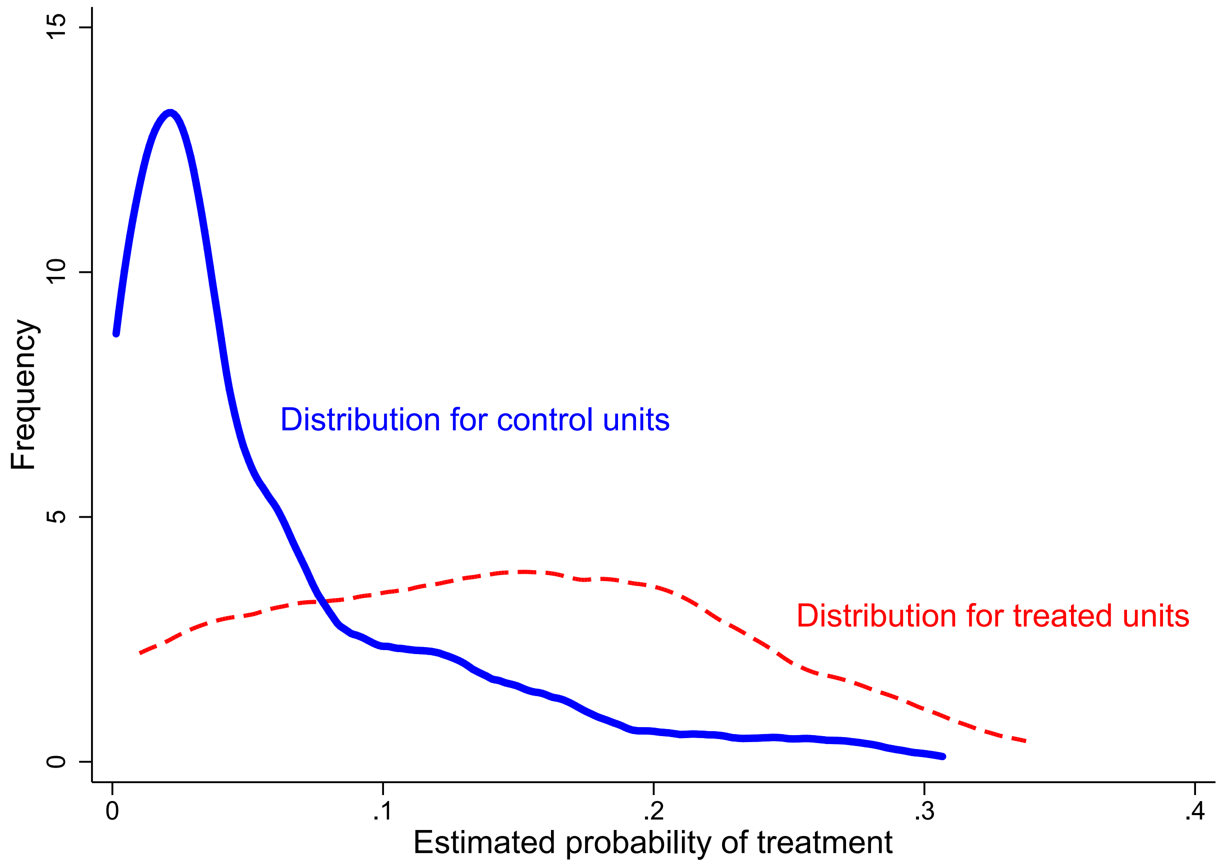


Figure A.2: *Treatment propensity score: First stage results for asset price responses.*

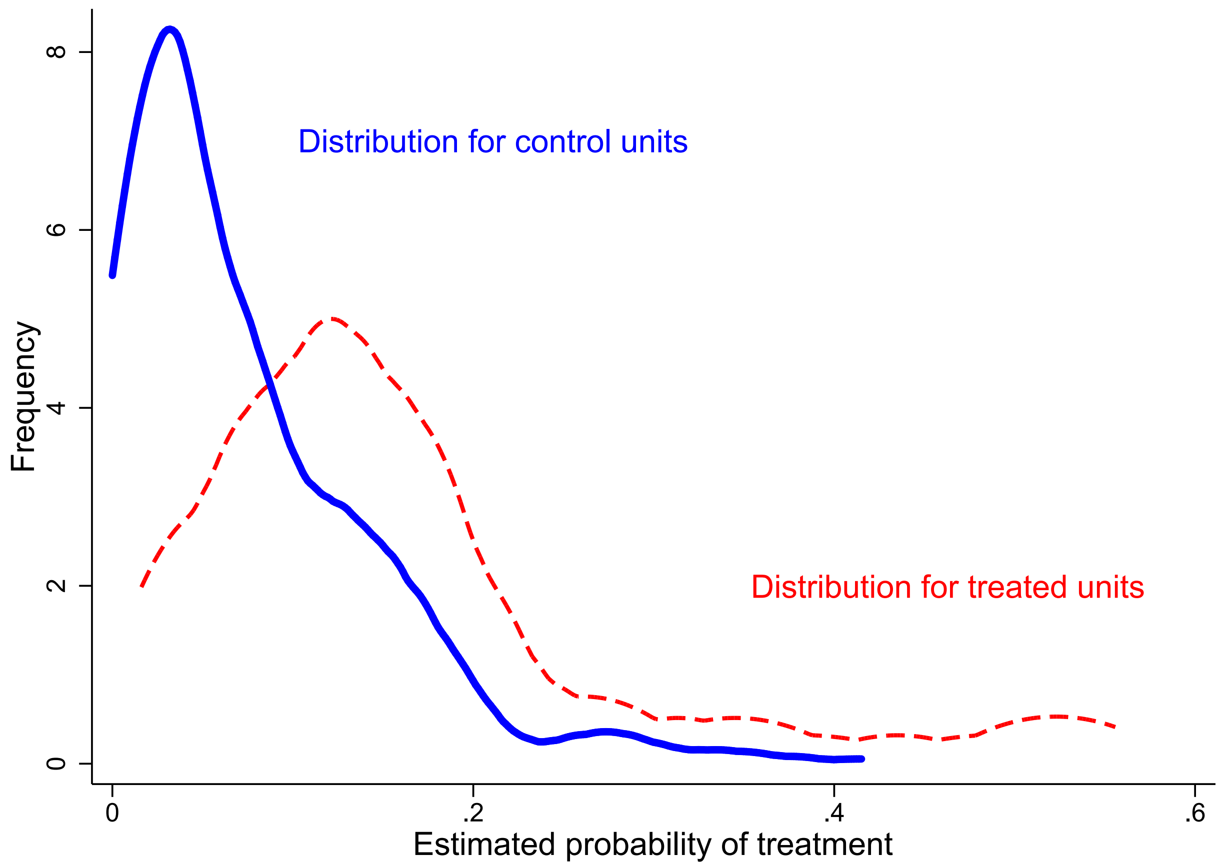
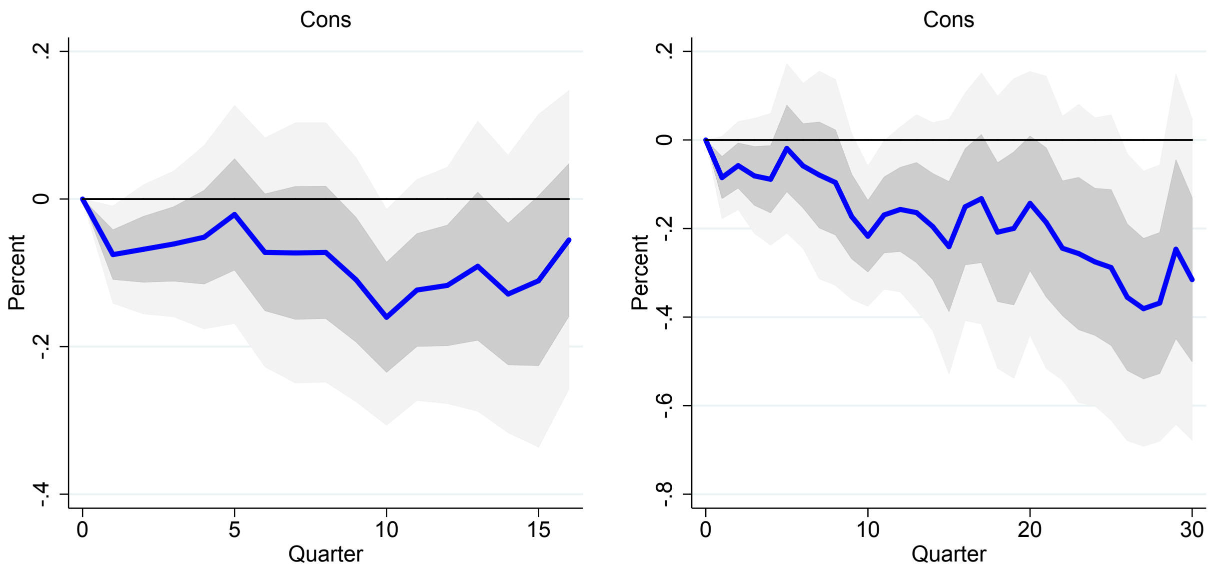
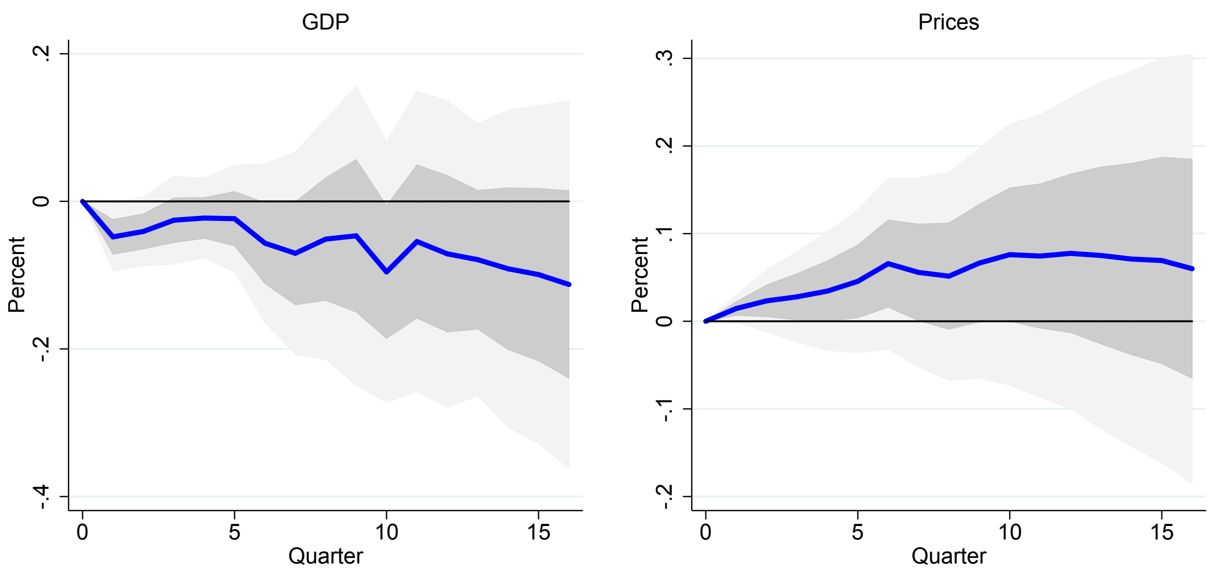


Figure A.3: *Local projection: Response of real consumption to a change in maximum loan-to-value ratios, quantified measure, 16 and 30 quarters horizons.*



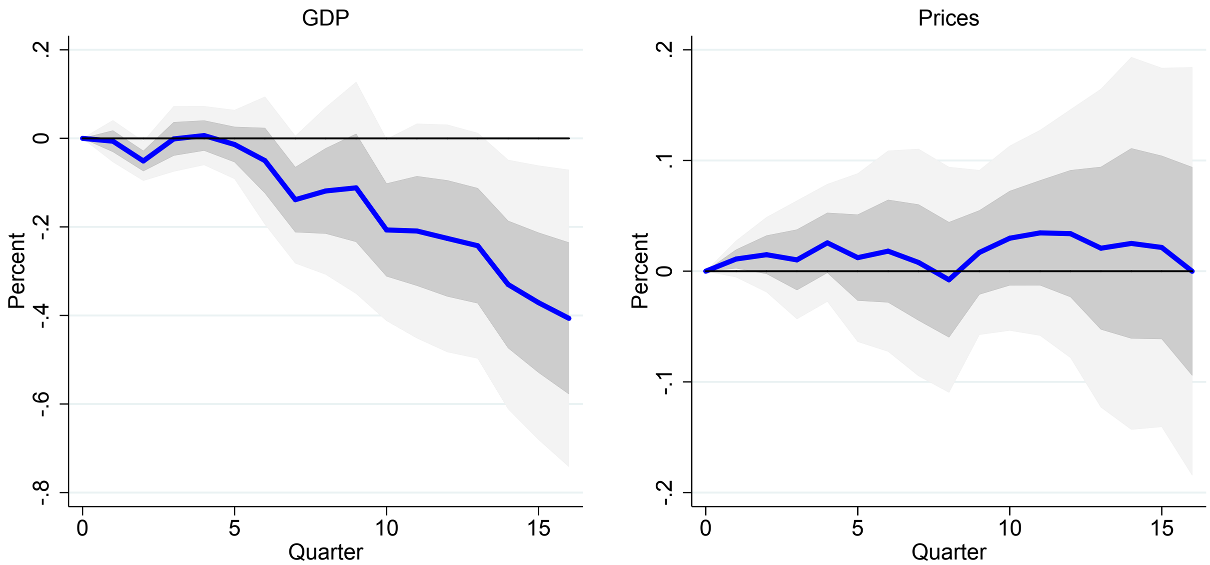
Notes: The blue lines display the coefficients of cumulative responses of real consumption following a change in maximum LTV ratios. The left panel shows a 16 quarters window, the right panel a 30 quarters window. Shaded areas refer to 1 standard deviation (dark) and 1.96 standard deviations (light).

Figure A.4: *Local projection: Responses of real GDP and the price level to a change in maximum loan-to-value ratios, quantified measure, announcement dates rather than implementation dates.*



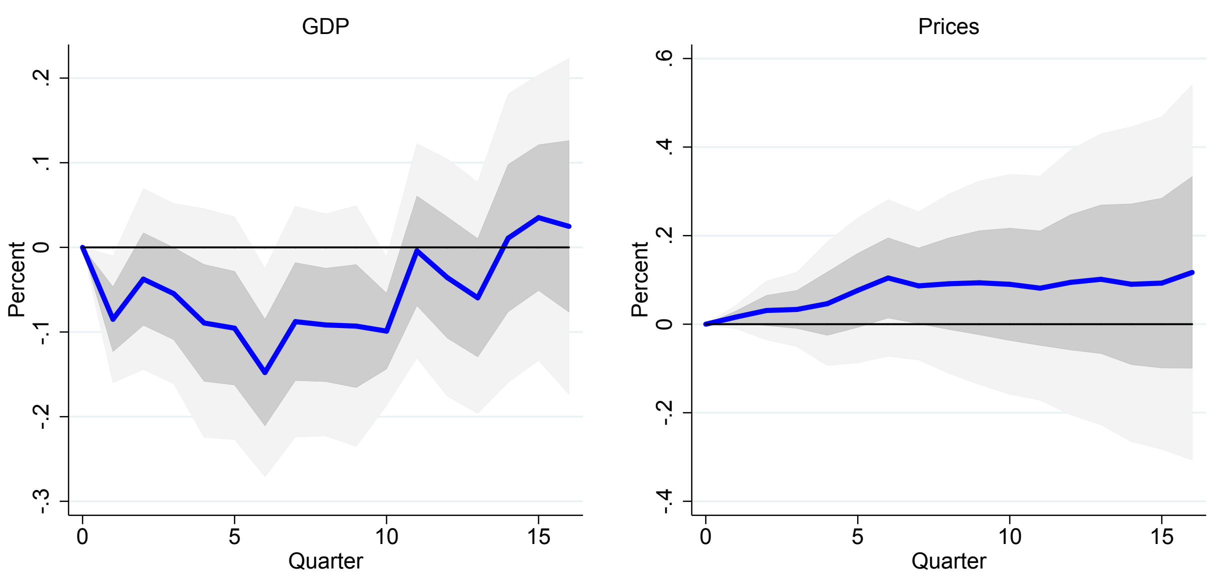
Notes: The blue lines display the coefficients of cumulative responses of real GDP and the price level over the 16 quarters following a change in maximum LTV ratios. Shaded areas refer to 1 standard deviation (dark) and 1.96 standard deviations (light).

Figure A.5: *Local projection: Responses of real GDP and the price level to a change in maximum loan-to-value ratios, quantified measure, boom periods.*



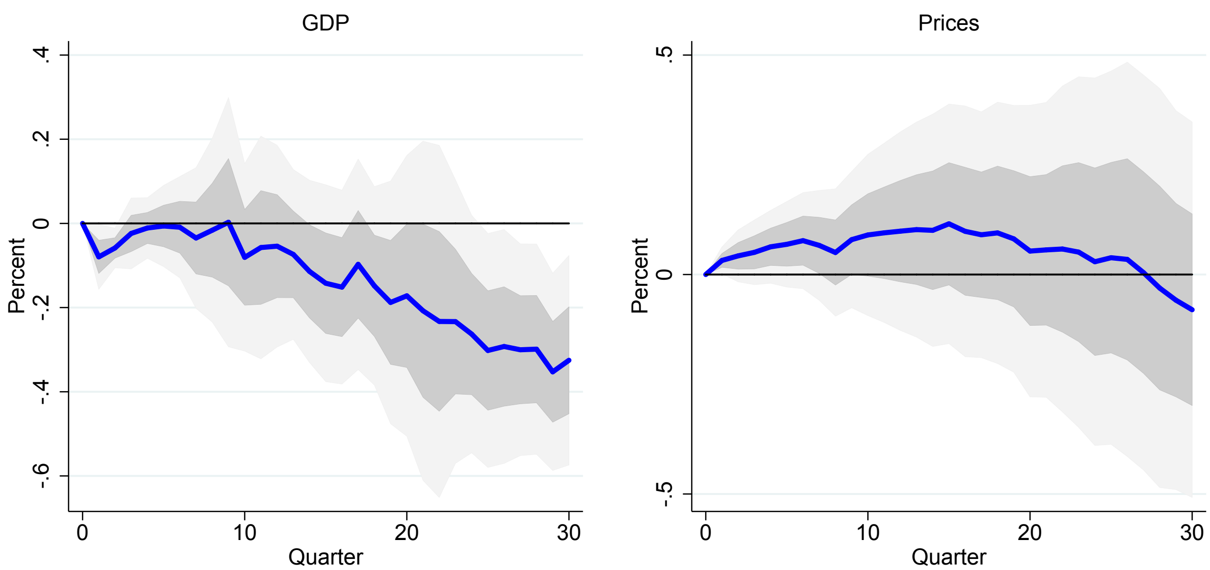
Notes: The blue lines display the coefficients of cumulative responses of real GDP and the price level over the 16 quarters following a change in maximum LTV ratios. Shaded areas refer to 1 standard deviation (dark) and 1.96 standard deviations (light). Estimates based on subsample of boom periods (output is above trend). Output trend is computed using the Hamilton (2017) procedure as explained in the main text.

Figure A.6: *Local projection: Responses of real GDP and the price level to a change in maximum loan-to-value ratios, quantified measure, slump periods.*



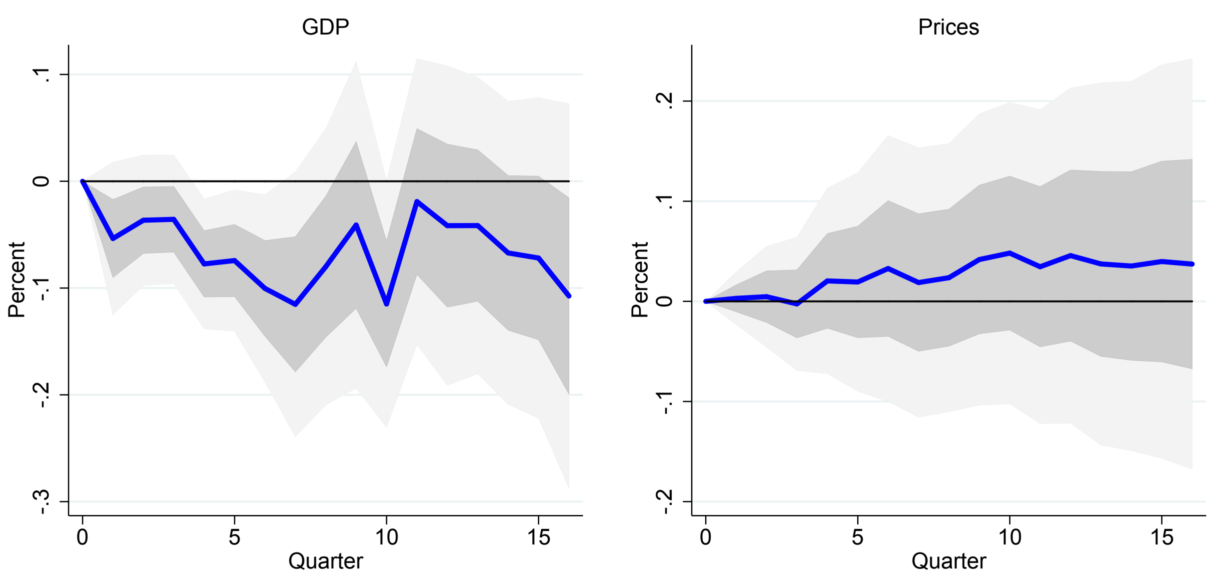
Notes: The blue lines display the coefficients of cumulative responses of real GDP and the price level over the 16 quarters following a change in maximum LTV ratios. Shaded areas refer to 1 standard deviation (dark) and 1.96 standard deviations (light). Estimates based on subsample of slump periods (output is below trend). Output trend is computed using the Hamilton (2017) procedure as explained in the main text.

Figure A.7: *Local projection: Responses of real GDP and the price level to a change in maximum loan-to-value ratios, quantified measure, 30-quarter horizon.*



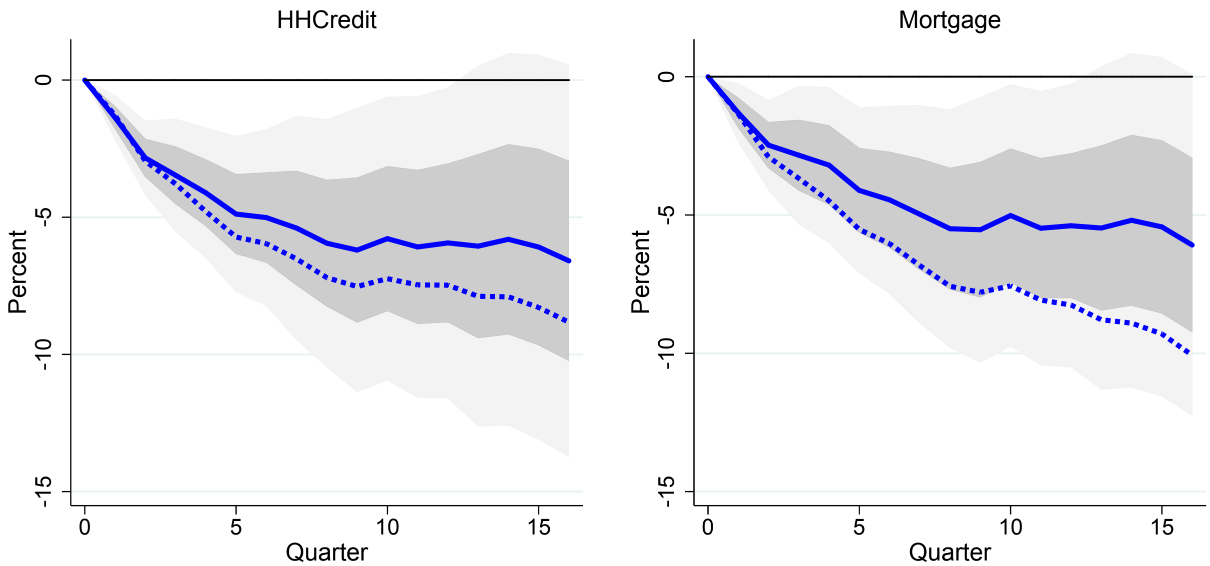
Notes: The blue lines display the coefficients of cumulative responses of real GDP and the price level over the 16 quarters following a 1 percentage point change in maximum LTV ratios. Shaded areas refer to 1 standard deviation (dark) and 1.96 standard deviations (light). Baseline specification as in Equation 1 with $h = 30$.

Figure A.8: *Local projection: Responses of real GDP and the price level to a change in maximum loan-to-value ratios, quantified measure, additional controls.*



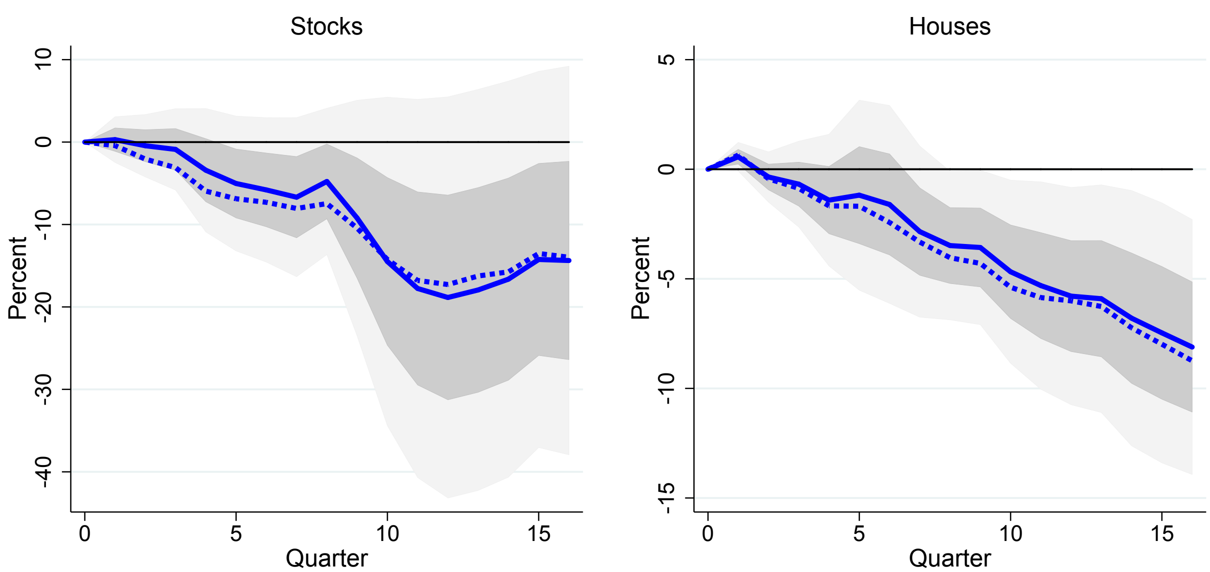
Notes: The blue lines display the coefficients of cumulative responses of real GDP and the price level over the 16 quarters following a change in maximum LTV ratios. Shaded areas refer to 1 standard deviation (dark) and 1.96 standard deviations (light). Baseline specification as in Equation 1, including additional control variables.

Figure A.9: *Inverse propensity weighted local projection – comparison: Responses of household and mortgage credit to a change in maximum loan-to-value ratios, tightening dummy.*



Notes: The solid blue lines display the coefficients of cumulative responses of household credit/GDP and mortgage credit/GDP over the 16 quarters following a tightening in maximum LTV ratios. The dotted lines show non-IPW estimates for comparison. The grey areas display one (dark) and 1.96 (light) standard deviation intervals.

Figure A.10: *Inverse propensity weighted local projection – comparison: Responses of real stock and house prices to a change in maximum loan-to-value ratios, tightening dummy.*



Notes: The solid blue lines display the coefficients of cumulative responses of real stock and house price indices over the 16 quarters following a tightening in maximum LTV ratios. The dotted lines show non-IPW estimates for comparison. The grey areas display one (dark) and 1.96 (light) standard deviation intervals.