WORKING PAPER

MACROPRUDENTIAL POLICY AND ITS IMPACT ON THE CREDIT CYCLE

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Abstract

We identify a novel set of macroprudential policy shocks and estimate their effects on credit cycle variables in a panel of 13 EU countries during 1999-2018. We find that a typical macroprudential policy tightening shock reduces bank credit-to-GDP by 1.8% points and household credit-to-GDP by 1.6% points over a period of four years. The non-financial corporations and total credit-to-GDP ratios, however, do not react significantly. Using state-dependent local projections, we further find that the effects on the credit-to-GDP ratios are stronger in credit cycle upturns than in downturns. We also detect a sizable leakage of firm credit from the banking to the non-banking sector next to a shift from firm to household credit.

Keywords: Macroprudential policy, Effectiveness, State dependency

JEL classification: C23, E58, G18, G28

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1 Introduction

Following the Great Financial Crisis (GFC), macroprudential policy caught the attention of policymakers in advanced economies. Due to a lack of macroprudential supervision, high and unsustainable levels of credit had built up (Borio and Lowe, 2002; Borio and Shim, 2007), aggravating the subsequent financial crisis and the accompanying deep recession. The scope and depth of the GFC have triggered extensive macroeconomic research on the causes of financial imbalances and the potential tools to impede their occurrence. An important role in this respect has been attributed to the credit cycle. Excessive credit growth has been identified as a good predictor of financial crises (Schularick and Taylor, 2012) while an increase in the household credit-to-GDP ratio seems to be followed by lower GDP growth and a higher rate of unemployment in the medium run (Mian et al., 2017).

In this paper, we focus on the effectiveness of macroprudential policy in ensuring financial stability in advanced economies. This constitutes an aspect of economic research which still lacks theoretical understanding as well as empirical evidence given the relatively recent experience of advanced economies with macroprudential policy. More specifically, we investigate in the first instance whether macroprudential tools have been able to affect credit-to-GDP ratios, i.e. curb the credit cycle, and therefore enhance financial stability for a group of EU countries. To this aim, we consider a panel of 13 EU countries between 1999-2018 and identify a novel set of macroprudential policy shocks based on a general macroprudential policy index that is constructed from the European Central Bank (ECB) database of Budnik and Kleibl (2018). In addition, we examine potential state-dependencies in the effectiveness of macroprudential policy by taking into account the specific phase of the credit cycle, i.e. discriminating between upturn and downturn phases, next to controlling for the business cycle and the household debt-to-income ratio.

We estimate the effects of these macroprudential policy shocks on general and sectoral credit-to-GDP ratios over a period of 16 quarters using the Local Projections (LP) approach of Jordà (2005). We discriminate between total credit to the private non-financial sector, total credit to non-financial corporations (NFCs), total credit to households and NPISH[1], and domestic bank credit

[1] Non-profit institutions serving households

to the private non-financial sector. The breakdown of total credit in its different constituent parts enables one to examine potentially different or diverging effects of macroprudential policy.

The use of panel LPs has several advantages. Panel data are first of all required to analyze the macroeconomic effects of macroprudential policy actions as information on different countries is essential to obtain a sufficient number of policy actions, especially for a sample of advanced economies. LPs further allow to examine the persistence of the effects. Many existing studies just look at the reaction on impact or assume a constant impact over time by regressing credit variables on the level of the macroprudential index (i.e. the number of restrictions applied to the economy at this point in time). We instead investigate the effects over a horizon of four years which allows us to observe the evolution of the reactions in the short to medium run. LPs further easily accommodate non-linearities.

The focus on a relatively narrow set of advanced economies on the other hand alleviates heterogeneity concerns that go along with large panels combining advanced and less advanced economies as used in related works (e.g. Cerutti et al., 2017; Akinci and Olmstead-Rumsey, 2018).[2] Distortions related to time-varying differences in, for example, institutional characteristics (e.g. the extent of financial development and openness of a country) and the employed types of macroprudential tools (e.g. the frequent use of capital controls by emerging economies in contrast to advanced economies) are minimized by the use of a more homogeneous group of countries.

Our results first of all show that the identified macroprudential policy shocks do curb the credit cycle and therefore diminish financial instability by reducing the ratios of household and domestic bank credit-to-GDP in a persistent manner. Over the period of 16 quarters, we find that the household credit-to-GDP ratio decreases by 1.6% points while the domestic bank credit-to-GDP ratio falls by over 1.8% points after a restrictive macroprudential policy shock. By contrast, our results reveal that the ratio of NFC credit-to-GDP does not react much and most often insignificantly. The reaction of total credit-to-GDP is also not significant. This last finding is likely to be driven by the high share of NFC credit (i.e. having a cross-country average of 58 to 61% over time in our

[2]Cerutti et al. (2017) for example have documented that the effects of macroprudential policy on credit growth are substantially smaller for a subset of advanced economies relative to their overall sample covering also emerging and developing economies.

sample) in total credit in the economy.

These findings are broadly in line with the related literature on the effects of macroprudential policy on credit growth. Cerutti et al. (2017) document that a one unit change of their general macroprudential policy index lowers total credit growth by 1.4% points after one year for a group of 31 advanced economies between 2000 and 2013. Household credit similarly lowers by 0.8% while credit to NFCs does not react in a significant way. Carreras et al. (2018) also document a significant immediate reduction of 0.2% in household credit after a general macroprudential tightening for a panel of 18 OECD economies between 2000q1 and 2013q4 and a decline with 1.1% after 8 quarters. Based on a sample of 22 advanced economies between 2000q1 and 2013q4, Akinci and Olmstead-Rumsey (2018) also find that bank credit growth decreases with 0.2% following a unit increase in their overall macroprudential index in the previous quarter whereas housing credit does no react significantly. Similarly, when focusing on household and mortgage credit, Richter et al. (2019) find a reduction of around 6% in household credit after three years following a restrictive change in the loan-to-value (LTV) ratio in a set of 56 emerging and advanced economies during 1990q1-2012q2. When looking more broadly at borrower-based measures (predominantly LTV and debt-service-to-income ratios) in 28 EU countries between 1990q1 and 2018q2, Poghosyan (2019) documents that a tightening action does not reduce total credit to the private sector in the short to medium term although it leads to a significant 1.5% reduction after three years.

Secondly, we find that the ratio of domestic bank credit to non-regulated credit[3] decreases by up to around 5% points in the years after a restrictive macroprudential policy shock. This points towards a "leakage" effect of macroprudential policy through credit supplied by domestic non-banks and/or foreign credit suppliers. Using ECB data on NFCs' balance sheets, we find that firms are able to find alternative funding sources when banks are confronted with macroprudential regulatory restrictions and substitute credit from domestic banks with borrowing from the domestic shadow banking sector and, to a smaller extent, with borrowing from abroad. These findings are economically meaningful given that, on average, 24% of NFC loans are borrowed from abroad and 41% of the domestic loans originate from non-MFI lenders for our sample of countries in 2014q4

[3]Non-regulated credit is in this context defined as the difference between total credit in the economy less domestic bank credit, hence excluding bank credit that is subject to regulation by the home country's regulatory banking supervision authorities.

(for households these numbers are 0.4% and 13%). The significance of leakage effects is in line with an earlier study by Cizel et al. (2019), in which the authors detect a sizable substitution effect from bank to non-bank credit in a panel of 28 advanced economies between 2000 and 2014. They find that non-bank credit growth is 1.8% higher after the introduction of a quantity-based macroprudential measure. Aiyar et al. (2014) find similar effects. Based on time-varying bank-specific minimum capital requirements in the UK between 1998-2007, they find a 1% decrease in credit of regulated banks while credit growth of foreign branches increased by 3%. In addition, our results indicate a substitution from household credit to credit to NFCs in response to a tightening shock based on the respective share in the total credit-to-GDP ratio.

Interestingly, we further find that all credit-to-GDP ratios react more negatively if a tightening macroprudential policy shock occurs in an upturn phase of the credit cycle than during a downturn phase. The results show significant decreases of respectively 2% points and 3% points after four years in the household and bank credit-to-GDP ratio when the shock takes place between a trough and a peak in the credit cycle and insignificant reactions when the shock occurs between a peak and a trough in the financial cycle. The sensitivity of the effects of the macroprudential tools to the extent of debt leverage in the economy and the phases of the business cycle is less clear-cut but indicates that macroprudential policy is more effective in constraining credit during phases of high leverage ratios whereas its effectiveness does not depend on the business cycle.

Lastly, we find no significant effects on real GDP and on the price level. The insignificant reaction of real GDP is in line with the findings of Richter et al. (2019) on the impact of a changing LTV ratio. The insignificant reactions of economic growth and prices imply that financial stability can be strengthened at a relatively low macroeconomic cost.

The remainder of the paper is structured as follows: in section 2, we present the credit data and introduce the macroprudential database. In section 3, we describe the employed econometric models and the identification of the macroprudential policy shocks. In section 4, we discuss the estimated effects on the credit variables and the wider economy. In section 5, we consider potential state-dependencies and check the robustness of our results. We conclude in section 6.

2 Data

We examine the effects of macroprudential policy shocks on credit dynamics in 13 EU countries between 1999q1-2018q4.[4] In the next subsections, we first elaborate on the credit data and the construction of relevant credit cycle proxies. Next, we discuss the data on macroprudential policy actions. The data section in the appendix provides further details on the data series and sources of all variables. All series are in real terms (deflated by the HICP).

2.1 Credit data

We make use of the widely-used "Long series on credit to the non-financial private sector" database of the Bank of International Settlements (BIS) which has the attractive feature that it offers comparable macro-level credit data for a large number of countries. More specifically, we use four different types of credit series: (i) Total credit from all sources (domestic and foreign) to the private non-financial sector (households and NFCs), (ii) total credit from all sources (domestic and foreign) to NFCs, (iii) total credit from all sources (domestic and foreign) to households and NPISH, and (iv) domestic bank credit to the private non-financial sector. Item (ii) and (iii) sum up to item (i) but allow for a separate sectoral evaluation. The difference between (i) and (iv) on the other hand informs us about credit originating from foreign banks and (domestic and foreign) non-banks, i.e. sectors which are not directly affected by a domestic macroprudential policy action concerning the banking sector.

The credit data are normalized by expressing them relative to GDP. In addition, to proxy credit cycles, these credit-to-GDP ratios are evaluated relative to their trend. The credit-to-GDP gap or *credit gap*, i.e. the difference of the actual credit-to-GDP ratio from its long-run trend, constitutes a measure of the degree of excess credit in the economy (Borio and Lowe, 2002). We deduce the trend component using a Hodrick-Prescott (HP) filter. Analyzing HP-filtered credit-to-GDP series is in line with the Basel III framework in which this gap is explicitly mentioned as an indicator for the implementation of counter-cyclical capital buffers. Moreover, Drehmann and Yetman (2018) show

[4] Austria, Belgium, Denmark, Finland, France, Germany, Ireland, Italy, the Netherlands, Portugal, Sweden, Spain, and the United Kingdom

that the one-sided HP-filtered credit-to-GDP gap has the highest power in predicting financial crises compared to other univariate indicators. Following the BIS methodology, the trend component is obtained by applying the one-sided HP-filter to the first 24 observations and then recursively adding one additional observation at a time. Afterwards, we delete the first 24 quarters to avoid starting point issues. Following the original work of Borio and Lowe (2002), a large smoothing parameter is used, i.e. $\lambda = 400,000$, taking into account the relatively long duration of credit cycles. The HP-filter has lately been criticized, most notably by Hamilton (2018), in that it produces spurious dynamic relations given assumptions on the smoothing parameters and the end-of-sample bias. By using the one-sided HP-filter and deleting the first 24 observations, we address those concerns.

2.2 Macroprudential Policy actions from MaPPED

The macroprudential policy actions used in this paper are taken from the Macroprudential Policies Evaluation database (MaPPED) compiled by Budnik and Kleibl (2018). The MaPPED is the most comprehensive data set in Europe listing almost 1,700 policy actions of a prudential nature since 1995, based on information provided by officials of all 28 EU member states' supervisory authorities.

More specifically, the MaPPED gives a detailed overview of all policy actions taken by the central banks and supervisory authorities of the EU member states deemed backward-looking as being of macroprudential nature, i.e. policy actions which could be classified as being a prudential policy tool with the objective to reach macroprudential goals or prudential tools whose structure and transmission channels closely resemble those of macroprudential instruments and which have a system-wide impact. This choice encompasses all macroprudential tools but also microprudential tools with a likely system-wide impact. In comparison to other macroprudential databases which just gives the implementation and termination of a policy instrument (e.g. Cerutti et al., 2017) the MaPPED follows each policy instrument over its "life cycle" by also accounting for the changes in the level or scope of each tool. The MaPPED further contains information on whether the macroprudential policy action is a tightening or loosening action or whether it had an ambiguous impact.

3 Econometric methodology

In this section, we first introduce the econometric model used to investigate the transmission of macroprudential shocks to the broader economy and we discuss its specifications. Afterwards, we elaborate on how we identify exogenous macroprudential shocks.

3.1 Model estimation

To estimate the effects of macroprudential policy shocks, we use the LP method proposed by Jordà (2005). Local projections are more recently widely used to estimate impulse response functions due to their flexibility and their robustness to misspecification in comparison to the VAR approach and for their convenience in estimating nonlinear specifications. In this work, we primarily study the path of the credit-to-GDP ratios and the credit gap conditional on a macroprudential policy shock (MPS) and macroeconomic controls. Equation (1) represents our benchmark model:

$$Y_{i,t+h} = \zeta^h MPS_{i,t} + \gamma^h(L)Y_{i,t-1} + \phi^h(L)ctrs_{i,t-1} + \alpha_i^h + \theta_t^h + \epsilon_{i,t+h} \tag{1}$$

for $h = 0, 1, \ldots, H$. Our dependent variable $Y_{i,t+h}$ is the level of Y (e.g. total credit-to-GDP ratio) at time $t + h$. We regress this variable on the macroprudential policy shock $MPS_{i,t}$ (as defined below) and the lagged values of Y. Moreover, we include as controls ($ctrs$) the log of real GDP, of the HICP and the policy interest rate[5] in levels. Further, we include the lags of the macroprudential policy shock in the controls to account for past implementations and the overall level of regulatory tightness in an economy. We also include a vector consisting of all other lagged macroprudential policy actions excluded narratively in the section below, to control for potential influences of other macroprudential policy events. We further add country-fixed effects α_i and time-fixed effects θ_t to control for country-specific factors and for unobserved common factors (e.g. the Great Financial Crisis, the European sovereign debt crisis, US monetary policy).

We correct for both cross-sectional and serial correlation in the error terms by using standard

[5]For the EMU and the UK we take the Wu and Xia (2016) shadow rate. For Denmark and Sweden, we take their respective policy interest rate.

errors corrected following Driscoll and Kraay (1998). We choose $H = 16$ quarters as horizon length for our local projections.[6] In our benchmark specification, we set the lag length to four quarters as is common for quarterly data. Moreover, we hold the sample constant by dropping all observations that are not included in the local projection step with the largest horizon. This leaves us with a panel of 819 country-quarter observations.

3.2 Identification of macroprudential policy shocks

We are interested in the quarter-on-quarter unexpected change in the macroprudential policy stance of an economy. Before quantifying the effects of macroprudential policy changes, we need to *purge* the macroprudential policy actions and address potential endogeneity concerns to be able to measure the causal effects of macroprudential policy. To this aim, we use the detailed information provided in the answers of the questionnaire of the MaPPED to narratively exclude those tools with a distinct nature. More specifically, we focus on the time span between the date of the announcement of the policy action and the date of its enforcement, the stated objective of a tool and the binding nature of the policy action.

The MaPPED lists for every policy action the quarter in which it was announced to the public and the quarter in which the action was enforced by the supervisory authority. Some tools are announced and enforced in the same quarter while in other cases there are sometimes years between those dates. We exclude the policy actions from our sample if those two dates do not fall in the same quarter. If announcement and enforcement dates are far from each other, banks might adopt these policies at different times, thus diluting the results. What is more, for some macroprudential tools banks would start immediately to reduce credit whereas for others, banks might be inclined to *increase* credit between announcement and enforcement of a restriction. A good example is the introduction of a LTV ratio. Banks might start expanding their lending in anticipation of future credit restrictions. Taking into account only the announcement dates therefore would lead us to pick up distinct reactions. Focusing only on enforcement dates on the other hand goes against the unexpected nature of shocks.

[6] Results of local projections namely become less reliable at long horizons (Ramey and Zubairy, 2018).

An additional criterion is to exclude measures for which the responsible institution has declared a counter-cyclical objective. In this case, the macroprudential tools are merely introduced as a reaction to macroeconomic developments. To avoid reversed causality issues, we exclude all macroprudential tools which were designed with a counter-cyclical objective. We further consider the binding nature of a tool and exclude those non-mandatory policy actions for which a breach does not lead to a penalty or fine. Including them would again mix up different types of reactions which we want to avoid. Table 3 in the appendix summarizes how many policy actions are excluded due to each criterion of the narrative identification mentioned above. Table 4 in the appendix lists all remaining policy actions.

We collect all policy tools in the MaPPED and count them per country-quarter observation. We give the same weight to the different types of policy actions (i.e. implementation, change in scope or level and termination). Meaning that e.g. an implementation of a tool which tightens the macroprudential policy stance and a tightening in the scope or level of an existing tool are counted in the same way.

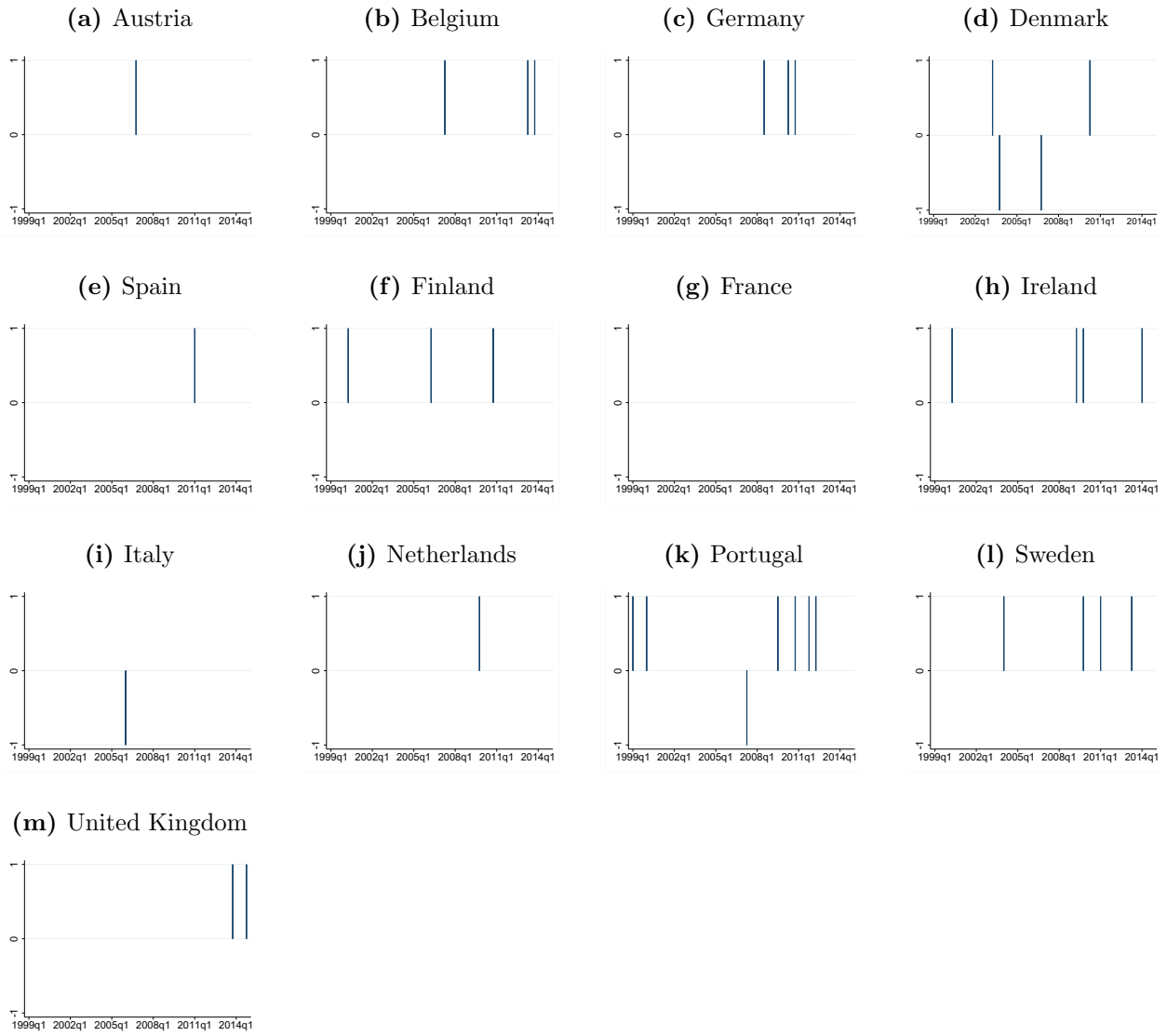
If there were more tightening than loosening policy actions during a quarter, the country-quarter observation counts as a tightening quarter and gets the value 1. If there were more loosening than tightening actions, the country-quarter observation gets the value -1. If there is an equal amount of loosening and tightening actions or there was no policy action, the country-quarter observation gets the value 0. We thereby follow the literature[7] by abstaining from weighting policy actions further.

Figure 1 gives an overview of the changes in the macroprudential policy stance for each of the 13 countries under analysis between 1999q1-2014q4.[8] There does not seem to be a systematic pattern concerning countries' frequency of changing the macroprudential policy stance, e.g. North vs. South. Austria and Spain for example introduced just a few policy actions during the sample period while Portugal or Denmark implemented relatively more policy measures. Most of the policy actions, however, have taken place in the second half of the sample, reflecting the arising

[7] e.g. Cerutti et al. (2017), Fendoglu (2017), Akinci and Olmstead-Rumsey (2018).

[8] As our estimation sample starts in 1999q1 and the ECB database officially ends in 2014q4, we only depict those tools falling into this time frame.

Figure 1: Macroprudential policy actions which comply with the narrative restrictions



Note: The bars show the direction of the change of the macroprudential policy stance in each country over time. A value of 1 means the macroprudential stance becomes tighter in that country and value of -1 means the macroprudential policy stance becomes looser in that country.

and aftermath of the GFC. In the robustness check section, we show that our findings are robust to an alternative calculation method of changes in the macroprudential policy stance based on the number of actions.

3.3 Granger Causality

Although we control for several real-economy variables and the narratively excluded implementations, our shocks might still be forecasted by the private sector. The policy actions under examination are regulations which take time to be approved by governmental bodies and might be already in the information set of economic agents prior to announcement due to the lengthy process of law making. To abate this endogeneity concern, we use information of the Bank Lending Survey (BLS) conducted by the ECB.

In this survey, responsible senior loan officers in 150 banks are specifically asked whether they expect that their credit standards for consumer credit, for house purchase credit and/or credit for enterprises will tighten or loosen in the upcoming quarter. If their expectations could anticipate the macroprudential actions, our shock would be endogenous. To test this, we regress our macroprudential shock in quarter t on the forecasts about credit standard changes made in quarter t-1 for quarter t (similar to Ramey (2011)). Additionally, we also regress our shock on the first principal component of these three indexes. The BLS data is available from 2003q1 onward, for the Euro area countries in our sample except Finland. This means that we have data for nine out of the 13 countries in our sample[9] for 48 quarters. Our number of country-quarter observations therefore reduces to 432. The first four lines of table 1 show the p-values of the coefficients of the regressions. The coefficients are all insignificant even on the 10% level meaning that the survey data does not predict our shocks.

Senior lending officers are further asked whether they tightened or loosened their credit standards in the last quarter. The index derived from that question allows us to check whether our shock Granger causes a change in banks' perception of credit standards by regressing the three-month backward-looking assessment about credit standards in period t+1 for period t on our macroprudential shock in period t. If our macroprudential policy shock tightens credit constraints, bank credit standards should also increase. The lower part of table 1 shows that this is the case. The coefficient is significant on the 5% level for credit for house purchase and for the first principal

[9] Austria, Belgium, Germany, Spain, France, Ireland, Italy, the Netherlands and Portugal. Meaning no comparable data for Denmark, Finland, Sweden and the United Kingdom.

Table 1: Granger Causality

Hypothesis		p-value
Do standards on consumer credit forecast the macroprudential shock?	No	0.322
Do standards on house purchase credit forecast the macroprudential shock?	No	0.237
Do standards on credit to NFCs forecast the macroprudential shock?	No	0.381
Does the first factor of credit standards forecast the macroprudential shock?	No	0.273
Does our shock forecast a change in standards for consumer credit ?	No	0.124
Does our shock forecast a change in standards for a house purchase?	**Yes**	0.042
Does our shock forecast a change in standards for credit to enterprises?	**Yes**	0.077
Does our shock forecast a change in the first factor of credit?	**Yes**	0.040
N		432

Note: The first four lines show the p-value for the regression of our macroprudential policy shock in t on the diffusion index of the forecast change in credit standards at time t-1 for period t for the respective type of credit. The last four lines do the same for the index of backward-looking changes in credit standards (asked in t+1 about changes in t) for the identified macroprudential policy shock in t. The data on credit standards is taken from the Bank lending survey of the ECB.

component of the three measures, and on the 10% level for credit for NFCs.

These findings indicate that senior loan officers could not anticipate the identified shocks or its effects. Moreover, the identified macroprudential policy shocks seem to have an impact on internal credit standards as assessed by bank officials in a backward-looking way. Given the simplicity of these regressions, the effects should not be overstated but they do offer a confirmation of the appropriateness of our identification strategy.

4 Do macroprudential policy tools affect the credit cycle?

By estimating the impulse response functions (IRFs) over 16 quarters, we examine the medium-term effects of the macroprudential policy shocks next to the immediate impact. In a first step, we analyze the reaction of the level of total credit, total credit to households and NPISH, total credit to NFCs and domestic bank credit to GDP to a macroprudential shock. In a second step, we investigate potential *leakages* from the banking to the non-banking and foreign credit sector by looking at aggregated balance sheet data of NFCs. Further, we estimate the IRFs for the *credit gaps* as a proxy for the impact of macroprudential tools on excessive credit growth and related

financial (in)stability. Lastly, we measure the reaction of the real economy variables output and prices.

4.1 Reaction of credit ratios

Figure 2 shows the IRFs of the cumulative change in the four aforementioned credit-to-GDP ratios over a horizon of 16 quarters after a tightening macroprudential policy shock, as estimated using equation (1). The solid blue line gives the point estimates for each h. The grey areas reflect the 90% error bands. The first two panels show the IRFs of the credit-to-GDP ratios for total credit and total credit to NFCs. The shapes of both IRFs are quite comparable and show a predominantly insignificant reaction to the macroprudential policy shock. The third and fourth panels depict the reaction of the credit-to-GDP ratios for total credit towards households and domestic bank credit. These ratios both show a persistently negative response to the macroprudential policy shock. The household credit-to-GDP ratio decreases gradually over time, becomes significant after 3 quarters and is around 1.6% points lower after 16 quarters compared to the non-shock scenario. The domestic bank credit-to-GDP ratio is around 1.8% points lower after 16 quarters after having slightly reverted back from a maximum decrease of over 3% points after three years.

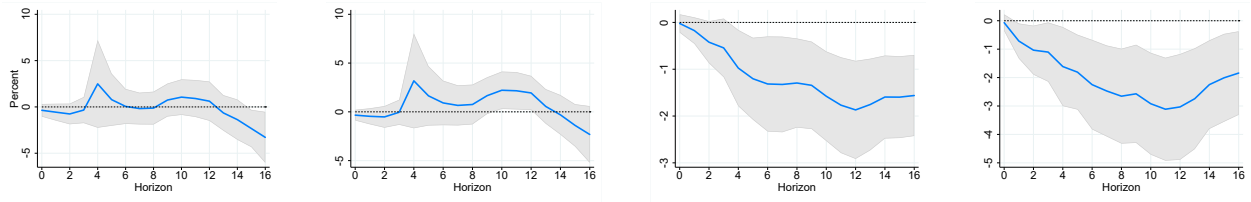
Figure 2: Impulse response functions of credit-to-GDP ratios to macroprudential policy shock

(a) Total credit-to-GDP

(b) NFC credit-to-GDP

(c) Household credit-to-GDP

(d) Dom. Bank credit-to-GDP



Note: The solid blue lines show the point estimates of the reaction of the variable to a macroprudential policy shock over a horizon of 16 quarters. The grey areas reflect the 90% error bands.

These results are in line with Cerutti et al. (2017) in the sense that household credit reacts stronger than corporate credit to a change in the overall macroprudential index. In contrast to their findings, however, total credit does not decline in response to a tightening macroprudential shock according to our model estimates. Akinci and Olmstead-Rumsey (2018) similarly find that

housing credit growth decreases following an increase in their general macroprudential policy index and this by 0.4% after one quarter. Moreover, bank credit growth declines by 0.3% following a increase in their overall index. For a subset of advanced economies, however, they find no significant reduction in housing credit and only a weakly significant change in bank credit growth by -0.2%. Carreras et al. (2018) find that a tightening in their aggregate macroprudential index in a set of OECD economies results in a 0.2% reduction in household credit on impact and a 1.1% reduction after two years.

The fall in the household and bank credit-to-GDP ratios relative to the more or less unaffected total and NFCs credit-to-GDP ratios signals a substitution effect of credit to households towards credit to NFCs and a leakage of regulated domestic bank credit towards unregulated domestic non-bank credit and foreign credit. Panel a of figure 3 shows the IRF for the ratio of domestic bank credit to the sum of domestic non-bank credit and foreign credit[10]. The ratio decreases for six quarters before leveling off at 5% points indicating a sizable *leakage* of domestic bank credit towards the non-banking and/or foreign financial system after a tightening macroprudential shock in the domestic economy. This leakage potentially undermines the effectiveness of macroprudential tools in stabilizing the domestic economy by preventing that the total credit-to-GDP ratio and therefore the leverage of the economy falls. Panels b and c show the changes of NFC credit and household credit as share of overall credit. From the IRFs, it becomes evident that there is a substitution of household credit to NFCs credit in the economy after the implementation of a macroprudential measure.

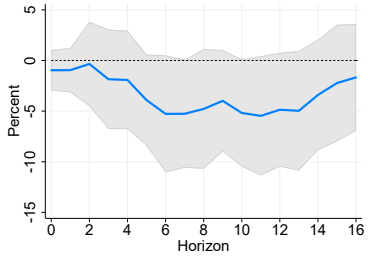
To investigate the nature of the leakage more in detail, we examine in figure 4 the effects on loans towards NFCs based on ECB balance sheet data.[11] We focus on loans to NFCs because corporations tend to have more access to alternative funding sources relative to households. These alternative sources, shadow banks and foreign banks, are mostly not restricted by domestic macroprudential regulations. To differentiate whether the leakage takes place domestically (shift from bank to non-bank borrowing) or abroad (shift from domestic to foreign credit), we look at four different types

[10]This sum is calculated as the difference between total credit and (domestic) bank credit

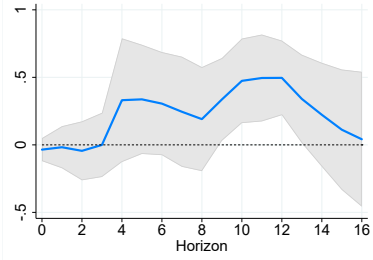
[11]The data is not available for the UK, and for Denmark only for 7 quarters. This reduces the sample under analysis to 690 country-quarter observations.

Figure 3: Impulse response functions of credit ratios to macroprudential policy shock

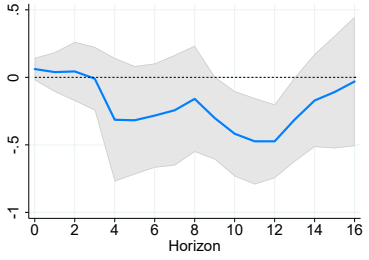
(a) Dom. Bank credit / Foreign and non-bank credit



(b) Non-financial corporation credit / Total credit



(c) Household credit / Total credit



Note: The solid blue lines show the point estimates of the reaction of the variable to a macroprudential policy shock over a horizon of 16 quarters. The grey areas reflect the 90% error bands.

of loans to domestic NFCs: (i) loans from domestic monetary financial institutions (MFIs) (ii) all other domestic loans, (iii) loans from other financial intermediaries (OFIs)[12] which is a subcategory of loans from non-MFIs and (iv) loans from foreign lenders. We divide the balance sheet positions by the overall stock of loans in the balance sheet of NFCs aggregated by country. This allows us to interpret the IRFs as the relative effect of macroprudential policy on the composition of NFCs' loan funding.

While the data is not entirely comparable to the BIS data as it just includes loans and not debt securities[13], the findings suggest a substantial replacement of regulated by unregulated lending meaning that - in the aggregate - NFCs borrow less from domestic banks after a macroprudential policy shock and start borrowing more from the domestic shadow banking sector. These results are in line with earlier work on the replacement effects of bank capital regulation in the U.K. ((Aiyar et al., 2014)) and for a panel of 37 countries (Reinhardt and Sowerbutts, 2015) and the finding in Cizel et al. (2019) of a substitution from bank to non-bank credit after a general macroprudential tightening in a sample of advanced economies. They might also explain the muted reaction of total non-financial credit after a macroprudential policy shock. Panel d of figure 4 further reveals that also the share of foreign credit increases. The coefficients are however not significant on the 10%

[12] An OFI is defined by the ECB (ECB glossary) as: *"a corporation or quasi-corporation which is mainly engaged in financial intermediation other than a MFI, a pension fund or an insurance company such as corporations engaged in financial leasing, financial vehicle corporations created to be holders of securitized assets, financial holding corporations, dealers in securities and derivatives (when dealing for their own account), venture capital corporations and development capital companies."*

[13] Loan data is longer available than data on debt securities.

level.

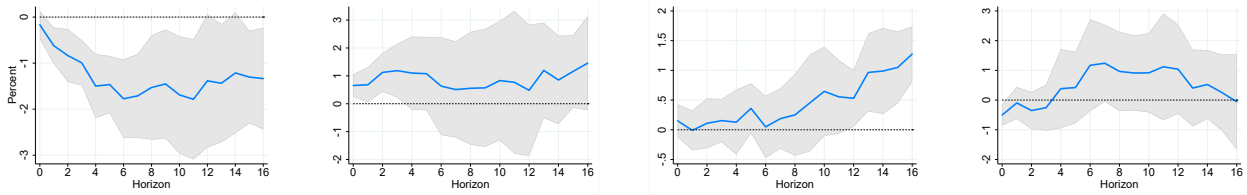
Figure 4: Impulse response functions of credit ratios to macroprudential policy shock

(a) MFI loans/ All loans to NFCs

(b) Non-MFI loans/ All loans to NFCs

(c) OFI loans/ All loans to NFCs

(d) Foreign loans/ All loans to NFCs



Note: The solid blue lines show the point estimates of the reaction of the variable to a macroprudential policy shock over a horizon of 16 quarters. The grey areas reflect the 90% error bands.

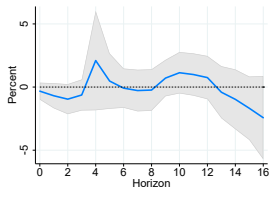
4.2 Reaction of cyclical components of credit ratios

The level responses of credit-to-GDP ratios are informative about the effectiveness of macroprudential tools in steering credit levels irrespective of their (long-run) level. Quantifying the effects of macroprudential tools on deviations from the long-run trend in addition allows for a more direct evaluation of the effectiveness of macroprudential policy in curbing excessive credit/stimulating below-trend credit evolution. The cyclical components, i.e. the credit gaps, are constructed based on the deviation from a one-sided recursively run HP-filtered trend (see section 2.1). Figure 5 shows the IRFs of the credit gaps for all four credit-to-GDP variables.

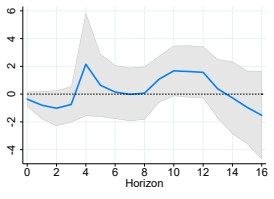
The first thing to notice is that the shape of the IRFs of the credit gaps strongly resembles the previous IRFs of the ratios. Similar to before, household credit and bank credit gaps are negatively affected by a tightening macroprudential action although the reactions are quantitatively smaller with a maximum of -0.6% for household credit and -1.5% for bank credit in the second year. Both reactions are, however, only marginally significant in the first half of the response horizon. The reactions of total credit and credit to NFCs are again almost completely muted. A typical tightening macroprudential policy action hence seems to decrease the cyclical component of household and bank credit in the first 1.5 year. Our results are different to the findings of Fendoglu (2017), who finds a more sizable reduction of the total credit gap of 2.4% on impact following the implementation of a macroprudential policy tool for a group of 18 emerging economies between 2000q1 and 2013q2.

Figure 5: Impulse response functions of credit-to-GDP gaps with smoothing parameter $\lambda = 4*10^5$ to macroprudential policy shock

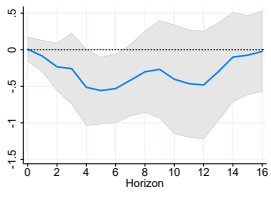
(a) Total credit-to-GDP gap



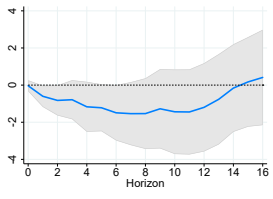
(b) NFC credit-to-GDP gap



(c) Household credit-to-GDP gap



(d) Bank credit-to-GDP gap



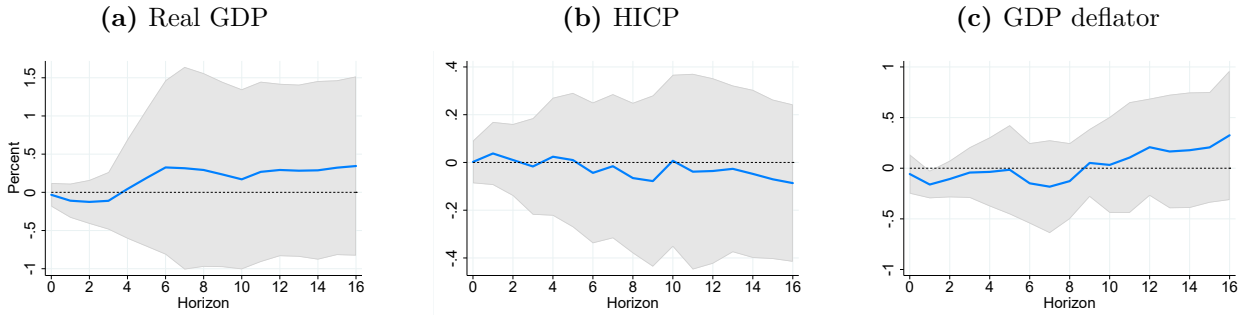
Note: The solid blue lines show the point estimates of the reaction of the variable to a macroprudential policy shock over a horizon of 16 quarters. The grey areas reflect the 90% error bands.

4.3 Do macroprudential shocks impact output and inflation?

Next to the effects on financial variables, the implementation of macroprudential tools is often thought to have repercussions for the real economy through the effects on financial conditions. In this section, we analyze the effects of macroprudential shocks on the traditional monetary policy targets, output and inflation. Evidence suggests that macroprudential policy shocks reduce economic activity (e.g. Sánchez and Röhn (2016)) by affecting investment. In more recent empirical work, however, this hypothesis is contested. Boar et al. (2017) find in a panel of 64 advanced and emerging economies that macroprudential policy can increase GDP growth and reduce its volatility in open and financially developed economies. Richter et al. (2019) only find little evidence of reduced GDP growth and an ambiguous effect on inflation due to a LTV shock.

We estimate the effect of macroprudential policy on the real economy variables by using equation (1). As dependent variables, we use real GDP, HICP and the GDP deflator all in log-levels. In this specification, we include credit to GDP as a control variable next to the lags of the respective dependent variable. Figure 6 shows the IRFs when the total credit-to-GDP ratio is used as control but results are robust to the use of the sectoral credit ratios. Real GDP does not react significantly to a tightening macroprudential policy shock. The HICP and GDP deflator show no significant reaction as well. Our findings hence indicate that the macroeconomic cost of macroprudential policy might be quite small or non-existent in general with no significantly negative effects on output and prices.

Figure 6: Impulse response functions of real economy variables to macroprudential policy shock



Note: The solid blue lines show the point estimates of the reaction of the variable to a macroprudential policy shock over a horizon of 16 quarters. The grey areas reflect the 90% error bands.

5 Does the effect depend on the state of the economy?

In this section, we look at possible state-dependent effects of macroprudential policy related to credit, leverage or business cycles. There exists to the best of our knowledge no theoretical framework which relates the effects of macroprudential policy to the different phases of these cycles despite the high importance of a correct understanding of their interdependence for the conduct of macroprudential policy. At the same time, state-dependencies of macroprudential tools' effectiveness are also a mostly unexplored field in empirical work given the relatively new field of macroprudential policy evaluation.[14] In the next subsection, we first describe the approach we take to measure potential state dependencies. Next, we evaluate the effects along the credit cycle by looking at asymmetric responses that depend on the state of real credit. Afterwards, we investigate potential nonlinear reactions depending on the extent of debt overhang and the state of the business cycle.

[14] A notable exception is Cerutti et al. (2017) in which the effects of macroprudential policy are allowed to depend on the growth rate of credit. First, by interacting their macroprudential policy index with the growth rate of total credit. Second, by interacting the index with dummy variables that equal 1 during times when credit growth is in the highest/lowest 10th percentile and zero otherwise. When looking at the effects in isolation, macroprudential policy is found to be more effective during periods of high credit growth and the effect of the macroprudential policy index is significantly different for times when credit growth is in the highest 10th percentile than for periods when credit growth was in the lowest 10th percentile of the sample. When combined, none of the effects turn out to be significant.

5.1 Measuring state dependencies

To answer the question whether macroprudential policy is more effective in certain states of the economy, we estimate a linear state-dependent local projection regression model. Different from regime-switching VAR models, state-dependent local projection models take the regime changes *endogenously* into account by averaging over the possible effects of a regime change after a shock. To construct our model, we augment equation (1) to account for two possible regimes.

$$\begin{aligned} Y_{i,t+h} =& D_{i,t-1}\Big[\alpha_i^{A,h} + \theta_t^{A,h} + \zeta^{A,h} MPS_{i,t} \\ &+ \gamma^{A,h}(L) Y_{i,t-1} + \phi^{A,h}(L) ctrs_{i,t-1}\Big] + \\ &(1 - D_{i,t-1})\Big[\alpha_i^{B,h} + \theta_t^{B,h} + \zeta^{B,h} MPS_{i,t} \\ &+ \gamma^{B,h}(L) Y_{i,t-1} + \phi^{B,h}(L) ctrs_{i,t-1}\Big] + \xi_{i,t+h} \end{aligned} \tag{2}$$

where $D_{i,t-1} \in \{0, 1\}$ is a dummy variable with value 1 if the economy is in state A and 0 if the economy is in state B. We include $D_{i,t}$ in the estimation lagged by one period to reduce endogeneity concerns. All other variables remain the same.

5.2 Does the reaction depend on the credit cycle?

We first look at the asymmetric effects arising from macroprudential policy shocks in credit cycle upturns and downturns. We define an upturn as the phase when the credit cycle is between a trough and a peak (including the peak) and a downturn as the phase when the credit cycle is between a peak and a trough (including the through).

We identify the credit cycle and its troughs and peaks following Hiebert et al. (2018) by applying the Harding and Pagan (2002) algorithm to the log of real total credit. We set the parameters as in the classical turning point analysis of Drehmann et al. (2012) and as in Hiebert et al. (2018) to have five quarters minimum length for a cycle (peak to peak or trough to trough) and two quarters minimum length for each phase (peak to through or trough to peak). Due to the shorter time frame of downturns in the financial cycle compared with upturns, we find 611 upturn quarters

with 16 tightening shocks and 4 loosening shocks, while we observe 195 downturn quarters with 12 tightening shocks. That we find tightening shocks in phases of downturns is not surprising given that the state of the cycle is ex-ante or even in real-time not measurable and just ex-post known. The distribution of upturn and downturn phases per country can be found in the appendix.

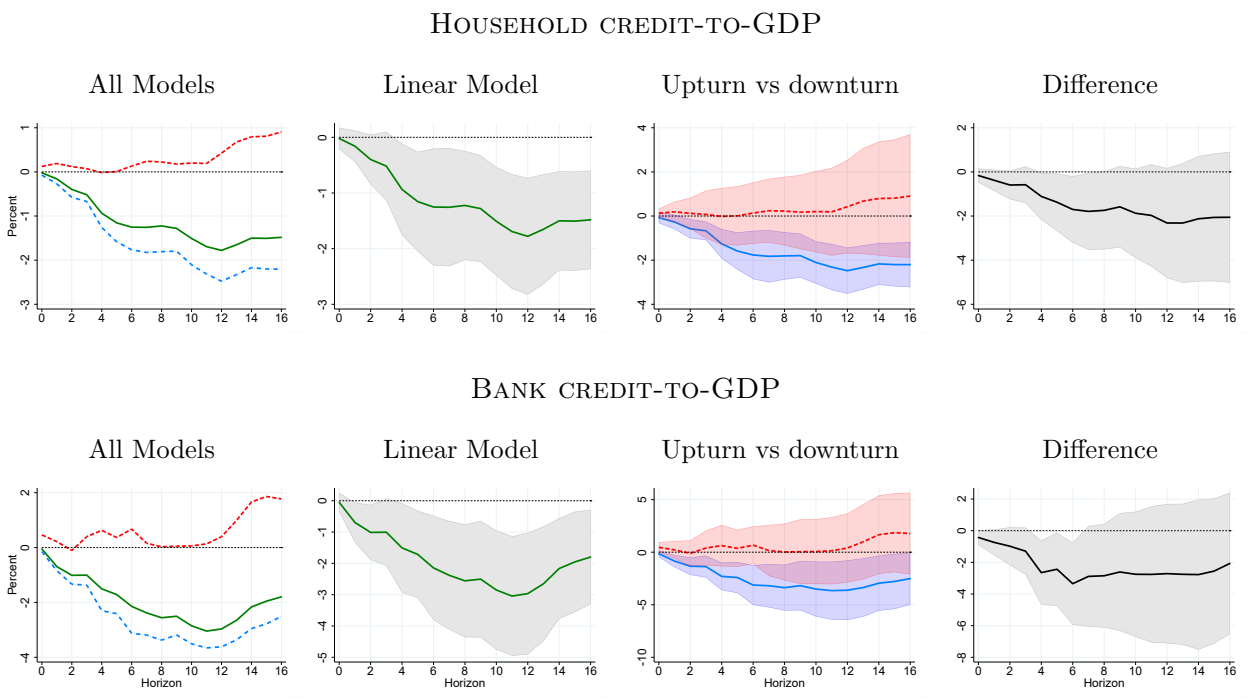
Given our earlier findings, we focus on the state-dependent reactions of the credit to households and domestic bank credit ratios to a macroprudential policy shock. Figure 7 gives the IRFs for each variable depicted in a separate row. In the different columns, we distinguish the reactions for each model. In the first column, the point estimates of the linear model (solid green line) estimated with equation (1) and the upturn (blue dashed line) and the downturn states (red dotted line) estimated with equation (2) are shown together. The second column depicts the IRFs of the linear model while the third column shows the IRFs for the upturn (blue) and downturn (red) states together with their confidence bands. The fourth column gives the IRFs of the difference between the reactions in the upturns and downturns, i.e. the difference between $\zeta^{A,h}$ and $\zeta^{B,h}$ and its 90% confidence band.[15]

The results show that the linear responses for household and bank credit-to-GDP ratios are predominantly driven by shocks during credit cycle upturns. The credit-to-GDP levels drop by respectively more than 2% points and 3% points during upturns whereas during a credit cycle downturn, there is no significant response. The interesting feature of the estimation of the cyclical phases is that the policymaker is in real-time not aware of the turning points. That should guarantee that the introduced policy actions should not depend on the cycle itself but can be seen as exogenous in the short run.

The most plausible explanation for the state-dependent asymmetric reaction are state-dependent binding credit constraints. In a credit cycle downturn, agents are in general already constrained whereas in a credit cycle upturn, macroprudential policy could introduce credit constraints and thereby curb the credit cycle more effectively.

[15] We calculate this difference directly using an auxiliary regression where we extend equation (1) by the same set of variables interacted with a dummy variable being one in a leveraging-state. The coefficient of the macroprudential shock interacted with this dummy variable gives the difference between the states.

Figure 7: State-dependent impulse response functions showing the asymmetric reaction based on the state of the credit cycle. Calculated with the Harding Pagan filter.



Note: In the first column, the point estimates of the linear model (solid green line) estimated with equation (1) and the upturn (blue dashed line) and the downturn states (red dotted line) estimated with equation (2) are shown together. The second column depicts the IRFs of the linear model while the third column shows the IRFs for the upturn (blue) and downturn (red) states together with their confidence bands. The fourth column gives the IRFs of the difference between the reactions in the upturn state and the downturn state, i.e. the difference between $\zeta^{A,h}$ and $\zeta^{B,h}$ and its 90% confidence band.

These results however come with a caveat. A concern is that macroprudential tools implemented during a rising credit cycle might increase the tightness by more than tools implemented during a falling credit cycle. Although we obviate this concern to some extent by using an ex-ante unknown measure for the credit cycle, it is impossible to dispel this critique with the data set at hand given that our macroprudential policy index does not allow to pick up the intensity of any macroprudential action.

5.3 How do other states of the economy influence the transmission of macroprudential policy?

Next to the potential dependencies on the credit cycle, there might be an asymmetric policy transmission arising from other macroeconomic state variables. Therefore, we repeat the estimation in the previous section for two alternative possible drivers of nonlinearities: the household debt overhang and the business cycle. Figure 8 depicts the results.

Household debt overhang As an indicator of the extent of household debt overhang, we use the *household credit gap* following the definition of the BIS.[16] As soon as the household credit gap is positive, the household credit-to-GDP ratio exceeds its long-run trend indicating higher leveraged households. On the other hand, a negative credit gap occurs if the household credit-to-GDP ratio is below its long-run trend. Phases of high leverage indicate phases in which households are more sensitive to adverse shocks and phases in which credit constraints do not seem to bind as strictly. The left upper panel of figure 8 shows the IRFs of the household credit-to-GDP ratio after a macroprudential tightening during a positive (blue) and a negative (red) household credit gap. The reactions during both phases are similar although more negative during phases of higher household credit-to-GDP ratios. The IRFs, however, are not significantly different from each other. The upper right panel shows the same IRF for bank credit. There is a stronger effect on the bank credit-to-GDP ratio during a positive household credit gap, especially in the longer run, while the effects during a negative gap are not significant.

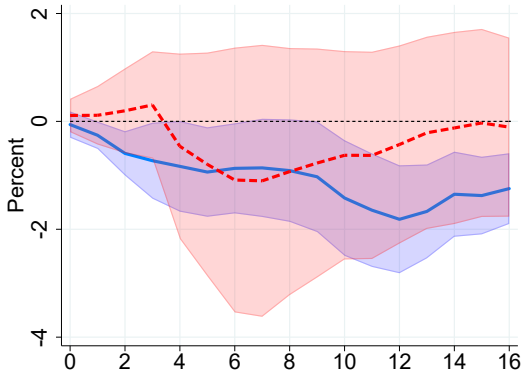
Business cycle Another potential driver of nonlinear responses is the business cycle. The effects of macroprudential policy actions might differ between an expansion and recession. We estimate the business cycle using again the Harding and Pagan (2002) filter on real GDP as proposed by Hiebert et al. (2018). A recession is defined as a phase between a peak and a trough and an expansion as the time between a trough and a peak. The results of estimating equation (2) can be found

[16] I.e. we define the household credit gap as the detrended series of household-to-GDP, detrended by the one-sided HP-filter with a large smoothing parameter, i.e., $\lambda = 400{,}000$.

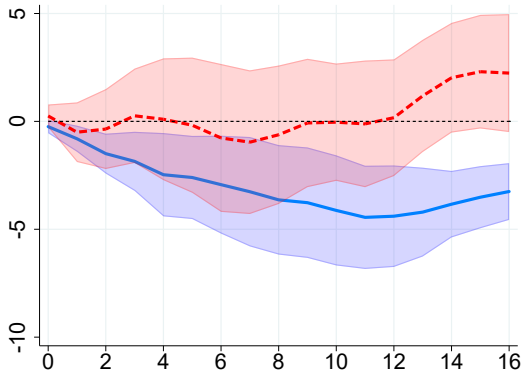
Figure 8: Impulse response functions to macroprudential policy shock depending on alternative states

HOUSEHOLD DEBT OVERHANG

(a) HH credit positive (blue) vs negative debt overhang (red)

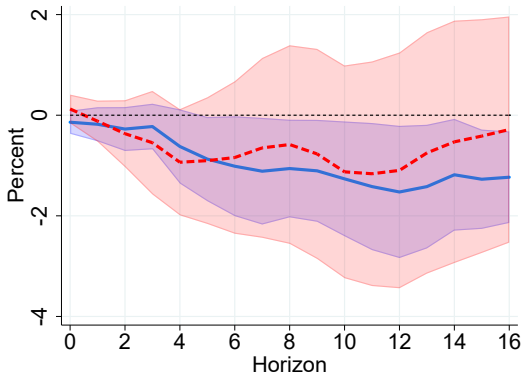


Bank credit positive (blue) vs negative debt overhang (red)

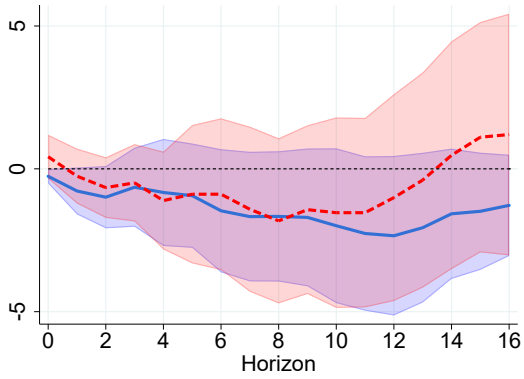


BUSINESS CYCLE

(b) HH credit expansion (blue) vs recession (red)



Bank credit expansion (blue) vs recession (red)



Note: The dashed red line and the red shaded area denote the IRFs and the 90% confidence band in the low state. The solid blue line and the blue shaded area show the IRFs and the 90% confidence bands in the high state.

in the second row of figure 8. The point estimates of the reactions of household credit and bank credit-to-GDP in expansions (blue) and during recessions (red) are very similar. Macroprudential policy effectiveness therefore does not seem to depend on the business cycle.

6 Robustness checks

To explore the sensitivity of our benchmark results, we conduct a series of checks. We test whether our results are sensitive to the composition of the sample, to the definition and direction of the shock, or to the credit cycle definition. Throughout the sensitivity analysis, we show the results for all four credit-to-GDP ratios.

6.1 Sample split

One concern might be that the GFC distorts our results due to, for example, structural changes or because the deleveraging after the crisis drives the documented relationship between macroprudential policy and credit-to-GDP ratios. We therefore re-estimate equation (1) while splitting our sample. The first row of figure 9 shows the IRFs using only data prior to 2008 and the second row depicts the IRFs using only data after 2007. The IRFs of our benchmark results are displayed in each graph in solid red while the confidence bands are displayed in dashed blue. For both subsamples, the results are broadly robust with one exception. In the sample after 2007, bank credit-to-GDP does not react significantly to a macroprudential shock. One reason might be that after the crisis, many tools where implemented during credit cycle downturns and, as seen before, bank credit-to-GDP effects seem to depend on the phase of the credit cycle.

6.2 Alternative calculation of the shock

We further check to what extent the definition of the size of each shock affects our results. In our benchmark regression, the macroprudential policy shock is a dummy variable which can take the values {-1,0,1}. An alternative approach that is used in the literature is to add all policy actions per country-quarter observation together, such that the shock is quantified in terms of the number of tightening and loosening actions. This approach hence takes not only into account whether the macroprudential stance changes but tries to weight the change based on the number of actions. The IRFs using this alternative shock measure can be found in the third row of figure 9. The results stay qualitatively the same and quantitatively very similar.

6.3 Tightening versus loosening

To check whether the results are driven by the direction of the shock, we re-estimate our benchmark regression including two shock series instead of one to namely one with only tightening policy actions and one with only loosening shocks. This enables us to pick up potential asymmetries related to the direction of the shock. To estimate the parameters for these two types of shocks, we augment equation (1) as follows:

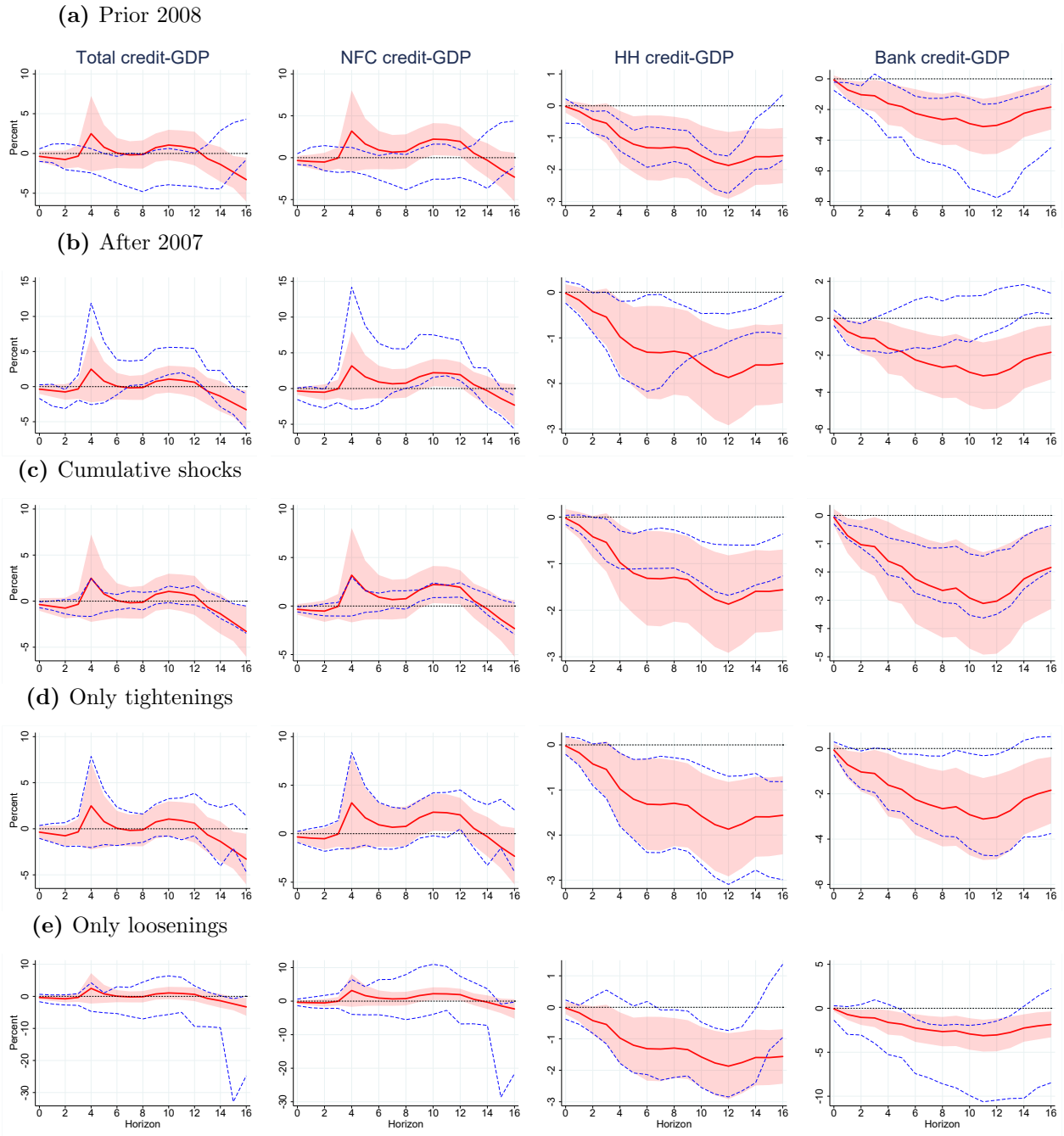
$$\begin{aligned} Y_{i,t+h} = \zeta_T^h MPS_{i,t}^{Tight} + \zeta_L^h MPS_{i,t}^{Loose} + \gamma^h(L) Y_{i,t-1} + \phi^h(L) ctrs_{i,t-1} \\ + \alpha_i^h + \theta_t^h + \epsilon_{i,t+h} \end{aligned} \tag{3}$$

The fourth row in figure 9 shows the IRFs for the tightening shocks ζ_T^h and the fifth row for the loosening shocks, i.e., ζ_L^h . The IRFs for tightening policy actions are almost identical to the benchmark results while the IRFs of the loosening policy actions have broad confidence bands. This can be explained by the low amount of loosening policy actions. The results nevertheless indicate that macroprudential policy shocks seem to affect credit in a symmetric way.

6.4 Alternative credit cycle definition

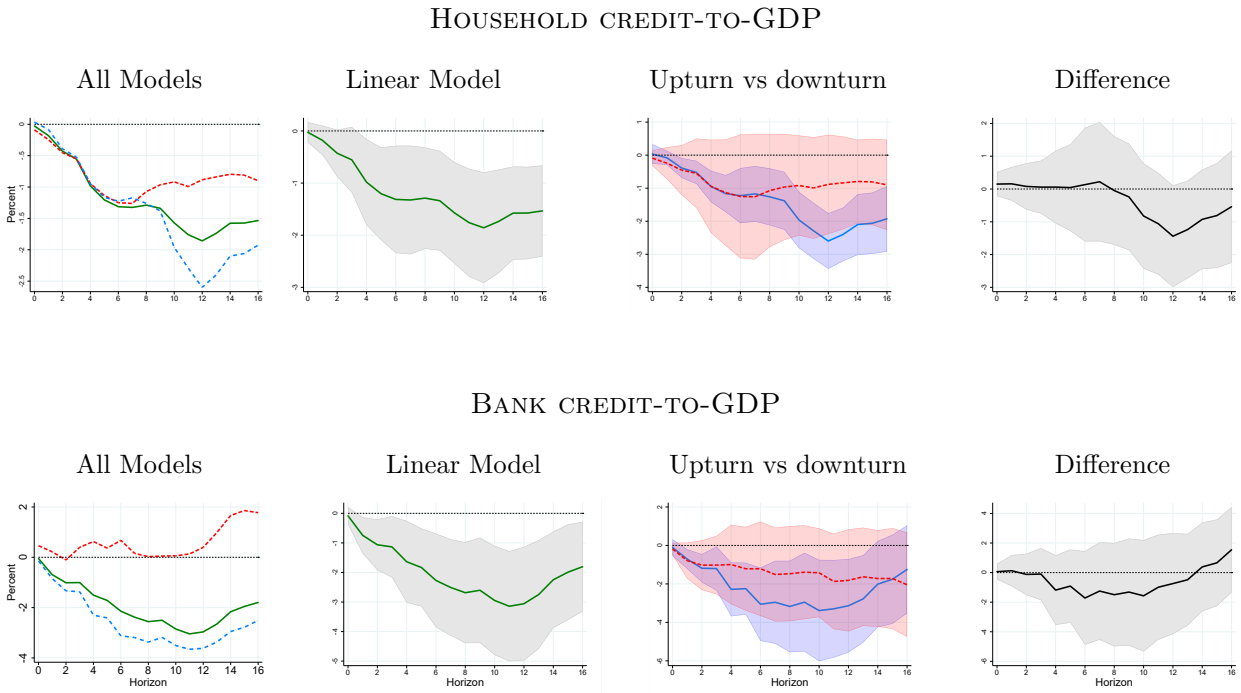
Our results might be sensitive to how we identify the credit cycle. An alternative method to define the credit cycle is to use the total credit-to-GDP ratio. The additional insight of using this ratio instead of the level of credit is that it puts debt in relation to income. We take the detrended series of the credit gap obtained by detrending total credit-to-GDP as before. Using these cyclical deviations from the trend, we identify phases of leveraging and deleveraging by applying again the Harding and Pagan (2002) algorithm to the detrended series, where we impose a minimum length of each phase of two quarters. The results can be seen in figure 10. The findings are qualitatively the same relative to the benchmark estimates of equation (2). Macroprudential policy seems to be more effective in leveraging (upturn) than in deleveraging (downturn) phases.

Figure 9: Robustness checks: Impulse response functions to macroprudential policy shock



Note: The solid red line and the red shaded area show the benchmark IRFs of the variables and their confidence band after a macroprudential policy shock. The dashed blue lines show the 90% confidence bands of the estimated effect of a macropudential shock in the respective robustness check. In the first row, the sample is restricted to data before 2008. In the second row, only data after 2007 is considered. The third row shows the robustness check for an alternative calculation of the shock. In the fourth and fifth row, the confidence bands for the model estimated in equation (3) are shown.

Figure 10: Robustness check: state-dependent impulse response functions to a macroprudential policy shock, leveraging vs. deleveraging based on detrended credit-to-GDP ratio



Note: In the first column, the point estimates of the linear model (solid green line) estimated with equation (1) and the upturn (blue dashed line) and the downturn states (red dotted line) estimated with equation (2) are shown together. The second column depicts the IRFs of the linear model while the third column shows the IRFs for the upturn (blue) and downturn (red) states together with their confidence bands. The fourth column gives the IRFs of the difference between the reactions in the upturn state and the downturn state, i.e. the difference between $\zeta^{A,h}$ and $\zeta^{B,h}$ and its 90% confidence band.

7 Conclusion

In this paper, we identify a novel set of macroprudential shocks for a consistent group of 13 EU countries using a narrative approach to infer the effects on total and sectoral credit evolutions. Based on these shocks, we find that tightening macroprudential tools not only reduce household credit-to-GDP and bank credit-to-GDP ratios but also curb the ratios' medium-term cyclical components. These findings are relevant as they demonstrate that macroprudential shocks can have sizeable and persistent effects on credit cycles in a group of advanced EU economies. We also provide more evidence to back up earlier findings of the existence of leakage effects following a macroprudential tightening whereby domestic bank credit in EU countries is to some extent substituted by non-bank and foreign credit. We further detect no significant reactions of GDP and prices after a macroprudential policy shock. Policymakers can therefore take our results as an indication that macroprudential tools in EU countries indeed had the desired effects in curbing the credit cycle for household credit an domestic bank credit. This not only in the short run (on impact) but also in the medium term. Total credit and credit to NFCs in contrast do not react significantly to a tightening macroprudential shock. Leakages to non-bank and foreign credit to NFCs might underlie this evolution.

Moreover, we find the effects of macroprudential policy to be state-dependent. While macroprudential policy actions are more effective during upturn and leverage phases of the credit cycle, there seems to be only small or no effects during downturn and deleveraging phases. These effects however do not seem to depend on business cycle phases. These findings are reassuring for policy makers as they indicate that macroprudential policy is effective when it should be.

Going forward, a detailed investigation of spillovers of macroprudential policy shocks between European countries and the disentanglement of announcement and enforcement effects seems warranted to look more closely into the transmission channels of macroprudential policy shocks within and outside Europe.

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Appendix A. Data

We deflated all relevant data using CPI taken from Eurostat (the HICP). We further seasonally adjusted all data using the X-13 ARIMA approach.

Total credit to the private non-financial sector Credit from all sectors to the private non-financial sector (BIS) quarterly data. All sectors mean domestic banks, all other sectors of the economy and non-residents. Credit covers the core debt, defined as loans, debt securities and currency & deposits.

Credit to non-financial corporations Credit from all sectors to private NFCs (BIS) quarterly data. All sectors mean domestic banks, all other sectors of the economy and non-residents. Credit covers the core debt, defined as loans, debt securities and currency & deposits.

Credit to households Credit from all sectors to households and (BIS) quarterly data. All sectors mean domestic banks, all other sectors of the economy and non-residents. Credit covers the core debt, defined as loans, debt securities and currency & deposits.

Domestic Bank credit Domestic bank credit to domestic private sector (BIS) quarterly data.

GDP Nominal GDP taken from Eurostat. GDP in domestic currency at current prices.

HICP Harmonized index of Consumer prices taken from the Statistical Data Warehouse of the ECB.

Policy rate For the Euro Area and the UK, we take the Wu and Xia (2016) shadow rate from the website of Cynthia Wu. For Denmark and Sweden, we use the respective policy rates.

Non-financial corporation balance sheet data We obtain all balance sheet information from the Quarterly Sector accounts (QSA) of the Statistical Data Warehouse of the ECB. The stock of overall loans (domestic or foreign) to domestic NFC is taken from the liability side of the aggregate NFC balance sheet for each country. The stock of domestic loans towards domestic NFC is obtained by looking at the asset side of the aggregate balance sheet of the total economy and taking the stock of loans towards domestic NFC. The stock of foreign loans is then just the difference between those

two balance sheet entries. We further obtain the total of domestic MFI and OFI loans granted to NFCs by looking at the respective aggregate asset sheet position. We further calculate the amount of domestic non-MFI by taking the stock of domestic loans to NFC and subtracting the stock of MFI loans to NFCs.

Table 2: Descriptive statistics

Variables	Obs	Mean	Std. Dev.	Min	Max	Source
Total credit-to-GDP ratio	819	171.95	50.42	68.52	330.93	BIS
Non-financial corporation credit-to-GDP ratio	819	104.09	32.53	49.08	229.60	BIS
Household credit-to-GDP ratio	819	67.90	26.39	19.44	139.27	BIS
Dom. bank credit-to-GDP ratio	819	101.26	32.30	42.63	199.30	BIS
Real GDP (logs)	819	12.21	1.04	10.58	13.80	Eurostat
HICP in logs	819	4.47	0.10	4.22	4.61	Eurostat
% of Stock of non MFI loans to NFCs	690	30.13	11.45	5.84	60.35	SDW (ECB)
% of Stock of OFI loans to NFCs	690	6.99	6.28	0.51	27.05	SDW (ECB)
% of Stock of foreign loans to NFCs	690	22.20	10.40	4.08	54.43	SDW (ECB)
% of Stock of MFI loans to NFCs	690	47.67	15.57	14.64	74.60	SDW(ECB)
Policy rate	819	1.92	2.08	-6.40	5.90	SDW (ECB)/ Cynthia Wu
Macroprudential policy actions						
Macroprudential shock	819	0.03	0.20	-1	1	MaPPED (ECB)
Announced but not enforced policy actions	819	0.08	0.31	-1	1	MaPPED (ECB)
Enforced but not announced policy actions	819	0.08	0.33	-1	1	MaPPED (ECB)

Appendix B. Tables and Figures

Table 3: Overview over the narrative reduction of policy actions in all 13 countries between 1999q1-2014q4.

Overall policy actions	282
- of which do not comply with timing restriction	213 of 282
- of which have no announcement date	64 of 282
- of which are non-mandatory and have no fine for non-compliance	31 of 282
- of which have a countercylical design	7 of 282
Total of policy actions complying with all narrative restrictions	55 of 282

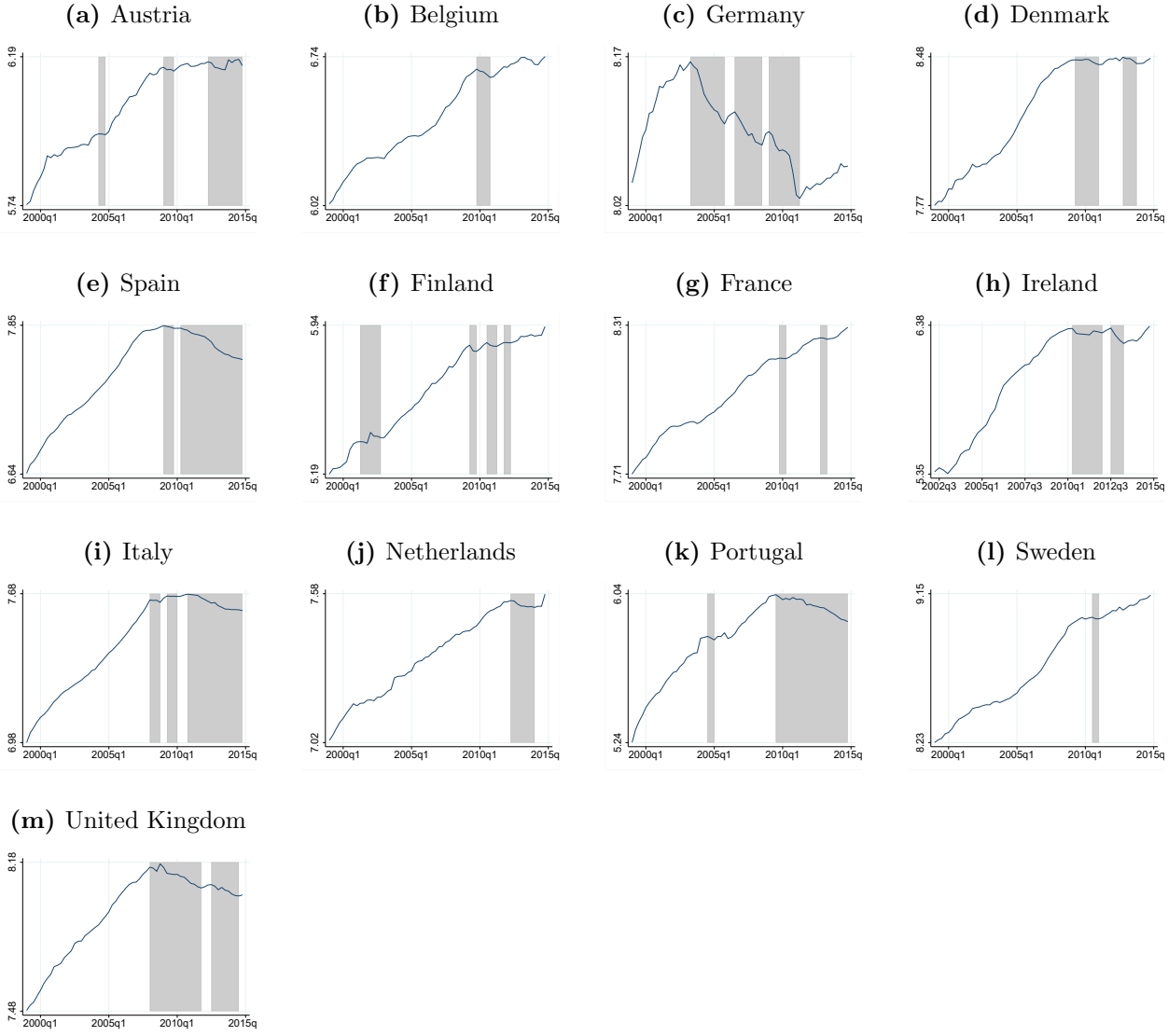
Note: The table shows the total amount of policy actions in the 13 countries between 1999q1 and 2014q4 and how many policy actions are excluded by each restriction. Some policy actions fail to comply with several of these restrictions.

Table 4: All identified Macroprudencial Policy actions

Country	Category	Subcategory	Direction	Period
AT	Risk weights	Risk weights on loans backed by commercial property	1	2006q4
BE	Minimum capital requirements	Tier 1 capital ratio	1	2013q2
BE	Minimum capital requirements	Common Equity Tier 1 capital ratio (CET1)	1	2013q2
BE	Risk weights	Risk weights on loans backed by residential property	1	2013q4
BE	Limits on large exposures and concentration	Single client exposure limits	-1	2011q4
BE	Limits on large exposures and concentration	Single client exposure limits	1	2011q4
BE	Limits on large exposures and concentration	Limits on qualified holdings outside the financial-sector	1	2007q2
BE	Limits on large exposures and concentration	Limits on qualified holdings outside the financial-sector	1	2007q2
DE	Minimum capital requirements	CAR	1	2010q4
DE	Other measures	Other regulatory restrictions on financial activities	1	2008q3
DE	Other measures	Other regulatory restrictions on financial activities	1	2010q2
DE	Other measures	Other regulatory restrictions on financial activities	1	2010q3
DE	Other measures	Other regulatory restrictions on financial activities	-1	2010q3
DK	Risk weights	Risk weights on loans backed by residential property	-1	2006q4
DK	Risk weights	Risk weights on loans backed by residential property	-1	2006q4
DK	Lending standards restrictions	Maturity and amortisation restrictions	-1	2003q4
DK	Limits on credit growth and volume	Asset-based reserve requirements	1	2010q2
DK	Limits on large exposures and concentration	Sector and market segment exposure limits	1	2010q2
DK	Limits on large exposures and concentration	Other exposure and concentration limits	1	2010q2
DK	LRLCMM	Other stable funding requirements incl. NSFR	1	2010q2
DK	LRLCMM	Liquidity ratios and deposit coverage ratios	1	2010q2
DK	LRLCMM	Other liquidity requirements	1	2003q2
ES	Minimum capital requirements	Core Tier 1 capital ratio	1	2011q1
ES	Capital buffers	Other capital surcharges and own funds requirements	1	2011q1
FI	Risk weights	Risk weights on loans backed by residential property	1	2000q2
FI	Loan-loss provisioning	Loan classification rules	1	2006q2
FI	LRLCMM	Other liquidity requirements	1	2010q4
FI	LRLCMM	Other liquidity requirements	1	2010q4
IE	Minimum capital requirements	rain ar CAR	1	2000q2
IE	Limits on large exposures and concentration	Single client exposure limits	1	2014q1
IE	Limits on large exposures and concentration	Intragroup exposure limits	1	2009q4
IE	Limits on large exposures and concentration	Funding concentration limits	1	2011q2
IE	Limits on large exposures and concentration	Funding concentration limits	-1	2011q2
IE	LRLCMM	Short-term liquidity coverage ratios incl. LCR	1	2009q2
IE	LRLCMM	Short-term liquidity coverage ratios incl. LCR	1	2009q2
IE	LRLCMM	Other liquidity requirements	1	2009q2
IT	LRLCMM	Other stable funding requirements incl. NSFR	-1	2006q1
NL	Limits on large exposures and concentration	Intragroup exposure limits	1	2009q4
PT	Risk weights	Risk weights on loans backed by residential property	1	2001q1
PT	Risk weights	Risk weights on loans backed by residential property	-1	2007q2
PT	Risk weights	Risk weights on loans backed by commercial property	-1	2001q1
PT	Loan-loss provisioning	General provisioning	1	1999q1
PT	Levy / Tax on financial institutions and activities	Tax on assets/liabilities	1	2010q4
PT	Limits on large exposures and concentration	Sector and market segment exposure limits	1	2010q4
PT	LRLCMM	Other liquidity requirements	1	2000q1
PT	LRLCMM	Other liquidity requirements	1	2009q3
PT	Other measures	Limits on deposit rates	1	2011q4
PT	Other measures	Limits on deposit rates	1	2012q2
SE	Risk weights	Risk weights on loans backed by residential property	1	2013q2
SE	Levy / Tax on financial institutions and activities	Tax on assets/liabilities	1	2009q4
SE	Limits on large exposures and concentration	Single client exposure limits	1	2011q1
SE	Limits on large exposures and concentration	Limits on qualified holdings outside the financial-sector	1	2004q1
SE	Limits on large exposures and concentration	Limits on qualified holdings outside the financial-sector	1	2004q1
UK	Risk weights	Risk weights on loans backed by commercial property	1	2014q4
UK	Leverage ratio	Leverage ratio	1	2013q4

Note: The overview only includes the macroprudential policy actions between 1999q1 and 2014q4 that comply with the narrative restrictions as discussed in section 3.2. Abbreviations: LRLCMM=Liquidity requirements and limits on currency and maturity mismatch. CAR= Capital Adequacy Rate. LCR= Liquidity Coverage Ratio. NSFR= Net Stable Funds Rate

Figure 11: Log of real total credit per country and phases of leveraging vs. deleveraging



Note: For each country, the blue line shows the log of real total credit and the grey areas depict deleveraging phases.