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Macroprudential Policies and Capital Controls Over Financial Cycles

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ABSTRACT: In this paper we assess the effectiveness of macroprudential policies and capital controls in supporting financial stability. We construct a large and granular dataset on prudential and capital flow management measures covering 53 countries during 1996-2016. Conditional on a credit boom, we study the impact of these policy measures on the probability of the credit boom ending in a bust. Our analysis suggests that macroprudential tools are effective from this perspective. If credit booms are accompanied by capital flow surges, in addition to macroprudential tools, capital controls on money market instruments including cross-border interbank lending tend to contribute to reducing the likelihood of a credit bust.

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WORKING PAPERS

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I. Introduction

The need to make the financial system more resilient and tame its inherent procyclicality vis-à-vis the real economy was one of the key lessons of the global financial crisis. In an effort to strengthen the financial sector, many policymakers have since implemented a new macroprudential policy approach that encompasses tools to limit systemic risk accumulation and to build buffers in the system during good times to minimize the impact of financial downturns on financial institutions, and hence the economy, once the cycle turns (IMF 2013). A growing body of literature on the effectiveness and calibration of macroprudential tools has emerged, contributing to our understanding of how to best adjust financial sector policies over the financial cycle.

Capital flows are one of the key drivers of financial cycles. Many episodes of financial upturns with strong credit and asset price growth have witnessed current account deteriorations amid strong private capital inflows, especially when intermediated by the banking system, and typically ending in financial busts (Martinez (2015); Dell'Ariccia et al. (2016)). Policymakers have often applied various capital flow management measures (CFM), referred to also as capital controls, to regulate extremely large inflows (capital surges) with the aim to prevent a possible overheating of the economy and to avoid an undue accumulation of vulnerabilities in the financial system. As such, certain capital control measures can indirectly serve a similar function as macroprudential tools, impacting traditional macroprudential intermediate targets such credit or asset price growth (IMF 2015).

In this paper, we assess the effectiveness of macroprudential tools and capital controls over the medium term by addressing the question whether they make the financial system more resilient. We differ from most other existing empirical studies on effectiveness that typically analyze the short-term (one to two year) effect of various policy tools on credit or asset price growth (Lim et al. (2011); Cerutti et al. (2017a, 2017b); Alam et al. (2019)). These studies address one of the two traditional objectives of macroprudential policy – to moderate the financial cycle by taming potentially excessive credit and asset price growth in financial upturns that has been often associated with the build-up of system-wide risks (CGFS 2010) – but have less to say about the second objective – to make the financial system more resilient by creating buffers at the level of lenders and borrowers – which is inherently more difficult to test.

We adopt an approach in which we zoom in on credit boom episodes only – i.e., periods in which systemic risk typically accumulates – and investigate whether using macroprudential measures and capital controls decrease the probability of a credit boom ending in a credit bust (credit crunch). While countries may not always be successful in moderating credit booms with macroprudential policy or capital controls, they may be able to create buffers and minimize vulnerabilities in the system – i.e., make the system resilient – limiting the extent to which a turn in the credit cycle impairs financial intermediation.[1]

To this end, by combining several existing datasets we construct a unique granular cross-country database on macroprudential measures (MMs) and capital controls (CCs) covering 53 countries and spanning over two

[1] A similar approach has been pursued by Dell'Ariccia et al. (2016), though they differ in the data sample used, the definition of booms and busts, and our inclusion of capital controls.

decades from 1996 to 2016. Our identification strategy requires a forward-looking window of at least three years to be able to find out whether a credit boom ended in a bust. Thus, while using credit data to identify booms and busts up to 2019, our analysis of policy interventions extends only to 2016.

Our main findings are as follows. First, we find that macroprudential policy is effective to help countries experiencing credit booms avoid ending in a credit bust. In terms of the concrete macroprudential policy tools, we find that capital-based tools, borrower-based tools such as loan-to-value (LTV) and debt-service-to-income (DSTI) caps, and reserve requirements have a significant effect. If credit booms are accompanied by capital flow surges, in addition to capital-based macroprudential tools, capital controls on money market instruments, including short-term cross-border interbank lending, tend to reduce the likelihood of a bust.

The paper is organized as follows: after the literature review (Section II), we describe the dataset (Section III) and present some stylized facts on the use of macroprudential policies and capital controls over time (Section IV). Section V describes how we identified credit and capital cycles, and Section VI discusses the use of macroprudential policies and capital controls over these cycles. Section VII presents our empirical findings regarding the effectiveness of macroprudential and capital control measures during credit booms, and Section VIII focuses on a subset of these booms, namely those that were accompanied by capital surges. Section IX concludes.

II. Review of Related Literature

Empirical research on the effectiveness of macroprudential policies has been growing rapidly over the past decade. Most studies apply panel regressions over a large number of countries to test the impact of various prudential tools on credit and asset price dynamics over the short-term. Given that most macroprudential interventions before the global financial crisis were implemented in emerging markets, especially in Central and Eastern Europe (CEE), Asia, and Latin America, existing studies typically focus on these regions. With a few exceptions mentioned below, capital controls were not included among the explanatory variables, and if so, not in the granular detail we present in this paper. As noted by Galati and Moessner (2018), some robust findings on the effects of macroprudential tools are emerging.

For example, early contributions in this literature documenting the impact of selected prudential policies on credit and house price growth include Hilbers et al. (2005), who focused on the rapid growth of private sector credit in 18 CEE countries, and Borio and Shim (2007), who studied18 mostly emerging markets in Europe and Asia. Lim et al. (2011) covered 49 countries over 2000-2010 at annual frequency, focusing on the effect of 10 types of macroprudential policy instruments on procyclicality of credit and three additional systemic risk measures (loan-to-deposit ratio, external funding of banks, and leverage). The data on policy interventions were collected in an IMF survey in early 2011. They found, among others, that caps on LTV and DSTI, ceilings on credit growth, reserve requirements, and countercyclical provisioning reduce the procyclicality between credit and GDP growth.

Tovar, Garcia-Escribano, and Martin (2012) showed that macroprudential policy, particularly reserve requirements, had a moderate but transitory impact on private bank credit growth in six Latin American economies, while Vandenbussche et al. (2012) found that certain types of macroprudential policies, including increased capital adequacy ratios and non-standard liquidity measures, influenced house price inflation in CEE countries. Claessens et al. (2013) used individual bank data in both advanced and emerging market economies, using a sample of about 2,300 banks in 48 countries and macroprudential policy measures collected in Lim et al. (2011), to show that policy measures such as maximum LTV and DSTI ratios and limits on foreign currency lending were effective in reducing leverage, assets, and non-core to core liabilities growth during booms.

Kuttner and Shim (2013) used a novel BIS database to analyze the impact of prudential (non-interest rate) policies in 57 countries over 1980-2011, building on an earlier version of the dataset in Shim et al. (2013). They found that housing credit responds strongly to limits on DSTI and that house prices are strongly impacted by house-related tax measures. Gersl and Jasova (2016) analyzed policy measures to curb quarterly bank credit growth in the private sector in 11 CEE countries in the pre-crisis period 2003–2007 using their own survey conducted in 2010. Their results indicate that asset classification and provisioning rules as well as loan eligibility criteria, such as caps on LTV and DSTI, have been effective in taming bank credit growth, especially if applied in combination.

Cerutti et al. (2017a) used a 2013 IMF survey to create an annual dataset of macroprudential policies in 119 countries over 2000-2013. This dataset records, for each year, whether different types of policies were in place without capturing if and when the instrument was adjusted. They found that a higher overall macroprudential policy index (i.e., more tools in place) is correlated with lower credit growth, especially in emerging market economies. They also used an index of financial openness as a proxy for capital controls, finding that the effect of macroprudential policies is lower in more open economies. In a follow-up study, Cerutti et al. (2017b) created a new database at quarterly frequency for 64 countries over 2000-2016 with a much higher level of detail when classifying the prudential instruments, but do not present any analysis of the effectiveness.

Bruno et al. (2017) used a Bank for International Settlements (BIS) macroprudential policy database presented in Shim et al. (2013) and a database of capital controls to study the effects of these policies on credit, banking flows, and bond flows in 12 Asian economies. They found that monetary policy, banking inflow controls, and macroprudential policies were used as complements in Asia from 2004 to 2013 and that bank inflow controls reduced the growth of bank inflows from 2004 to 2007, but not recently. Akinci and Olmstead-Rumsey (2018) covered 57 countries over 2000-2013 at quarterly frequency, testing the effectiveness of seven categories of macroprudential tools in curbing credit growth and house price appreciation. They constructed a new database of macroprudential tools based on data from the 2011 IMF survey presented in Lim et al. (2011) combined with BIS data presented by Shim et al. (2013); the 2013 IMF survey (Global Macroprudential Policy Instruments GMPI); the databases constructed by Cerutti et al. (2017a, 2017b); and national sources. Their results suggest that provision requirements, LTV caps, and risk weights on mortgages have an impact on overall bank credit growth, while only targeted housing-related policies such as caps on LTV and DSTI constrain housing credit growth and house price appreciation. For a subset of 19 emerging market countries, they also included an overall capital flow restriction index constructed by Ahmed and Zlate (2014), which uses information from the IMF's Annual Report on Exchange Arrangements and Exchange Restrictions (AREAER) database. However, this index was not found to be statistically significant.

Fendoğlu (2017) constructed a database at quarterly frequency using the IMF and BIS datasets for 18 emerging markets over 2000-2013 to study the impact of macroprudential tools on credit cycles. He found that borrower-based measures and reserve requirements are particularly effective. Carreras et al. (2018) assessed the impact of macroprudential policy interventions on house prices and household credit growth in 19 mostly advanced (OECD) countries over 1990-2014, using separately the datasets from the IMF (Cerutti et al. (2017a, 2017b)) and the BIS (Shim et al. (2013)). They found that most of the traditional prudential tools do have a significant impact on credit and house prices. Using the datasets from the IMF, Olszak et al. (2019) found that macroprudential policies – especially caps on LTV and DTI – reduce the procyclical impact of the capital ratio on bank lending.

Poghosyan (2019) tested the effectiveness of lending restrictions measures, such as caps on LTV and DSTI, in 28 EU countries over 1990-2018, using a newly constructed database by the European Central Bank (Budnik and Kleibl (2018)). The measures are shown to be effective in decelerating credit and house price growth. Meuleman and Vander Vennet (2020) used the same database to investigate the impact of macroprudential policy on measures of banks' systemic risk constructed from stock prices. They found that announcements of macroprudential policy actions have a downward effect on bank systemic risk.

Alam et al. (2019) constructed a new integrated Macroprudential Policy (iMaPP) database covering 134 countries at monthly frequency over 1990-2016 by merging some of the previously constructed databases mentioned above with a new regular Annual Macroprudential Policy Survey conducted by the IMF since 2017. Their results confirm earlier findings that borrower-based measures do have an impact on credit growth while the effect on house prices is weaker.

The methodological approach adopted by us is closest to the study by Dell'Ariccia et al. (2016), who analyzed credit booms and what type of credit booms end up in a bust. They constructed a proxy for macroprudential policy using a mixture of prudential and capital control measures from the IMF's Annual Report on Exchange Arrangement and Exchange Restrictions dataset combined with additional data sources and test whether its inclusion has an effect on the probability of ending in a bust. Belkhir et al. (2020) have a similar focus as our paper (albeit with a different methodology), exploring whether macroprudential policy is conducive to a lower incidence of systemic banking crises. They find that while macroprudential policies exert a direct stabilizing effect, they also have an indirect destabilizing effect, which works through the depressing of economic growth. Nier et al. (2020) show that tightening of macroprudential policies dampens the positive link between the appreciation of the local exchange rate and the subsequent increase in the domestic credit gap.

Our analysis also relates to the literature that studies the effect of capital controls on capital flows as well as on whether capital controls can decrease the risks of financial instability stemming from periods of strong capital inflows (surges) that typically fuel credit growth. Binici et al. (2010), using a novel database of capital controls based on the IMF's Annual Report on Exchange Arrangements and Exchange Restrictions (AREAER), did not find a significant effect of capital controls on capital inflows. Fernández et al. (2015)(2) find that capital controls are remarkably acyclical, with booms and busts in aggregate activity associated with virtually no movements in capital controls. Some studies, such as Habermeier et al. (2011) and Zhang and Zoli (2016), look at the impact of capital controls on credit growth, generally finding little evidence of a direct impact. This is consistent with Qureshi et al. (2011), who found evidence that domestic prudential measures (e.g., LTV, credit growth limits and reserve requirements) are better tools to be used to restrain the intensity of aggregate credit growth while capital controls can serve to decrease other types of vulnerabilities in the financial system such as the share of FX loans in total loans or excessive reliance on external debt. They also showed that both prudential measures and capital controls at place just before the Global Financial Crisis 2008/2009 muted the impact of the crisis on the real economy as measured by the decline in GDP in 2008-2009 compared to the country's average growth over 2003–07.

More recently, new studies have been published on the joint effects of macroprudential tools and capital controls. IMF 2017 discusses the complementarity between both policies in minimizing risks from capital flows. Bergant et al. (2020) provide evidence on the effectiveness of macroprudential regulation in dampening the effects of global financial shocks (among which capital surges) on economic activity, differentiating across macroprudential measures. They compare the benefits of macroprudential regulation to the ones of capital controls, finding no evidence for the latter. Brandao-Marques et al. (2020) evaluate net benefits of macroprudential policies and capital controls (in addition to monetary, fiscal, and exchange rate policies) using a quantile regression approach. Their results show that tightening macroprudential policy dampens downside risks to growth stemming from loose financial conditions while tightening capital controls has small net benefits. Das et al. (2022) test whether a preemptive use of macroprudential tools and capital flow management measures reduced emerging markets and developing countries' external finance premia during the "shock" periods of Taper Tantrum in May 2013 and Covid-19 Crisis in March 2020. They show that capital controls on inflows helped reduce the external finance premium during those two risk-off shocks while the effect of macroprudential tools is not clear.

III. Data Construction

Our dataset is at an annual frequency from 1996-2016 and covers 53 countries (27 emerging markets and 26 advanced economies) for which both prudential measures and capital controls are available (see Table A1).[2]

Prudential measures

Our dataset on the use of prudential policy draws on information from five different databases, which we use to compile a comprehensive set of macroprudential measures and to track policy loosening (-1) (i.e., less stringent macroprudential requirements); tightening (1); and unchanged policy (0). The five sources of databases include Shim et al. (2013), Akinci and Olmstead-Rumsey (2018), Cerutti et al. (2017a), Cerutti et al. (2017b) and Alam et al. (2019). While Alam et al. (2019) constitutes the primary source of data on (macro)prudential policies, we still cross-check it against the additional available databases for potential inconsistencies across them. For those identified discrepancies, we further verified the data using national sources. Four of the datasets allow to capture both macroprudential policy tightening and loosening. Cerutti et al.'s (2017a) database is the exception which captures the existence of policy instruments from the time of their introduction until they are discontinued (if applicable), but it does not reflect any changes (loosening or tightening) of measures already in place.

We perform the following three steps to define our macroprudential measures. First, we convert the data from quarterly and monthly datasets to annual frequency, as the underlying datasets have various frequencies. Second, we redefine the variables from Cerutti et al. (2017a) by first-differencing them because they show stock levels rather than policy changes as in the other datasets. Third, as the five databases cover a number of different measures of prudential policy, we group them into the following 8 categories: reserve and liquidity requirements (RR); limits on credit growth (CG); loan-to-value caps (LTV); debt-to-income or debt service-to-income ratios (DTI); capital instruments (CAP); provisioning (PR); exposure and concentration limits, including on interbank exposure (EXP); and other measures, including taxes, consumer loans measures (OTH) (see Table A2 for detailed definitions). Thus, for each category, the indicators can take on the values of {-1,0,1}.[3]

The mapping of the measures from the aforementioned databases to these categories is detailed in Table A3 in the Appendix. In several cases, we merged more detailed breakdowns into broader categories. For instance, Shim et al. (2013) report one macroprudential policy measure on reserve requirements of banks and another one on liquidity requirements. We merge these two into one measure (RR) by summing up the values from the two measures if there is data available for at least one of the variables. A positive sign of the sum indicates a policy tightening (coded as 1), a negative one stands for policy loosening (coded as -1) and 0 implies that there

[2] We have to perform our analysis at annual frequency because the capital controls dataset by Fernández et al. (2015)(1) is at annual frequency. In terms of geographical coverage, our final database covers Asia-Pacific (13 countries), Europe (27), North & Latin America (9), and Middle East and Africa (4).

[3] In this paper, we use the term "measure" to reflect a category (and thus discuss the eight alternative measures) even if in practice there will generally be more than one tool within one category.

is no policy change for a given measure and time period, resulting in a non-continuous dummy variable with the values {-1,0,1}. A similar procedure is applied to the rest of the variables and databases.

Finally, we create an overall MPM (Macro-Prudential Measures) index as a sum of the values over the eight categories. Thus, this index can take on discrete values between -8 and +8 and captures net tightening in a year: for example, if there was tightening in three categories (+3) and loosening in one (-1) in a given year, the net tightening will sum to 2.

Similar to other studies in this area, the usual limitations apply also to our macroprudential policy dataset. First, the intensity of use of each instrument is not fully captured, so the magnitudes of tightening at different times or by different countries can differ. Second, our dataset allows us to control for changes in policy instruments, but it is not able to control for the overall level of restrictions already in place. Third, by summing up across subcategories, the overall MPM does not capture that some categories may have higher economic impact than others.

Capital controls

Our capital control measures are based on the dataset compiled by Fernández et al. (2015)(1),[4] which allows to track controls on capital inflows and outflows across separate asset categories over 1996 to 2016. The dataset is based on *de jure* information from the IMF's Annual Report on Exchange Arrangements and Exchange Restrictions (AREAER). Presence of a capital control restriction is coded as 1 while 0 implies an absence of restrictions. The dataset distinguishes restrictions on capital inflows and outflows transactions. In this paper we will concentrate only on capital inflows as these are more relevant for fueling credit growth. Thus, a restriction on capital inflows within each separate instrument type implies either a restriction on purchases locally by nonresidents (plbn) or a restriction on sales or issuance abroad by residents (siar).[5] To make the capital controls stock measure comparable with our flow dataset on macroprudential measures, we first-difference the individual inflow measures in the Fernández et al. (2015)(1) database. This allows us to capture both policy tightening and loosening. We recode the resulting flow equivalents into a {-1,0,1} dummy to denote a tightening (1) if the difference is positive; no change (0) if it is zero; and a policy loosening (-1) if the difference is negative.

Our focus is on inflow measures, and as the data disaggregation in Fernandez et al. varies by asset category, we proceed as follows. The Fernandez et al. dataset contains capital control measures for 10 asset categories: (1) equity, shares or other securities of a participating nature (eq); (2) bonds or other debt securities (bo) which have an original maturity longer than one year; (3) money market instruments (mm) which cover short-term securities and short-term interbank cross-border lending; (4) collective investment securities such as mutual funds and investment trusts (ci); (5) derivatives (de) which includes operations in rights, warrants, swaps of

[4] The latest version of the dataset can be found at http://www.columbia.edu/~mu2166/fkrsu/.

[5] In general, there are likely to be indirect effects: stricter outflow controls might also reduce inflows as reversing the investment becomes more costly. We focus here only on the direct impact of inflow controls on inflows.

bonds and other debt securities etc.; (6) financial credit and credit other than commercial credit (fc); (7) commercial credits for operations directly linked with international trade transactions or with the rendering of international services (cc); (8) guarantees, sureties and financial back-up facilities (gs); (9) direct investment (di); and (10) real estate transactions (re).[6]

For the first five asset categories listed above, two measures on inflows are provided in the dataset: on purchase locally by a non-resident and sale or issue abroad by residents. We take an average of these two measures on each of these five asset categories of capital inflows (e.g., bond purchase by non-residents and bond issuance abroad by residents) and recode it into a {-1,0,1} dummy. If this average is positive the dummy gets a value of 1, implying tightening, 0 yields a dummy being equal to 0 (or no policy action) and, finally, a negative average corresponds to policy loosening or a dummy taking on a value of -1. As a third and final step, we merge these 10 categories into 8 instrument types. Namely, controls on commercial (cc) and financial credit (fc) categories are merged into one instrument. If the cumulative impact of the two instruments is negative, we count it as a policy loosening, while a positive sum of the two instruments is recorded as tightening. Similarly, we combine guarantees (gs) with derivatives (de). Thus, we obtain dummies for each of the 8 instrument categories that show whether there were capital controls introduced or removed in each given category. The mapping of the instruments can be found in Table A4 in the Appendix.

Similar to the macroprudential measures, we construct an overall capital controls index (CCs in inflows) by summing up the individual categories. The overall CC index again can range from -8 to +8.

Macroeconomic variables

To answer the research question on the efficiency of macroprudential measures on financial stability, we control for several macroeconomic characteristics of the countries covered in the analysis. In particular, for data on credit to the private sector we rely on the IMF's International Financial Statistics (IFS) Database, which is extended using data from Haver Analytics where needed.

For data on capital flows, we rely on the IMF's Financial Flows Analytics (FFA) database. In particular, we use gross private (non-official) capital inflows and debt creating inflows. Non-official private capital inflows are obtained by subtracting other inflows to the official sector (other liabilities of central bank, government, and special drawing rights) from total capital inflows. Debt-creating inflows are the sum of portfolio debt inflows and other types of flows into the non-official sector, which are typically dominated by banking flows but also include non-financial corporate debt.

We further collect data on real GDP growth and nominal GDP levels from the IMF's World Economic Outlook Database (WEO).

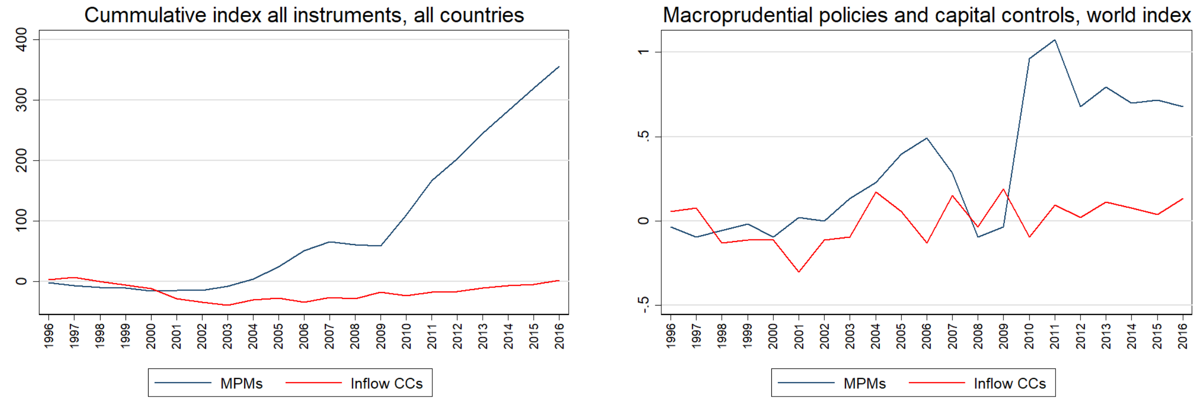
[6] For more detailed information on classification see Fernández et al. (2015)(1) and Schindler (2009)

IV. Macroprudential and Capital Control Measures Over Time

The evolution of both the MPM and CC indices, as shown in Figure 1, are in line with the common understanding of the financial-sector policy developments. During the late 1990s, continued liberalization in the financial sector and the capital account are reflected in a gradual net loosening of both macroprudential (which are to be understood as more general “prudential” policies, as the term “macroprudential policies” was not yet used in the 1990s) and capital flow management policies across countries. However, during the 2000s the development of the two policies diverged - the removal of restrictions on inflows of foreign capital into domestic economies continued up until the global financial crisis, while macroprudential policies tightened as authorities started to respond to the accumulation of vulnerabilities during the financial upturn phase, especially between 2002 and 2007 (Borio and Shim (2007)). Also, the Asian financial crisis of the late 1990s (and additional crises that followed) was an impetus for many countries, especially in that region, to introduce macroprudential measures from early 2000s. During the global financial crisis of 2008/2009 macroprudential policies loosened across our sample while capital controls remained largely unchanged.

After the global financial crisis, in net terms, both policies started to tighten again. The steep increase in the use of macroprudential measures was driven especially by the implementation of a new macroprudential framework in many countries to build up an arsenal of policy tools to tame financial cycles and prevent systemic risk accumulation in the future (IMF 2013). Moreover, some, mainly advanced, countries became worried by the developments in mortgage lending and housing markets in the post-crisis period in an environment of very accommodative monetary policies and started to use macroprudential tools more actively. In addition, existing financial regulation was tightened to support financial stability, in part, by implementing the new Basel III. Given the quite volatile post-crisis capital flows, many countries in our sample have gone back to using capital inflow controls more frequently in line with the new policy consensus on the use of capital controls to prevent accumulation of vulnerabilities during surges and mute capital flow reversals (IMF 2012).

Figure 1. Dynamics of Macroprudential Policies and Capital Control Measures

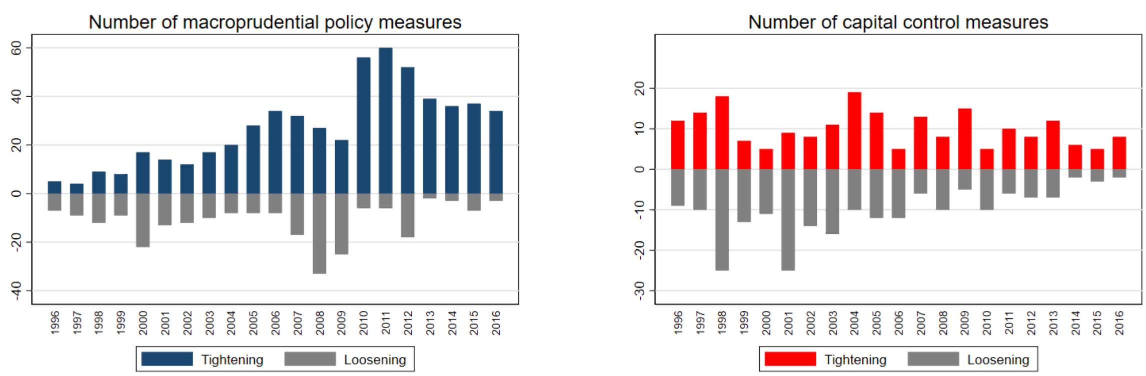


Source: MPMs and CCs datasets

Note: The left panel shows the cumulative level of MPM and CC indices in net terms across all countries in our sample for which the index is available. The right panel shows the average net tightening in each year across all countries, with the value of the respective index for each year is constructed as a sum across all countries divided by the number of countries.

The trends described above hide substantial heterogeneity across countries and across measures used. Figure 2 shows the disaggregated picture illustrating that policy tightening in some countries or categories went hand in hand with loosening in others. This holds for both types of policies but is more pronounced in the area of capital controls.

Figure 2. Policy Tightening vs. Loosening



Source: MPMs and CCs datasets

Note: The tightening (loosening) column is created by summing the values for all categories of the given policy across all countries where tightening (loosening) occurred in the given year

In terms of the individual macroprudential measures (categories), reserve requirements are the most actively used instrument, as measured by the number of times RR policy was tightened or loosened, followed by capital-based tools and LTV caps (Table 1). Explicit limits on credit growth appear to be the least actively used macroprudential measure in our sample.

Table 1. Use of Macroprudential Policy by Instrument, 1996-2016

	RR	CG	LTV	DTI	CAP	PR	EXP	OTH	All
-1	115	3	36	10	14	10	7	52	247
0	888	1086	993	1056	920	1061	815	1002	7821
1	110	24	84	47	179	42	58	59	603
Average N of times used	**4.2**	**0.5**	**2.3**	**1.1**	**3.6**	**1.0**	**1.3**	**2.1**	

Source: MPMs dataset

Note: The table shows the number of tightening, loosening or no action across all countries and years. The Average N of times used row shows how often each instrument was used (tightened or loosened) on average per country during the observed period (number of actions (both loosening and widening)/number of countries where data on the instrument is available).

Turning to the instruments that make up the controls on capital inflows, overall, we see considerably less policy activity over 1996-2016 compared to macroprudential measures (Table 2). The total number of policy changes is around half of those in the macroprudential policy area. Loosenings and tightenings are roughly balanced in total as well as across the individual categories of capital controls. Controls on derivatives are the most commonly used instruments, followed by capital controls on short-term money market and equity flows.

Table 2. Use of Capital Inflow Controls by Instrument, 1996-2016

	eq	bo	mm	ci	de	ccfc	di	re	All
-1	29	24	33	34	42	28	15	13	218
0	1051	941	1041	1046	1023	1063	1078	1075	8318
1	31	28	35	28	39	20	20	19	220
Average N of times used	**1.1**	**1.0**	**1.3**	**1.2**	**1.5**	**0.9**	**0.7**	**0.6**	

Source: CCs dataset

Note: The table shows the number of tightening, loosening or no action across all countries and years. "Average N of times used" expresses how often each instrument was used (tightened or loosened) on average per country during the observed period (number of actions (both loosening and widening)/number of countries where data on the instrument is available).

V. Identifying Credit and Capital Cycles

The existing literature applies various methodologies to measure credit cycles and identify periods of strong (credit booms) and weak credit growth (credit crunches or busts), typically combining a detrending method and selecting (arbitrary) thresholds. For example, Mendoza and Terrones (2012) or Arena et al. (2015) declare an episode a credit boom if the (log of) real credit per capita exceeds its long-run trend (estimated ex post with a two-sided Hodrick-Prescott (HP) filter) by a multiple (1.65) of the (country-specific) standard deviation. In contrast, Dell'Ariccia et al. (2012) rely on using information available to policymakers in real time, identifying those episodes as booms if the credit-to-GDP ratio in each country is greater than its rolling 10-year cubic trend by 1.5 times its standard deviation (combined with additional criteria such as minimum credit growth rate). These methods, however, require a sufficient number of years to estimate the trends reliably and those relying on filters suffer from the end-point bias, giving different signals in real time compared to the ex-post assessment.

In our analysis, we opt for a simple identification mechanism that relies on changes in the credit-to-GDP ratio using data available to policy makers in real time, a method often used in IMF surveillance (IMF GFSR (2011)). We identify an episode as a credit boom if

- it lasts at least 2 years;
- at least in one year, the change of the credit-to-GDP ratio is larger than 3 ppts; and
- in all other years of the boom, the change of the credit-to-GDP ratio is larger than 2 ppts.

We make two further adjustments to this definition. First, we exclude years in which an increase in credit to GDP of more than 2 ppts was driven by the contraction of the denominator, i.e., by economic recession (defined as an annual decline in the real GDP growth by at least 1%). Second, we combine two adjacent boom periods into one if there is no more than a one-year gap between them and if (nominal) credit growth in the gap year is positive. Moreover, to define credit booms we use macroeconomic and credit data starting from 1990 (whenever available) despite our policy tools database starting only in 1996.

Similarly, we identify an episode as a credit bust if

- it lasts at least 2 years;
- at least in one year, the change of the credit-to-GDP ratio is less than -3 ppts; and
- in all other years of the credit bust, the change of the credit-to-GDP ratio is less than -1 ppts.

We merge two busts with no more than a one-year gap into one if (nominal) credit growth in that year is negative.

The thresholds chosen for identification of credit booms are similar to those frequently used in financial stability assessments (e.g., by the IMF). We aim to identify episodes where credit is growing more rapidly than can be deemed sustainable and would typically start to trigger a policy discussion and, eventually, a policy action. We do target tails of the distribution of the changes in the credit-to-GDP ratio, but not too extreme ones - our 3 ppts threshold corresponds roughly to the 70^{th} percentile of the distribution. Even if in many cases this definition does not necessarily imply a “boom” in its classical meaning of an extraordinarily large credit expansion, we - for simplicity - will henceforth call these episodes credit booms.

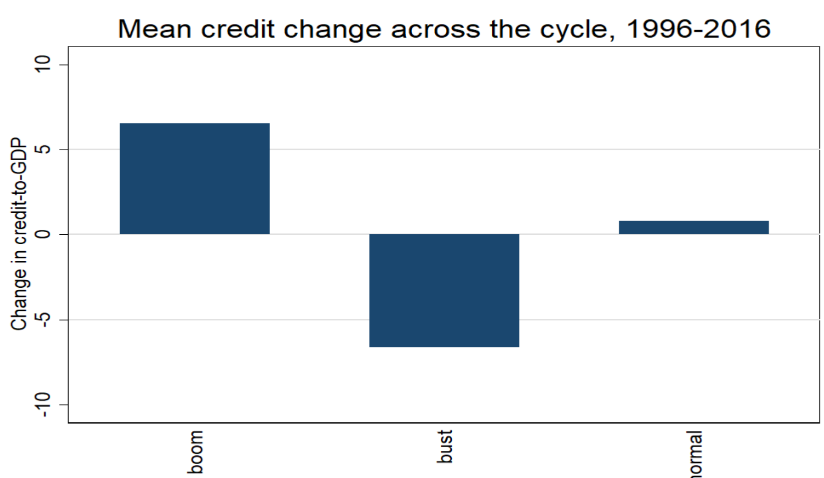
The thresholds used in the definition of busts differ from those for the booms to reflect the different pattern of credit busts, namely, a steep decline followed by another year or two of subdued performance, compared to a continuous credit expansion in booms. Also, the -3 ppts threshold is close to the 15^{th} percentile, thus our definitions of bust episodes are more extreme cases relative to booms and represent realizations further out in the distribution tails, capturing true "busts."[7]

To investigate whether a credit boom ended in a bust, we allow for a window of three years after a boom ended within which a bust can occur. This creates a particular challenge for the end-year of our sample, as there are several countries in a credit boom in 2016. We proceed as follows: as we have at our disposal credit data until end-2019, we identify booms only until 2016 and then assess whether a credit bust has followed a boom or not based on the 2017-2019 credit data. We remove those episodes where it is impossible to identify if the boom ends in a bust or not within our sample, i.e., we do not include the booms that continued beyond 2016.

Our identification strategy leaves us with 72 boom episodes identified over 1996-2016 for the 53 countries covered in our analysis. Out of the 72 identified booms, 26 ended in a bust within 3 years following the boom episode (i.e., about 36%). This finding is in line with Dell'Ariccia et al. (2016) who find that a third of credit booms end in a financial crisis.

The average duration of a credit boom in our sample is 7 years (min 2 years, max 14 years) and that of a credit bust 4 years (min 2 years, max 10 years). About three quarters of credit booms are between 2 and 11 years, while three quarters of busts last 2-5 years. Despite our asymmetric definitions of booms and busts, the average absolute annual change in credit to GDP is in both cases around 7 percentage points. In normal times, the annual change of credit-to-GDP ratios is about 1 percentage point (Figure 3).

Figure 3. Change in credit to GDP



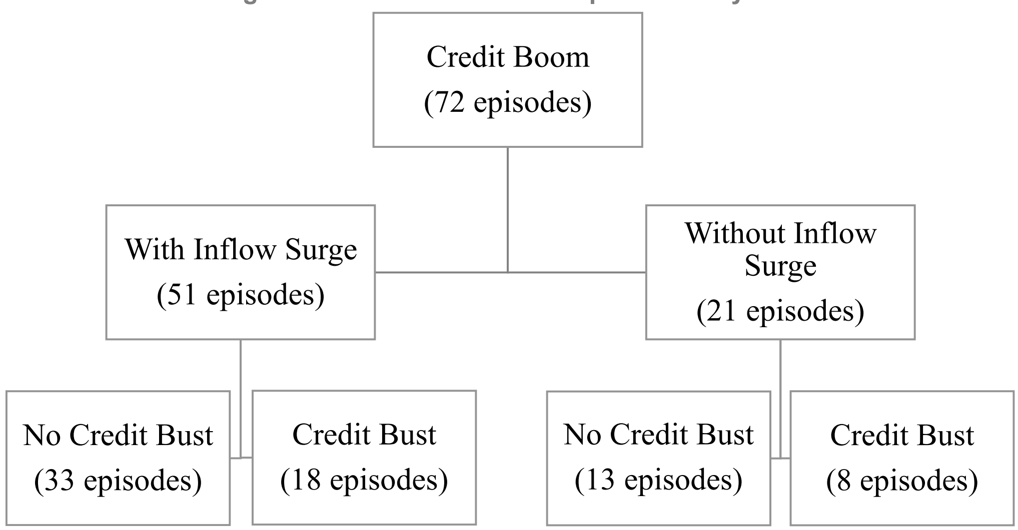
Sources: IFS, Haver Analytics

[7] As a robustness check, we also applied a stricter definition. In addition to lasting at least 2 years, a credit boom needs to see an increase in the credit-to-GDP ratio of more than 5 ppts in one year and 3 ppts in the other years. During a credit bust, the credit-to-GDP ratio needs to decline by more than 5 ppts in at least one year and more than 2 ppts in the other years.

Strong capital inflows often fuel credit booms. To account for this in our analysis, we define periods of strong capital inflows (surges) as episodes in which the difference of the gross private capital inflow-to-GDP ratio exceeds its 5-year rolling mean by one standard deviation calculated on that 5-year rolling window, following the methodology applied in Forbes and Warnock (2012).[8]

Unlike credit boom episodes, we do not restrict the number of years for which the condition needs to hold as capital flow surges can often be short-lived. This leaves us with over 170 episodes of surges in private capital inflows. The average duration of our surges is slightly below 2 years. Of the 72 credit boom episodes defined earlier, 51 coincide with a surge in private capital inflows within the first three years of the boom. We use this subsample in the last part of our analysis to track the effectiveness of macroprudential tools and capital controls during these episodes (see Figure 4).

Figure 4. Credit and Private Capital Flow Cycles



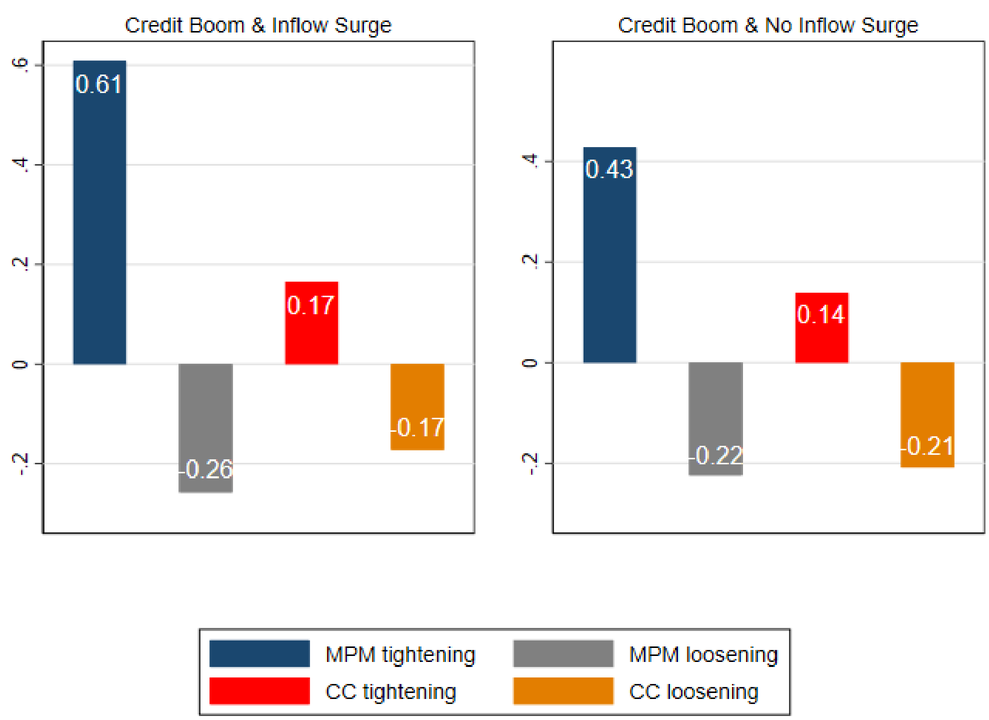
VI. Macroprudential and Capital Control Measures over Credit and Capital Cycle

The analysis of the use of macroprudential and capital control policies during boom years reveals some interesting findings (Figure 5). First, in line with expectations, macroprudential policy is typically applied

[8] Alternatively, Ghosh et al. (2012) set the threshold on capital inflows at the top 30th percentile for a given country's inflow, provided that it also falls in the top 30th percentile for the entire sample. Given our aim to identify surges in real-time rather than ex post, this approach is less suitable for our purposes.

countercyclically, i.e., it tends to be tightened during credit booms, although the sample also contains a few cases of no action or, in fact, loosening. Out of the 72 boom episodes identified, in nearly half of the cases (35) there was a MPM tightening in net terms, while during 24 boom episodes no macroprudential policy action was taken and in 13 a net loosening was recorded. However, the net loosening recorded for those 13 booms masks a certain heterogeneity in policy actions: in many of those cases, the authorities did tighten some measures but relaxed others. Second, somewhat tighter macroprudential policies are on average implemented in years when a credit boom coincides with a capital surge.

Figure 5. Use of Macroprudential and Capital Control Measures During Credit Booms, 1996-2016



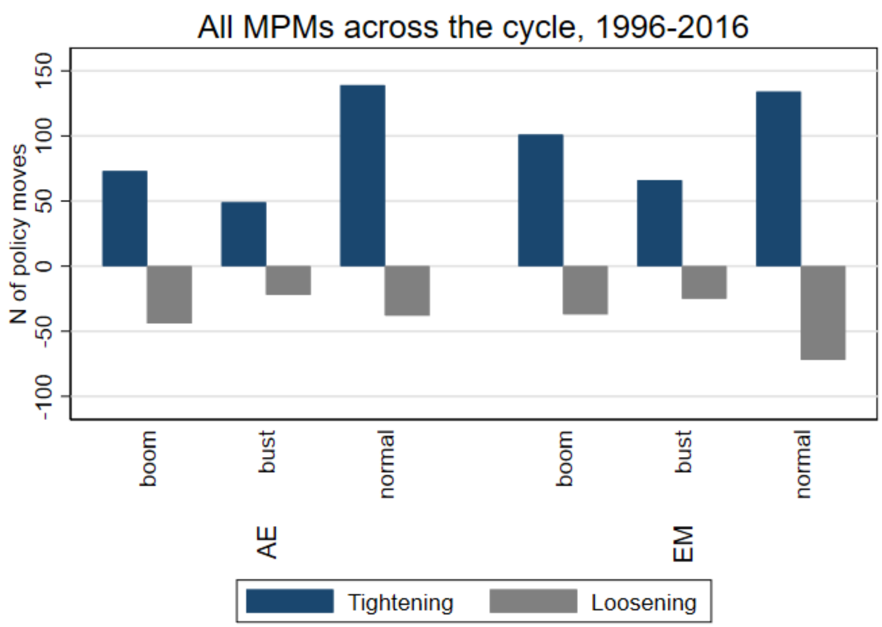
Source: MPMs and CCs datasets

Note: Average number of policy moves (tightening or loosening) per year

Third, we do see more gross tightening (and less gross loosening) of capital controls during credit booms accompanied by capital surges compared to credit booms without capital surges. Please note that *CC* is defined as the cumulative change of capital controls (activation or relaxation) during the boom episodes. However, in line with findings by Fernández et al. (2015)(2), capital controls seem to be mostly acyclical, so we have on average a zero net tightening during credit booms with capital inflow surges. But we do see some net loosening during credit booms without surges, so the capital controls policy in credit booms with surges – even if being neutral - is on average tighter. This may reflect the common policy view supported also by the IMF guidance on the use of various policies in times of strong capital inflows (IMF 2012), where macroprudential policies would typically be used more actively during capital surges with financial stability challenges before capital controls are activated.

A disaggregated picture across advanced countries and emerging markets as well as across booms, busts, and other (normal) times further shows that, somewhat surprisingly, the majority of the macroprudential policy tightening in advanced economies occurred during normal times (Figure 6). This was driven in large part by the implementation of international rules such as Basel II and III, which extended across both non-boom and post-crisis years. The figure also shows that emerging markets are on average more active in using macroprudential policies during credit booms than advanced economies.

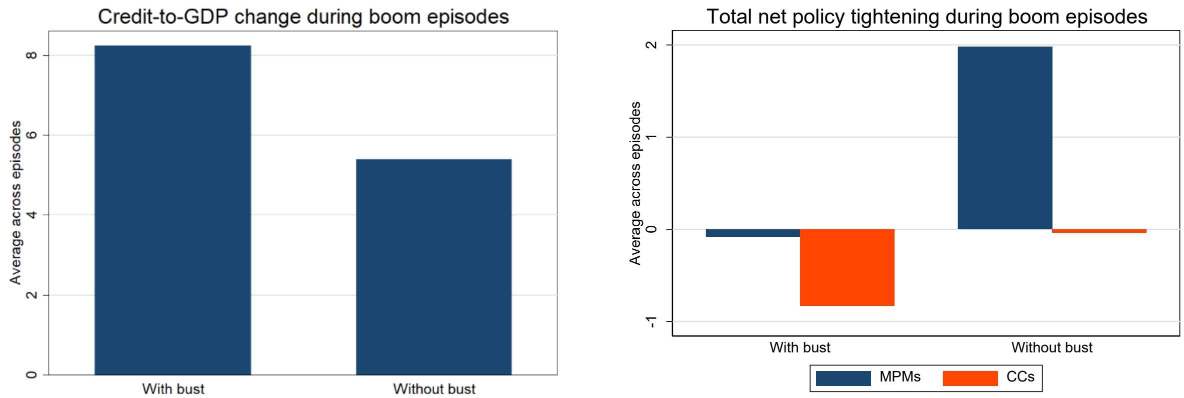
Figure 6. Macroprudential Policies over the Cycle in Advanced Economies and Emerging Markets



Source: MPMs dataset

Before proceeding with a formal econometric approach to assess the risks associated with certain credit booms ending in a credit bust, Figure 7 indicates what the main drivers could be. First, our data shows that credit increased on average more rapidly in the boom episodes that end in a bust relative to those that do not, with the average annual change in the credit-to-GDP being about 3 percentage points higher in the "bad" booms. Second, credit booms that ended in a bust were typically accompanied by much looser macroprudential and capital control policies compared to the credit booms without a bust. That is, bad booms tended to be both more intense and accompanied by less of a macroprudential and capital control policy response.

Figure 7. Good and Bad Credit Booms



Sources: IFS, Haver Analytics, MPMs dataset, CCs dataset

VII. Policy Effectiveness During Credit Booms

In this section, we analyze whether the use of macroprudential and capital flow management policies has increased resilience of the banking systems and helped countries in credit booms, in some cases accompanied by capital surges, avoid a costly credit bust. Adopting an event study approach, we zoom in on the identified 72 credit boom episodes, treating them as cross-sectional observations and running a binary probit model to project the probability of a credit boom ending in a credit bust. In addition to our key variables of interest (MPMs and CCs) we control for a typical set of indicators that are likely to affect the riskiness of credit booms. These include the intensity of the credit boom (measured by the average change of the credit-to-GDP during the boom) and its duration in years.

We estimate the following probit model:

$$\Phi(BoomBust_i = 1) = \alpha + \beta Controls_i + \mu MPM_i + \eta CC_i + e_i$$

where our dependent variable is a dummy that equals 1 when a credit boom was followed by a bust within 3 years after the boom and 0 otherwise. $Controls_i$ includes the two abovementioned control variables. MPM_i contains various measures of macroprudential policy, including both the overall index and separate categories in alternative specifications. In the baseline specification, we use the (cumulative) sum of the net policy tightening over the whole duration of the boom episode. We also control for an overall index of capital control measures CC_i.

Results in Table 3 confirm that larger and longer credit booms are more likely to end in a bust. We find evidence that the use of macroprudential instruments decreases the probability of a bad boom, while capital controls are not significant. When evaluated at the mean of other explanatory variables, with average probability of a bad boom being 33%, net tightening of macroprudential policy by 1 (i.e., for example tightening one instrument out of the eight considered) during a credit boom reduces the probability by about 7% on average.[9]

However, not all instruments appear equally effective. Repeating the regression one by one across the eight subcategories of macroprudential tools reveals that the activation of capital instruments, DTI caps, LTV caps, and reserve requirements (in this order of economic significance) help reduce the probability of a bad boom.[10] This is largely in line with the general view that capital tools help create buffers in the system and make banks more resilient, with less need for deleveraging when the financial cycle turns into its downward phase. The DTI and LTV caps play a dual role – as borrower-based measures, they can help tame the financial cycle upturns. At the same time, they indirectly help create buffers in banks' balance sheets by increasing the resilience of borrowers to income shocks (DTI) and by better collateralizing the mortgage portfolios and thus decreasing credit losses in times of borrowers' defaults (LTV). Finally, reserve requirements (which also include other liquidity tools in our dataset) would also work along both objectives of macroprudential policy, i.e., moderating credit growth as well as creating (liquidity) buffers for bad times, which would decrease the need for banks to deleverage their balance sheet and cause a credit bust in times of liquidity tensions. For these instruments, tightening by one decreases the probability of a bad boom by 22% in the case of using CAP and DTI tools, and 18% and 11% in the case of LTV and RR, respectively.

[9] We also checked for nonlinearity of the results by including interaction term of MPM and CC overall indices, but it appeared to be not significant.

[10] Due to a relatively small number of observations, we do not combine all possible policy variables in one regression to prevent the problem of over-identification. We instead run separate regressions with identical control variables and a single MPM policy instrument variable at a time.

Table 3. Net policy tightening: sum over the boom

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Credit-to-GDP change (avg)	0.113***	0.115***	0.0976**	0.100***	0.128***	0.118***	0.0889**	0.0927**	0.0785*	0.0954**
	(0.0430)	(0.0430)	(0.0392)	(0.0387)	(0.0479)	(0.0447)	(0.0410)	(0.0398)	(0.0420)	(0.0398)
Boom duration	0.0944*	0.0890*	0.0930*	0.113**	0.122**	0.105**	0.137**	0.115**	0.128**	0.0970*
	(0.0525)	(0.0538)	(0.0519)	(0.0505)	(0.0517)	(0.0510)	(0.0535)	(0.0519)	(0.0539)	(0.0526)
MPMs (cum.)	-0.184**	-0.187**								
	(0.0776)	(0.0774)								
CCs on inflows (cum.)		-0.0557	-0.0267	-0.0591	-0.0253	-0.0526	-0.0413	-0.0175	-0.0539	-0.0127
		(0.0996)	(0.0936)	(0.100)	(0.0957)	(0.0991)	(0.0951)	(0.0966)	(0.0983)	(0.0955)
RR (cum.)			-0.292*							
			(0.177)							
CG (cum.)				-0.719						
				(0.620)						
LTV (cum.)					-0.492*					
					(0.252)					
DTI (cum.)						-0.580*				
						(0.332)				
CAP (cum.)							-0.615**			
							(0.286)			
PR (cum.)								0.0953		
								(0.302)		
EXP (cum.)									-0.150	
									(0.375)	
OTH (cum.)										-0.189
										(0.210)
Constant	-1.450***	-1.437***	-1.611***	-1.610***	-1.757***	-1.607***	-1.498***	-1.643***	-1.650***	-1.533***
	(0.457)	(0.457)	(0.442)	(0.432)	(0.476)	(0.450)	(0.449)	(0.463)	(0.467)	(0.444)
Observations	72	72	72	72	72	72	72	72	65	72
R2-pseudo	0.195	0.198	0.148	0.134	0.164	0.152	0.186	0.118	0.123	0.127

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Dependent variable: probability of a credit boom ending in a bust (dummy that equals 1 when a credit boom was followed by a bust within 3 years after the boom and 0 otherwise).

Source: Authors' estimates

To check the robustness of our results, we ran several alternative specifications of these regressions. First, we used the average (mean) use of the instruments applied during a boom (i.e., per year) rather than the sum (Table 4). While the sum measures the activity in macroprudential policy (i.e., the number of instruments used per boom, some of them even repeatedly), the mean may better capture the intensity as it is normalized by the duration of the boom. The results are broadly similar, with DTI caps not being significant anymore.

Table 4. Net policy tightening: mean over the boom

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Credit-to-GDP change (avg)	0.123***	0.118**	0.0915**	0.0959**	0.159***	0.122**	0.0894**	0.0880**	0.0741*	0.0949**
	(0.0455)	(0.0466)	(0.0407)	(0.0401)	(0.0604)	(0.0514)	(0.0421)	(0.0407)	(0.0432)	(0.0425)
Boom duration	0.0799	0.0921*	0.118**	0.122**	0.111**	0.104*	0.117**	0.133**	0.139***	0.113**
	(0.0523)	(0.0535)	(0.0512)	(0.0509)	(0.0522)	(0.0529)	(0.0513)	(0.0532)	(0.0530)	(0.0528)
MPMs (avg)	-0.483**	-0.463*								
	(0.235)	(0.238)								
CCs on inflows (avg)		0.379	0.526	0.360	0.355	0.318	0.471	0.473	0.379	0.447
		(0.364)	(0.359)	(0.363)	(0.377)	(0.377)	(0.362)	(0.356)	(0.383)	(0.356)
RR (avg)			-1.104*							
			(0.600)							
CG (avg)				-3.906						
				(3.755)						
LTV (avg)					-2.063**					
					(1.010)					
DTI (avg)						-1.555				
						(1.212)				
CAP (avg)							-1.965*			
							(1.065)			
PR (avg)								0.468		
								(0.902)		
EXP (avg)									1.368	
									(1.559)	
OTH (avg)										-0.680
										(0.885)
Constant	-1.461***	-1.518***	-1.712***	-1.637***	-1.878***	-1.650***	-1.460***	-1.735***	-1.765***	-1.615***
	(0.456)	(0.465)	(0.457)	(0.442)	(0.506)	(0.458)	(0.459)	(0.477)	(0.481)	(0.452)
Observations	72	72	72	72	72	72	72	72	65	72
R2-pseudo	0.169	0.180	0.173	0.153	0.187	0.154	0.179	0.137	0.142	0.140

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Dependent variable: probability of a credit boom ending in a bust (dummy that equals 1 when a credit boom was followed by a bust within 3 years after the boom and 0 otherwise).

Source: Authors' estimates

Second, we used the gross tightening of macroprudential policies, rather than net tightening, ignoring the policy loosening steps, for both the sum (total) and average (mean) tightening during the booms. One reason to include such a variation of our policy variables is to make the results comparable to studies that coded the macroprudential intervention in the binary 0-1 form. The results presented in Table A5 and Table A6 in the Appendix confirm a role for capital instruments and LTV caps.

Third, we used the stricter definition of credit booms and credit busts described in the Section V. This brings the number of identified booms down to 53, out of which 17 ended in a bust. 26his further lowers the number of observations, and thus the results of this robustness check need to be interpreted with caution. Running the four alternative specifications (the combination of the sum versus mean use of the tools and net versus gross tightening) yields again very similar results as the baseline specification as far as capital instruments are concerned, with mixed results for other instruments found significant in the baseline (in some specifications, the DTI caps rather than LTV caps now turn significant, in others not even the DTI caps are significant).[11]

Furthermore, we conducted additional robustness checks to verify the validity of our baseline results, which included testing for omitted variable bias by controlling for indicators capturing monetary policy, level of economic development, economic activity, external imbalances, and global volatility.[12] Monetary policy could have been used to lean against the wind and affect the probability of the credit booms turning into busts. We check whether the changes in policy rates during the boom episodes have a statistically significant impact on the probability of a boom ending in a bust. The results suggest no significant impact, while leaving the remaining findings qualitatively unchanged. Our sample is comprised of both advanced economies and emerging markets. To test whether the two country groups might have different factors driving the probability of credit booms ending in busts, we introduce a dummy variable capturing countries' level of development. The dummy variable indeed appears to be significant and positive, implying that booms in emerging market economies are more likely to end in a bust. The remaining findings of the analysis stay unaffected. Real GDP growth dynamics and current account balances during the boom episodes also appear to have no significant impact on the probability of a credit boom ending in a bust. Global volatility, captured by the VIX index, could increase the probability of a credit booms ending in a bust depending on the regression specification, overall, however, there is no qualitative impact on our conclusions.

Overall, the robustness checks confirm a strong role for macroprudential instruments, among which both capital-based and borrower-based tools as well as reserve requirements appear to be effective in decreasing the probability of the credit boom turning into a credit bust.

VIII. Policy Effectiveness during Credit Booms with Capital Surges

In what follows, we focus only on the credit booms that were accompanied by a private capital inflow surge. The rest of the analysis follows the logic of the previous section, with the exception that we now look at a smaller set of boom episodes (51, out of which 18 ended in a bust). These types of episodes offer the policymakers the option to extend their policy instrument set and activate capital controls, which could help indirectly tame credit booms. Thus, we include capital controls both jointly (as an index) and one by one into our analysis.

As Table 5 with net tightening using a sum of tools applied over the boom suggests, in the subset of credit boom episodes that coincides with a capital flow surge, the overall results from the previous section largely

[11] In one regression, the provisioning instrument also turns significant, but with incorrect sign, a result driven by a very small number of observations. We report the results of the cumulative sum and mean over the boom for net tightening in Table A7 and Table A8 in the Appendix; results of the other two specifications (with gross tightening) are available upon request from the authors.

[12] The results can be made available upon request.

hold. Importantly, macroprudential measures such as capital instruments, caps on DTI, and reserve requirements continue to decrease the probability of a credit boom (accompanied with a surge) ending in a bust, but we also find exposure limits to be a significant policy measure in this regression.

While the total index of capital controls is not significant, when zooming on individual categories, tightening controls on inflows via money market can play a statistically significant role in decreasing the probability of a bad boom. This could be related to taming the inflow of “hot money” to the banking sector that is usually withdrawn as the boom ends, potentially intensifying the economic fallout.

Table 5. Net policy tightening during booms with surges: sum over the boom

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Credit-to-GDP change (avg	0.155*	0.136*	0.155**	0.161**	0.179**	0.150*	0.0921	0.131*	0.144*	0.149**	0.162*	0.143	0.173**	0.153*	0.150*	0.153*
	(0.0836)	(0.0732)	(0.0677)	(0.0786)	(0.0813)	(0.0806)	(0.0786)	(0.0720)	(0.0835)	(0.0707)	(0.0850)	(0.0979)	(0.0879)	(0.0834)	(0.0852)	(0.0838)
Boom duration	0.192**	0.171**	0.172***	0.186***	0.177***	0.239***	0.233***	0.163**	0.204***	0.177**	0.191**	0.161**	0.199**	0.195***	0.198***	0.206***
	(0.0766)	(0.0672)	(0.0656)	(0.0657)	(0.0668)	(0.0763)	(0.0802)	(0.0670)	(0.0758)	(0.0763)	(0.0751)	(0.0759)	(0.0814)	(0.0748)	(0.0746)	(0.0772)
MPMs (cum.)	-0.320**								-0.328**	-0.299**	-0.323**	-0.286**	-0.383***	-0.329**	-0.336**	-0.326**
	(0.134)								(0.138)	(0.135)	(0.138)	(0.131)	(0.147)	(0.138)	(0.145)	(0.139)
CCs on inflows (cum.)	-0.0338	-0.0333	-0.0638	-0.00560	-0.0502	-0.0481	-0.0798	-0.00496								
	(0.128)	(0.113)	(0.124)	(0.113)	(0.121)	(0.118)	(0.128)	(0.114)								
RR (cum.)		-0.425*														
		(0.218)														
CG (cum.)			-1.994													
			(2.208)													
LTV (cum.)				-0.393												
				(0.274)												
DTI (cum.)					-0.771*											
					(0.440)											
CAP (cum.)						-0.908**										
						(0.390)										
EXP (cum.)							-1.036*									
							(0.610)									
OTH (cum.)								-0.128								
								(0.240)								
re (cum.)									0.209							
									(0.487)							
di (cum.)										-1.574						
										(1.887)						
eq (cum.)											-0.410					
											(0.656)					
bo (cum.)												-0.182				
												(0.607)				
mm (cum.)													-1.855*			
													(1.118)			
ci (cum.)														-0.190		
														(0.663)		
de (cum.)															0.287	
															(0.491)	
ccfc (cum.)																0.230
																(0.513)
Constant	-2.190***	-2.349***	-2.235***	-2.296***	-2.325***	-2.296***	-2.271***	-2.104***	-2.216***	-2.084***	-2.275***	-2.013***	-2.434***	-2.199***	-2.169***	-2.239***
	(0.704)	(0.649)	(0.584)	(0.639)	(0.647)	(0.663)	(0.690)	(0.622)	(0.706)	(0.647)	(0.724)	(0.742)	(0.784)	(0.708)	(0.714)	(0.723)
Observations	51	51	51	51	51	51	46	51	51	51	51	45	51	51	51	51
R2-pseudo	0.384	0.281	0.265	0.252	0.270	0.345	0.295	0.222	0.386	0.404	0.389	0.338	0.440	0.385	0.389	0.386

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Dependent variable: probability of a credit boom ending in a bust for the subset of booms that were accompanied by capital inflow surges (dummy that equals 1 when a credit boom was followed by a bust within 3 years after the boom and 0 otherwise).
Source: Authors' estimates.

Again, in terms of economic significance, net tightening of a single macroprudential instrument during a credit boom episode that is accompanied by a capital flow surge reduces the probability of a boom ending in a bust by around 11% from the initial 29% probability when evaluated at the means of explanatory variables. The effects of the individual instruments – capital instruments, caps on DTI, reserve requirements and exposure limits (31%, 29%, 15% and 35%, respectively) – are broadly in line with those in the previous section.

The results appear broadly robust to alternative specifications. First, we again expressed the policy variables as means (rather than sums) over the boom. The results confirm our initial findings for two of the four macroprudential measures found significant in the previous specification – the capital instruments and DTI caps – and for the controls on money market instruments as regards the capital management measures (Table 6). Second, we also ran the two specifications with gross rather than net tightening. In those cases, while the results for macroprudential tools hold for capital and exposure limits (and LTV caps become significant), capital controls do not appear to be significant. Third, we introduced two alternative specifications of capital control measures into the regressions: controls on capital outflows were added in addition to the controls on inflows; and capital inflow measure was replaced with a net overall index of capital controls. CCs on outflows or the overall index remain not significant similar to CCs on inflows and do not affect the significance or magnitude of other variables included in the analysis. Finally, we also considered surges only in debt inflows (part of the total private capital inflows), as these are mostly comprised of cross-border banking flows that typically boost credit growth. Here, the results are qualitatively similar. [1]

To summarize, the robustness checks again confirm a strong role for macroprudential instruments and money-market-oriented capital flow management tools for credit booms accompanied by capital flow surges, with more detailed research needed to explore the role of individual macroprudential instruments to limit the probability of a credit boom with a capital inflow surge turning into a credit bust.

[1] The results of all robustness checks are available upon a request from the authors.

Table 6. Net policy tightening during booms with surges: mean over the boom

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Credit-to-GDP change (avg)	0.173*	0.111	0.141**	0.171**	0.284**	0.173**	0.0956	0.124*	0.178*	0.175**	0.201**	0.189*	0.212**	0.182*	0.178*	0.181**
	(0.0948)	(0.0736)	(0.0687)	(0.0863)	(0.115)	(0.0867)	(0.0769)	(0.0724)	(0.0933)	(0.0827)	(0.0970)	(0.113)	(0.102)	(0.0932)	(0.105)	(0.0922)
Boom duration	0.170**	0.205***	0.184***	0.183***	0.169**	0.192***	0.204***	0.175***	0.165**	0.140*	0.162**	0.143*	0.166**	0.161**	0.162**	0.180**
	(0.0732)	(0.0689)	(0.0662)	(0.0662)	(0.0713)	(0.0696)	(0.0713)	(0.0676)	(0.0729)	(0.0719)	(0.0703)	(0.0734)	(0.0741)	(0.0701)	(0.0702)	(0.0727)
MPMs (avg)	-1.268***								-1.267***	-1.214**	-1.324***	-1.240**	-1.529***	-1.277***	-1.362**	-1.154**
	(0.486)								(0.482)	(0.482)	(0.503)	(0.507)	(0.549)	(0.486)	(0.530)	(0.491)
CCs on inflows (avg)	0.220	0.347	0.153	0.355	0.158	0.195	-0.00927	0.312								
	(0.550)	(0.500)	(0.486)	(0.466)	(0.537)	(0.474)	(0.505)	(0.455)								
RR (avg)		-1.774**														
		(0.794)														
CG (avg)			-11.26													
			(16.95)													
LTV (avg)				-1.855												
				(1.305)												
DTI (avg)					-7.887**											
					(3.818)											
CAP (avg)						-3.825**										
						(1.728)										
EXP (avg)							-4.144									
							(3.715)									
OTH (avg)								-0.556								
								(1.154)								
re (avg)									0.184							
									(1.299)							
di (avg)										-17.73						
										(20.50)						
eq (avg)											-2.919					
											(2.820)					
bo (avg)												0.612				
												(1.937)				
mm (avg)													-8.051*			
													(4.624)			
ci (avg)														-0.673		
														(3.648)		
de (avg)															2.222	
															(1.819)	
ccfc (avg)																2.770
																(2.704)
Constant	-2.217***	-2.429***	-2.202***	-2.325***	-2.732***	-2.212***	-2.144***	-2.109***	-2.221***	-2.072***	-2.406***	-2.161***	-2.538***	-2.214***	-2.175***	-2.336***
	(0.737)	(0.665)	(0.594)	(0.663)	(0.774)	(0.670)	(0.669)	(0.623)	(0.740)	(0.684)	(0.781)	(0.803)	(0.830)	(0.736)	(0.770)	(0.756)
Observations	51	51	51	51	51	51	46	51	51	51	51	45	51	51	51	51
R2-pseudo	0.389	0.314	0.263	0.259	0.337	0.328	0.256	0.227	0.387	0.409	0.401	0.343	0.441	0.387	0.414	0.406

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Dependent variable: probability of a credit boom ending in a bust for the subset of booms that were accompanied by capital inflow surges (dummy that equals 1 when a credit boom was followed by a bust within 3 years after the boom and 0 otherwise).
Source: Authors' estimates.

IX. Conclusion

This paper contributes to the literature on the effectiveness of macroprudential measures and capital controls in mitigating risk accumulation in the financial system. We constructed a new large and granular cross-country dataset on prudential and capital flow management measures covering 53 countries for two decades from 1996 to 2016 by combining various extant datasets. We analyzed the joint effectiveness of macroprudential and capital control measures in a granular way, with eight types of macroprudential tools and eight types of capital controls. Instead of focusing on short-term effects of policy interventions on intermediate targets such as credit or house prices growth, reflecting the macroprudential objective to tame financial cycles, we applied a medium-term perspective and focused on what tools make the financial system more resilient. We operationalized it by investigating whether the use of either macroprudential or capital control measures helps decrease the probability of credit booms – some accompanied with capital inflow surges – ending in a credit bust.

We found that macroprudential policies are effective in countries experiencing credit booms to avoid ending in a credit bust. They also help in cases where credit booms are accompanied by capital flow surges, but additionally also capital controls on short-term money market instruments, including cross-border interbank lending, tended to reduce the likelihood of a bust. Capital-based macroprudential tools, borrower-based tools such as caps on DTI and LTV as well as reserve requirements appear to play a role in making the financial system resilient in times of credit booms, both with and without capital surges, but more research is needed on their channels of influence as they have not been found significant in all our specifications. Overall, the findings underscore the importance of macroprudential policy in mitigating the adverse impact of credit booms.

Our analysis also opened new questions that could be explored in follow-up research. For example, are there additional characteristics of countries (apart of their level of development, i.e., specific features of their institutional, regulatory or governance framework) and/or credit boom episodes (such as type of credit driving the boom, initial level of financial integration, or the degree of initial financial development) that would co-determine whether macroprudential tools and capital controls are effective? What is the best (most effective) “sequencing” and combinations of macroprudential instruments and capital controls over the credit booms? Are some credit busts worse than others (e.g., accompanied by undesirable macroeconomic outcomes or banking crises), and which macroprudential policies or capital controls would reduce the probability of such busts? What is the effect of additional policies, such as fiscal or exchange rate policy, on reducing the probability of a credit boom ending in a credit bust?

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Appendix

Table A1. Countries included in the sample

Emerging markets	Advanced economies
Argentina	Australia
Brazil	Austria
Bulgaria	Belgium
Chile	Canada
China	Denmark
Colombia	Finland
Czech Republic	France
Hungary	Germany
India	Greece
Indonesia	Hong Kong SAR
Latvia	Iceland
Lebanon	Ireland
Malaysia	Israel
Mexico	Italy
Nigeria	Japan
Peru	Korea
Philippines	Netherlands
Poland	New Zealand
Romania	Norway
Russia	Portugal
Slovenia	Singapore
South Africa	Spain
Thailand	Sweden
Turkey	Switzerland
Ukraine	United Kingdom
Uruguay	United States
Vietnam	

Table A2. Definitions of macroprudential measures

Macroprudential measures	Abbreviation	Definition
Reserve and liquidity requirements	RR	Requirements for the banks to hold at least a fraction of their liabilities with the central bank, typically differentiated by the type, maturity and currency of deposits and other funding; imposed on stocks or flows of liabilities (marginal reserve requirements). Liquidity tools are aimed at mitigating banks' liquidity risks and are typically set as a minimum ratio of liquid assets relative to total assets or selected (less stable) liabilities. Both reserve and liquidity requirements affect the funds available for lending on to the private sector.
Limits on credit growth	CG	Quantitative limit set directly on the growth of credit over a specified period or a maximum increase in lending over specific period of time.
LTV caps	LTV	Caps on loan-to-value ratio limits the maximum amount that can be lent to the borrowers against their (typically real estate) collateral. An LTV ratio of 70% would imply lending to fund 70% of the purchase value (the rest being a downpayment), while 100% would allow for full lending equivalent to the value of the collateral.
DTI or DSTI caps	DTI	Caps on debt-to-income (or debt-service-to-income) ratio limits the size of debt (or debt service payments) relative to household income.
Capital instruments	CAP	Measures aimed at affecting the capitalization levels of banks. Countercyclical capital buffers require banks to hold more capital during upturns. Leverage ratios limits the banks from exceeding a fixed minimum leverage ratio. Capital surcharges on Systemically Important Financial Institutions force them to hold a higher capital level than other financial institutions. Sectoral capital buffers or higher risk weights on various types of exposures also belong here, as they require higher capital to be held against such exposures.
Provisioning	PR	Adjustments (typically an increase) of specific provisions created for bad loans beyond traditional provisioning rates, such as for high LTV loans, loans in foreign currency or loans to certain sectors; adjustments to general provisioning rates, including a dynamic (countercyclical) element that requires banks to set aside reserves from profits in good times in order to cover realized losses from borrower default in bad times.
Exposure and concentration limits	EXP	Limit the fraction of assets held by specific borrowers or sectors (concentration limit) or fraction of liabilities held by other banks (interbank exposure limit).
Other	OTH	This is a residual category and includes various tax measures applied on financial institutions as well as selected consumer loan measures.

Table A3. Dataset on MPMs: Mapping of tools to source databases

Defined macroprudential measures	Code	Shim et al. 2013	Akinci & Olmstead-Rumsey 2015	Cerutti et al. 2015	Cerutti et al. 2017	Alam et al. 2019
Reserve and liquidity requirements	RR	RR Liq		RR RR_REV	rr_foreign rr_local	Liquidity LTD RR
Limits on credit growth	CG	CRg	B2_4	FC CG		LCG LoanR LFC
LTV caps	LTV	Ltv	B1_1	LTV LTV_CAP	ltv_cap	LTV
DTI or DSTI caps	DTI	Dsti	B1_2	DTI		DSTI
Capital instruments (includes buffers, risk weighting, leverage ratio)	CAP	RW	B1_4 B2_1	CTC LEV SIFI	sscb_res sscb_cons sscb_oth cap_req	CCB Conservation Capital LVR SIFI
Provisioning	PR	Prov	B1_5 B2_2	DP		LLP
Exposure and concentration limits, incl. intrabank	EXP	Expo		INTER CONC	concrat ibex	
Other (taxes, consumer loans measures)	OTH	Tax	B2_3	TAX		TAX OTH LFX
Number of countries	**65**	**60**	**50**	**119**	**64**	**134**
Time period	**1995-2014**	**1990-2012**	**2000-2014**	**2000-2013**	**2000-2014**	**1990-2016**
Frequency	**A**	**M**	**Q**	**A**	**Q**	**M**

Table A4. Dataset on CCs: Mapping of tools to source databases

Indicator Inflow restrictions	Code	Variable name in FKRSU 2015	Description
Overall	**FKRSU_i**		(eq+bo+mm+ci+de+ccfc+di+re)/8
Equity	**eq**	**eq_plbn**	Purchase locally by nonresidents (equity)
		eq_siar	Sale or issue abroad by residents (equity)
Bond	**bo**	**bo_plbn**	Purchase locally by nonresidents (bonds)
		bo_siar	Sale or issue abroad by residents (bonds)
Money market	**mm**	**mm_plbn**	Purchase locally by nonresidents (money market instruments)
		mm_siar	Sale or issue abroad by residents (money market instruments)
Collective investments	**ci**	**ci_plbn**	Purchase locally by nonresidents (collective investments)
		ci_siar	Sale or issue abroad by residents (collective investments)
Derivatives	**de**	**de_plbn**	Purchase locally by nonresidents (derivatives)
		de_siar	Sale or issue abroad by residents (derivatives)
		gsi	Guarantees, sureties and financial backup facilities
Commercial and financial credits	**ccfc**	**cci**	Commercial credits inflow restrictions
		fci	Financial credits inflow restrictions
Direct investment	**di**	**dii**	Direct investment inflow restrictions
Real estate	**re**	**re_plbn**	Purchase locally by nonresidents (real estate)

Table A5. Gross policy tightening: sum over the boom

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Credit-to-GDP change (avg)	0.106***	0.108***	0.0916**	0.100**	0.138***	0.114**	0.0902**	0.0928**	0.0766*	0.0921**
	(0.0410)	(0.0417)	(0.0390)	(0.0390)	(0.0498)	(0.0448)	(0.0424)	(0.0402)	(0.0426)	(0.0407)
Boom duration	0.128**	0.119**	0.109**	0.114**	0.127**	0.105**	0.138**	0.106**	0.125**	0.105**
	(0.0506)	(0.0516)	(0.0508)	(0.0517)	(0.0523)	(0.0509)	(0.0542)	(0.0510)	(0.0551)	(0.0510)
MPMs (cum.)	-0.137**	-0.145**								
	(0.0693)	(0.0726)								
CCs on inflows (cum.)		0.125	0.124	0.0965	0.0841	0.0774	0.238	0.0988	0.0855	0.0960
		(0.127)	(0.125)	(0.120)	(0.127)	(0.121)	(0.152)	(0.119)	(0.122)	(0.119)
RR (cum.)			-0.247							
			(0.235)							
CG (cum.)				-0.814						
				(0.598)						
LTV (cum.)					-0.659**					
					(0.313)					
DTI (cum.)						-0.493				
						(0.334)				
CAP (cum.)							-0.789**			
							(0.321)			
PR (cum.)								0.121		
								(0.282)		
EXP (cum.)									-0.201	
									(0.353)	
OTH (cum.)										0.0163
										(0.303)
Constant	-1.487***	-1.529***	-1.593***	-1.666***	-1.823***	-1.647***	-1.601***	-1.679***	-1.667***	-1.641***
	(0.443)	(0.451)	(0.444)	(0.440)	(0.485)	(0.455)	(0.464)	(0.460)	(0.473)	(0.453)
Observations	72	72	72	72	72	72	72	72	65	72
R2-pseudo	0.166	0.177	0.136	0.150	0.184	0.151	0.218	0.125	0.129	0.123

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Dependent variable: probability of a credit boom ending in a bust (dummy that equals 1 when a credit boom was followed by a bust within 3 years after the boom and 0 otherwise).

Source: Authors' estimates

Table A6. Gross policy tightening: mean over the boom

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Credit-to-GDP change (avg)	0.118***	0.111**	0.0846**	0.0960**	0.184***	0.115**	0.0937**	0.0880**	0.0732	0.0860**
	(0.0447)	(0.0467)	(0.0405)	(0.0408)	(0.0687)	(0.0511)	(0.0449)	(0.0419)	(0.0449)	(0.0431)
Boom duration	0.0853*	0.101*	0.118**	0.129**	0.112**	0.110**	0.125**	0.135**	0.143***	0.128**
	(0.0517)	(0.0527)	(0.0508)	(0.0516)	(0.0524)	(0.0522)	(0.0520)	(0.0529)	(0.0532)	(0.0523)
MPMs (avg)	-0.452*	-0.413								
	(0.257)	(0.265)								
CCs on inflows (avg)		0.925*	1.008**	0.942*	0.890	0.879*	1.289**	1.043**	1.125*	1.000**
		(0.518)	(0.495)	(0.516)	(0.563)	(0.528)	(0.596)	(0.516)	(0.591)	(0.510)
RR (avg)			-1.154							
			(1.114)							
CG (avg)				-5.206						
				(4.765)						
LTV (avg)					-2.822**					
					(1.296)					
DTI (avg)						-1.265				
						(1.114)				
CAP (avg)							-2.769**			
							(1.302)			
PR (avg)								0.528		
								(0.888)		
EXP (avg)									0.894	
									(1.511)	
OTH (avg)										0.0903
										(1.076)
Constant	-1.371***	-1.631***	-1.715***	-1.852***	-2.129***	-1.830***	-1.719***	-1.964***	-1.988***	-1.867***
	(0.462)	(0.503)	(0.486)	(0.475)	(0.563)	(0.489)	(0.509)	(0.516)	(0.528)	(0.486)
Observations	72	72	72	72	72	72	72	72	65	72
R2-pseudo	0.155	0.194	0.176	0.191	0.233	0.180	0.232	0.168	0.176	0.164

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Dependent variable: probability of a credit boom ending in a bust (dummy that equals 1 when a credit boom was followed by a bust within 3 years after the boom and 0 otherwise).

Source: Authors' estimates

Table A7. Net policy tightening in strong booms: sum over the cycle

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Credit-to-GDP change (avg)	0.133***	0.134***	0.127***	0.119**	0.147***	0.161***	0.122**	0.130***	0.108**	0.117**
	(0.0494)	(0.0497)	(0.0468)	(0.0475)	(0.0544)	(0.0552)	(0.0492)	(0.0495)	(0.0528)	(0.0473)
Boom duration	0.0589	0.0589	0.0790	0.105	0.102	0.0476	0.120	0.138*	0.180*	0.0820
	(0.0853)	(0.0849)	(0.0819)	(0.0788)	(0.0808)	(0.0870)	(0.0806)	(0.0831)	(0.0929)	(0.0813)
MPMs (cum.)	-0.153*	-0.151								
	(0.0925)	(0.0942)								
CCs on inflows (cum.)		0.0782	0.0928	0.108	0.104	0.0364	0.163	0.207	0.0601	0.119
		(0.160)	(0.156)	(0.162)	(0.155)	(0.169)	(0.160)	(0.168)	(0.159)	(0.155)
RR (cum.)			-0.325							
			(0.236)							
CG (cum.)				-0.00190						
				(0.520)						
LTV (cum.)					-0.384					
					(0.265)					
DTI (cum.)						-1.069**				
						(0.532)				
CAP (cum.)							-0.612*			
							(0.365)			
PR (cum.)								0.703*		
								(0.405)		
EXP (cum.)									0.748	
									(0.686)	
OTH (cum.)										-0.267
										(0.277)
Constant	-1.729***	-1.752***	-2.031***	-1.988***	-2.087***	-1.830***	-1.901***	-2.404***	-2.402***	-1.851***
	(0.623)	(0.627)	(0.613)	(0.621)	(0.639)	(0.635)	(0.636)	(0.695)	(0.730)	(0.619)
Observations	53	53	53	53	53	53	53	53	48	53
R2-pseudo	0.191	0.194	0.177	0.146	0.180	0.223	0.202	0.196	0.167	0.163

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Dependent variable: probability of a credit boom ending in a bust (dummy that equals 1 when a credit boom was followed by a bust within 3 years after the boom and 0 otherwise).

Source: Authors' estimates

Table A8. Net policy tightening in strong booms: mean over the cycle

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Credit-to-GDP change (avg)	0.132***	0.133***	0.129***	0.122**	0.134**	0.158***	0.134**	0.127**	0.107*	0.120**
	(0.0499)	(0.0509)	(0.0486)	(0.0481)	(0.0543)	(0.0577)	(0.0531)	(0.0501)	(0.0550)	(0.0485)
Boom duration	0.0650	0.0741	0.101	0.108	0.0980	0.0608	0.0896	0.150*	0.200**	0.118
	(0.0861)	(0.0867)	(0.0807)	(0.0798)	(0.0816)	(0.0870)	(0.0834)	(0.0857)	(0.0949)	(0.0864)
MPMs (avg)	-0.256	-0.232								
	(0.221)	(0.222)								
CCs on inflows (avg)		0.519	0.613	0.518	0.527	0.368	0.886	0.730	0.352	0.578
		(0.517)	(0.516)	(0.523)	(0.515)	(0.545)	(0.592)	(0.539)	(0.580)	(0.513)
RR (avg)			-1.027							
			(0.686)							
CG (avg)				-1.500						
				(2.579)						
LTV (avg)					-0.448					
					(0.634)					
DTI (avg)						-1.805				
						(1.270)				
CAP (avg)							-3.273**			
							(1.571)			
PR (avg)								1.575		
								(1.108)		
EXP (avg)									2.958	
									(1.953)	
OTH (avg)										0.138
										(0.762)
Constant	-1.781***	-1.871***	-2.157***	-2.018***	-2.041***	-1.935***	-1.837***	-2.419***	-2.531***	-2.088***
	(0.628)	(0.646)	(0.642)	(0.629)	(0.633)	(0.641)	(0.677)	(0.710)	(0.749)	(0.679)
Observations	53	53	53	53	53	53	53	53	48	53
R2-pseudo	0.160	0.176	0.194	0.164	0.166	0.193	0.255	0.191	0.196	0.159

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Dependent variable: probability of a credit boom ending in a bust (dummy that equals 1 when a credit boom was followed by a bust within 3 years after the boom and 0 otherwise).

Source: Authors' estimates

PUBLICATIONS

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