Income-based tools to mitigate housing market risks: Where might we have been without them?*

Ryan Banerjee
Bank for International Settlements

Moaz Elsayed
Banque de France

Valère Fourel
Banque de France

Niamh Hallissey
Central Bank of Ireland

Ceara Hui
Hong Kong Monetary Authority

Stanley Ka
Hong Kong Monetary Authority

Kyeongtae Lee
Bank of Korea

Fergal McCann
Central Bank of Ireland

Chris McDonald
Reserve Bank of New Zealand

Philip Ng
Hong Kong Monetary Authority

Matthieu Segol
Banque de France

Tuli Saha
Bank of England

Alexandra Varadi
Bank of England

Lu Zhang
De Nederlandsche Bank

September 15, 2025

Abstract

Income-based borrower-based measures (I-BBMs), such as debt-service-to-income (DSTI) and debt-to-income (DTI) limits, have emerged as key macroprudential tools for mitigating housing market risks. However, evidence on their impact remains relatively scarce. This paper fills this gap by quantifying the macroeconomic stabilising benefits against the costs in terms of restricting high DSTI/DTI lending in seven economies that have implemented these measures. Using a novel framework that integrates micro-level borrower data and identification strategies into a structural vector autoregression model, we find that I-BBMs helped stabilise housing markets and economic activity during the Covid-19 pandemic and subsequent housing boom by counteracting the procyclicality of bank lending standards. Notably, this stabilisation came at the expense of constraining only a modest share of high DSTI/DTI loans.

Keywords: Macroprudential policy, Housing market, Borrower-based measures, Financial cycles, Business cycle fluctuations.
JEL Codes: E32, E44, G21, G28.

*The views expressed in this paper are those of the authors and do not necessarily represent those of the Bank for International Settlements, Banque de France, Bank of Korea, Bank of England, Central bank of Ireland, De Nederlandsche Bank, Hong Kong Monetary Authority and the Reserve Bank of New Zealand. We thank Katarina Bergant (discussant), Kristin Forbes, Anil Kashyap, Luc Laeven, Philip Lane, David Miles (discussant), Eric Rosengren and seminar participants at the DNB-Riksbank-Bundesbank-Central Bank of Ireland Macroprudential Conference for helpful comments and assistance from Edward Gaffney and Emil Bandoni.

Corresponding author: Ryan Banerjee (E-mail: ryan.banerjee@bis.org).

1. Introduction

Historically, the buildup of household leverage has led to busts, lower output growth, and higher unemployment (Jordà et al. (2016), Mian et al. (2017)). A growing body of research highlights the central role of debt servicing obligations in driving these adverse dynamics (Drehmann et al. (2023)), especially in risky tail of borrowers (Cumming and Hubert (2022)). For many households with mortgages, debt servicing payments consume a substantial portion of their income. When these obligations become difficult to meet, the risk of default rises. Even when defaults are avoided, the financial strain of servicing debt often forces households to curtail consumption, thereby weakening aggregate demand and spreading economic fragility to the broader economy (Cloyne et al. (2019), Drehmann et al. (2023), Bracke et al. (2024)).

Given the strong link between debt servicing difficulties and financial instability, macroprudential authorities have increasingly turned to tools which limit the issuance of loans with high debt service obligations relative to incomes (Committee on the Global Financial System (2023)). These income-based borrower-based measures (I-BBMs) typically take the form of debt-service-to-income (DSTI) limits or debt-to-income (DTI) limits, which restrict lenders from extending loans that exceed specified DSTI or DTI thresholds.

Despite the growing popularity of I-BBMs amongst macroprudential policymakers, the empirical literature, particularly on their stabilising benefits, remains relatively scarce. Understanding the benefits, as well as potential costs is important, as politicians have sometimes proved wary in granting this specific tool to macroprudential authorities due to fears that I-BBMs could limit access to housing credit for key constituents (Committee on the Global Financial System (2023)).

This paper evaluates the trade-offs of I-BBMs by jointly analysing key costs and benefits through a novel framework. On the cost side, we examine how these measures restricted borrowers' access to high DSTI or DTI loans – a politically sensitive and highly visible issue for governments. On the benefit side, we estimate the extent to which limiting access to such loans reduced the volatility of key macroeconomic variables. We additionally provide further insights into the cost-benefit balance by assessing the impact of I-BBMs on the growth-volatility trade-off.

To jointly measure these costs and benefits, we leverage the recently developed "meso-econometric" framework of Elsayed et al. (2025), which integrates the micro-level identification strategies enabled by detailed administrative data (e.g., Defusco et al. (2019), Tzur-Ilan (2023), Gaffney (2022), HCSF (2024), Levina et al. (2019)) within a structural vector autoregression (SVAR) model. This approach combines the precision of microeconomic analysis with the broader general equilibrium insights of macroeconomic frameworks.

We apply this framework to examine the impact of I-BBMs in seven economies that have been at the forefront of using these tools to address housing market risks.[1] By analysing results across multiple economies, we enhance the external validity of our findings and whether the cost-benefit trade-offs are broadly similar. This is especially important given the significant variations in housing markets, which arise from differences in legal frameworks, regulatory environments, and the types of housing finance available.

Our study, therefore, bridges the divide between detailed single-country micro-level studies on the impact of BBMs (e.g. Defusco et al. (2019), Tzur-Ilan (2023), Gaffney (2022), HCSF (2024), Levina et al. (2019)) and those relying on multi-country macro-level data (e.g. Kuttner and Shim (2016), Akinci and Olmstead-Rumsey (2018), Richter et al. (2019), Brandao-Marques et al. (2021), International Monetary Fund (2024)). Compared to detailed microeconomic studies, our findings provide a synthesis of the key mechanisms driving macroeconomic cycles across different economies and their effects within diverse institutional settings. In contrast to multi-country macro-level studies, which are constrained by the lack of detailed data we have access to and thus focus only on the presence or absence of borrower-based measures, our analysis delves into how binding these tools were in practice. This enables us to better assess broader stabilisation effects over the cycle. Relative to existing studies assessing the micro-to-macro effects (e.g. Gross and Población (2017),

[1]The seven economies are: France, Hong Kong, Ireland, Korea, the Netherlands, New Zealand, and the United Kingdom.

Giannoulakis et al. (2023)) we do not rely on surveys and modelled projections, but instead use data observed by supervisory authorities on the actual distribution of borrowers both before and after the implementation of BBMs. By precisely analysing how binding these tools were in practice, our paper takes a significant step forward in assessing how these policies influenced macroeconomic outcomes ex post.

The method of Elsayed et al. (2025) that we use employs a novel two-step identification strategy to isolate I-BBM induced shocks, leveraging in each stage identification techniques of macro- and micro-econometric studies respectively. In the first stage, we estimate lending standards shocks by applying the macroeconometric external instrument identification strategy for SVARs (Stock (2008) and Mertens and Ravn (2013)), where we use measures based on banks' reported changes in mortgage lending standards as an external instrument, similar to Bassett et al. (2014) and Varadi (2024).[2]

In the second stage, we decompose lending standards shocks into two components: changes in banks' own lending standards that would have occurred independently of the I-BBM (referred to as bank lending standard shocks) and changes in lending standards induced by the I-BBM itself (I-BBM induced shocks). Taking cues from the microeconometric identification strategies employed in Defusco et al. (2019), Tzur-Ilan (2023), Gaffney (2022) and HCSF (2024), we use granular data that exploits the heterogeneous impact of the I-BBM across the loan distribution. Specifically, we compare the evolution of lending to borrower segments near the threshold of the DSTI/DTI policy – the treatment group – against segments further away from the threshold – the control group. This decomposition allows us to isolate the causal effect of the I-BBM on lending standards from changes in bank' own lending practices. One caveat is that since our analysis is based solely on information from borrowers who ultimately secure housing loans, our decomposition of lending standards excludes information about the influence of I-BBMs on lending at the extensive margin.

With this decomposition of lending standards shocks, we first use our SVAR to examine the

[2]Note that for Hong Kong, due to data availability we use sign and zero restrictions to estimate credit supply shocks in the first stage.

macroeconomic impact of lending standards shocks. Across all economies studied, tighter lending standards lead to declines in the volume of housing loans and house prices. In terms of broader macroeconomic effects, tighter lending standards lead to declines in residential investment, real incomes, and real GDP across all economies.

Next, we analyse the relationship between I-BBM induced lending shocks and bank lending standards that would have occurred independently of the I-BBMs. Our findings indicate that, on average, I-BBMs have tended to offset the effects of bank lending standards shocks since their introduction. Given the procyclicality of bank lending standards (Berger and Udell (2004)), this result provides a first glimpse into the potential stabilising effects of I-BBMs.

Finally, we use historical decompositions of our SVAR to quantify costs and benefits of I-BBM policies. Specifically, we use our model to generate counterfactual data in the absence of the I-BBM shocks, which we then compare to the actual data. In terms of the costs, our baseline estimates indicate that on average across economies, I-BBMs have constrained the origination of high DSTI/DTI loans by around 0.5 to 6% of total new lending since their implementation. These estimates are consistent with, though somewhat towards the lower end of, estimates from bottom-up approaches. For example, for France, HCSF (2024) estimates that around 6% of total new loans were constrained by the DSTI limit.

While I-BBM policies appear to have constrained access to high DSTI/DTI loans for some households, our findings highlight significant stabilisation benefits. Our baseline estimates indicate that across the economies in our study, I-BBMs policies may have led to a 10% fall in the volatility of real income growth, house price growth and residential investment growth between 2019 and 2024, a period which saw significant swings in interest rates and real income.

To put our estimates of the volatility reduction into perspective, Stock and Watson (2003) estimate that during the great moderation (1984-2002), the standard deviation of real GDP growth in the United States, was around 35% lower compared to the 1960-1983 period, while declining by between 30% to 50% in France, Germany, Italy, Japan and the United Kingdom. Thus, our estimates indicate that I-BBMs may have dampened macroeconomic volatility by about one quarter of that which occurred during the great moderation.

Our historical decompositions shed further light into the mechanisms through which I-BBMs helped to stabilise the macroeconomy during this period. They show that I-BBMs did constrain access to high DSTI/DTI loans during the housing market boom of 2021. However, subsequently, our SVAR historical decompositions indicate that I-BBMs supported access to high DSTI/DTI loans and supported house prices and housing loan growth as the post-Covid inflation surge eroded real incomes and interest rates rose. Thus, our findings indicate that I-BBM policies contributed to stabilising housing markets and the broader economy, which in turn smooth the credit cycle - one of the key objectives of macroprudential policy. Due to these automatic-stabiliser like properties, I-BBMs may have further reduced the need for policymakers to recalibrate other macroprudential policy through the economic cycle such as the countercyclical capital buffer.

Additionally, the historical decompositions suggest that I-BBMs enhanced the output-volatility tradeoff during this period. Our counterfactual analysis reveals that, in the absence of I-BBMs, the Sharpe ratio for real income growth would have been lower. This indicates that I-BBMs effectively reduced macroeconomic volatility without hindering economic growth.

These quantitative estimates provide important empirical support for a transmission mechanism previously highlighted in theoretical DSGE models incorporating housing and credit constraints (e.g., Millard et al. (2024), Greenwald (2018)): namely, that BBM limits help to mitigate fluctuations in the housing market and income (or output) volatility, thereby contributing to improved household welfare. In our specific case, I-BBMs would be expected to limit the volatility of household debt service relative to incomes, which in turn would enable households to better smooth consumption through the various macroeconomic and financial shocks.

Clearly conducting any counterfactual analysis over the Covid-19 pandemic period requires a health warning. The size of the pandemic shock and the unprecedented fiscal and monetary support during this period makes it particularly challenging to quantify the stabilising effects of these measures. Nevertheless, robustness tests which exclude the Covid-19 pandemic period confirm that I-BBMs helped to stabilise key macroeconomic variables.

The paper is organised as follows: section 2 briefly summarises the related literature and our contribution;section 3 provides an overview of the use of I-BBMs in the seven economies of our study; section 4 describes the data we use; section 5 sets out our empirical and identification strategy; section 6 provides preliminary evidence of the stabilising effects of I-BBMs; while section 7 presents our quantification of the costs and benefits of I-BBMs; section 8 uses our estimates to provide a narrative to how I-BBMs have influenced economic activity during the Covid-19 pandemic, the post-pandemic housing market boom and subsequent tightening of monetary policy; section 9 examines the robustness of our baseline results; and finally, section 10 concludes.

2. Related literature

Our paper contributes to several strands of literature analysing the effects of macroprudential policy. Specifically, it adds to the microeconometric literature that examines the cross-sectional effects of borrower-based measures (BBMs). Prior studies have explored the impact of loan-to-value (LTV) measures on the cross-section of borrowers (e.g., Tzur-Ilan (2023) and van Bekkum et al. (2024)) and the effects of income-based borrower-based measures (I-BBMs) (e.g., Defusco et al. (2019), Peydró et al. (2023), Nier et al. (2019)). The microeconometric methods employed in these studies allow for a detailed assessment of how these measures influence the origination of high DSTI/DTI mortgages relative to other types of loans. Additionally, these studies often explore potential spillover effects, such as the impact on house prices, by comparing regions with varying levels of exposure to the policies. However, the cross-sectional nature of these studies limits any analysis of general equilibrium effects such as the overall impact on house prices or credit and the role of borrower-based measures in dampening cyclical fluctuations.

Relative to this body of work, our paper makes a novel contribution by integrating elements from these microeconometric studies into a standard macroeconomic framework. This approach allows us not only to capture the intensity of I-BBMs on the distribution of borrowers but also to evaluate the broader, economy-wide effects of these policies. In doing so, we bridge the gap between microeconomic insights and macroeconomic outcomes, providing a more comprehensive understanding of the role of I-BBMs in stabilising the economy.

Our paper is also closely related to the work of Gross and Población (2017) and Giannoulakis et al. (2023), who provide a framework for assessing the micro-to-macro effects of borrower-based measures (BBMs). These studies focus on evaluating the macroeconomic impact of BBMs by using microeconomic household data to identify which borrowers might be constrained by these measures before they were implemented, and hence the potential impact on aggregate household credit. The inferred impact on household credit is subsequently incorporated into a separate Bayesian VAR to analyse the potential macroeconomic effects of the policy.

In contrast, the methodology of Elsayed et al. (2025), which we adopt, utilises microdata to capture the actual impact of I-BBMs on household credit and the broader economy. By leveraging microdata to identify BBM shocks, this approach enables a direct evaluation of the influence of I-BBMs on macroeconomic outcomes. This perspective offers valuable insights into the realised effects of BBMs, serving as a complement to analyses that focus on their potential impacts.

We also contribute to the growing literature on the macroeconomic effects of macroprudential borrower-based measures (BBMs). A substantial body of research has examined how changes in macroprudential policies, including I-BBMs, affect credit and asset prices (e.g., Akinci and Olmstead-Rumsey (2018), Bruno et al. (2017)) as well as broader economic activity (e.g., Richter et al. (2019), Brandao-Marques et al. (2021), International Monetary Fund (2024)). However, this literature does not isolate the specific impact of I-BBMs. Instead, it has either focused solely on loan-to-value (LTV) limits – given their more frequent use and adjustments – or does not differentiate between the effects of LTVs and I-BBMs. Kuttner and Shim (2016) is one exception, who do separately assess the effect of tightenings or loosenings of DSTI limits on housing credit and house prices. More importantly, the nature of the data used in this literature is such that, these studies typically analyse the impact of a binary dummy variable when they are introduced or change. This, particularly limits the analysis of I-BBMs, which are rarely changed after implemention.

Crucially, our paper diverges from the existing macroeconomic literature by not relying solely on changes in policy calibrations to identify effects. Our paper makes a novel contribution by not only analysing the impact of introducing I-BBMs, but exploiting granular data to capture the intensity of these measures in constraining lending over the cycle. This allows us to assess the stabilising properties of I-BBMs even in the absence of policy recalibration, providing a more comprehensive perspective of their role in stabilising economic activity.

Finally, our paper contributes to the literature on the costs versus the benefits of borrower-based policies. The microeconometric studies tend to focus on the costs of these measures, such as the share of constrained households or reallocation away from lower income households. Similarly, many macroeconomic studies have focused on the costs in terms of lower output. For example, Richter et al. (2019) examine the output costs of tightening LTV limits. A few papers do additionally examine the stabilising benefits. International Monetary Fund (2024) examines whether tighter LTV limits are associated with a lower sensitivity of consumption to monetary policy shocks, while Brandao-Marques et al. (2021) examines the tradeoffs between lower average GDP growth but lower tail risks as measured by GDP-at-risk.

Our paper contributes by bridging the gap between these two strands of literature. We integrate the microeconometric analysis of I-BBMs, which focuses on their impact in constraining high DSTI/DTI loans, with the macroeconomic perspective that captures the economy-wide stabilisation benefits of these policies. This approach complements existing studies by providing arguably a closer representation of the cost-benefit calculus faced by policymakers (see Committee on the Global Financial System (2023)).

3. Income-based macroprudential policies

Macroprudential authorities have employed various tools to restrict the issuance of loans with high debt service obligations relative to borrower incomes. This section outlines key details of the specific policies implemented across the economies included in our study, highlighting their design and application.

The most obvious distinction is that some authorities have implemented limits on the debt service-to-income (DSTI) ratio, while others have opted for limits on the debt-to-income (DTI) ratio. Among those that have introduced direct DSTI limits at loan origination, the specifics of the policies often reflect the structure of housing finance in the respective economies.

For instance, in France, where nearly all housing loans feature fixed interest rates for the entire loan term, the DSTI limit is based solely on the DSTI ratio at origination. In contrast, in Hong Kong, where the majority of housing loans have floating interest rates or only fixed for a short period, the DSTI limit incorporated a stressed interest rate. This stress test added a buffer to the current interest rate on the loan to calculate a stressed DSTI, ensuring borrowers can continue to repay if interest rates were to increase in the future.[3] Similarly, Korea has implemented DSTI limits at loan origination. To reflect potential increases in interest rates, a stressed interest rate has been applied to floating-rate loans since 2024, which account for roughly one-third of housing loans.

DSTI limits are usually paired with loan maturity limits. If loans have regular amortisation schedules, borrowers can reduce their DSTI ratios by lengthening the maturity of the loan. To address this loophole, authorities have introduced maturity caps alongside DSTI limits. In France, the maximum maturity of the loan is capped at 25 years. In Hong Kong, it is capped at 30 years while in Korea, the maximum maturity for DSTI calculation is capped at 40 years for loans with

[3] On 28 February 2024, the Hong Kong Monetary Authority suspended the interest rate stress testing requirement for property mortgage loans, having considered the relatively low probability of a further increase in mortgage interest rates in Hong Kong in the near term.

original maturities exceeding 40 years.

DTI limits are another form of DSTI constraint. Specifically, a DTI limit can be expressed as a DSTI limit based on a fixed interest rate and a given loan maturity. Because of this property, DTI limits have often been the preferred I-BBM in economies with a higher prevalence of floating or short-term fixed rate housing loans, such as Ireland, New Zealand, and the United Kingdom.

The Netherlands also employs de facto DTI limits, derived from DSTI-based standards (the so called Nibud norms), with notable distinctions. The limits are tailored to mortgage interest rates, household income, borrower characteristics, particularly proximity to retirement. In addition, the limits are recalibrated annually to reflect broader macroeconomic developments and distributional considerations. Although the limits were formally introduced in 2013, lenders had already been using them as guidelines in the years prior.

The Netherlands is not alone in tailoring I-BBM limits based on borrower type. Many macroprudential authorities differentiate between first-time buyers and other borrowers. For instance, in Ireland, first-time buyers are permitted to borrow at higher DTI multiples compared to second-and-subsequent buyers. In New Zealand, DTI limits vary between owner-occupiers and investors. In Hong Kong, for many years the DSTI limits depended on whether the property is intended for self-use or other purposes, though more recently, the limits have been standardised across borrowers. In Korea, the DSTI limit currently applies only to larger loans exceeding 100 million KRW (approximately $70,000); however, certain types of loans, such as lump-sum rental deposit loans (commonly known as jeonse), interim payment loans for new constructions, and housing loans provided by public institutions, are exempt from the DSTI requirement.

Some macroprudential authorities allow a portion of new lending to have DSTI/DTIs above the I-BBM limits, providing flexibility for specific cases. These exemptions are referred to as flexibility margins in France, allowances in Ireland, speed limits in New Zealand, and flow limits in the United Kingdom.[4] In some cases, these exemptions are further tailored to specific borrower groups. For

[4]See Committee on the Global Financial System (2023) for specific details on these exemptions.

instance, in France, at least 70% of the flexibility margin must be allocated to loans for primary residences, with at least 30% specifically reserved for first-time buyers. In other economies, I-BBMs are implemented on a 'comply or explain' basis, which also provides the possibility of lending above I-BBM limits.

4. Data

A key contribution of our paper lies in leveraging microdata on the distribution of new loans based on borrowers' DSTI or DTI ratios at origination. This approach allows us to go beyond merely analysing changes in these measures. It also enables us to capture the extent to which I-BBMs constrain lending over time, providing a more comprehensive assessment of their impact. This is particularly relevant, as I-BBMs have often been implemented as structural policies or guardrails, designed to maintain a fixed calibration of the instrument over time.

For France the DSTI series are based on bank-level reporting implemented by the ACPR starting in 2012. Since 2022, the reporting covers all banks. Before that, it covered the largest institutions. For the period prior to 2012, data on the distribution come from an annual ACPR survey and are interpolated to obtain them at a quarterly frequency.

For Hong Kong, the data are obtained from the Hong Kong Monetary Authority (HKMA). Through its Granular Data Reporting (GDR) programme, the HKMA has started collecting loan-level outstanding residential mortgage data at a monthly frequency since 2020. To extend the series back in time, the pre-2020 data is constructed using information on loans outstanding in 2020. Thus, for these earlier years, the data may not be fully representative of the profile of all originated mortgages. Due to this consideration, we use our granular data for Hong Kong from 2014 onwards.

For Ireland, data on the LTI distribution of new mortgages cover mortgages for property purchases and self-build purposes only, excluding buy-to-let purposes. These data cover mortgages both in and outside the scope of the mortgage measures. Data were originally provided by five major retail banks operating in Ireland to describe their balance sheet exposures to mortgages, beginning in 2011 (for three banks) and 2015 (for the other banks). Earlier data, from 2006, are based on estimates of loans that remained on the banks' balance sheets until the first observation date. This means that these earlier years, the sample may be less representative of the profile of all originated mortgages. From 2015 onwards, the Central Bank of Ireland's monitoring template data is used.

For Korea, we use a borrower-level panel dataset spanning from 2012 to 2024 on a quarterly basis, representing approximately 2.4% of the population. A loan is classified as new lending if the borrower appears for the first time and the loan is originated in the same quarter. For the Netherlands, we use loan-level data obtained from the De Nederlandsche Bank (DNB), covering the period from 2009 Q1 to 2022 Q1. The dataset includes borrower income, age, loan characteristics, which we use to calculate the DTI at origination. Data for the UK comes from UK Finance, Office of National Statistics and Bank of England's calculations from the Financial Conduct Authority's Product Sales Data which covers the period Q2 2005 to Q3 2024.

The other data we use in our analysis come from national sources. They consist of the interest rate on new loans for house purchases, housing loans granted to households deflated by CPI, gross disposable income of households deflated by CPI, real gross fixed capital formation for dwellings, national house price indices deflated by CPI and the yield on the 10-year government bond index.

5. Estimation and Identification

In this paper, we adopt the identification strategy proposed by Elsayed et al. (2025) to examine the impact of I-BBMs on the economy. This approach involves two key steps which we outline below. First, lending standards shocks are identified within an SVAR framework using the external instrument methodology following Bassett et al. (2014). Second, Elsayed et al. (2025) assume that these lending standards shocks can be decomposed into two components: shocks to banks' own lending standards (bank lending standards shocks), representing what would have occurred in the absence of I-BBMs, and shocks to lending standards directly attributable to the influence of I-BBMs (I-BBM induced shocks). We use their methods to disentangle these two components.

After identifying I-BBM induced shocks following the methodology outlined in Elsayed et al. (2025), we undertake two key exercises to evaluate the costs and potential stabilising effects of these policies. First, we analyse whether I-BBM induced shocks have historically offset or amplified bank lending standards shocks. Second, we utilise historical decompositions from our SVAR model to assess the degree to which I-BBMs have restricted household access to high DSTI/DTI loans and to evaluate their macroeconomic stabilising benefits.

5.1 Identifying lending standards shocks

To identify lending standards shocks we use the SVAR external instrument identification strategy of Stock (2008) and Mertens and Ravn (2013). For each of the seven economies in our study, we individually estimate a VAR of the form

$$y_t = b + \sum_{k=1}^{p} A_k y_{t-k} + \sum_{k=0}^{q} B_k x_{t-k} + u_t \tag{1}$$

where the endogenous variables, y_t, in the VAR include: quarterly growth rates in house prices based on the national house price index, residential investment, outstanding housing loans,[5] household income, which are all deflated by the national consumer price index. We also include the lending rate on new housing loans. In addition, we include the yield on the 10-year government bond as an exogenous variable, x_t in the VAR, b is a vector of constants and u_t is the vector of reduced form residuals. In our baseline specification we include four lags of the endogenous and exogenous variables.

To recover the structural lending standards shocks from the VAR, for each economy we use responses to bank lending surveys (BLS) to derive an external instrument.[6] As the net diffusion

[5] For Ireland we use new lending.

[6] Note that Hong Kong does not survey on banks' credit standards for loans to households. Instead, we identify credit supply shocks from a BVAR with sign- and zero-restrictions, where we identify a credit supply shock as one

index of reported credit standards in the BLS is likely to be endogenous to the state of the economy, we construct our instrument by first purging the BLS credit standards net diffusion index for any correlations with reported changes in household demand for loans as well as lags of key macroeconomic variables. Specifically, first regress the BLS net diffusion index for credit standards on housing loans on the contemporaneous BLS net diffusion for changes in housing loan demand and on lags of real GDP growth, changes in the unemployment rate, changes in the short-term interest rate and the 10-year interest rate. We then take the residual from this regression as our instrument.[7]

To achieve point identification of the lending standards shocks from the SVAR we follow Plagborg-Møller and Wolf (2022) and impose a recoverability assumption to derive our instrument for the lending supply shock.[8] Table 1 shows that, in five of the six economies for which we implement the external instrument approach, the first-stage F-statistic is around or above 10, suggesting that instrument is not weak. However, for New Zealand, the F-statistic of just under four suggests that care must be taken when interpreting the responses to lending standards shocks.

Model	**F-Statistic**
HK	Identified via sign/zero restrictions
IE	16.91
KR	9.49
NL	13.56
NZ	3.69
UK	10.49
FR	11.03

Table 1: **Instrument strength of the proxy variable used for the identification of the lending standard shocks:** First-stage F-statistics

The impulse responses that we obtain from the SVARs for each economy are consistent with

where the volume of mortgage credit and the price move in opposite directions.

[7] We use estimates of the shadow rate for the period when short-term interest rates were at the zero lower bound for France, Netherlands and the United Kingdom. We also include lags of equity market volatility indices for these economies as well as New Zealand.

[8] See Elsayed et al. (2025) for additional details.

the literature. Across all the economies, a tightening of lending standards result in declines in the volume of housing loans and house prices (Figure 1). However, the impact on mortgage lending rates differs across economies. For the majority of economies (five out of seven), lending rates increase. In these economies, lending standards shocks can be viewed as credit supply shocks, reducing the quantity of credit and increasing its cost. However, for two economies (Ireland and New Zealand), aggregate shocks are associated with declines in the average lending rate. This might be due to a reallocation of lending towards safer borrowers. This would be consistent with Acharya et al. (2022) who find such reallocation when macroprudential measures were introduced in Ireland.

Measures of economy activity tend to weaken in response to a tightening in lending standards. Real house prices tend to experience a sustained decline in their growth rates. The lower two panels of Figure 1 indicate that a contractionary lending standards shock tends to result in a reduction in the growth rates of residential investment across all the economies in our study. Our results also suggests that real incomes also tend to decrease in response to a contractionary lending standards shock, with the exception of Korea.

5.2 Identifying I-BBM induced shocks

The identification strategy of Elsayed et al. (2025) crucially assumes that when banks respond to bank lending surveys, their answers incorporate the influence of the I-BBM policy when they report a tightening or loosening of lending standards. Specifically, they assume that the shocks to lending standards on housing loans recovered in subsection 5.1 are the sum of banks' own lending standards that would have prevailed in the absence of the I-BBM (bank lending standards shocks), and shocks to lending standards directly attributable to the influence of I-BBMs (I-BBM induced shocks).

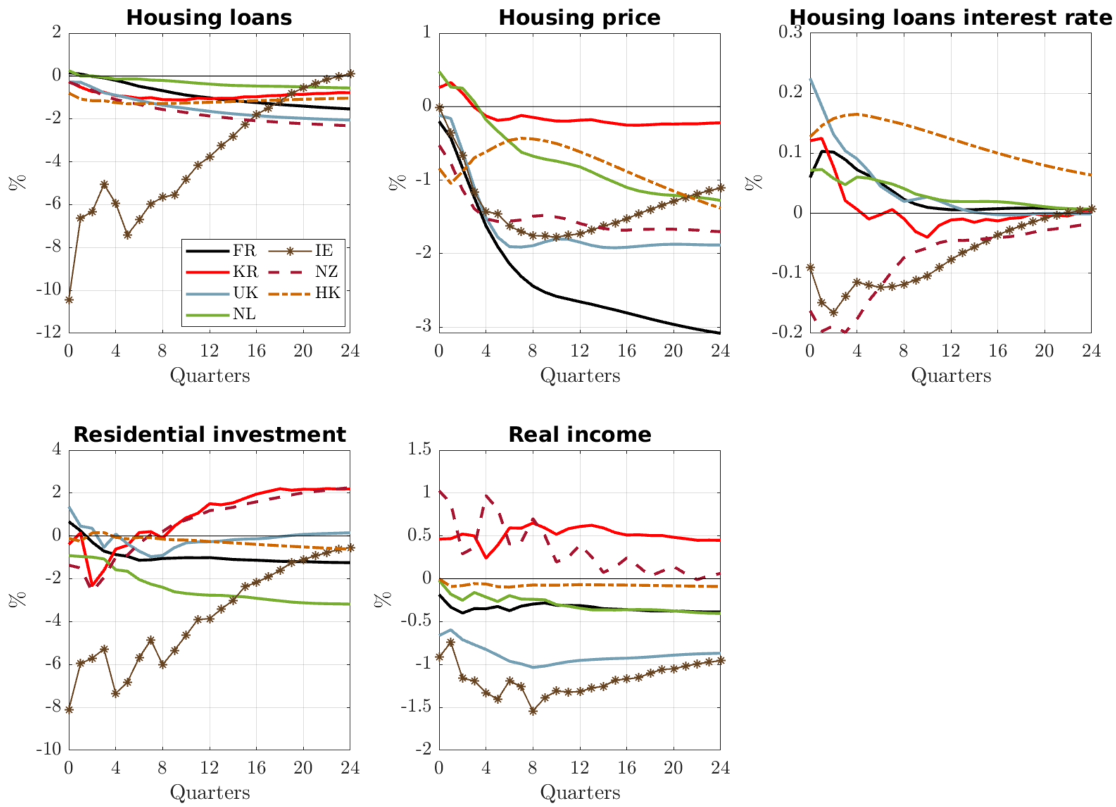

Figure 1: **Lending standards shocks drive the housing cycle:** Impulse responses to a 1 standard deviation-contractionary lending standards shock. Housing loans: quarterly growth rate of real amounts of outstanding housing loans except for Ireland where the dotted line shows the growth rate of new housing loans. Housing price: quarterly growth rate in the economy wide price index in real terms. Housing loans interest rate: interest rate on new housing loans. Residential investment: quarterly growth rate in real residential investment. Real income: quarterly growth rate in real household income, except for Ireland where the dotted line shows the quarterly growth rate of real personal expenditures.

$$\underbrace{\epsilon_{1,t}}_{\text{lending standards shock}} = \begin{cases} \epsilon_{1,t}^{BANK}, & t < T^*, \\ \underbrace{\epsilon_{1,t}^{BANK}}_{\text{Bank induced}} + \underbrace{\epsilon_{1,t}^{BBM}}_{\text{I-BBM-induced}}, & t \geq T^*, \end{cases} \tag{2}$$

where $\epsilon_{1,t}$ is the lending standards shock identified in subsection 5.1, which is made up of a bank lending standards shock, $\epsilon_{1,t}^{BANK}$ driven by changes in banks' own risk management practices and an I-BBM shock, $\epsilon_{1,t}^{BBM}$. For periods before the introduction of the I-BBM, $t < T^*$, the observed lending standard shock is only driven by $\epsilon_{1,t}^{BANK}$. However, for periods after the introduction of the I-BBM, $t \geq T^*$, it is assumed that the lending standards shock is the sum of bank-induced component and a component driven by the I-BBM.

5.2.1 Treatment and control groups

To disentangle the I-BBM shock from the bank lending standards shock, we turn to insights from the microeconometric literature. This literature uses either "treatment" and "control" groups or bunching around borrower-based limits to assess how BBMs impact the distribution of DSTI/DTI for newly originated loans (Defusco et al. (2019), Tzur-Ilan (2023), Gaffney (2022), HCSF (2024)). To assess the impact of BBMs on the distribution, this literature constructs counterfactual DSTI/DTI distributions by comparing the evolution of lending to borrowers not likely to be impacted by the DSTI/DTI limit, such as those far below the limit, to the evolution of lending with DSTI/DTIs close to the limit. By comparing the evolution of the group close to the I-BBM limit (treatment group) to that far below the limit (control group), they derive estimates of the share of lending impacted by the I-BBM policy.

We follow a similar strategy and identify treatment and control groups based on the specific details of the I-BBM policy implemented in each of the economies in our study. In all economies, the control group is identified as those with DSTI/DTI ratios at origination that are far from the I-BBM limit (Table 2). However, for the treatment group, we exercise judgment based on how the measure have been implemented and interpreted across the different economies. For some economies, we select the share of loans above the I-BBM limit. These include France, Ireland, New Zealand and the United Kingdom where flexibility margins and flow limits allow some lending to be above the I-BBM limit. For others that do not have flexibility margins, the treatment group is defined by the share of loans just below the I-BBM limit, which captures the extent to which policy measures lead to bunching in the origination of loans with DSTI/DTIs just under the I-BBM limit.

Table 2 also lists the date T^* that we use to classify the start of the I-BBM in each economy.[9]

	Control Far below limit	**Treatment** Just below limit	Above limit	T^*
France	DSTI $<$ 20% and 10 $<$ Maturity $\leq$ 15 years		DSTI $>$ 35% and Maturity $>$ 25 years	2019 Q4
Hong Kong[1]	Mortgages not affected by macroprudential policy changes and DSTI within 10% of current limit	Mortgages affected by DSTI policy change and DSTI within 10% of current limit		2015 Q1
Ireland	2.5 $<$ LTI $\leq$ 3		LTI$>$ 3.5	2015 Q1
Korea	5% $<$ DSTI $\leq$ 15%		DSTI $>$ 40%	2019 Q4
Netherlands	DTI $<$ 60% of borrower-specific Nibud limit	90% $<$ DTI $\leq$ 100% of limit		2013 Q1
New Zealand[2]	Owner occupiers: DTI $<$ 3 Investors: DTI $<$ 3		DTI $>$ 6 DTI $>$ 7	2024 Q3
United Kingdom	LTI $<$ 3		LTI $>$ 4.5	2014 Q3

Table 2: **Definition of control and treatment groups used for the identification of I-BBM induced shocks.** T^* denotes the quarter after which we decompose the lending standards shock into in its bank-induced and I-BBM-induced components. Prior to T^*, the bank lending standard shock is equal to the lending standards shock. In all economies the treatment series is the share of loans originated far from the I-BBM limit as defined in the second column. The treatment series depends on the specific implementation of the measure in the economy. It is either defined as the share of loans in bucket just below the I-BBM limit (third column) or the share above the limit (fourth column). [1] Lending standards shock decomposition for Hong Kong is not implemented in the results of the current draft. [2] Post I-BBM sample too short for reliable estimates of BBM component. In the Netherlands, the Nibud defines the the maximum percentage of annual disposable income that can be spent on mortgage payments (interest and amortisation) which vary systematically with mortgage interest rates, borrower income and age, and are updated and published annually.

Figure 2 plots the evolution of the treatment and control groups for France. The top-left panel plots the share of lending in the treatment group, i.e. loans with DSTI ratios greater than the 35% limit. Prior to the implementation of the DSTI limit, around 25% of newly originated loans exceeded the limit in each quarter. However, once the limit was introduced in 2020 Q1, this plunged to 15%. The bottom-left panel plots the evolution of lending to the control group, i.e., loans with a DSTI of less than 20%, far below the DSTI limit in France. In the years prior to the DSTI measure, the share had also been roughly stable, at around 11% of total lending. The control series

[9] It is worth noting that the identification strategy outlined below does not assume that the treatment and control groups are fully comparable and behave in the same way, before and after the implementation of the I-BBM policy; rather, it allows for differential responses – both in direction and magnitude – to the various macroeconomic structural shocks identified in the VAR model, including those related to lending standards.

also declined after 2021 Q1, though by significantly less and by a slower rate than the treatment group. As the French DSTI limit also included a maturity component, the right-hand panels show the share of lending to the treatment group with maturities greater than 25 years, while the lower panel shows the share of lending to the control group. Figure 3 shows the treatment and control groups for Korea, with the share of lending to both groups falling by around 6 percentage points after the introduction of the DSTI limit in late 2019.

For the Netherlands, we classify the treatment group as the bunching group with DTI's just below the I-BBM limit. As the limits in the Netherlands depend on borrower characteristics, we define it as borrowers with DTIs between 90 to 100% of the Nibud limits.[10] Although the DTI limit was formally introduced in 2013, lenders had already been using them as guidelines in the years prior. Relative to these limits, the share of lending to households in the bunching only starts to rise strongly around three years later in 2016 (Figure 4). This feature highlights the potential challenges in identifying the impact of BBMs from macroprudential policy changes, as they may not initially bind upon implementation.

[10]The Netherlands applies de facto DTI limits based on DSTI norms - commonly referred to Nibud norms - which define the the maximum percentage of annual disposable income that can be spent on mortgage payments (interest and amortisation). The DTI limits vary systematically with mortgage interest rates, borrower income and age, and are updated and published annually. We define treatment and control group based on each borrower's applicable DTI cap. The treatment group includes loans with a relative DTI between 90% and 100% of the limit, while the control group consists of loans with relative DTIs well below 60% of the norm.

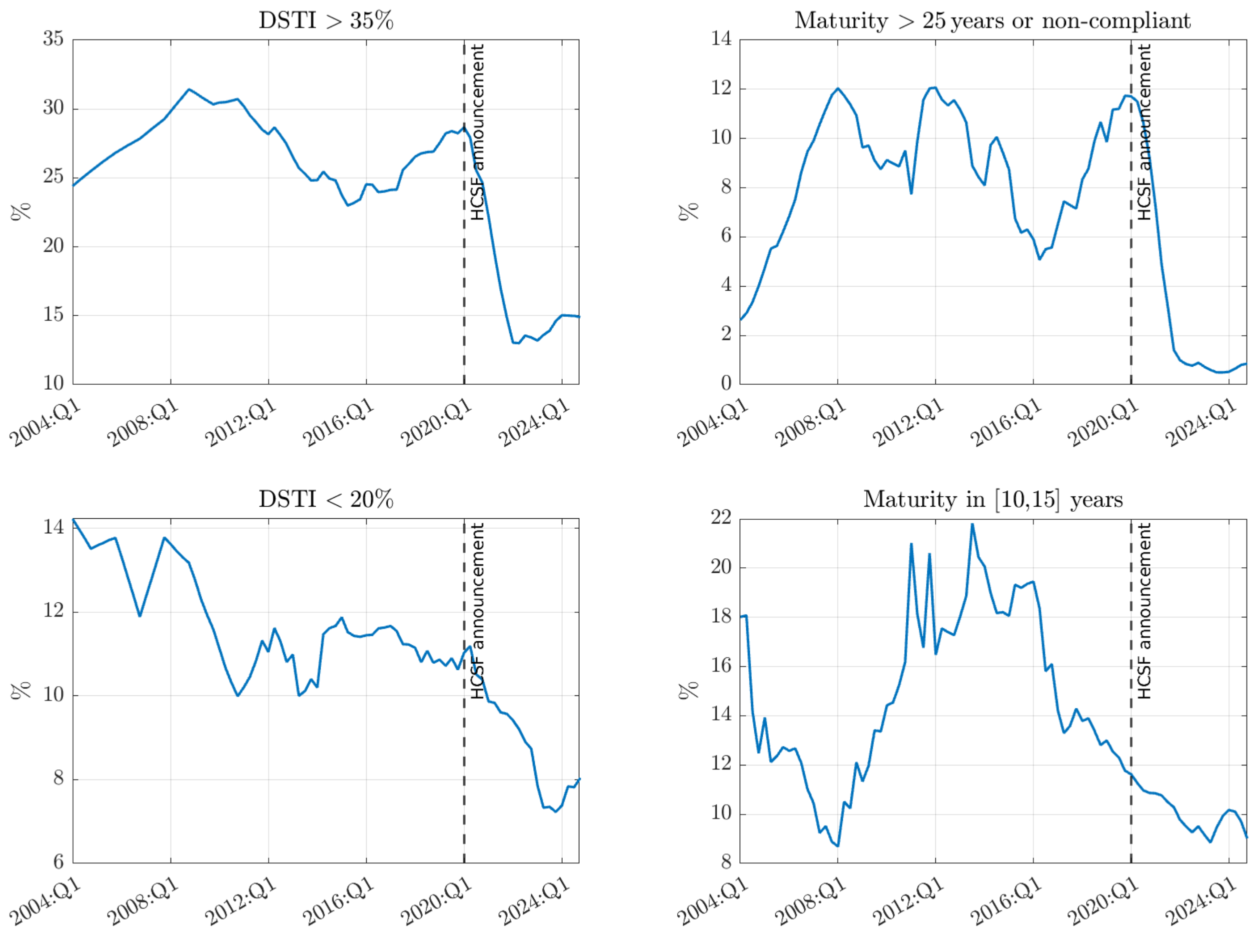

Figure 2: France: Share of total lending to treatment group (top) and control group (bottom)

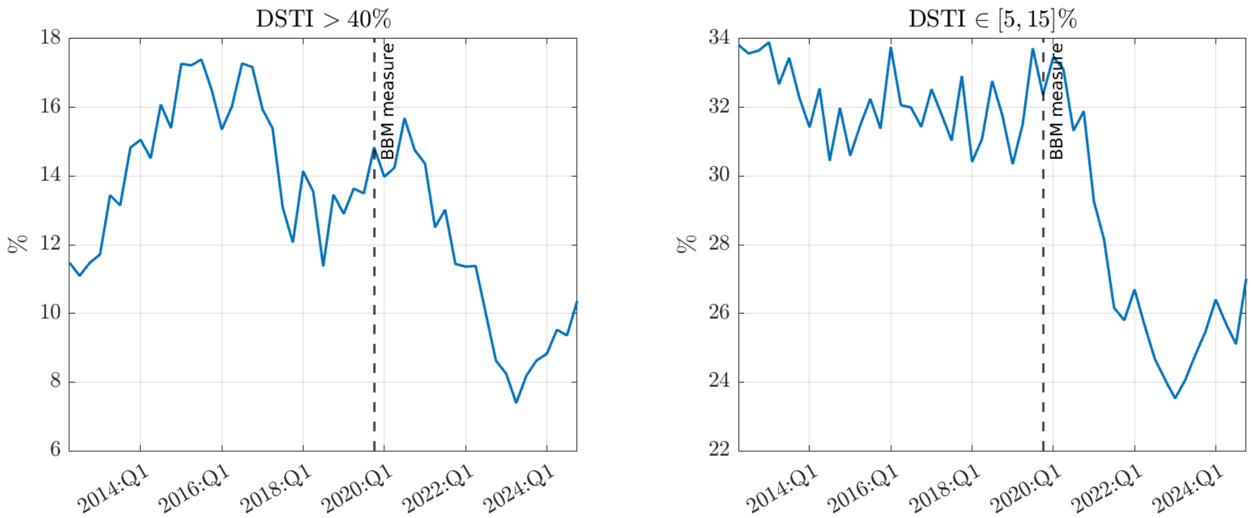

Figure 3: Korea: Share of total lending to treatment group (left) and control group (right)

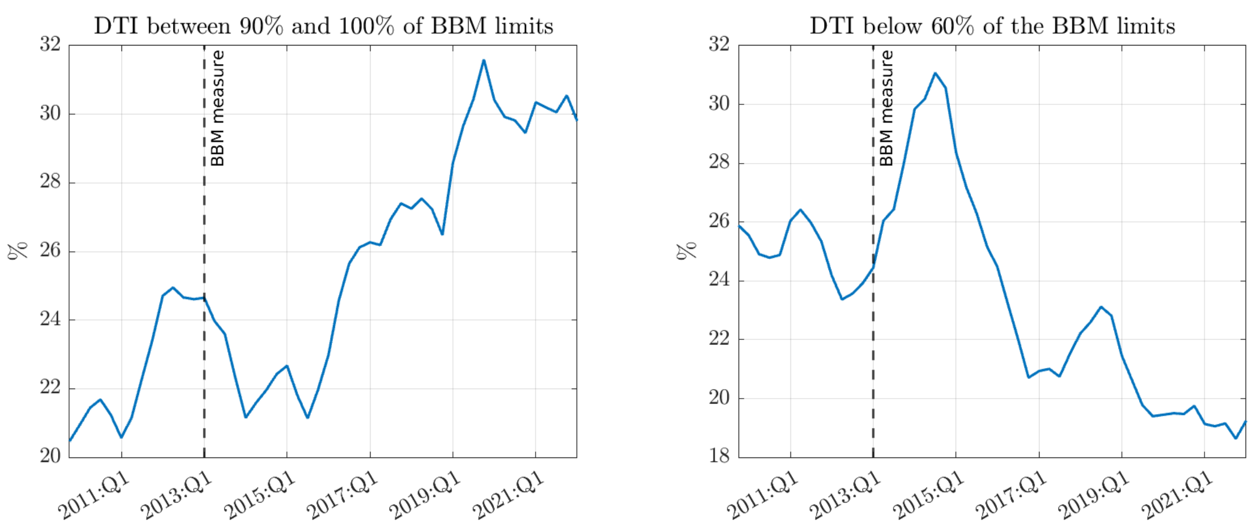

Figure 4: Netherlands: Share of total lending to treatment group (left) and control group (right)

5.2.2 Strategies to identify I-BBM induced shocks

We now briefly outline the two strategies we use to identify I-BBM-induced lending shocks. These strategies draw on the evolution of lending to the treatment and control groups.[11] As the share of loans in both the treatment group, f_t^{treat}, and control group, $f_t^{control}$, could themselves be driven by macroeconomic conditions, we first purge the fraction of loans in treatment and control groups for co-movements with lags of the endogenous macroeconomic variables in our VAR, i.e. $\{y_\tau\}_{\tau<t}$, with lags of and current values of our exogenous variable, $\{x_\tau\}_{\tau\leq t}$ and with the portions of the contemporaneous VAR residuals purged from the lending standard shocks, $\{\tilde{u}_t\}$ to account for the other macroeonomic structural shocks

$$\tilde{f}_t^j = f_t^j - E(f_t^j|\{y_\tau, f_\tau\}_{\tau<t}, x_t, \tilde{u}_t) \quad j \in \{treat, control\} \tag{3}$$

where $\tilde{f}_t^j$ thus captures variation in the treatment and control groups orthogonal to the macroeconomic structural shocks of the VAR not corresponding to the lending standards shocks. To identify the I-BBM shock from the lending standards shock, Elsayed et al. (2025) assume that the control series are uncorrelated with this component of the lending standard shocks such that:

$$E[\tilde{f}_{i,t}^{control}\epsilon_{1,t}^{BBM}] = 0 \tag{4}$$

whereas for the treatment series

$$E[\tilde{f}_{i,t}^{treat}\epsilon_{1,t}^{BBM}] \neq 0 \tag{5}$$

As a result, the treatment series load on both $\epsilon_{1,t}^{BANK}$ and $\epsilon_{1,t}^{BBM}$

$$\tilde{f}_t^{treatment} = \alpha_{treat,1}\epsilon_{1,t}^{BANK} + \alpha_{treat,2}\epsilon_{1,t}^{BBM} + \psi_{treat,t} \tag{6}$$

[11] See Elsayed et al. (2025) for additional details.

while the control series only load on $\epsilon_{1,t}$

$$\tilde{f}_t^{control} = \alpha_{control,1}\epsilon_{1,t}^{BANK} + \psi_{control,t} \tag{7}$$

Elsayed et al. (2025) then propose two strategies to recover the BBM shock. Both methods rely on estimating the equations 6 and 7 separately in the pre- and post-BBM samples but with different constraints:

1. The "variance minimisation method", our baseline identification strategy, involves selecting the sequence of $\epsilon_{i,t}^{BBM}$ and $\alpha_{treat,2}$ that minimises the difference between the variance-covariance matrix of innovations to bank lending standard shocks estimated in the pre- and post-BBM samples.

2. The "K-method" assumes that the lending standards shock $\epsilon_{1,t}$ is a weighted sum of the $\epsilon_{1,t}^{BBM}$ and $\epsilon_{1,t}^{BANK}$, with weights k and $(1-k)$ respectively. To pin down the parameters, the method assumes that $\alpha_{control,1}$ does not change between the pre-and post-BBM sample periods and that

$$\epsilon_{1,t}^{BBM} = \begin{cases} 0, & t < T^*, \\ k\,\epsilon_{1,t}, & t \geq T^*. \end{cases} \tag{8}$$

This implies that for $t < T^*$

$$\tilde{f}_{\text{control},t} = \alpha_{\text{control},1}\,\epsilon_{1,t} + \zeta_{\text{control},t},$$

$$\tilde{f}_{\text{treat},t} = \alpha_{\text{treat},1}\,\epsilon_{1,t} + \zeta_{\text{treat},t}.$$

and for $t \geq T^*$

$$\tilde{f}_{\text{control},t} = \alpha_{\text{control},1}\,\big(1-k\big)\,\epsilon_{1,t} \;+\; \zeta_{\text{control},t},$$

$$\tilde{f}_{\text{treat},t} = \big[\alpha_{\text{treat},1}\,\big(1-k\big) \;+\; \alpha_{\text{treat},2}\,k\big]\,\epsilon_{1,t} \;+\; \zeta_{\text{treat},t}.$$

Hence, if the parameter $k > 0$ (resp. $k < 0$), the I-BBM-induced component tends to move in the same (resp. opposite) direction as the lending standards shocks. However, if $|k| > 1$, the two components of the lending standard shocks move in opposite directions, and the I-BBM-induced shocks can be interpreted as acting as a stabiliser with respect to the bank-induced lending standards shocks.

5.3 Additional considerations

While the empirical strategy of Elsayed et al. (2025) represents a significant step towards identifying the influence of macroprudential policy on the economy, caution is warranted when interpreting the results.

One important limitation of the empirical methodology is that it captures the effects of I-BBMs only on borrowers who actually take out housing loans – that is, it estimates the impact of macroprudential policy on the share of constrained loans conditional on borrowing (*intensive margin*). While our VAR model includes total lending volumes, and can therefore capture the overall impact of I-BBMs on lending activity, the second step of the empirical approach deployed to estimate the share of loans constrained by I-BBMs does not account for the policy's influence on the ultimate decision to borrow in the first place. Consequently, the methodology may underestimate the full effect of I-BBMs by excluding their potential role in limiting credit access at the extensive margin.

Another concern is that our estimation strategy does not account for the influence of other macroprudential policies. This is important because measures such as LTV limits have often been implemented at the same time as I-BBMs. For France and the United Kingdom, this is not an issue as there are no macroprudential LTV limits. But for other economies in our study, LTV limits were either introduced at the same time or have been adjusted alongside changes in I-BBM limits.[12]

How might ignoring the potential impact of LTV limits affect our results? By overlooking LTV constraints, we risk misclassifying some housing loans as unconstrained simply because their values fall well below the I-BBM limit, when in reality they may be restricted by LTV requirements. If the LTV limit constrains lending to the control group, while the treatment group is largely impacted by the I-BBM limit, this would tend to result in an underestimation of the I-BBM effects as lending growth in the control group would tend to be weaker. Another possibility is that the loans that end up in our treatment group are impacted by both limits, such that even in the absence of the I-BBM, households would not have sufficient liquid assets to meet the LTV limit. In this case, by omitting the LTV limit, we may overestimate the impact of the I-BBM on lending.

6. Do I-BBM induced shocks offset bank lending standards shocks?

Bank lending standards tend to be procyclical (Berger and Udell (2004)). As a first step in assessing the potential stabilizing effects of I-BBM-induced shocks, we examine whether the identified I-BBM-induced shocks, $\epsilon_{1,t}^{\text{BBM}}$, amplify or counteract bank-induced lending standards shocks, $\epsilon_{1,t}^{\text{BANK}}$. The shock series derived from both the minimum variance approach and the K-method suggest a stabilising effect, indicating that I-BBM-induced shocks tend to offset the procyclicality of bank-induced lending standards.

Figure 5 plots the I-BBM-induced shocks against the bank-induced lending standards shocks obtained from the variance-minimisation method. The strong negative correlation obtained using this approach indicates that, on average, I-BBM-induced shocks counterbalance bank-induced lending standards shocks. Similarly, Table 3 reports estimates of the parameter k from the K-method,

[12] Note that such concerns have, to some extent been addressed for Hong Kong by choosing selecting control and treatment groups that exclude lending to that could be impacted by LTV limits.

which measures the extent to which I-BBM-induced shocks offset bank-induced lending standards shocks. In most economies, point estimates of k exceed one, implying that I-BBM-induced shocks are negatively correlated with bank-induced lending standards shocks. Even when k falls below one, the estimates remain close to unity, indicating a low contribution of the bank-induced component to the overall lending standards shocks and therefore a low correlation between its two components. In summary, results from both the variance-minimisation technique and the K-method consistently indicate that I-BBM-induced shocks tend to offset bank-induced lending standards shocks, supporting their stabilising role.

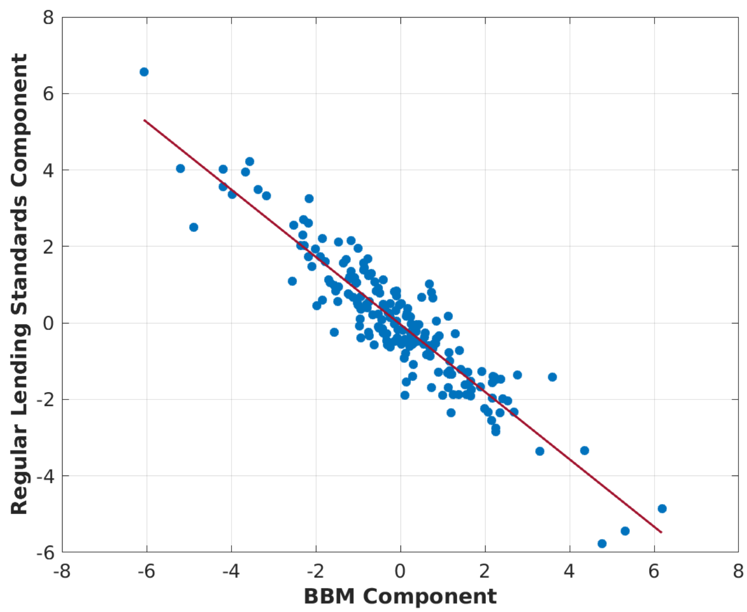

Figure 5: **I-BBM-induced shocks tend to offset bank-induced lending standards shocks:** This figure shows the identified I-BBM-induced shocks plotted against the identified bank-induced lending standards shocks. Each dot plots the shocks in a given quarter and economy.

Economy	**Estimated k**	**Confidence Interval**
France	1.04	[0.40, 1.68]
Hong Kong	0.998	[0.74, 1.28]
Ireland	0.94	[0.04, 1.85]
Korea	1.36	[0.20, 2.52]
Netherlands	1.14	[0.54, 1.74]
United Kingdom	1.29	[0.69, 1.99]

Table 3: Estimates of k with corresponding one standard deviation confidence intervals

7. Quantifying the costs and benefits of I-BBM policies

In this section, we use the historical forecast decompositions from the SVAR to evaluate the costs and benefits of I-BBM policies. Specifically, we compare actual outcomes to counterfactual scenarios in which I-BBM induced shocks are absent. With these series we assess the costs – by measuring the extent to which I-BBMs restricted borrowers' access to high DSTI or DTI loans – and the benefits – by assessing the extent to which I-BBMs stabilised key macroeconomic variables. We further shed light on the cost-benefit tradeoffs by comparing Sharpe ratios of key macroeconomic variables in the actual and counterfactual data. Our baseline findings are derived using the variance-minimisation method. Alternative results based on the K-method are presented in section 9.

7.1 The costs of I-BBM policies

Our econometric framework provides a natural measure of perhaps the most visible and politically sensitive cost of I-BBM policies – the share of high DSTI/DTI lending constrained by the I-BBM measures. To estimate this cost, we first compute the counterfactual share of loans in the treatment group that would have occurred in the absence of the I-BBM policy by assigning zero to the I-BBM-induced lending standard shocks in Equation 6. We then compute the associated costs of the policy – the share of high DSTI/DTI lending constrained by the I-BBM measures – as the difference between the share of lending in the treatment group in the actual series compared to counterfactual series.

For economies where the treatment group is defined as the share of new loans above the I-BBM limit, the counterfactual is simply computed as the share of new loans in this basket that would have prevailed in the absence of I-BBM induced shocks. For this group, we defined the share of constrained loans as the difference between the share of new lending to the treatment in the counterfactual minus the share of new lending to the treatment group in the actual data. For economies where the treatment group is defined as lending in the bucket just below the I-BBM limit (i.e. the bunching group), we define the share of constrained lending to be the difference between the share of new loans in the bunching group in the actual data and the share new lending in the bunching group in the counterfactual series.

On average, the estimated share of constrained lending is relatively limited. Pooling all data across all of the six economies and all time periods, Panel A of Figure 6, shows that the mean share of high DSTI/DTI lending that was constrained by I-BBMs was on average around 2% of total new lending in any quarter since the measures were introduced ($t \geq T^{*}$).[13] The median is somewhat lower at just above 0.5% of new lending. The interquartile rate indicates that in at least 75% of quarters across the six economies, less than 1% of high DSTI/DTI lending was constrained.

Panel B shows summary statistics of the maximum share of constrained lending since the introduction of I-BBM measures. It shows that the average of the maximum shares of constrained new lending recorded across the six economies was about 5% of total new lending, while the median of the maximum shares was about 1%. It is important to note that there are also periods when the share on constrained households are instead higher in the counterfactual series that exclude I-BBM induced shocks (see section 8).

Our estimates of the share of constrained loans due to the I-BBM policy are consistent with, though somewhat towards the lower end of estimates obtained via bottom-up approaches. For example, for France, estimates based on the bottom-up approach indicate that around 6% of total

[13] Alternatively expressed, the share of high DSTI/DTI lending in total new lending is on average 2 percentage points larger in the counterfactual compared with to the actual data.

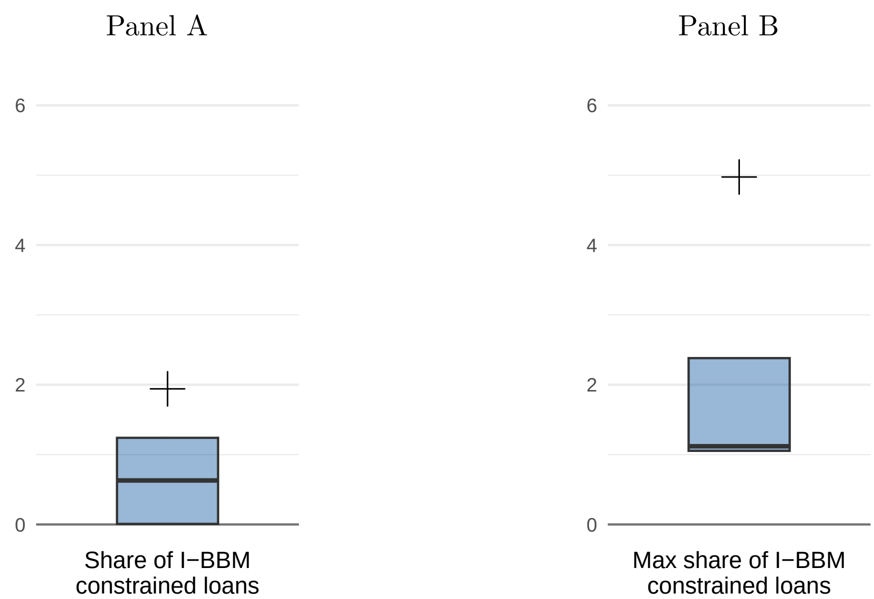

Figure 6: **Costs of I-BBMs:** Share of lending that was constrained by the I-BBMs. Panel A of this figure plots the mean, the median and interquartile range (denoted by a +, horizontal line and box, respectively) of the constrained share of new lending computed from pooling observations across all of the six economies (FR, GB, HK, IE, KR and NL) for which we compute I-BBM-induced shocks and all time periods for $t \geq T^*$. For economies where the treatment group is defined as the share of new loans above the I-BBM limit, the share of constrained lending is defined as the difference between the share of new lending to the treatment in the counterfactual minus the share of new lending to the treatment group in the actual data. For economies where the treatment group is defined as lending in bunching group just below the I-BBM limit, the share of constrained lending is defined as the difference in the share of new loans in the bunching group in the actual data minus the share new lending in the bunching group in the counterfactual series. Panel B plots the mean, median and interquartile ranges of the maximum share of constrained lending from each of the six economies.

new loans were constrained by the DSTI limit (HCSF (2024)).

7.2 The benefits of I-BBM policies

We now turn to assessing the benefits of I-BBM policy. We quantify the benefits by assessing the extent to which I-BBM policy stabilised the evolution of macroeconomic variables in our VAR. We do this over the period spanning from 2019 Q1 to 2024 Q3. This period witnessed significant macroeconomic shocks, including the Covid-19 pandemic, the post-Covid inflation surge and a significant tightening of monetary policy to bring inflation back down.

Our estimates suggest that I-BBMs meaningfully stabilised key macroeconomic variables over this period. Figure 7 shows that, on average, the standard deviation of real income growth and residential investment growth in the actual data was around 7% to 10% lower than that of the counterfactual series computed in the absence of the I-BBM induced shocks. Moreover, our estimates suggest that I-BBMs also stabilised house price growth by a similar magnitude. The interquartile ranges further indicate that the decline in the volatility of these variables is quite consistent across the six economies in our sample. For aggregate housing loan growth, however, we do not find any consistent pattern across the six economies.

To put our estimates of the reduction of volatility into perspective, Stock and Watson (2003) estimate that during the Great Moderation (1984-2002), the standard deviation of real GDP growth declined by around 35% in the United States, compared to the 1960-1983 period, while declining by between 30% to 50% in France, Germany, Italy, Japan and the United Kingdom. Thus, our estimates indicate that the reduction in volatility due to I-BBMs on income, may about one quarter of the volatility reduction that occurred during the Great Moderation.

We find a similar pattern when computing the volatility using the interquartile range, suggesting that these findings are not driven by a few outliers. Quantitatively, Figure 8 shows that, on average, the volatility of the real data for real income growth, house prices and residential investment growth is around 25% lower than that of the counterfactual series computed in the absence of I-BBM induced shocks. Turning to housing loan growth, measuring volatility with the interquartile range suggests that, if anything, I-BBMs may have resulted in higher volatility.

The results presented above indicate that I-BBMs result in a trade off. On the one hand, our estimates suggest that I-BBMs constrain access to high DSTI/DTI loans for some households by between 1% to 6% on average across the six economies. On the other hand they appeared to have dampened the volatility of real income growth and real house price growth by around 10%.

Figure 7: **Macroeconomic stabilisation from I-BBMs: impact on macroeconomic volatility (standard deviation).** This figure plots the volatility (as measured by the standard deviation) of the actual data as a ratio of the volatility of the corresponding counterfactual series computed in the absence of I-BBM induced shocks. The volatility is computed over the period 2019 Q1 to 2024 Q3. The figure shows the mean, the median and interquartile range (denoted by a +, horizontal line and box, respectively) of these ratio relative to the counterfactual series across the six economies for which we estimate I-BBM-induced shocks. Ratios less than one indicate that the volatility of the actual data was less than that of the counterfactual series. Ratios more than one indicate the opposite.

7.3 The growth volatility trade-off of I-BBMs

We also use our framework to assess the costs and benefits in terms of lost output relative to the stabilising benefits. To do so, we compute the Sharpe ratios for – the ratio of the mean growth rate of the variable over its standard deviation – for the key macroeconomic variables in our VAR model. Assessing the impact of I-BBMs on the growth volatility trade-off is somewhat similar in spirit to assessing the costs and benefits using GDP-at-risk analysis (Brandao-Marques et al. (2021)), though the latter focuses on the impact of these measures on downside risks.

We compute the Sharpe ratios for the observed data and the counterfactual series for each economy over the period since the introduction of I-BBMs, $t \geq T^*$, until 2024 Q3. In order to assess the impact of I-BBMs, we compare the difference between the Sharpe ratios computed from the actual data to the Sharpe ratios obtained from the counterfactual series.

On average, our estimates indicate that I-BBMs improve the growth volatility trade-off. The

Figure 8: **Macroeconomic stabilisation from I-BBMs: impact on macroeconomic volatility (IQR).** This figure plots the volatility (as measured by the interquartile range) of the actual data as a ratio of the volatility of the corresponding counterfactual series computed in the absence of I-BBM induced shocks. The volatility is computed over the period 2019 Q1 to 2024 Q3. The figure shows the mean, the median and interquartile range (denoted by a +, horizontal line and box, respectively) of these ratio relative to the counterfactual series across the six economies for which we estimate I-BBM-induced shocks. Ratios less than one indicate that the volatility of the actual data was less than that of the counterfactual series. Ratios more than one indicate the opposite.

Sharpe ratios for house price growth, housing loan growth, real incomes and residential investment are on average, somewhat higher in the actual data relative to the counterfactuals in the absences of I-BBMs. A higher Sharpe ratio implies that the macroeconomic variable delivers a higher average growth rate relative to its volatility, indicating a more efficient risk-return profile. The improvement in the Sharpe ratio is strongest for real income growth. In the case of housing loan growth, improvements in the Sharpe ratios are more uneven across economies. For some economies, the I-BBM policy seems to have reduced the ratio of loan growth rate to volatility, suggesting a slight deterioration in the risk-return trade-off.

Figure 9: **Impact of I-BBMs on Sharpe ratios.** This figure plots the difference in the Sharpe ratio of the actual data and the Sharpe ratio of the counterfactual series computed in the absence of I-BBM induced shocks. The Sharpe ratios are compute over the period since the implementation of I-BBMs, $t \geq T^*$. The figure shows the median and interquartile ranges of this difference in Sharpe ratios computed across the six economies for which we estimate I-BBM-induced shocks. Values greater than zero indicate that the Sharpe ratios in the actual data are higher than those computed with the counterfactual data. Values less than zero indicate the opposite.

8. Covid and the post-pandemic inflation surge

In this section, we use our estimates to analyse how I-BBM policy has influenced the evolution of key variables over the past half decade. Since 2020, economies have been buffeted by a sequence of substantial shocks. In early 2020, the COVID-19 pandemic triggered a sharp contraction in economic activity as governments worldwide implemented lockdown measures to contain the public health crisis. In some economies, significant monetary and fiscal stimulus measures helped jump-start economic activity and contributed to booming housing markets. Inflation surged to levels not seen in over thirty years, prompting a significant tightening of monetary policy in 2022 and 2023. To evaluate the impact of the I-BBM policy on the evolution of key variables during this period, we compare the actual data outcomes with the counterfactual series derived from historical decompositions that exclude the estimated I-BBM-induced shocks, following a similar approach as the one outlined above.

Our estimates indicate that I-BBMs constrained access to loans as housing markets boomed, but have more recently helped support access to high DSTI/DTI loans. Figure 10 plots the average evolution of constrained households from 2016 Q1 to 2024 Q3 after standardising the economy-level data. The figure shows that, on average across economies, the share of new loans affected by I-BBM policies remained relatively stable from 2016 through 2018. From 2018 onwards, however, our estimates indicate that the share of constrained loans increased notably, coinciding with an upswing in housing markets. During the initial months of the COVID-19 pandemic, the proportion dipped briefly but quickly rebounded. It then rose further during the pandemic-driven housing market boom, peaking in the third quarter of 2021 according to our estimates. Afterwards, as inflation surged and monetary policy tightened, our results indicate a decline in the proportion of loans that were constrained by I-BBM policy compare to the counterfactual.[14] This suggests that I-BBMs became less binding as the post-Covid inflation surge took hold and monetary policy tightened. By the end of the estimation period in the third quarter of 2024, the share of households impacted by I-BBMs had fallen to its lowest level since these measures were introduced.

I-BBM policies also appear to have contributed to the stabilisation of key macroeconomic variables since 2023. Figure 11 illustrates the evolution of the observed macroeconomic data with respect to the counterfactual series for each economy, constructed under the assumption of no I-BBM. The solid line plots the average difference between actual and counterfactual outcomes across economies in each quarter, while the shaded area represents one standard deviation around the mean.

The top-left panel of Figure 11 depicts the difference in house price growth dynamics between observed outcomes and those estimated in the absence of I-BBM policies. In both 2016 and 2019, house price growth was generally weaker than in the counterfactual scenario. More notably, since

[14]Hong Kong follows the Linked Exchange Rate System (LERS), which keeps the Hong Kong Dollar (HKD) trading within the Convertibility Zone of HK$ 7.75-7.85 to one USD dollar. The LERS restricts the Hong Kong Monetary Authority's discretion on matters of monetary policy. In accordance with the LERS and through an automatic interest rate adjustment mechanism, HKD interbank interest rates generally track their USD counterparts, with short-dated interest rates also respond to local supply and demand.

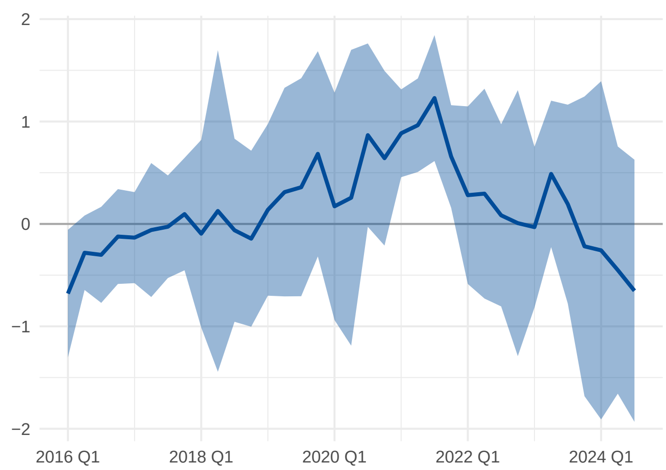

Figure 10: **Time series variation in the share of constrained lending due to the I-BBM policy.** This figure plots the mean and plus/minus one standard deviation interval of the standardised share of constrained new lending in each quarter across the six economies for which we estimate I-BBM shocks. For economies where the treatment group is defined as the share of new loans above the I-BBM limit, the share of constrained lending is defined as the difference between the share of new lending to the treatment in the counterfactual minus the share of new lending to the treatment group in the actual data. For economies where the treatment group is defined as lending in the bunching group just below the I-BBM limit, the share of constrained lending is defined as the difference in the share of new loans in the bunching group in the actual data minus the share new lending in the bunching group in the counterfactual series. Between 2016 Q1 and 2018, Q1, the sample consists of estimates from four economies. From 2019 Q1, the sample consists of six economies.

2023, annual house price growth has been, on average, around 3 percentage points higher than it would have been without I-BBMs. This suggests that the policies helped stabilise house prices during recent periods higher interest rates.

Our model also indicates that I-BBMs led to somewhat slower housing loan growth between 2016 and 2019. Between 2019 and 2021, however, the policies appear to have supported slightly stronger lending growth, before again exerting a dampening effect during 2022-2023. From 2024 onwards, in contrast, housing loan growth could have been substantially weaker in the absence of I-BBM measures.

The lower panels of Figure 11 further suggest that while I-BBMs may have initially restrained loan growth, they have since played a role in supporting broader macroeconomic outcomes. In particular, since 2020, the policies appear to have contributed modestly to stronger real income growth and, more recently, have helped sustain residential investment.

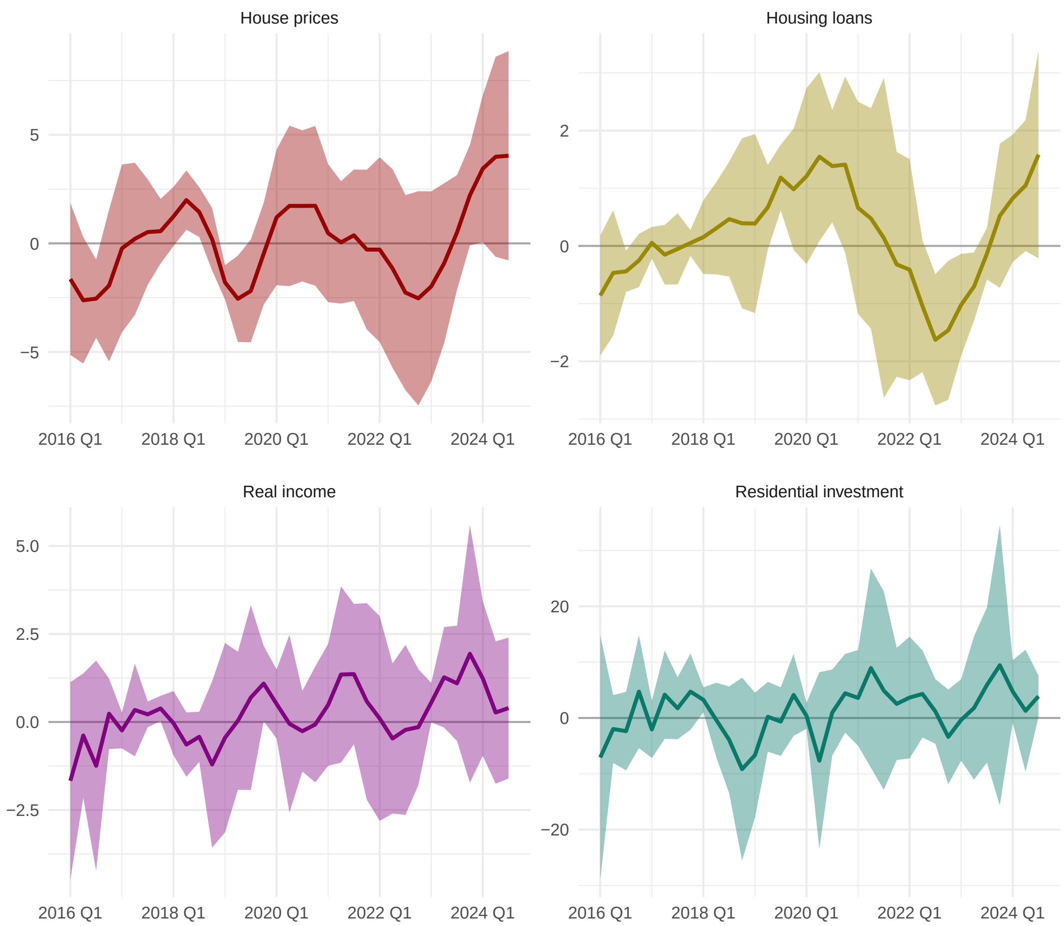

Figure 11: **Time-series impact of I-BBMs on macroeconomic variables.** This figure plots the mean and the plus/minus one standard deviation interval of the growth rates of the actual data relative to the counterfactual series computed in the absence of I-BBM policy in each quarter across the six economies for which we estiamte I-BBM shocks. Between 2016 Q1 and 2018, Q1, the sample consists of estimates from four economies. From 2019 Q1, the sample consists of six economies.

9. Robustness

We assess the robustness of our findings in several ways. First, we include robustness tests to assess whether the highly unusual Covid-19 pandemic and unprecedented policy support may bias our findings toward finding strong stabilising effects of I-BBMs. Second, given the novelty of the estimation framework, we assess the robustness of our findings to the alternative K-method identification of the I-BBMs shocks.

9.1 Robustness to excluding the Covid-19 pandemic

Clearly, conducting any counterfactual analysis over the Covid-19 pandemic period requires a health warning. The size of the shock and the unprecedented fiscal and monetary support during this period makes it particularly challenging to quantify the stabilising effects of these measures. A priori, it isn't clear how this unprecedented intervention may influence our results. One possibility is that we overestimate the impact of I-BBMs. For example, our VAR may indicate unusual dynamics for real incomes in particular, when fiscal support kicks, we may incorrectly assign this to the influence of I-BBMs if the evolution of lending in high DSTI/DTI is particularly weak during this period. To assess whether the highly unusual dynamics during the pandemic and and unprecedented support influences our findings, we present our baseline results based on the pre-Covid sample.

Overall, our main findings are robust to excluding the Covid-19 pandemic period. Figure 12 shows that the estimated share of loans constrained by I-BBMs remains broadly unchanged to the results from the full sample. Turning to the stabilisation benefits in the pre-Covid sample, where perhaps the concern of confounding effects is greatest, Figure 13 confirms the reduction in the volatility of real income growth and residential investment growth in the pre-Covid sample, and of similar magnitude to the post 2019 estimates in Figure 7. By contrast, Figure 13, shows that in the pre-Covid sample, the impact of I-BBMs may have been the opposite as during post 2019 Q1 sample, with higher volatility of house price growth and lower loan growth volatility. Although confounding factors may be relevant to explain this shift, it is equally plausible that the workings of the I-BBMs during the pandemic housing boom itself drove the shifting dynamics.

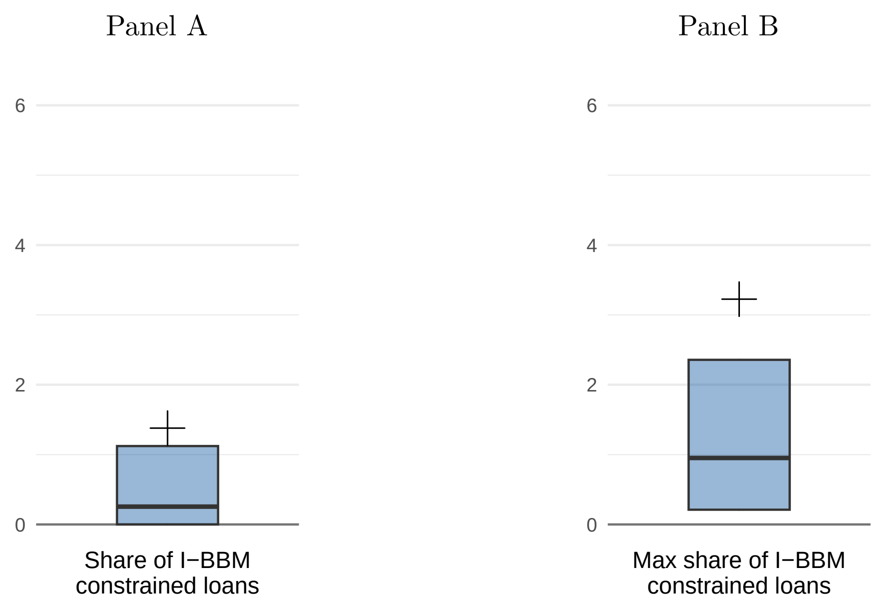

Figure 12: **Costs of I-BBMs, pre-Covid sample:** Share of lending that was constrained by the I-BBMs. Panel A of this figure plots the mean, the median and interquartile range (denoted by a +, horizontal line and box, respectively) of the constrained share of new lending computed from pooling observations across all of the economies for which we compute I-BBM-induced shocks and all time periods for $t \geq T^*$ until 2019 Q4. For economies where the treatment group is defined as the share of new loans above the I-BBM limit, the share of constrained lending is defined as the difference between the share of new lending to the treatment in the counterfactual minus the share of new lending to the treatment group in the actual data. For economies where the treatment group is defined as lending in bunching group just below the I-BBM limit, the share of constrained lending is defined as the difference in the share of new loans in the bunching group in the actual data minus the share new lending in the bunching group in the counterfactual series. Panel B plots the mean, median and interquartile ranges of the maximum share of constrained lending from the economies with I-BBMs before 2019 Q4.

9.2 Alternative estimation strategy

The estimated influence of I-BBM induced shocks are broadly consistent across the two identification strategies of Elsayed et al. (2025). Figure 14, plots the distribution of the share of constrained new lending pooled across economies and time periods based on the K-method. With this method, the estimated share of constrained loans tends to be similar, though somewhat smaller, both on average (panel A) and for the maximum values for each economy (panel B) when compared to the variance minimisation method.

Similarly, the K-method yields more modest estimates of the stabilisation effects. Compared to the variance-minimisation approach, Figure 15 shows a smaller reduction in the volatility of real

Figure 13: **Macroeconomic stabilisation from I-BBMs pre-Covid sample: impact on macroeconomic volatility (standard deviation).** This figure plots the volatility (as measured by the standard deviation) of the actual data as a ratio of the volatility of the corresponding counterfactual series computed in the absence of I-BBM induced shocks. The volatility is computed over the period from to $t \geq T^*$ until 2019 Q4. The figure shows the mean, the median and interquartile range (denoted by a $+$, horizontal line and box, respectively) of these ratio relative to the counterfactual series across the six economies for which we estimate I-BBM-induced shocks. Ratios less than one indicate that the volatility of the actual data was less than that of the counterfactual series. Ratios more than one indicate the opposite.

income growth between 2019 and 2024. Moreover, K-method estimates suggest that house price growth and, in particular, residential investment growth may have been more volatile than in the counterfactual scenarios that exclude I-BBM-induced shocks.

Although the stabilisation benefits appear more modest, the K-method estimates nonetheless indicate a broad improvement in the Sharpe ratios of key macroeconomic indicators (Figure 16). For instance, in line with the variance-minimisation approach, the Sharpe ratios for real income growth and house prices tend to be higher in the observed data than in the counterfactual scenarios without I-BBM measures.

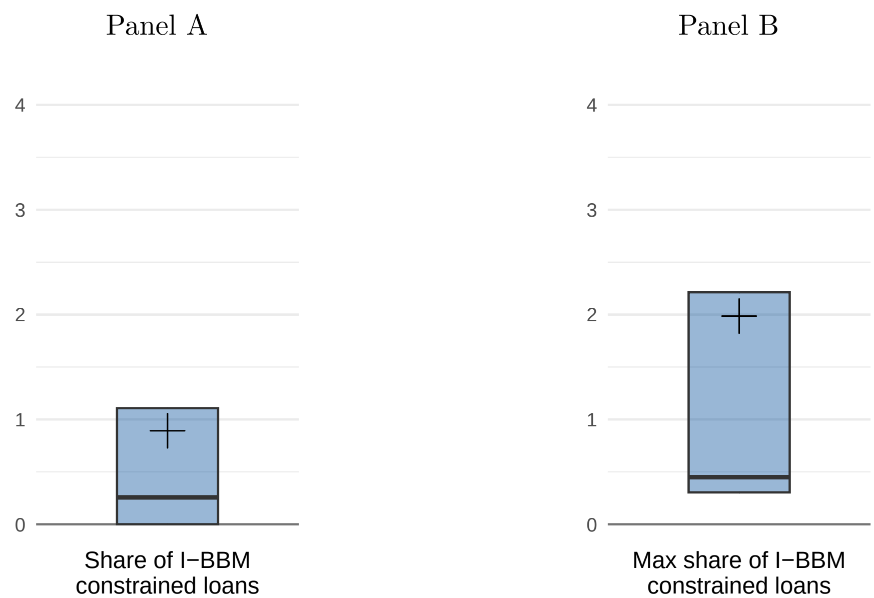

Figure 14: **K-method estimates of the share of constrained lending due to the I-BBM.** This figure plots results based on the K-method to estimate I-BBM induced shocks. Panel A of this figure plots the mean, the median and interquartile range (denoted by a +, horizontal line and box, respectively) of the constrained share of new lending computed from pooling observations across all of the six economies for which we compute I-BBM-induced shocks and all time periods for $t \geq T^*$. For economies where the treatment group is defined as the share of new loans above the I-BBM limit, the share of constrained lending is defined as the difference between the share of new lending to the treatment in the counterfactual minus the share of new lending to the treatment group in the actual data. For economies where the treatment group is defined as lending in bunching group just below the I-BBM limit, the share of constrained lending is defined as the difference in the share of new loans in the bunching group in the actual data minus the share new lending in the bunching group in the counterfactual series. Panel B plots the mean, median and interquartile ranges of the maximum share of constrained lending from each of the six economies.

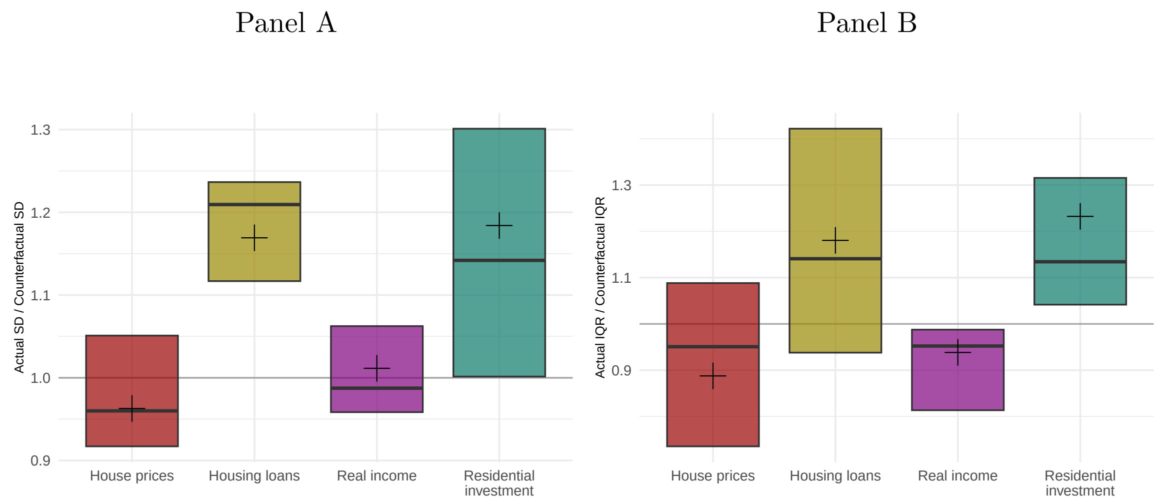

Figure 15: **K-method estimates of the impact I-BBMs on macroeconomic volatility.** Figure A plots the volatility (as measured by the standard deviation) of the actual data as a ratio of the volatility of the corresponding counterfactual series computed in the absence of I-BBM induced shocks, while Panel B plots the ratio of the actual and counterfactural volatilities (as measure by the interquartile range). In both panels, volatility of each series is computed over the period 2019 Q1 to 2024 Q3. The figure shows the median and interquartile ranges of these ratios across the six economies for which we estimate I-BBM-induced shocks. Ratios less than one indicate that the volatility of the actual data was less than that of the counterfactual series. Ratios more than one indicate the opposite.

Figure 16: **Influence of I-BBMs on Sharpe ratios based on K-method estimates.**This figure plots the difference in the Sharpe ratio of the actual data and the Sharpe ratio of the counterfactual series computed in the absence of I-BBM induced shocks. The Sharpe ratios are estimated over the period since I-BBMs were implemented. The figure shows boxplots of the median and interquartile ranges of this difference in Sharpe ratios computed across the six economies for which we estimate I-BBM-induced shocks. Values greater than zero indicate that the Sharpe ratios in the actual data are higher than those computed with the counterfactual data. Values less than zero indicate the opposite.

10. Conclusion

In conclusion, our paper contributes to addressing significant gaps in the analytical frameworks available to policymakers, as highlighted by Committee on the Global Financial System (2023). Specifically, we make progress in two key areas. First, we examine the automatic stabilising properties of income-based borrower-based measures (I-BBMs), demonstrating their role in counteracting the procyclicality of banks' lending standards. This finding highlights the potential of I-BBMs to act as automatic stabilisers, thereby reducing the need for frequent recalibration of macroprudential policies in response to changing economic conditions.

Second, we take an important step towards quantifying the costs and benefits of income-based borrower-based measures. In doing so, we aim to assist policymakers in evaluating the trade-offs inherent in these measures and in effectively communicating their macroprudential policy decisions. These contributions align with two of the key requests from macroprudential policymakers outlined in Committee on the Global Financial System (2023).

While our approach provides valuable insights, there remains scope for further refinement. Future research could better capture treatment versus control dynamics by incorporating additional data series and dimensions into the analysis, including the contribution of LTV and other macroprudential measures.

The two-step identification approach of Elsayed et al. (2025), that we use, makes undeniable progress in helping to better identifying the macroeconomic effects of borrower-based measures by incorporating information on the intensity of the measures from the microeconomic literature. Yet, there still exist some challenges in capturing information on the extensive margin and in evaluating uncertainty around the estimates of the I-BBMs impacts. Going forward there could well be benefits if information from the control and treatment groups could be directly incorporated into the VAR framework. Furthermore, expanding the analysis to include other macroeconomic variables, such as consumption and loan arrears, would offer a more comprehensive assessment of the broader economic impact of I-BBMs.

By addressing these areas, future work can build on the foundation laid by in Elsayed et al. (2025) and this paper, further improving the tools available to policymakers for calibrating, evaluating and communicating the role of borrower-based measures in the macroprudential policy toolkit. To the extent that the elaboration of costs and benefits can help to justify the measures to politicians and the public, building on this work could help enhance the operational independence of the macroprudential authorities.

References

Acharya, Viral V., Katharina Bergant, Matteo Crosignani, Tim Eisert and Fergal Mccann. (2022). "The Anatomy of the Transmission of Macroprudential Policies". *Journal of Finance*, 77 (5), pp. 2533–2575.

Akinci, Ozge, and Jane Olmstead-Rumsey. (2018). "How effective are macroprudential policies? an empirical investigation". *Journal of Financial Intermediation*, 33, pp. 33–57.

Bassett, William F., Mary Beth Chosak, John C. Driscoll and Egon ZakrajÅ¡ek. (2014). "Changes in bank lending standards and the macroeconomy". *Journal of Monetary Economics*, 62, pp. 23–40.

Berger, Allen N., and Gregory F. Udell. (2004). "The institutional memory hypothesis and the procyclicality of bank lending behavior". *Journal of Financial Intermediation*, 13 (4), pp. 458–495.

Bracke, Philippe, Matthew Everitt, Martina Fazio and Alexandra Varadi. (2024). "When refinancing meets monetary tightening: heterogeneous impacts on spending and debt via mortgage modifications". *Bank of England, Staff Working Paper, No. 1,105.*

Brandao-Marques, Luis, Machiko Narita, Erlend Nier and Gaston Gelos. (2021). "Leaning Against the Wind: An Empirical Cost-Benefit Analysis". CEPR Discussion Papers, 15693, C.E.P.R. Discussion Papers.

Bruno, Valentina, Ilhyock Shim and Hyun Song Shin. (2017). "Comparative assessment of macroprudential policies". *Journal of Financial Stability*, 28, pp. 183–202.

Cloyne, James, Clodomiro Ferreira and Paolo Surico. (2019). "Monetary policy when households have debt: New evidence on the transmission mechanism". *The Review of Economic Studies*, 87 (1), pp. 102–129.

Committee on the Global Financial System. (2023). "Macroprudential policies to mitigate housing market risks". CGFS Papers, 69, Bank for International Settlements.

Cumming, Fergus, and Paul Hubert. (2022). "House prices, the distribution of household debt and the refinancing channel of monetary policy". *Economics Letters*, 212, p. 110280.

Defusco, Anthony A, Stephanie Johnson and John Mondragon. (2019). "Regulating household leverage". *The Review of Economic Studies*, 87 (2), pp. 914–958.

Drehmann, Mathias, Mikael Juselius and Anton Korinek. (2023). "Long-term debt propagation and real reversals". CEPR Discussion Papers, 18075, C.E.P.R. Discussion Papers.

Elsayed, Moaz, Valère Fourel and Matthieu Segol. (2025). "The impact of borrower-based macroprudential tools: A macroeconometric approach". Mimeo, Banque de France.

Gaffney, Edward. (2022). "Loan-to-income limits and mortgage lending outcomes". Financial Stability Notes, 10, Central Bank of Ireland.

Giannoulakis, Stelios, Marco Forletta, Marco Gross and Eugen Tereanu. (2023). "The effectiveness of borrower-based macroprudential policies: a cross-country analysis using an integrated micro-macro simulation model". Working Paper Series, 2795, European Central Bank.

Greenwald, Daniel. (2018). "The mortgage credit channel of macroeconomic transmission". *MIT Sloan Research Paper No. 5184-16*.

Gross, Marco, and Javier Población. (2017). "Assessing the efficacy of borrower-based macro-prudential policy using an integrated micro-macro model for european households". *Economic Modelling*, 61, pp. 510–528.

HCSF. (2024). "Chapitre 3: Évaluation des effets de la mesure HCSF relative aux conditions d'octroi des crédits à l'habitat". Rapport annuel September 2024, Haute Conseil de Stabilité Financière.

International Monetary Fund. (2024). "Feeling the pinch? Tracing the effects of monetary policy through housing markets". WEO Chapter, 2, International Monetary Fund.

Jordà, Òscar, Moritz Schularick and Alan M. Taylor. (2016). "The great mortgaging: housing finance, crises and business cycles". *Economic Policy*, 31 (85), pp. 107–152.

Kuttner, Kenneth N., and Ilhyock Shim. (2016). "Can non-interest rate policies stabilize housing markets? evidence from a panel of 57 economies". *Journal of Financial Stability*, 26, pp. 31–44.

Levina, Iren, Robert Sturrock, Alexandra Varadi and Gavin Wallis. (2019). "Modelling the distribution of mortgage debt". Bank of England working papers, 808, Bank of England.

Mertens, Karel, and Morten O. Ravn. (2013). "The dynamic effects of personal and corporate income tax changes in the united states". *American Economic Review*, 103 (4), p. 1212â€"47.

Mian, Atif, Amir Sufi and Emil Verner. (2017). "Household debt and business cycles worldwide*". *The Quarterly Journal of Economics*, 132 (4), pp. 1755–1817.

Millard, Stephen, Margarita Rubio and Alexandra Varadi. (2024). "The macroprudential toolkit: Effectiveness and interactions". *Oxford Bulletin of Economics and Statistics*, 86.

Nier, Erlend, Radu Popa, Maral Shamloo and Liviu Voinea. (2019). "Debt Service and Default: Calibrating Macroprudential Policy Using Micro Data". IMF Working Papers, 2019/182, International Monetary Fund.

Peydró, José-Luis, Francesc Rodriguez-Tous, Jagdish Tripathy and Arzu Uluc. (2023). "Macroprudential policy, mortgage cycles, and distributional effects: Evidence from the united kingdom". *The Review of Financial Studies*, 37 (3), pp. 727–760.

Plagborg-Møller, Mikkel, and Christian K. Wolf. (2022). "Instrumental variable identification of dynamic variance decompositions". *Journal of Political Economy*, 130 (8), pp. 2164–2202.

Richter, Björn, Moritz Schularick and Ilhyock Shim. (2019). "The costs of macroprudential policy". *Journal of International Economics*, 118, pp. 263–282.

Stock, James. (2008). "Recent developments in structural VAR modeling". chap. Lecture 7: What's New in Econometrics: Time Series. NBER.

Stock, James H., and Mark W. Watson. (2003). "Has the business cycle changed and why?"

Tzur-Ilan, Nitzan. (2023). "Adjusting to macroprudential policies: Loan-to-value limits and housing choice". *The Review of Financial Studies*, 36 (10), pp. 3999–4044.

van Bekkum, Sjoerd, Marc Gabarro, Rustom M. Irani and José-Luis Peydró. (2024). "The real effects of borrower-based macroprudential policy: Evidence from administrative household-level data". *Journal of Monetary Economics*, 147 (S).

Varadi, Alexandra. (2024). "Identifying the transmission channels of credit supply shocks to household debt: Price and non-price effects". *European Economic Review*, 166.