Can non-interest rate policies stabilize housing markets?

Evidence from a panel of 57 economies*

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Abstract

Using data from 57 economies over three decades, we investigate the effectiveness of nine non-interest rate policies on house prices and housing credit using conventional panel, mean group and event study methods. We find that changes in the maximum debt-service-to-income ratio have the largest and most robust effects on housing credit, with a typical tightening action lowering the real growth rate by 4 to 7 percentage points over the subsequent four quarters. None of the policies consistently affects house prices with the exception of housing-related tax increases, which slow real house price appreciation by 2 to 3 percentage points.

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1 *Introduction*

Following the housing boom and bust of the mid-2000s, the drawbacks of relying on interest rates alone to ensure financial stability have become increasingly clear. As documented elsewhere, the quantitative impact of interest rates on house prices is economically significant but not large enough to achieve a meaningful degree of restraint.[1] An interest rate hike of sufficient size to meaningfully dampen house price growth would therefore run the risk of causing a recession. As Federal Reserve Chairman Ben Bernanke (2010) put it, monetary policy is a "blunt tool" for stabilizing housing markets.[2] Moreover, countries with exchange rate targets (either explicit or implicit) lack the freedom to use the interest rate as a policy tool.

The recognition of interest rates' limitations has left policymakers searching for other policy tools to tame housing and other asset markets, either independently or as a complement to interest rate policy. A great deal of attention has been focused on non-interest rate policies, such as reserve requirements and maximum loan-to-value (LTV) ratios, which have been on high and growing demand in many economies. Given the central role of the housing market in the recent crises, it is no surprise that many of these policies are aimed squarely at reining in the housing sector. The critical question is whether these non-interest rate tools really work in modulating house prices and housing credit growth.

This paper is closely related to the rapidly expanding literature on macroprudential policy, whose overarching goal is to limit systemic risk in the financial system as a whole.[3] The two main objectives of macroprudential policy are, first, to promote the resilience of the financial system by mandating higher levels of liquidity, capital and collateralization; and second, to restrain the build-up of financial imbalances by slowing credit and asset price growth. This paper deals with the second of these two objectives, focusing specifically on imbalances in the housing market. At the same time, it looks at a broad range of policy actions, not just those traditionally associated with macroprudential regulation. These include changes in taxes and subsidies affecting the housing market, and other actions, such as changes in reserve requirements, that are not explicitly justified

[1]See Kuttner (2014) and the references contained therein.

[2]Partly for this reason, many macroeconomists have argued that the interest rate should not be used to address such developments (e.g. Bernanke & Gertler (1999), Blanchard *et al.* (2010), Galí (2013), Ito (2010), Posen (2006) and Svensson (2010)). Others have argued that there is a role for interest rate policy in ensuring financial stability (e.g. Borio (2011), Eichengreen *et al.* (2011), King (2013), Mishkin (2011), Stein (2013) and Woodford (2012)).

[3]See IMF-BIS-Financial Stability Board (2011) for a more complete discussion of macroprudential policy.

by macroprudential objectives. We therefore refer to the policies in our paper as credit and housing-related tax policies, rather than as narrowly-defined macroprudential tools.

A growing body of research has documented the use of tools other than the short-term interest rate in various countries and examined their effectiveness in damping credit growth and house prices. Among the first was Hilbers *et al.* (2005), who documented that ten of the 18 central and eastern European (CEE) countries responded to house price booms with regulatory policy actions. In the same vein, Crowe *et al.* (2011) found that out of 36 countries that had experienced real estate booms, 24 had responded with policy measures intended dampen the property market.

Focusing on six countries in Latin America, Tovar *et al.* (2012) showed that macroprudential policy in general, and reserve requirements in particular, had a moderate but transitory impact on private bank credit growth in the region. More recent work on the CEE economies by Vandenbussche *et al.* (2012) found that certain types of macroprudential policies, including capital adequacy ratios and non-standard liquidity measures, influenced house price inflation. And taking an international perspective, Borio & Shim (2007) documented 12 types of macroprudential policy actions taken by 18 European and Asian countries going back as far as 1988. Their event study analysis showed that macroprudential measures reduced credit growth by 4 to 6 percentage points in the years immediately following their introduction, while house prices decelerated in real terms by 3 to 5 percentage points.

Lim *et al.* (2011) used data from a survey conducted by the International Monetary Fund (IMF) in 2010 to document that 40 of the 49 countries surveyed had taken (broadly defined) macroprudential measures in the preceding 10 years. Using panel regression analysis, they found that a variety of macroprudential tools, including reserve requirements, dynamic provisioning, maximum LTV ratios, maximum debt-service-to-income (DSTI) ratios and limits on foreign currency lending had measurable effects on the growth rate or cyclicality of private sector credit and leverage.

Taking a disaggregated approach, Claessens *et al.* (2013) analyzed the use of macroprudential policy aimed at reducing vulnerabilities in individual banks in both advanced and emerging market economies, using a sample of about 2,300 banks in 48 countries and macroprudential policy measures documented by Lim *et al.* (2011). They showed that policy measures such as maximum LTV and DSTI ratios and limits on foreign currency lending are effective in reducing leverage, asset and non-core to core liabilities growth during booms, and that few policies help stop declines in bank leverage and assets during downturns.

This paper's goal is to provide a systematic assessment of the efficacy of credit and housing-related tax policies on housing credit and house prices. The analysis uses a new dataset on the usage of nine of these policy types by 60 countries over a period going as far back as 1980, making it the most comprehensive study to date in terms of both scope and time span. While in some respects similar to Lim *et al.* (2011), our focus is on housing credit and house prices rather than overall private sector credit. Our study employs three different empirical approaches as a check on the results' robustness.[4] The main findings are, first, that the maximum DSTI ratio is the policy tool that most consistently affects housing credit growth, with a typical policy tightening slowing housing credit growth by roughly 4 to 7 percentage points over the following four quarters. Second, the evidence suggests that an increase in housing-related taxes can slow the growth of house prices, although this finding is somewhat sensitive to the choice of econometric method.

The plan of the paper is as follows. Section 2 describes each of the nine policies analyzed and sketches a theoretical framework illustrating the channels through which the policies operate. Section 3 describes the data used in the analysis, focusing on the key characteristics of the policy action dataset. Section 4 describes the econometric methods and reports the results. Section 5 concludes.

2 *Credit and housing-related tax policies: taxonomy and transmission*

The purposes of this section are first, to propose a three-way classification of non-interest rate policies; second, to describe the ways in which the policies are transmitted to the housing market; and third, to discuss the conditions under which the policies are likely to be effective. One set of policies works by limiting banks' supply of housing credit. A subset of these tools affects all forms of credit, while the other focuses more narrowly on housing credit. Another group of policies is aimed at the demand for housing credit, working via households' budget constraints. Naturally, all of the policies involving restrictions on the volume of credit rely on the existence of credit-constrained households. Those aimed at the supply side are also affected by the economy's financial structure. None has a direct impact on prices

[4] This paper builds on Kuttner & Shim (2012), which explored a similar set of issues. The present paper uses a significantly expanded version of the policy action dataset used in the earlier work and brings additional econometric methods to bear on the analysis.

The third set of policies consists of taxes and subsidies that affect the cost of home ownership and housing transactions in one way or another. Unlike those affecting credit supply and demand, these policies are likely to affect house prices regardless of whether households are credit-constrained.

2.1 Supply-side credit policies

2.1.1 General

The three tools falling into this category are credit growth restrictions, reserve requirements and liquidity requirements. All affect banks' loan supply, but since none is aimed specifically at the housing sector, we refer to them as general credit policies.

Credit growth restrictions have sometimes been imposed during lending booms in an effort to limit the expansion of private sector credit. This may take the form of a numerical ceiling on the rate of credit growth or the amount of an increase in lending over some period of time. These policies also entail specifying the penalties that would be incurred for violating the limit.

Reserve requirements compel banks to hold a minimum fraction of their liabilities as liquid reserves. These are normally held either as reserve deposits at the central bank or as vault cash. The regulation generally specifies the size of required reserves according to the type of deposits (e.g. demand, savings or time deposits), their currency of denomination (domestic or foreign currency) and their maturity. Liquidity requirements typically take the form of a minimum ratio of highly liquid assets, such as government securities and central bank paper, to certain types of funding sources (usually deposits and deposit-like liabilities). Both regulations serve a prudential purpose, increasing banks' ability to withstand cash outflows during periods of stress. The main difference between liquidity and reserve requirements is that the former requires the bank to keep funds at the central bank whereas the latter obliges them to hold liquid marketable securities.

Reserve and liquidity requirements operate by influencing banks' cost of funds, and so their effects on loan volume is less direct than credit growth restrictions. To see how these tools work, consider a stylized bank, whose assets, A, are divided between loans, L, marketable securities, Q and reserves, R. It obtains funds either by taking deposits, D, or by raising equity, E. The bank's profits are

$$\Pi = r_L \cdot L + r_R \cdot R + r_Q \cdot Q - r_D \cdot D - r_E \cdot E \quad , \tag{1}$$

where r_L, r_R and r_D are the interest rates on loans, reserves and deposits, respectively. The cost of equity funding is r_E. Reserves generally pay a below-market interest rate, so $r_R < r_L$. In addition, a reasonable assumption is that the return on liquid marketable securities, such as short-term government bonds, is less than the loan return, so that $r_Q < r_L$.

Letting ψ denote the reserve-to-deposit ratio, μ the securities-to-deposit ratio and χ the equity-to-asset ratio, the return on assets can be expressed as

$$\Pi/A = r_L - [r_D + \psi(1-\chi)(r_L - r_R) + \mu(1-\chi)(r_L - r_Q) + \chi(r_E - r_D)] \ , \tag{2}$$

where the difference between the loan rate and the cost of funds is represented by the collection of terms in the square brackets. Besides the deposit rate, the cost of funds includes the opportunity cost of holding reserves, $\psi(1-\chi)(r_L - r_R)$ (the "reserve tax"). If ψ were determined by a regulatory-mandated minimum reserve ratio, then an increase in that minimum would increase the cost of funds, shifting the loan supply curve inward and reducing the equilibrium volume of lending. The same applies to an increase in the statutory liquidity requirement. Assuming it were binding, an increase in the minimum μ would increase the opportunity cost associated with holding liquid marketable securities, $\mu(1-\chi)(r_L - r_Q)$, similarly increasing the cost of funds and reducing the equilibrium volume of loans.

The effectiveness of these general credit policies in practice is likely to be limited by five factors. First, their efficacy rests on the existence of borrowing constrained households. Second, because these policies apply to *all* bank lending, their impact on the housing market may be diluted by changes in other forms of lending. In the U.S., for example, mortgage lending accounted for barely one-third of total commercial bank credit as of November 2013.

Third, reserve and liquidity requirements work only to the extent that the requirements are binding. Banking systems differ greatly across countries along this dimension. As of April 2012, Korea's banking system held virtually no excess reserves, while that of the Philippines held only 4.6 billion pesos ($100 million). Until the quantitative easing policies that began in 2008, the US banking system held only a trivial amount of excess reserves. In contrast, the figure for Thailand is 2 trillion baht ($45 billion), the equivalent to almost one fifth of GDP. Fourth, reserve and liquidity requirements work only if banks lack alternative, non reservable funding sources. This is not likely to be the case in the U.S., where only one-eighth of commercial banks' total deposits are in the form of reservable transactions accounts.

Fifth, the tools' effectiveness relies on the absence of non-bank alternative financing sources and securitization. Consequently, one would not expect these policies to have much of an impact where there is a large shadow banking system, as is the case in the U.S. where roughly half of all residential mortgages and mortgage backed securities are held by entities other than commercial banks and thrifts.

2.1.2 Targeted

The three tools falling into this category are limits on exposure to the housing sector, the risk weights applied to housing loans and loan loss provisioning requirements. Like the general credit policies described earlier, these tools all affect banks' loan supply. But since they are aimed specifically at the housing sector, we refer to them as targeted credit policies. They serve a prudential purpose as well, as they reduce banks' vulnerability to downturns in the housing sector.

Exposure limits place a quantitative cap on the share of banks' assets allocated to the housing or property sector, expressed as a percentage of either total assets or equity. This tool is analogous to restrictions on credit growth in the sense that it has a direct impact on the volume of lending. Reductions in exposure limits have been used in efforts to slow the expansion of housing loans by banks, and to limit the losses from housing loans in the event of a price correction and a surge in defaults. Loosenings, such as the Bank of Thailand's February 2008 move to exclude residential housing loans from the pre-existing 20% exposure limit, have also been used to support the market.

In contrast, risk weighting and provisioning requirements work by making it more costly to extend credit, analogous in that respect to reserve and liquidity requirements. In particular, higher risk weights on housing loans compel a bank to increase equity for a given level of loans. Their effects therefore stem from the fact that, for well documented reasons, equity finance is more expensive than deposits. Consequently, any regulation that increases the mandatory minimum capital ratio (χ in equation 2) raises the cost of funds. Similarly, imposing a higher loan-loss provisioning ratio on housing loans requires banks to set aside a larger portion of their profits as a cushion against defaults, which also increases the implicit cost of that category of lending. Tightening risk weighting or provisioning requirements therefore shifts the loan supply curve inward, decreasing the equilibrium volume of housing loans.

One example of a tightening of provisioning requirements is the Reserve Bank of India's September 2006 decision to increase the required loan loss provisioning ratio applied to housing loans in excess of 2 million rupee from 0.55% to 0.70%. This action came less than two years after the Bank's December 2004 move to increase the risk weights applied to housing loans from 50% to 70%. Risk weights are often differentiated by the actual LTV ratio of individual loans. For example, the portion of a housing loan's LTV ratio that are higher than a certain threshold (say, 80%) may carry a higher risk weight.

Risk weighting and provisioning requirements share some of the same limitations as reserve and liquidity requirements. Both sets of tools rely on the existence of a significant number of credit-constrained households, work only if the relevant requirements are binding, and can be offset by increases in non-bank sources of housing credit and securitization. The targeted credit policies are more likely to have a significant impact on the housing sector, however, since they apply specifically to the extension of housing credit. In addition, because the requirements are based on the volume of loans rather than deposits, they will not be undercut by access to non-reservable funding sources.

2.2 Demand-side credit policies

The two policies used to influence the demand for housing credit are limits on the maximum LTV and DSTI ratios. Both work through the household's budget constraint, limiting households' ability to borrow funds for house purchases. However, there are some subtle but important differences in the way they work, which are readily illustrated by a stripped-down two-period model of housing demand. The household's utility function includes housing and non-durable consumption,

$$\max_{c_1,c_2,h} u(c_1) + \frac{1}{1+\rho} u(c_2) + v(h) \ . \tag{3}$$

In the absence of borrowing limits, the household maximizes the utility function subject to the budget constraint,

$$c_1 + p_1 h + \frac{1}{1+r} c_2 \leq \frac{p_2 h}{1+r} + y_1 + \frac{1}{1+r} y_2 \ , \tag{4}$$

where c_1, c_2 and h represent first-period consumption, second-period consumption, and housing purchases, respectively. Taking the house prices in periods 1 and 2, p_1 and p_2, and the interest rate, r, as exogenous, maximization leads to the first-order conditions

$$u'(c_1) = \frac{1+r}{1+\rho}u'(c_2) \text{ and } u'(c_1) = \left[p_1 \frac{r-\pi}{1+r}\right]^{-1} v'(h) \ , \tag{5}$$

where π is the (expected) rate of house price appreciation. As usual, the first-order conditions can be combined with the budget constraint to yield expressions for households' demand for housing and consumption in each period as functions of the relevant prices and the interest rate. These expressions in turn imply a desired level of borrowing, $B = c_1 + p_1 h - y_1$.

In the presence of a cap on the LTV ratio, θ, the budget constraint must also satisfy $B \leq \theta p_1 h$. If this is binding, then $B = \theta p_1 h$ and $\partial B / \partial \theta = p_1 h > 0$. Naturally, a reduction in θ entails less spending on housing and first-period consumption. The response of h can be determined by replacing the c_1 in the objective function with $y_1 - (1-\theta) p_1 h$ and differentiating,

$$(1-\theta)u'(y_1 - (1-\theta)p_1 h) = \frac{1}{p_1}v'(h) + \frac{1+\pi}{1+\rho}u'(p_2 h + y_2) \ . \tag{6}$$

In general, c_1 and h will both decline, the extent determined by the degree of substitutability between housing and consumption.

An important property of the maximum LTV constraint is that it is relaxed by a rise in house prices, so that $\partial B / \partial p_1 = \theta h > 0$. To take a concrete example, suppose a household faces a binding maximum LTV ratio of 80%, allowing it to borrow \$400 thousand for a house costing \$500 thousand. A 20% increase in the value of the house, from \$500 thousand to \$600 thousand, would allow the household to borrow \$480 thousand, an increase of \$80 thousand. The maximum amount of borrowing would increase to \$420 thousand even if the maximum LTV ratio were reduced by 10 percentage points from 80% to 70%. Rapidly rising house prices can therefore fuel a credit boom and neutralize efforts to dampen it through reductions in the maximum LTV ratio.

Turning to limits on the DSTI ratio, a regulatory maximum ϕ requires that $B \leq \phi y_1 / r$. If the requirement is binding, then $B = \phi y_1 / r$ and $\partial B / \partial \phi = y_1 / r$. All else equal, a reduction in ϕ requires that the household cut back on either housing purchases or consumption, or both. Analogous to the LTV limit, the response of h can be determined by replacing c_1 in the objective function with $y_1(1+\phi/r) - p_1 h$ and differentiating,

$$u'((1+r^{-1}\phi)y_1 - p_1 h) = \frac{1}{p_1}v'(h) + \frac{1+\pi}{1+\rho}u'(p_2 h + y_2) \ . \tag{7}$$

First-period consumption and housing purchases will decline when ϕ is reduced, by amounts determined by the shape of the utility function.

The budget constraint under a DSTI limit reveals an important link between interest rates and credit growth. A reduction in r will decrease the user cost of home ownership and increase housing demand and credit, even in the absence of a binding constraint on the DSTI ratio. With borrowing limited by the DSTI cap, a reduction in r directly relaxes the budget constraint by reducing the debt service burden for a given amount of borrowing: $\partial B/\partial r = -\phi y_1/r^2$. Consequently, when housing credit is capped by a maximum DSTI ratio, an interest rate cut may have a larger impact on credit than when the constraint is not binding.

The main requirement for DSTI and LTV limits to affect housing credit is that households are borrowing constrained. Unlike the supply-side measures discussed above, they are unaffected by factors specific to the financial system, such as the degree to which reserve requirements are binding or the availability of non-bank sources of finance (assuming the LTV and DSTI restrictions apply to all sources of housing credit). It is therefore more likely that these demand-side policies would be effective in restraining housing credit than the supply-side policies.

2.3 Housing-related tax policies

All of the policies discussed in Sections 2.1 and 2.2 have their primary effects on the supply of and demand for credit. None has a direct impact on the price side. These policies will therefore affect house prices only to the extent that they affect the overall demand for houses. Although restrictions on the supply of or demand for credit will affect individual households' housing demand, in general equilibrium their impact on aggregate market demand and prices will depend on the response of households whose borrowing capacity remains unconstrained. To the extent that unconstrained households' demand for houses increases when prices fall, an increase in their demand for housing could offset the reduction coming from constrained households, thus attenuating the price effects.

Housing-related tax policies, on the other hand, will affect prices directly through the user cost

(UC) of home ownership. This is easy to see in the standard UC definition,

$$UC = (i_t + \tau_t^p)(1 - \tau_t^y) + \delta - \pi_{t+1}^e \ , \tag{8}$$

in which i is the nominal interest rate applied to housing loans, τ^p is the property tax rate, τ^y is the income tax rate taking into account the deductibility of mortgage interest and property taxes, δ is the rate of physical depreciation, and π_{t+1}^e is the expected rate of house price appreciation. To the extent that households readily substitute between renting and owning, the UC will be equal to the rent-to-price ratio, so that a reduction in the UC will lead to an increase in prices. Reductions in the property tax rate and the relevant income tax rate (e.g. more generous deductibility of mortgage interest) should therefore lead directly to price increases, regardless of whether households are credit-constrained.

Housing-related taxes take many different forms besides property taxes and mortgage interest deductibility. Transactions or "stamp" taxes are not uncommon. (Equation 8 could be generalized to include these charges, amortized over the expected period of home ownership.) Hong Kong SAR, for example, raised the stamp tax rate from 2.75% to 3.75% in April 1999 in an effort to dampen house price growth. The category also includes direct subsidies and incentives for first-time home buyers. Ireland, for example, effectively raised the cost of home ownership in April 1987 by abolishing the mortgage subsidies that had been introduced in 1981 and 1982.

3 Data

This paper's empirical analysis brings together three different categories of data: the policy actions discussed in Section 2, house price and housing credit data, and several macroeconomic time series. The combined dataset covers the 57 advanced and emerging market economies listed in Table 1, extends through 2011Q4, and, for some countries, goes as far back as 1980Q1. This section highlights some of the salient properties of the data, summarizes the data sources, and describes the criteria used in selecting the economies and time periods used in the analysis.

3.1 The policy action dataset

The heart of the empirical analysis and a major contribution of the paper is a new, comprehensive dataset containing 1,111 non-interest rate policies of the types categorized in Section 2.[5] The dataset incorporates information from a variety of sources, drawing wherever possible on official documents from central banks, regulatory authorities and ministries. These include annual reports, financial stability reports, monetary policy bulletins, supervisory authorities' circulars, budget reports, ministry of finance announcements on tax changes and press releases from these institutions. We also consulted Borio & Shim (2007), a survey by the Committee on the Global Financial System (CGFS) on macroprudential policy conducted in December 2009, Hilbers *et al.* (2005), Crowe *et al.* (2011), Lim *et al.* (2011) and Tovar *et al.* (2012). Where these secondary sources were used, we cross-checked the information from the secondary sources against the information obtained from official documents.

One benefit of using primary sources is that the dataset should, in principle, provide a complete list of all relevant policy actions officially promulgated by national authorities. Information obtained from ad hoc surveys, on the other hand, is likely to be less comprehensive. For example, the IMF survey described in Lim *et al.* (2011) includes only those actions taken for explicit macroprudential purposes, and therefore excludes a large number of policy changes that are likely to have affected the housing market. Moreover, the use of official publications makes it possible to obtain full and accurate information on each of the potentially relevant policy actions. These details allow us to apply uniform criteria when determining which measures to include and how to record them consistently. Another benefit of using official publications is that it provides precise information on the implementation date of each policy action.

One disadvantage of using official sources is that English translations of the relevant documents are not uniformly available for the earliest parts of the sample. Another is that, for a limited number of countries, archives available on the websites of the relevant authorities or offline publication archives available from the BIS library may have one or two missing years. For these reasons, a few policy actions may have been omitted.

[5] The dataset used in this paper is based on the 60-country public dataset described in Shim *et al.* (2013) and available at `http://www.bis.org/publ/qtrpdf/r_qt1309i.htm`. The public version covers a shorter time span (starting in 1990) and excludes changes in housing-related tax policy. The lack of housing credit and house price data for three countries limits our analysis to 57 of the 60 economies.

The heterogeneity of the information contained in the database requires that we impose a consistent set of criteria in determining what constitutes a policy action. In the case of reserve requirements, for example, we consider only changes in the statutory reserve requirement ratio and reserve base. We do not consider changes in the remuneration rates, reserve maintenance periods or averaging methods because we focus on policy actions directly targeting the aggregate quantity of funds available for lending. However, it should be noted that this distinction is not clear-cut to the extent that reserve requirements also operate, in effect, by influencing the cost of lending. We also include both average reserve requirements, where a certain reserve requirement ratio applies to all outstanding amount of eligible liabilities, and marginal reserve requirements, with which additional liabilities banks assume after certain dates or the amount of liabilities exceeding the level of banks' liabilities as of certain dates are subject to often very high reserve requirement ratios.

Housing-related tax policies are similarly diverse, encompassing taxes (such as capital gains tax, wealth tax and value added tax related to housing), subsidies (on first-time home buyers and young couples and also on mortgage interest payment), fees (such as stamp duties and registration fees) and tax deductibility of mortgage interest payments. We only include in the database only nationwide measures targeting middle-income or high-income groups who are potential homebuyers. Tax measures applied to one or two cities or subsidies given specifically to low-income families are not included.

Even with the application of uniform selection criteria, the specifics of policy actions differ across countries and over time. For example, the dataset includes the introduction of a maximum LTV ratio as well as the subsequent reductions and increases in the ratio. Also, in some economies total household income is used in calculating the DSTI, while in others the borrower's income is used. Including these data in a regression model therefore requires some degree of standardization and aggregation.

Our solution is to create monthly variables that take on three discrete values: 1 for tightening actions, -1 for loosening actions and 0 for no change.[6] The monthly observations are summed to create quarterly time series. This means that if multiple actions in the same direction were taken within a given quarter, the variable could take on the values of 2 or -2, or even 3 and -3. It also

[6]Some of the policy measures that are more standard across countries, such as reserve requirements, would be more amenable to a numerical representation.

means that a tightening action and a loosening action taken within the same quarter would cancel each other out. Changes in reserve requirements account for nearly all closely spaced actions. With only a few exceptions, no more than one of the other types of policy actions is observed in any given quarter.

The heterogeneous nature of the policies makes it hard to characterize the typical size of the policy actions. In a large number of cases, the policy change consisted of either the adoption or the suspension of a particular requirement. In the case of the maximum LTV ratio, for example, 27 of the actions in the database are introductions of a new requirement. Of the 41 actions involving the adjustment of the maximum ratio, the average change is approximately 12 percentage points. Changes in the DSTI limit are even harder to quantify, as most actions are introductions, discontinuations, or changes along dimensions other than the ratio itself. Similarly, the diversity of housing-related taxes makes it hard to characterize the magnitude of a typical policy action. Of those that are readily quantified, the average change in housing-related taxes is approximately 8.5 percentage points. However, this figure is based on aggregating rate changes for a variety of different taxes (stamp, land value, property, capital gains), tax relief measures, subsidies and fees.

Of the 1,111 policy actions in the dataset, 55% (607) are tightening, and 45% (504) are loosening.[7] Reserve requirements are by far the most frequently used of the nine categories of policy, accounting for 641 of all policy actions. Liquidity requirements and restrictions on credit growth, on the other hand, are used relatively infrequently. Since all three of these tools are likely to have similar effects on the supply of housing credit, for the purposes of the empirical work we aggregate them into a single "general credit" variable. With 148 actions in the dataset, housing-related tax policies are also relatively common. Changes in exposure limits are rare, and only 20 instances are documented in the dataset.

The use of these policies varies a great deal between countries. Twenty-four countries averaged five or fewer policy changes per decade. A few countries were very active users of these policies. Thirteen economies account for over half of all the policy actions in the dataset. Four averaged 20 or more actions per decade. The Asia-Pacific economies have been especially energetic in their use of non-interest rate policies. The CEE countries have also made frequent use of non-interest

[7] A detailed breakdown of policy actions and the dates of coverage for each country are available in the appendix. See also Shim *et al.* (2013).

rate policies, especially reserve requirements.

The frequency with which these policies have been used has steadily increased since the 1980s and the mix of policies has also changed. General credit policies (reserve requirements, liquidity requirements and limits on credit growth) have become less common compared with other types of policies, while targeted credit policies (maximum LTV and DSTI ratios, risk-weighting, exposure limits and provisioning requirements) have come into increasing use. Relative to the total number of policy actions, the number of housing-related tax measures has remained stable between 10% and 15% since the 1990s.

Table 2 reports the correlations between the cumulative policy indicators, constructed by summing current and previous quarters' policy changes. Most of these correlations are relatively small, indicating that the policies are not in general seen as complements. A conspicuous exception is the 0.58 correlation between DSTI and LTV actions, suggesting that the two policies are often used in conjunction. The correlations of 0.23 and 0.29 suggest that changes in provisioning requirements also sometimes accompany adjustments in DSTI and LTV limits.

3.2 Housing credit, house price and macroeconomic data

Another contribution of the paper is its use of an extensive new dataset of house prices. The starting point is the BIS property price database.[8] We enlarged this dataset using data from statistics from official sources and, in a few instances, from proprietary private sector sources such as CEIC. When multiple housing price indices are available for a given economy, for example nationwide versus major city indices, we use indices for major cities, as these are the areas that would be most susceptible to overvaluation and are often addressed by targeted credit policy and housing-related tax policy. Data are available for 57 economies, although the time series are quite short in many cases.[9]

The property price data are highly imperfect. The methods used in the construction of the price indices (e.g. quality adjustment) vary greatly between economies, as does the definition of the relevant housing market (e.g. flats versus detached houses). Moreover, in some cases two or more series must be spliced together in order to yield a usable time series. Conclusions involving the level of property prices are therefore problematic, especially when it comes to cross-country

[8] Available at `http://www.bis.org/statistics/pp.htm`.

[9] Data sources and dates of coverage are documented in the appendix.

comparisons. Recognizing these limitations, we will proceed on the assumption that these series can serve as informative indicators of cyclical swings in the residential property market, if not the price levels.

Household credit data were compiled from a similarly diverse set of sources. The primary source is the BIS data bank, supplemented with series from Datastream, CEIC and central bank websites. Data are available for 57 countries, although for several economies the series only begin in the late 2000s. And as with the price data, the sources and definitions are not always consistent across countries.[10]

The empirical work also uses a number of macroeconomic time series. One is the consumer price index, obtained from the IMF's International Financial Statistics database. The IMF is also the source for most of the short-term interest rate series, supplemented in several cases with data from national sources or Haver Analytics. Ideally, our analysis would also include a measure of personal disposable income. These data are difficult or impossible to obtain for the majority of countries we are looking at, however. We therefore used as a proxy annual real per capita gross national income (GNI) from World Bank's World Development Indicators database, interpolated to a quarterly frequency.

3.3 Sample selection criteria

Data availability is the main constraint on the scope of our analysis. The policy action database contains information on 60 countries, but the United Arab Emirates lacks interest rate data while Uruguay and Saudi Arabia have no house price or housing credit data, narrowing our sample to 57 economies. For most countries, the 2011Q4 endpoint is constrained by the GNI data, which were only available through that date at the time of writing.

For some countries, data are available, but the time series are too short to be of any use. House price and housing credit data availability is the binding constraint in most cases, although in a few instances the sample is limited by the lack of interest rate data. Economies with fewer than 16 usable quarterly observations (accounting for the loss of observations from lags and differencing) are excluded. This criterion eliminates Brazil from the house price regression. Similarly, Iceland and Serbia are dropped from the regressions involving housing credit.

[10] The definitions and starting and ending dates for the housing credit series are also listed in the appendix.

Even where data are available, there are often good reasons to discard a portion of the sample. We exclude periods of extreme macroeconomic instability, as these are accompanied by extremely high inflation and interest rates. Argentina's 1990 and 2002 crises are prime examples of such episodes. In order to prevent these anomalous observations from unduly influencing the results, we postpone the regression start dates to avoid these periods.

Poor data quality is another reason for truncating the sample. While there is no good way to independently verify the reliability of the data, many of the series exhibit extreme volatility during the first few quarters for which they are available. Some of the observed spikes may be the result of very rapid growth from a small base, or an artifact of small samples or thin markets. This is a common issue among the CEE economies. The regression starting date is delayed in these cases in order to exclude these periods. Large changes that appear to genuinely reflect conditions in the housing market are not dropped.

Table 3 reports descriptive statistics for the housing credit, house price and macroeconomic data, after the sample selection criteria are applied. Even with the elimination of the most extreme observations, there is still a great deal of volatility in the data, especially in house prices and housing credit. The standard deviations of the annualized quarterly percentage changes in these two variables are 15.6 and 12.3, respectively. The sample even contains changes in these two variables in excess of 100%.

4 Empirical methods and results

This section presents estimates of the policies' effects on housing credit and house prices. We use three different empirical methods in an effort to assess the robustness of our results. The first involves conventional panel data regressions. The second uses a mean group estimator, which allows for cross-country heterogeneity in the model coefficients. The third can be described as a panel event study analysis in which the results of country-specific event studies are aggregated to estimate the average effect for the sample. We also explore the possibility of asymmetric responses to policy tightenings and loosenings.

The three methods yield similar point estimates for the policies' effects. The degree of precision varies, however, with the panel regressions producing smaller standard errors than the other two methods. To preview, we find that DSTI limits exert a significant effect on housing credit growth, a result that holds regardless of the method used. With the exception of housing-related tax changes, none of the policies consistently affects house price growth.

4.1 Conventional panel regression analysis

We begin with a standard reduced-form regression model of credit growth,

$$\begin{aligned} \Delta \ln C_{i,t} &= \alpha_i + \rho \Delta \ln C_{i,t-1} + \beta_1 r_{i,t-1} + \beta_2 r_{i,t-2} + \\ &\quad \beta_3 \Delta \ln y_{i,t-1} + \beta_4 \Delta \ln y_{i,t-2} + \sum_{j=1}^{4} \gamma_j X_{i,t-j} + \varepsilon_{i,t} \end{aligned} \tag{9}$$

in which the quarterly credit growth rate for country i, $\Delta \ln C_i$, is expressed as a function of its own lag, two lags of the short-term interest rate, r_i, two lags of real personal income growth, $\Delta \ln y_i$, and a variable X_i representing one (or more) of the policy variables described in Section 3. To account for cross-country differences in the average rate of credit growth, a country-specific fixed effect, α_i is included.[11] An analogous specification is used for the house price, P_i.[12]

Reduced-form regressions such as equation 9 are always susceptible to the critique that the regressors are likely to be endogenous. Specifically, policymakers may adjust interest rates or implement credit and housing-related tax policies in response to conditions in the housing market (or in response to omitted variables that are correlated with house prices or credit fluctuations). This is especially true in those economies, such as many in the Asia-Pacific region, where policymakers have actively changed LTV, provisioning and reserve requirements in their efforts to curb housing market excesses. This endogeneity may bias the parameter estimates, making it problematic to interpret the γ coefficients as a reliable gauge of the policies' effectiveness.

Fortunately, there is reason to believe that the endogeneity problem will lead the estimates to *understate* the policies' effectiveness. Consider a tightening of the LTV requirement (a decrease in the maximum LTV ratio and a positive value of the LTV variable) for example. If the policy had the desired effect, it would reduce housing credit ceteris paribus. But if policymakers tended to tighten the LTV requirement when housing credit was already expanding rapidly, this would give

[11] Strictly speaking, the inclusion of the lagged dependent variable would bias the fixed-effect estimator. But given the relatively long time series dimension of the data, the amount of bias should be small.

[12] Theoretically, the user cost model implies a cointegrating relationship between house prices and rents, which would suggest including the log of the rent-to-price ratio as an additional regressor. Results not presented in this paper indicate that the inclusion of this term has no significant effect on the parameter estimates of interest.

rise to a positive correlation between our LTV variable and credit, partially (or fully) offsetting the desired policy effect. In the (implausible) limiting case in which policymakers managed to set the maximum LTV ratio in such a way as to completely stabilize credit, the estimated regression coefficient on the LTV variable would be zero. An accurate statistical assessment of the policies' effects would require some exogenous variation in the policy measures (regulatory "policy shocks"). Unfortunately, it is hard to think of any circumstances that would give rise to such exogenous policy shifts.

4.1.1 Housing credit

Before examining the effects of the policy variables, we estimate baseline fixed-effect regressions for housing credit and house price growth omitting the policy variables. The fitted equation for housing credit (with standard errors in parentheses)

$$\Delta\ln C_{i,t} = \underset{(0.05)}{0.59}\ \Delta\ln C_{i,t-1} - \underset{(0.19)}{0.67}\ r_{i,t-1} + \underset{(0.21)}{0.56}\ r_{i,t-2} + \underset{(0.19)}{0.59}\ \Delta\ln y_{i,t-1} - \underset{(0.14)}{0.004}\ \Delta\ln y_{i,t-2}$$

$$N = 3700 \quad \bar{R}^2 = 0.56 \quad \text{SEE} = 10.2\ ,$$

yields reasonable estimates of housing credit dynamics and the effects of interest rates and personal income growth. With a coefficient of 0.59 on its lag, credit growth exhibits a moderate amount of positive serial correlation. Interest rate hikes slow credit growth. The negative -0.67 coefficient on r_{t-1} along with the positive coefficient of 0.56 on r_{t-2} (both statistically significant) together indicate that it is the *change* in the short-term interest rate that is relevant for credit growth, rather than the level.[13] The variables are defined in such a way that the coefficients represent the effect on the *annualized* credit growth. A coefficient of -0.67 on the change in the interest rate therefore indicates that a 1 percentage point increase in the short-term interest rate is associated with a 0.67 percentage point reduction in credit growth in the following quarter. (Naturally, the positive coefficient on lagged credit growth implies that the medium- and long-run effects of a sustained increase in the interest rate will exceed -0.67.) Finally, the positive coefficient of 0.59 on the first lag of $\Delta\ln y$ indicates that a 1 percentage point increase in the rate of personal income growth translates into an increase in the rate of housing credit growth of over half a percentage point.

[13] A formal statistical test fails to reject the hypothesis that $\beta_2 = -\beta_1$.

Having verified the plausibility of the baseline model, the next step is to include the policy variables, X, in the regression.[14] The results are summarized in Table 4. The first numeric column reports the number of each type of policy action included in the regression.[15]

The next two numeric columns (labelled "individually") summarize the estimated $\hat{\gamma}$ coefficients when each is included one at a time in the regression given by equation 9. Thus, each of the seven lines in the table corresponds to a separate regression. Rather than give the individual parameter estimates, which are of little intrinsic interest, we report two functions of the estimates summarizing the magnitude of the policies' effects. The second numeric column shows the sum of the coefficients, which would represent the long-run impact of a permanent unit tightening, ignoring the dynamics. Although it provides a rough gauge of the magnitude and statistical significance of the effects, it is not particularly useful for assessing the likely effect over a policy-relevant horizon. We therefore also report a second summary statistic, shown in the third numeric column, constructed to gauge the expected effect on the growth rate over a one-year horizon, taking the dynamics into account. This is a function of $\hat{\gamma}_1$ through $\hat{\gamma}_4$, as well as $\hat{\rho}$:

$$\text{4Q effect} = \frac{1}{4}\left[\hat{\gamma}_1(1+\hat{\rho}+\hat{\rho}^2+\hat{\rho}^3)+\hat{\gamma}_2(1+\hat{\rho}+\hat{\rho}^2)+\hat{\gamma}_3(1+\hat{\rho})+\hat{\gamma}_4\right] \ . \tag{10}$$

The delta method is used to calculate the standard errors.

Reassuringly, all the parameter estimates from the one-policy-at-a-time regressions have the correct (negative) sign, indicating that tightening and loosening policy actions can moderate housing credit cycles. Of these, four are statistically significant at at least the 5% level: LTV limits, DSTI limits, exposure limits, and housing-related taxes. The largest effect comes from the DSTI limits, with a unit tightening reducing credit growth by 6.8 percentage points over the subsequent four quarters. Next come exposure limits, with a 4.6 percentage point effect. (Some caution is

[14] The estimated coefficients on the non-policy variables, including the interest rate, are largely unchanged by the inclusion of the policy variables.

[15] The number of policy actions appearing in the regressions is considerably smaller than the number of actions in the database. There are two reasons for this. One is the limited coverage of the time series data. As discussed in Section 3, the availability of housing credit and price data varies a great deal between countries. Two of the 57 economies were excluded altogether from the credit regressions for lack of sufficient data. The spotty coverage also limits the time series dimension of the remaining 55 countries. The second reason is that aggregation to the quarterly frequency reduces the number of events. This is relevant when an action in one direction was followed within the quarter by an action in the other direction. For example, a tightening of reserve requirements in January and a loosening in March would net to zero for the quarter.

warranted, however, as the sample contains only 12 changes in exposure limits.) Taxes and LTV limits come in at approximately 2 percentage points.

The estimates are largely unchanged when all seven policy variables are included in the same regressions (the columns labeled "jointly"). The main difference is that the LTV limit variable is insignificant, both economically and statistically. A likely explanation is that the tendency for DSTI and LTV requirements to be adjusted in tandem (consistent with the positive correlation reported in Table 2) so that when included individually, the LTV variable picks up the effect of the omitted DSTI policy. This is consistent with the positive correlation between the two policies evident in Table 2.

While not a direct implication of the theoretical frameworks sketched in Section 2, there are reasons to suspect that tightening and loosening actions may have asymmetric effects. To the extent that reductions in reserve requirements tended to occur during economic downturns, banks may find themselves constrained by factors other than reserve requirements, such as low loan demand or an erosion of the capital base. Similar logic applies to the other policies as well. One might therefore expect loosenings to have smaller effects than tightenings.

We explore this possibility by estimating an expanded version of equation 9 in which the X's are distinguished by direction. Rather than a single X with positive and negative values representing tightenings and loosenings respectively, we define two separate X's: one with positive values for tightening episodes and zero otherwise, and a second with negative values for loosenings and zero otherwise. Defined in this way, one would expect negative coefficients on both variables; the question is whether those on the loosening variable are smaller in magnitude and statistical significance.

Table 5 reports the results from a set of regressions allowing for asymmetric effects. The estimates do indeed suggest some degree of asymmetry. For three of the four policies with statistically significant effects in Table 4, the effects of tightenings are significant, while those of loosenings are not. In some cases, the coefficients on the loosening variables have the wrong sign, but in no case is the effect significant at the 5% level. Only the relaxation of exposure limits has a discernible impact. (The caveat about the small number of actions in the sample is now even more applicable.) The standard errors associated with the loosening coefficients are generally larger than those of the tightening coefficients, however. This is at least in part due to the smaller number of policy actions (e.g. 32 tightenings but only six loosenings for DSTI limits). Consequently, the hypothesis of symmetric effects is rejected at the 5% level only for the risk-weighting variable in the individual regressions, and for the risk-weighting and LTV variables in the joint regression.

4.1.2 House prices

As with the credit regressions discussed in Section 4.1.1, we begin with the estimation of a fixed-effect regression for house price growth, analogous to equation 9, including the 56 economies with sufficient data. The results are as follows (with standard errors in parentheses):

$$\Delta \ln P_{i,t} = \underset{(0.05)}{0.48} \; \Delta \ln P_{i,t-1} - \underset{(0.20)}{0.49} \; r_{i,t-1} + \underset{(0.20)}{0.24} \; r_{i,t-2} + \underset{(0.20)}{0.60} \; \Delta \ln y_{i,t-1} - \underset{(0.17)}{0.17} \; \Delta \ln y_{i,t-2}$$

$$N = 3935 \quad \bar{R}^2 = 0.30 \quad \text{SEE} = 10.2 \; .$$

The estimates are similar to those for housing credit. Price changes are positively serially correlated, albeit somewhat less than credit growth. The coefficient on the first lag of the short-term interest rate is negative and statistically significant; the coefficient on the second is positive, but insignificant. Finally, a 1 percentage point increase in personal income growth tends to be followed by a 0.6 percentage point increase in house prices in the subsequent quarter.

Table 6 reports the results from a set of regressions that includes the policy variables, X.[16] As with housing credit, the X's are first included one by one (the "individually" columns), and then all at once (the "jointly" columns). It is evident that none of the policies has a tangible impact on house prices. The sum of the coefficients for the LTV variable is statistically significant at the 10% level, but the four-quarter effect is economically small and statistically insignificant. Exposure limits have the desired effect and the magnitudes are economically meaningful, but due to the large standard errors the hypothesis of a zero effect cannot be rejected.

Things are slightly more promising in the specification that allows for asymmetric effects of tightening and loosening actions. As shown in Table 7, the sum of the coefficients for the tightening of LTV requirements is somewhat larger than in the symmetric specification (−4.10 versus −2.18), but the four-quarter effect remains insignificant. Another difference is that the sum of the coefficients for increasing housing-related taxes is negative and statistically significant at the

[16] The estimated coefficients on the non-policy variables are again largely unchanged by the inclusion of the policy variables.

5% level, suggesting that higher taxes slow house price growth. The estimated four-quarter effect is significant at only the 10% level, however. As in the regressions for housing credit, loosening exposure limits has a discernible positive impact on house price growth. (The caveat about the small number of actions in the sample is again applicable.) There is some evidence for asymmetric responses, with the symmetry hypothesis rejected at the 5% level for the exposure limit and tax variables.

4.2 Mean group regression analysis

A potentially serious issue in panel regressions such as those estimated in Section 4.1 is cross-country heterogeneity in the model parameters, which may lead to biased and inconsistent estimates. One solution to this problem is to use the mean group (MG) estimator proposed by Pesaran & Smith (1995). This involves estimating a separate time series regression for each country of the form

$$\begin{aligned} \Delta \ln C_{i,t} \quad = \quad & \alpha_i + \rho_i \Delta \ln C_{i,t-1} + \beta_{i,1} r_{i,t-1} + \\ & \beta_{i,2} r_{i,t-2} + \beta_{i,3} \Delta \ln y_{i,t-1} + \beta_{i,4} \Delta \ln y_{i,t-2} + \sum_{j=1}^{4} \gamma_{i,j} X_{i,t-j} + \varepsilon_{i,t} \quad , \end{aligned} \tag{11}$$

in which the coefficients are now also indexed by i. (Again, we use an analogous model for house prices, P.) Aggregating the country-specific estimates gives the MG estimator. The aggregation can be done either on an unweighted basis, or weighted as in the random coefficient specification of Swamy (1970). Either way, the estimated parameters' covariance matrix is used in the calculation of the standard errors.

This method obviously places much greater demands on the data. First, rather than estimate nine coefficients for the entire sample, we must estimate nine coefficients for *each country* — 513 if regressions were run on the 57 countries for which we have either credit or price data. Second, each country's time series must be long enough to allow for the estimation of equation 11. We set the threshold for inclusion at 20 usable observations. Third, in order to estimate the relevant γ coefficients, there must be at least one policy action in the sample for which there are sufficient time series data. Consequently, the number of degrees of freedom drops sharply both because of the loss of observations and because of the additional parameters to be estimated. It is therefore not surprising that in terms of statistical significance, the results from the MG method tend to be weaker than those from the conventional panel regressions of Section 4.1.

4.2.1 Housing credit

Table 8 reports the MG estimates of equation 11. As shown in the first two columns, except in the case of housing-related taxes, the loss of usable data associated with the MG method reduces the number of policy actions used in the estimation. Because of the requirement that each of the country-level time series regressions contains at least one policy action, the set of countries used in the calculation depends on the type of policy under consideration.

Moving rightward, the next two numeric columns in the table report the sum of the γ's and the four-quarter effect defined by equation 10, with an unweighted aggregation of the country-level parameter estimates. The next two contain the weighted (Swamy) estimates.[17]

The change in estimation method turns out to have relatively little effect on most of the parameter estimates of interest. The four-quarter effect of a unit DSTI change, for example, is -6.65 for the unweighted MG procedure versus -6.76 for the conventional panel regression. Neither is far off from the weighted estimate of -5.54. The MG estimates are much less precise than those of the panel regression, however. This makes a tangible difference for some of the variables, especially DSTI where the standard errors are approximately twice as large. The estimated four-quarter effect goes from being statistically significant at the 1% level in the panel regression to being significant at only the 5% level in the unweighted MG results. The remaining coefficients are statistically significant at the 10% level in the weighted results.

One sees a similar tendency in some of the other estimates, including those of the LTV and exposure limits. Somewhat surprisingly, the results turn out to be stronger for general credit policies. In that case, the unweighted estimated four-quarter effect is -1.60 (statistically significant at the 10% level) compared with a statistically insignificant -0.55 in the panel regression. The effect is not statistically significant in the weighted case, however, owing to the larger standard errors.

4.2.2 House prices

Table 9 reports the MG estimates of an equation analogous to equation 11, replacing housing credit growth with house price growth. Having obtained such weak results using conventional

[17] The Rats `meangroup` and `swamy` procedures were used for the calculations.

panel regression analysis, it is unlikely that the change in method would improve matters. As expected, with one conspicuous exception none of the estimated effects is significant at even the 10% level.

The lone exception is housing-related tax changes, which now have a large and highly statistically significant impact on house price growth. Taken at face value, an incremental tightening would slow house price growth by 3 percentage points. Unfortunately, this result disappears in going to the weighted results, as doing so both shrinks the parameter estimates and increases the standard errors. (The event study analysis in the following section gives some insights as to why the results are sensitive to the weighting scheme.)

4.3 Event study analysis

The third method used to assess the policies' effects is a panel event study analysis. As discussed in MacKinlay (1997), the conventional event study involves identifying discrete events and then partitioning time series into two mutually exclusive subsamples: a set of estimation windows over which a forecasting model is fit, and a set of event windows spanning some period of time following the event. The events' effects are calculated by subtracting the forecast values from the actual values during the event window. For an event occurring in 2004Q1, for example, a four-quarter event window would span 2004Q2 through 2005Q1, and the model would be estimated on data through 2003Q4 and after 2005Q1. In a panel setting, the results from individual (in our case country-specific) event studies are aggregated to create an estimate of the average.

The event study method has several advantages over other techniques. One is that it is (arguably) less susceptible to endogeneity since the events are excluded from the estimation of the econometric model. The second is that it imposes less of a parametric structure than either the panel regression or MG methods. And like the MG technique, it allows for cross-country heterogeneity in the underlying model.

The most common use of event study analysis involves high-frequency financial data, in which the data are plentiful and the events widely spaced. The application of the method to our study poses two challenges. One is that there are far fewer observations relative to the number of events. This reduces the amount of data in the estimation window, making it more difficult (and in some cases impossible) to estimate a reasonable forecasting model.

Closely spaced events create a second difficulty. Continuing with the previous example, the question is what to do when a second (or third) event occurs within four quarters after the first (e.g. in 2005Q1 and again in 2005Q3). In our analysis, we begin the event window one quarter after the last event in such a sequence (e.g. 2005Q4). This further reduces the number of observations available for estimating the forecasting model. It also means that the number of events is considerably smaller than the number of policy actions. In order to estimate a plausible forecasting model, we restrict the analysis to countries with 20 usable observations in the union of the estimation windows. The set of countries represented is therefore even narrower than in the MG analysis.

The forecasting model fitted to the data is the same as the one used in the MG analysis, equation 11 (and an analogous version for house prices). A four-quarter event window is used, making the estimates quantitatively comparable to the estimated four-quarter effects from the panel regression and MG analysis. The comparison is done on the basis of static forecasts, which use the actual data for the lagged dependent variable.[18] Tightening and loosening actions are treated as distinct events, so the procedure intrinsically allows for asymmetric effects.

There are two ways to aggregate the country-level estimates. One is to take the simple, unweighted average (but still use the estimated country-specific variances in the calculation of the overall standard error). These are reported as the "unweighted" estimates. An alternative is to use the (inverse of) the country-specific standard deviations as weights (analogous to weighted least squares regression), creating the estimates reported in the "weighted" column.

4.3.1 Housing credit

Table 10 reports the results of the event study analysis of credit growth. The first two numeric columns show the number of countries and events used in estimating the policies' effects. Largely due to a propensity for frequent adjustments in reserve requirements, the number of general credit events is less than 20% of the number of policy actions. For the other policies, the number of distinct events is roughly one half to two thirds of the number of actions in the corresponding MG analysis.

The salient result from the table is the strong negative effect of DSTI tightening. The unweighted estimates imply that averaged across the 13 countries in the sample, a typical decrease

[18] Dynamic forecasts (using the fitted values as the lagged dependent variable) yield similar results.

in the maximum DSTI ratio is associated with a 3.7 percentage point reduction in the rate of real housing credit growth (4.2 percentage points for the weighted estimate). The effect is statistically significant at the 1% level. The effects are somewhat smaller but still not far from the panel regression and MG estimates. None of the other estimated tightening effects is statistically significant.

Only two DSTI loosening events survive the paring-down process, unfortunately, and so those estimates (as well as those for exposure limits) are left unreported. One anomaly is the statistically significant *reduction* in credit growth following a loosening of the LTV requirement. (Note that since the numbers represent the difference in growth rates, one would expect to see *positive* numbers in the loosening columns.) None of the other estimated loosening effects is significant.

The relatively small number of observations and high volatility of the dependent variable make the event study analysis highly sensitive to outliers. Nowhere is this more evident than is the response of housing credit to tightenings in general credit conditions. Each dot in Figure 1 is the estimated country-specific effect of the general credit tightening actions, ordered by size. The bars represent the 90% confidence intervals. The figure shows that most of the responses are either negative or close to zero. Bulgaria is a conspicuous outlier, however. There, housing credit continued to grow at a double-digit pace even after a 4 percentage point increase in the reserve requirement ratio in September 2007. Needless to say, it would be inappropriate to exclude anomalous observations *ex post*. But it is worth noting that if Bulgaria *were* dropped, the estimated impact on housing credit of a general credit tightening would have been a highly significant -4.7 percentage points in the unweighted case and -3.25 percentage points in the weighted case.

4.3.2 House prices

Table 11 presents an analogous set of results for house prices. The table shows that only housing-related tax increases (or subsidy reductions) have statistically significant effects. The magnitudes are strikingly similar to those estimated using the MG method: -3.6 percentage points in the unweighted case (statistically significant at the 1% level) and -1.9 percentage points in the weighted case (significant at the 10% level).

A recurring theme in both the MG and event study analyses is the difference between the weighted and unweighted estimates. Figure 2 illustrates the reason for this discrepancy. (As in Figure 1, the dots are the estimated country-specific effects and the bars represent the 90% confidence intervals.) The overall tendency is clearly negative. And although zero is within the confidence interval for all but two of the countries, the simple average is statistically significant. A number of the large negative responses are associated with large standard errors, however. The weighted procedure discounts these observations proportionally, which tends to shrink the estimates.

5 Conclusions

Utilizing a new database of credit and housing-related tax policy measures and three alternative econometric approaches, this paper has provided a systemic assessment of the efficacy of credit and housing-related tax policies on housing credit and house prices. The evidence shows that certain types of targeted credit and/or tax policies can affect the housing market, and could potentially be used as tools to promote financial and macroeconomic stability.

Using conventional panel regressions, we found that housing credit responds in the expected way to changes in the maximum DSTI ratio, the maximum LTV ratio, exposure limits, and housing-related taxes. However, these results are somewhat sensitive to the choice of econometric technique. In the MG and event study analyses, only changes in the maximum DSTI ratio have statistically significant effects on housing credit. Depending on the method used, an incremental tightening in the DSTI ratio is associated with a 4 to 7 percentage point deceleration in credit growth over the following year. Loosenings have a comparable but imprecisely estimated effect in the opposite direction. An increase in housing-related taxes is the only policy with a measurable impact on house prices, with an incremental tightening associated with a 2 to 3 percentage point reduction in house price growth. Tax reductions have no discernible impact on house prices.

From a policy perspective, the negative results are in some respects as important as the positive ones. One such finding is that instruments affecting the supply of credit generally by increasing the cost of providing housing loans (reserve and liquidity requirements and limits on credit growth) have little or no detectable effect on the housing market. Nor do risk-weighting and provisioning requirements, which target the supply of housing credit. Exposure limits, which work not by the cost of lending but through the quantity restrictions on banks' loan supply, may be an exception in this regard, although the small number of documented policy actions makes it hard to draw firm conclusions. Measures aimed at controlling credit supply are therefore likely to be ineffective.

Of the two policies targeted at the demand side of the market, the evidence indicates that reductions in the maximum LTV ratio do less to slow credit growth than lowering the maximum DSTI ratio does. This may be because during housing booms, rising prices increase the amount that can be borrowed, partially or wholly offsetting any tightening of the LTV ratio.

None of the policies designed to affect either the supply of or the demand for credit has a discernible impact on house prices. This has implications for the degree to which credit-constrained households are the marginal purchasers of housing or for the importance of housing supply, which is not explicitly considered in this study. Only tax changes affecting the cost of buying a house, which bear directly on the user cost, have any measurable effect on prices.

These conclusions all pertain to the policies' average effects in a sample of 57 heterogeneous economies. There is no reason to believe the effects will be the same everywhere, of course. A policy that is ineffective in one country may be highly effective in another, and vice versa. The essential next step is to understand how policy effectiveness is influenced, narrowly, by legal, institutional and financial structural features of the housing market and, more broadly, by the financial system.

Table 1: Economies included in the analysis

Region (number)	Economies
Asia-Pacific (13)	Australia, China, Hong Kong SAR, India, Indonesia, Japan, Korea, Malaysia, New Zealand, the Philippines, Singapore, Thailand, Chinese Taipei
Central & eastern Europe (15)	Bulgaria, Croatia, the Czech Republic, Estonia, Hungary, Latvia, Lithuania, Poland, Romania, Russia, Serbia, Slovakia, Slovenia, Turkey, Ukraine
Latin America (6)	Argentina, Brazil, Chile, Colombia, Mexico, Peru
Middle East and Africa (2)	Israel, South Africa
North America (2)	Canada, United States
Western Europe (19)	Austria, Belgium, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Italy, Luxembourg, Malta, the Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, United Kingdom

Table 2: Correlations between policy measures

	Gen cred	LTV	DSTI	Expo lim	Risk wt	Prov	Tax
General credit	1						
LTV ratio	0.08	1					
DSTI ratio	0.15	0.58	1				
Exposure limits	−0.11	0.07	0.11	1			
Risk-weighting	0.01	0.08	0.08	−0.06	1		
Provisioning	0.08	0.23	0.29	0.04	0.13	1	
Housing-related tax	−0.01	0.00	0.15	−0.00	−0.00	0.06	1

Notes: The correlations are calculated for the 57 economies used in the empirical analysis. The underlying monthly data are summed to obtain quarterly series. The general credit category includes credit growth limits, reserve requirements and liquidity requirements.

Table 3: Descriptive statistics

Variable	Mean	SD	Fractiles 50%	5%	95%	Min	Max	Obs
Real housing credit growth	9.5	15.6	7.2	−7.9	38.3	−81.5	116.2	3730
Real house price growth	2.0	12.3	1.6	−15.2	20.6	−100.7	101.7	4067
Real personal income growth	2.4	3.2	2.5	−2.8	7.8	−19.3	14.8	4447
Short-term interest rate	6.3	5.3	4.8	0.5	17.4	0.0	39.5	4363
Inflation rate	4.3	4.7	3.1	−0.6	13.6	−11.7	33.4	4556

Notes: Housing credit growth, house price growth, real income growth and inflation are expressed in annualized quarterly growth rates in percentage terms. The interest rates are expressed in percentage terms. The sample covers the period with valid data for either housing credit or house prices, whose start dates are given in the appendix.

Table 4: Panel regression results for housing credit with symmetric effects

Policy	Actions	Individually Sum	Individually 4Q	Jointly Sum	Jointly 4Q
General credit	378	−1.58	−0.55	−1.24	−0.37
		(1.14)	(0.51)	(1.44)	(0.49)
LTV limits	80	−4.69***	−2.11**	−0.29	−0.04
		(1.80)	(0.85)	(2.16)	(1.03)
DSTI limits	38	−14.19***	−6.76***	−12.74***	−6.19***
		(3.52)	(1.67)	(4.19)	(1.92)
Exposure limits	12	−10.94***	−4.64***	−10.34***	−4.41***
		(2.70)	(1.62)	(2.99)	(1.76)
Risk-weighting	44	−1.46	−0.63	0.20	0.05
		(3.84)	(1.53)	(4.11)	(0.96)
Provisioning	28	−3.38	−1.24	−2.99	−1.03
		(3.95)	(1.66)	(4.19)	(1.74)
Housing-related tax	108	−5.24**	−2.39**	−5.03**	−2.28**
		(2.46)	(1.09)	(2.48)	(1.09)

Notes: The dependent variable is annualized quarterly growth rate in real housing credit. Robust standard errors are in parentheses. Asterisks indicate statistical significance: *** for 1%, ** for 5% and * for 10%.

Table 5: Panel regression results for housing credit with asymmetric effects

	Tightening					Loosening				
		Individually		Jointly			Individually		Jointly	
Policy	Actions	Sum	4Q	Sum	4Q	Actions	Sum	4Q	Sum	4Q
General credit	179	−2.24*	−1.05**	−2.14*	−0.86*	199	−0.20	0.18	0.22	0.37
		(1.34)	(0.51)	(1.25)	(0.48)		(1.60)	(0.71)	(1.58)	(0.67)
LTV limits	59	−7.13***	−3.04***	−2.33	−0.97	21	4.10	1.36	11.39*	4.74*
		(1.50)	(0.66)	(1.62)	(0.69)		(7.48)	(3.35)	(6.50)	(2.78)
DSTI limits	32	−13.42***	−6.19***	−10.98***	−5.05***	6	−17.17	−8.89	−18.75	−9.52
		(3.68)	(1.74)	(4.19)	(1.93)		(16.17)	(8.16)	(14.82)	(7.55)
Exposure limits	6	1.05	−0.59	2.57	0.69	4	−16.74***	−7.11*	−19.21***	−7.93**
		(10.72)	(4.40)	(9.96)	(4.07)		(6.13)	(3.69)	(5.60)	(3.23)
Risk-weighting	31	−6.78	−2.54	4.59	−1.58	13	11.34	4.03	10.73	3.60
		(3.97)	(1.56)	(4.00)	(1.53)		(7.69)	(3.09)	(7.79)	(3.12)
Provisioning	22	−5.45*	−1.64	−4.51*	−1.19	6	5.29	1.03	4.84	0.80
		(3.21)	(1.18)	(3.01)	(1.01)		(12.46)	(5.56)	(13.79)	(5.86)
Housing-related tax	48	−7.10**	−2.70**	−5.98**	−2.19*	60	−3.63	−2.13	−3.93	−2.24
		(2.93)	(1.31)	(2.55)	(1.15)		(3.74)	(1.77)	(3.78)	(1.76)

Notes: The dependent variable is annualized quarterly growth rate in real housing credit. Robust standard errors are in parentheses. Asterisks indicate statistical significance: *** for 1%, ** for 5% and * for 10%. The hypothesis of symmetric effects for the sum of the coefficients and the average four-quarter effect is rejected at the 5% level for LTV limits and risk-weighting.

Table 6: Panel regression results for house prices with symmetric effects

		Individually		Jointly	
Policy	Actions	Sum	4Q	Sum	4Q
General credit	420	−0.37	−0.07	−0.24	−0.01
		(0.82)	(0.31)	(0.80)	(0.30)
LTV limits	85	−2.18*	−0.58	−2.01	−0.54
		(1.20)	(0.55)	(1.98)	(0.84)
DSTI limits	42	−2.67	−0.70	−1.70	−0.47
		(4.17)	(1.54)	(4.91)	(1.86)
Exposure limits	19	−7.62	−3.18	−8.76	−3.56
		(6.83)	(2.94)	(6.78)	(2.99)
Risk-weighting	45	6.99	1.71	8.07*	2.08
		(4.39)	(1.70)	(4.42)	(1.71)
Provisioning	34	−0.61	−0.58	−0.63	−0.52
		(3.18)	(1.34)	(3.29)	(1.33)
Housing-related tax	120	−1.34	−0.33	−1.24	−0.32
		(1.46)	(0.53)	(1.54)	(0.60)

Notes: The dependent variable is annualized quarterly growth rate in the real house price. Robust standard errors are in parentheses. Asterisks indicate statistical significance: *** for 1%, ** for 5% and * for 10%.

Table 7: Panel regression results for house prices with asymmetric effects

	Tightening					Loosening				
		Individually		Jointly			Individually		Jointly	
Policy	N	Sum	4Q	Sum	4Q	N	Sum	4Q	Sum	4Q
General credit	202	−0.68	−0.10	−0.73	−0.10	218	0.22	0.09	0.59	0.25
		(0.86)	(0.30)	(0.89)	(0.26)		(1.30)	(0.54)	(1.30)	(0.51)
LTV limits	60	−4.10**	−1.08	−3.42**	−0.82	25	5.02	1.64	6.03	1.89
		(1.88)	(0.69)	(1.43)	(0.63)		(5.67)	(2.28)	(5.81)	(2.40)
DSTI limits	33	−2.81	−0.59	−0.13	0.19	9	−1.55	0.60	−2.16	−0.70
		(5.67)	(2.08)	(5.94)	(2.31)		(6.11)	(2.30)	(6.51)	(2.50)
Exposure limits	9	1.19	0.33	1.41	0.44	10	−13.71**	−5.68*	−19.88**	−7.88**
		(10.95)	(4.17)	(11.34)	(4.43)		(6.77)	(3.19)	(8.70)	(4.43)
Risk-weighting	32	0.89	0.18	0.92	0.17	13	20.47	5.26	23.95**	6.43
		(4.72)	(1.62)	(4.78)	(1.73)		(12.73)	(4.65)	(11.85)	(4.25)
Provisioning	28	2.56	0.84	0.91	0.21	6	−15.16	−6.96	−10.77	−5.34
		(4.29)	(1.57)	(4.71)	(1.78)		(14.41)	(6.55)	(13.68)	(6.62)
Housing-related tax	52	−7.60**	−2.70*	−7.78**	−2.83*	68	3.18*	1.46	3.63*	1.57
		(3.49)	(1.42)	(3.67)	(1.50)		(1.95)	(0.94)	(2.04)	(0.96)

Notes: The dependent variable is annualized quarterly growth rate in the real house price. Robust standard errors are in parentheses. Asterisks indicate statistical significance: *** for 1%, ** for 5% and * for 10%. The hypothesis of symmetric effects for the sum of the coefficients is rejected at the 5% level for the exposure limit and tax variables

Table 8: Mean group regression results for housing credit

			Unweighted		Weighted	
Policy	Countries	Actions	Sum	4Q	Sum	4Q
General credit	42	349	−4.18**	−1.60*	−3.47	−1.43
			(2.13)	(0.90)	(2.91)	(1.20)
LTV limits	23	77	0.24	0.09	−0.16	−0.23
			(4.32)	(1.81)	(5.85)	(2.52)
DSTI limits	15	35	−13.69*	−6.65**	−11.61*	−5.54*
			(7.08)	(3.06)	(6.57)	(3.33)
Exposure limits	6	10	−7.71	−2.98	−6.89	−2.72
			(7.62)	(3.14)	(7.81)	(3.34)
Risk-weighting	22	42	−7.57*	−2.93*	−5.28	−2.00
			(3.98)	(1.73)	(4.41)	(2.00)
Provisioning	12	24	0.05	−0.43	0.33	−0.35
			(6.93)	(3.25)	(5.86)	(2.79)
Housing-related tax	28	108	−3.05	−1.47	−2.48	−1.19
			(2.46)	(1.06)	(2.92)	(1.30)

Notes: The dependent variable is annualized quarterly growth rate in real housing credit. Standard errors are in parentheses. Asterisks indicate statistical significance: *** for 1%, ** for 5% and * for 10%.

Table 9: Mean group regression results for house prices

			Unweighted		Weighted	
Policy	Countries	Actions	Sum	4Q	Sum	4Q
General credit	47	404	−1.72	−0.52	−0.45	−0.10
			(1.86)	(0.68)	(2.28)	(0.89)
LTV limits	25	79	6.40	2.54*	3.80	1.67
			(4.09)	(1.49)	(3.97)	(1.50)
DSTI limits	17	41	−0.27	0.09	−0.67	0.07
			(6.39)	(2.59)	(7.19)	(2.90)
Exposure limits	7	15	5.82	1.28	4.17	0.80
			(10.65)	(3.89)	(14.08)	(5.57)
Risk-weighting	21	40	1.21	−0.10	2.33	0.45
			(4.96)	(1.86)	(5.10)	(1.93)
Provisioning	14	30	−0.99	−0.38	−1.83	−0.65
			(6.46)	(2.24)	(6.66)	(2.40)
Housing-related tax	31	119	−7.75***	−3.05***	−4.73	−1.87
			(2.85)	(1.09)	(4.02)	(1.72)

Notes: The dependent variable is annualized quarterly growth rate in the real house price. Standard errors are in parentheses. Asterisks indicate statistical significance: *** for 1%, ** for 5% and * for 10%.

Table 10: Panel event study results for housing credit

	Tightening				Loosening			
Policy	Countries	Events	Unweighted	Weighted	Countries	Events	Unweighted	Weighted
General credit	11	15	−0.81	−2.71	30	57	0.77	−0.07
			(4.12)	(2.00)			(1.20)	(0.77)
LTV limits	15	23	−0.66	−0.86	12	16	-3.79^{***}	-2.03^{*}
			(1.40)	(1.24)			(1.45)	(1.22)
DSTI limits	13	15	-3.67^{***}	-4.20^{***}	2	2	…	…
			(1.43)	(1.05)			…	…
Exposure limits	5	5	−1.03	−1.61	3	3	…	…
			(1.49)	(0.98)			…	…
Risk-weighting	19	24	−0.32	−1.09	7	8	1.41	1.96
			(1.11)	(0.87)			(8.55)	(4.79)
Provisioning	7	8	−0.39	−1.30	5	5	0.97	1.73
			(1.17)	(0.79)			(2.64)	(2.64)
Housing-related tax	19	29	−2.40	−1.90	17	29	0.82	−0.29
			(1.52)	(1.21)			(3.07)	(1.78)

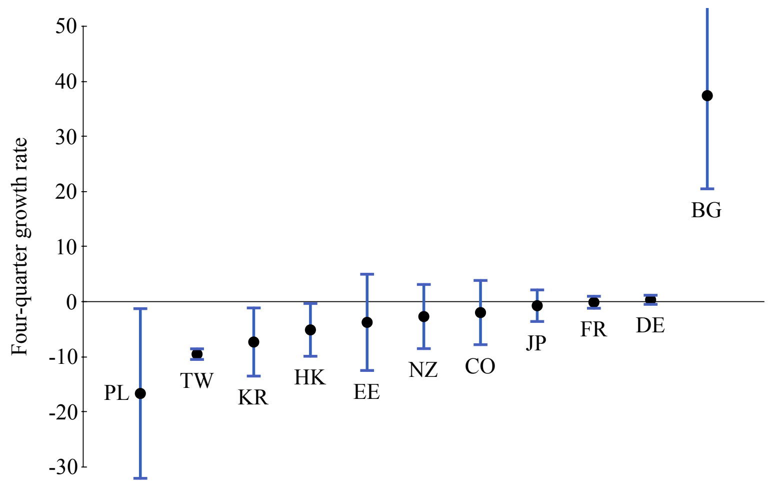
Notes: The dependent variable is annualized quarterly growth rate in real housing credit. Standard errors are in parentheses. Asterisks indicate statistical significance: *** for 1%, ** for 5% and * for 10%.

Table 11: Panel event study results for house prices

	Tightening				Loosening			
Policy	Countries	Events	Unweighted	Weighted	Countries	Events	Unweighted	Weighted
General credit	12	18	−1.66	−0.59	33	60	−1.08	−0.04
			(1.38)	(1.01)			(1.45)	(0.90)
LTV limits	16	23	−0.15	0.41	11	19	−1.48	−1.35
			(2.01)	(1.18)			(1.80)	(1.35)
DSTI limits	12	14	3.30	0.98	3	3	...	...
			(3.67)	(1.75)			...	...
Exposure limits	5	5	0.38	−1.24	2	2	...	...
			(4.71)	(2.65)			...	...
Risk-weighting	18	22	0.07	−1.54	7	8	1.46	−0.55
			(1.11)	(0.99)			(2.43)	(1.48)
Provisioning	10	12	−2.40	−0.87	4	4	−2.54	1.26
			(2.00)	(1.12)			(5.96)	(1.58)
Housing-related tax	16	28	−3.61***	−1.88*	18	33	−2.53	0.61
			(1.39)	(1.12)			(2.65)	(1.82)

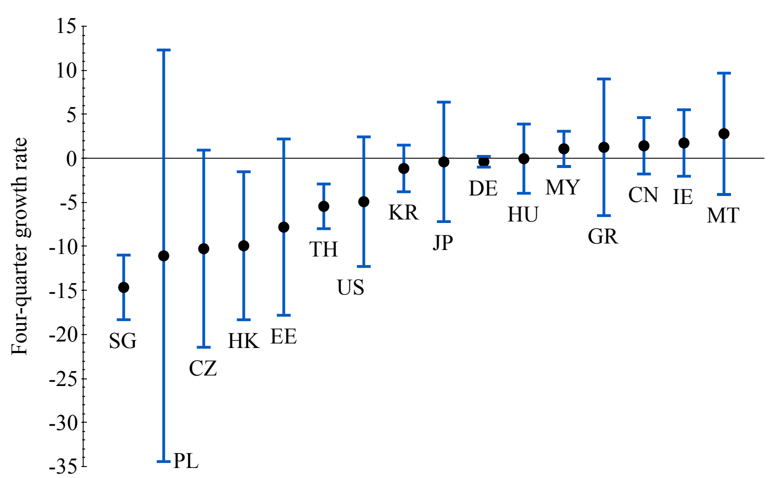
Notes: The dependent variable is annualized quarterly growth rate in the real house price. Standard errors are in parentheses. Asterisks indicate statistical significance: *** for 1%, ** for 5% and * for 10%.

Figure 1: Event study responses of housing credit to a general credit tightening



Notes: BG = Bulgaria; CO = Colombia; DE = Germany; EE = Estonia; FR = France; HK = Hong Kong SAR; JP = Japan; KR = Korea; NZ = New Zealand; PL = Poland; TW = Chinese Taipei.

Figure 2: Event study responses of house prices to housing-related tax increases



Notes: CN = China; CZ = Czech Republic; DE = Germany; EE = Estonia; GR = Greece; HK = Hong Kong SAR; HU = Hungary; IE = Ireland; JP = Japan; KR = Korea; MT = Malta; MY = Malaysia; PL = Poland; SG = Singapore; TH = Thailand; US = United States.

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