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Digging Deeper—Evidence on the Effects of Macroprudential Policies from a New Database

This paper introduces a comprehensive database of macroprudential policies, which covers 134 countries from January 1990. Using a novel numerical indicator of the tightness of loan-to-value (LTV) regulations, we estimate the policy effects of incremental tightening in LTV limits, employing a propensity score-based method to address endogeneity concerns. The results point to economically significant and nonlinear effects on household credit, with a declining per-unit impact for larger tightening measures. The analysis indicates that policy leakage effects could be a factor behind the nonlinear effects. We finally find that the side effects of macroprudential policies on consumption and output are relatively small.

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DESPITE CONSIDERABLE PROGRESS OVER THE past years in assessing the effectiveness of macroprudential policies, many questions remain open.¹ In particular, the literature has so far not fully succeeded in rigorously quantifying the effects of various macroprudential measures. This is due in part to a reliance on incomplete data sets in terms of the coverage of countries and measures. Moreover, previous research has mostly used dummy-type policy action indices, which do not allow for an estimation of quantitative effects of policies, a key issue for policymakers. In addition, endogeneity problems often hamper a proper assessment of the effects: macroprudential measures may often be taken in response to developments in credit and asset prices. If not properly addressed econometrically, this will tend to result in biased estimates, typically understating the effects of macroprudential measures.²

In this paper, we address some of these shortcomings by making progress on four fronts. First, we present a new comprehensive database of macroprudential policies, combining information from various sources. Second, we evaluate the quantitative impact of a 1 percentage point (ppt) reduction in loan-to-value (LTV) limits, one of the most widely used macroprudential policy instruments. While previous studies provide qualitative evidence for the effects of LTV limits in containing credit growth, a quantification of the effects is still limited, since most studies use a dummy-type indicator of policy changes.³ Our new database contains rich numerical information on LTV limits, allowing us to estimate the per-unit effects of varying these limits on household credit, and to investigate potential nonlinearities in those effects. Third, we address the endogeneity problem for estimating policy effects by using a propensity score-based method that has been proposed in the literature (Robins, Rotnitzky, and Zhao 1994) to identify causal effects. Fourth, we make progress toward assessing side effects of macroprudential policies, by investigating the impact of macroprudential tools, including changes in LTV limits, on private consumption.⁴ Assessing the effects

1. For a literature survey, see Galati and Moessner (2018).

2. Studies trying to address this problem include Richter, Schularick, and Shim (2019) and some studies using micro data, such as Allen et al. (2020), Basten and Koch (2015), and Epure et al. (2018).

3. While most empirical studies provide per-action policy effects using dummy-type policy action indicators, a few studies go beyond per-action effects. Glocker and Towbin (2015) construct a weighted average of reserve requirements in Brazil and estimate their per-unit effects on macrofinancial variables. Vandenbussche et al. (2015) construct intensity-adjusted policy change indicators for 29 categories of prudential measures and assess their impacts on house prices in 16 countries in Central, Eastern, and Southeastern Europe. Richter, Schularick, and Shim (2019) build a quantified LTV change variable and carry out a cross-country empirical analysis of the effect of changes in LTV limits on GDP, inflation, credit, and asset prices.

4. See Richter, Schularick, and Shim (2019) for another recent approach to assess the effects of LTV actions.

on consumption is a first step toward a more comprehensive assessment of the costs and benefits of macroprudential policy, which is outside of the scope of this paper.⁵

Our new database, the integrated Macroprudential Policy (iMaPP) database, has three advantages over other available databases. First, it provides a comprehensive coverage in terms of instruments, countries, and time periods. It combines information from five existing databases, that are cross-checked against additional sources, such as authorities' official announcements and IMF country documents, and it also integrates all information contained in the IMF's new Annual Macroprudential Policy Survey database.⁶ Second, the iMaPP database provides the average LTV limit prevailing in a given country at any given point in time, whereas most other databases only provide dummy-type policy action indicators.⁷ Third, the iMaPP database is being updated annually using information from the IMF's Annual Macroprudential Survey.⁸

Using these new and comprehensive data, we find strong and nonlinear effects of LTV tightening on household credit, and modest side effects on consumption. For the most common magnitude of LTV action in our sample—a tightening of less than 10 ppts—a 1 ppt LTV tightening cumulatively reduces household credit growth by about 0.5 ppts after four quarters. For larger actions—a tightening of between 10 ppts and 25 ppts—the cumulative decline in household credit growth per 1 ppt tightening is found to be smaller, at 0.2 ppts.

The smaller per-unit effects on household credit of a larger LTV tightening could be driven by policy leakages, because a strong tightening could incentivize credit from abroad or from nonbank lenders to which LTV limits may not apply. By examining policy effects on household credit by type, we find supporting evidence, albeit indirect in nature due to the limited number of large tightening episodes. Specifically, we find that tightening LTV limits leads to a stronger reduction in growth for the types of household credit typically covered by the regulatory LTV limits (e.g., bank mortgage loans). While we also examine other possible reasons (peculiarities associated with the introduction cases of new LTV limits, potential concentration of narrowly applied

5. See Alpanda and Zubairy (2017), Svensson (2017), and Brandao-Marques et al. (2020) for a cost-benefit assessment of macroprudential policy as well as other policies.

6. For more information on the Annual Macroprudential Policy Survey, see IMF (2018a) and online at www.elibrary-areaer.imf.org/Macroprudential/Pages/Home.aspx

7. Although a few databases provide intensity-adjusted policy action indicators (Vandenbussche et al. 2015 and Richter, Schularick, and Shim 2019), they do not provide the level information of the LTV limits, which the iMaPP database does. Taking the average over various regulatory limits was considered by Glocker and Towbin (2015) for reserve requirements, while the iMaPP is the first to apply it to the levels of LTV limits. See Online Appendix A Table A4 for more information on how the iMaPP database compares to other existing databases.

8. The iMaPP database initially covered data through December 2016 when it was first released as an associated data set of Alam et al. (2019). The updated versions with the latest data have been released annually on the iMaPP website. See www.elibrary-areaer.imf.org/Macroprudential/Pages/Home.aspx or www.imf.org/iMaPP

LTV limits in the group of larger tightening), we conclude that they are not the main drivers behind the nonlinear effects.

We finally provide a comprehensive analysis of the side effects of macroprudential policies on consumption and output. We find that the effects of changes in LTV limits on consumption are much smaller than the effects on credit. For example, estimates of per-unit effects of changing LTV ratio caps on consumption are not statistically significant. We also find mostly modest effects on consumption and output when examining the effects “per policy action” of a broader range of macroprudential tools captured by the iMaPP database. These results confirm and extend previous findings in the literature, such as those reported by Richter, Schularick, and Shim (2019), who find small effects on output of changes in LTV caps.

The rest of the paper is structured as follows. Section 1 compares features of the iMaPP database to other available databases and explains the construction of the average LTV limit series. Section 2 discusses the methodologies used for estimation. Section 3 quantifies the effects and the side effects of a 1 ppt reduction in the LTV limits. The section shows diminishing per-unit effects of LTV tightening and presents a deeper analysis of structural factors behind the nonlinear policy effects. Section 4 revisits the standard regression analysis of effects per policy action, using multiple indices of macroprudential policy actions available from the iMaPP database, and Section 5 concludes.

1. IMAPP DATABASE

1.1 Key Features of the Database

This paper introduces a new database of macroprudential policy measures: the iMaPP database.⁹ It integrates information from five major existing databases and enriches this with information from the IMF’s new Annual Macroprudential Policy Survey (Online Appendix A).¹⁰

The iMaPP database provides the most comprehensive source of information on the use of macroprudential instruments to date.¹¹ It covers the main instruments discussed in IMF (2014), classifying them into 17 categories, and providing information on these measures for 134 countries at a monthly frequency from January 1990 to the latest available. For selected instruments (e.g., capital requirements), it also provides the subcategories of general, household-sector, and corporate-sector measures

9. Our definition of macroprudential policy is the “use of primarily prudential tools to limit systemic risk,” following IMF (2013) and IMF-FSB-BIS (2016).

10. The five existing databases are Lim et al. (2011, 2013), the Global Macroprudential Policy Instrument (GMPI) survey conducted by the IMF in 2013, Shim et al. (2013), and the database by the European Systemic Risk Board (ESRB). See Online Appendix A for more information.

11. See Online Appendix A Table A4 for a comparison of the coverages of existing databases and the iMaPP database.

to allow researchers to examine the effects of instruments that target sector-specific exposures. For each category, it provides dummy-type policy action indicators and descriptions. It is important to note that the iMaPP database includes policy instruments (such as reserves requirements) that can be macroprudential in nature but also serve other purposes, because macroprudential policy instruments can overlap with those of other policies, such as monetary policy and capital flow management measures (IMF 2012, 2017).

In addition, the iMaPP database provides a novel quantitative measure of the regulatory limit on LTV ratios of real estate loans by banks and nonbank financial institutions—one of the most common macroprudential instruments, typically used to control risks in mortgage markets—for 66 countries from January 1990. Since many countries have multiple LTV limits for different mortgage loan categories (e.g., loans for owner-occupied properties, those for buy-to-let investments), we compute for each period the simple average of the prevailing regulatory LTV limits for all existing categories in each country and track the evolution through time of the average LTV limit for each country.¹²

The average LTV limit series is new. While most existing policy action indices only indicate the direction of a policy change, or at most the intensity of the policy change (Vandenbussche, Vogel, and Detragiache 2015, and Richter, Schularick, and Shim 2019), our average LTV limit series informs about the level of regulatory LTV limits as well as the magnitude of policy changes. Since the series tracks the prevailing limits on the LTV ratios, it allows us to quantify in greater detail the effects of changes in regulatory limits. In addition, because it contains the level information, it enables further examination of possible nonlinear effects of changes in regulatory limits (see Section 3).

However, there are also a few caveats. First, the iMaPP database does not cover every initial implementation, especially if instruments were introduced before the start of the sample period. Second, the database only includes policy actions that have been verified and cross-checked with official documents. In some cases, data availability was constrained, for example, by reporting differences. Third, the average LTV limit series may overstate the importance of LTV limits that only apply to a small group of loans. This is because the simple average gives equal weight to all categories, while regulatory changes affecting a large subset of loans could be considered more important than those applied to a smaller subset.¹³

12. LTV limits are mostly applied to residential housing loans extended by banks, while some countries also apply the LTV limits to residential housing loans extended by nonbanks (e.g., loans extended by insurance companies). The average LTV limit series does not reflect LTV limits on other types of loans (e.g., car loans), but these limits are explained in the descriptions and included in dummy-type policy action indicators. Please see Online Appendix A for more information.

13. One possible refinement could be to construct a weighted average of regulatory LTV limits, based on market shares of loan categories. However, such a weighted average requires time series of loan shares, which are not readily available in many countries.

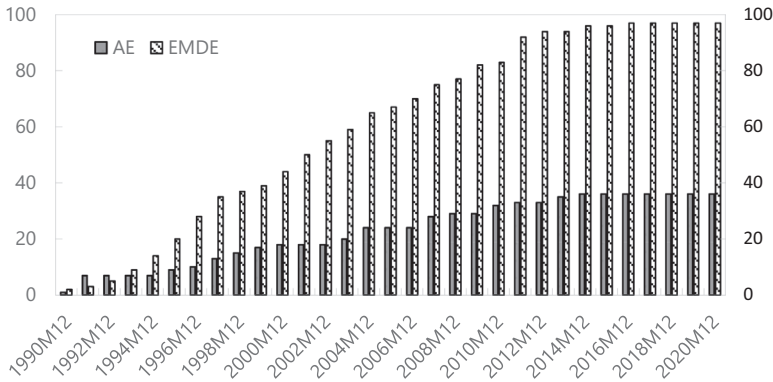


Fig 1. Number of Economies that Have Used Macroprudential Policy.
 SOURCES: The iMaPP database (see Online Appendix A for the original sources) and authors' calculations.
 NOTES: The figure shows the number of economies that have used any macroprudential policy instrument (except for reserve requirements) at least once during the sample period. There are in total 134 economies (36 AEs and 98 EMDEs) in the iMaPP database. AE = advanced economy; EMDE = emerging market and developing economy.

1.2 How Has Macroprudential Policy Been Used?

The iMaPP database reveals a growing prevalence of macroprudential policy worldwide. The number of economies that have used any macroprudential policy tool has increased steadily since 1990 (Figure 1). Interestingly, even before the global financial crisis, many countries had already implemented at least one macroprudential instrument—23 out of 36 advanced economies (AEs) and 61 out of 98 emerging market and developing economies (EMDEs) as of December 2006. The global financial crisis led to a broader recognition of the importance of financial stability, prompting global efforts to introduce macroprudential policies, including the Basel III countercyclical capital buffer (CCyB) and liquidity coverage ratio (LCR) requirement. As a result, by end 2016, all the 134 economies in the sample had activated or used at least one such tool.

Various instruments have been used both in AEs and EMDEs, while the most used instruments differ across these groups of countries (Figure 2). As of December 2020, limits on foreign exchange (FX) positions remain the tools most widely used among EMDEs, reflecting their exposure to vulnerabilities from external shocks, including volatile capital flows and exchange rates (Cerutti, Claessens, and Laeven 2017a). Basel III measures, such as the LCR and systemically important institution (SIFI) surcharges, and their adjustments since the onset of the COVID-19 pandemic in March 2020, are the most commonly deployed tools in AEs.¹⁴ Housing-related tools, such

14. See Online Appendix A Table A3 for the definitions of the iMaPP categories. For example, the Basel III's LCR requirement is captured in the "Liquidity" category, and the additional capital requirements for systemically important financial institutions are captured in the "SIFI" category. The high use of "Other" among AEs reflects the fact that many countries introduced restrictions on dividend distributions by financial institutions, complementing the relaxation of capital requirements, to ensure capital buffers were available to continue lending and absorb losses due to the pandemic shocks.

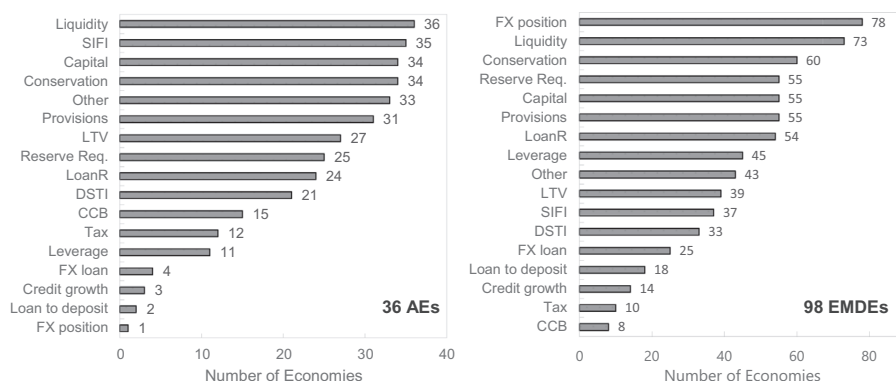


Fig 2. Prevalence of Use by Instrument, December 2020.

SOURCES: The iMaPP database (see Online Appendix A for the original sources) and authors' calculations.

NOTES: The figure shows the number of economies that have used the specified instrument as of December 2020. AE = advanced economy; EMDE = emerging market and developing economy.

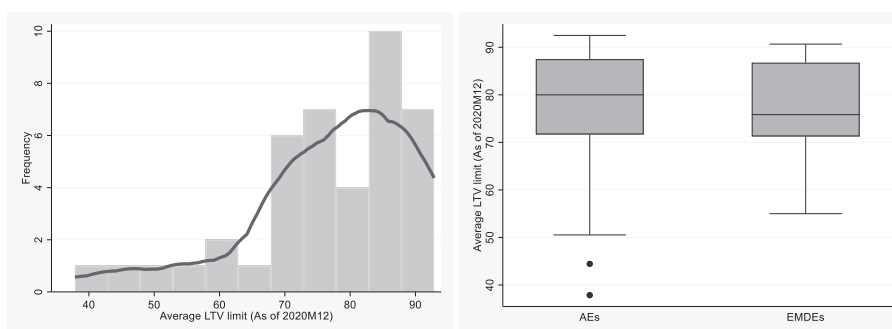


Fig 3. Distribution of the Average LTV Limit, December 2020.

SOURCES: The iMaPP database (see Online Appendix A for the original sources) and authors' calculations.

NOTES: The left panel shows the histogram of the average LTV limit of less than 100%, together with its kernel density estimate. The right panel shows the distributions for AEs and EMDEs. The box represents the interquartile interval, the inner line represents the median, and the outer lines represent the minimum and the maximum values. The dots represent outliers. AE = advanced economy; EMDE = emerging market and developing economy.

as LTV, have become common across both groups of countries, reflecting awareness since the global financial crisis of the need to contain housing sector vulnerabilities.

The iMaPP database also shows that regulatory LTV limits take a wide range of values across countries. For the 36 economies with an average LTV limit of less than 100% as of December 2020, the median is about 75%, but the distribution ranges from 47% to 95% (Figure 3, left panel).¹⁵ In particular, LTV limits in EMDEs appear tighter than those in AEs (Figure 3, right panel).

15. When no regulatory limits exist in a country, the average LTV limit is set to 100, which means no down payment requirement for secured loans. See Online Appendix A.

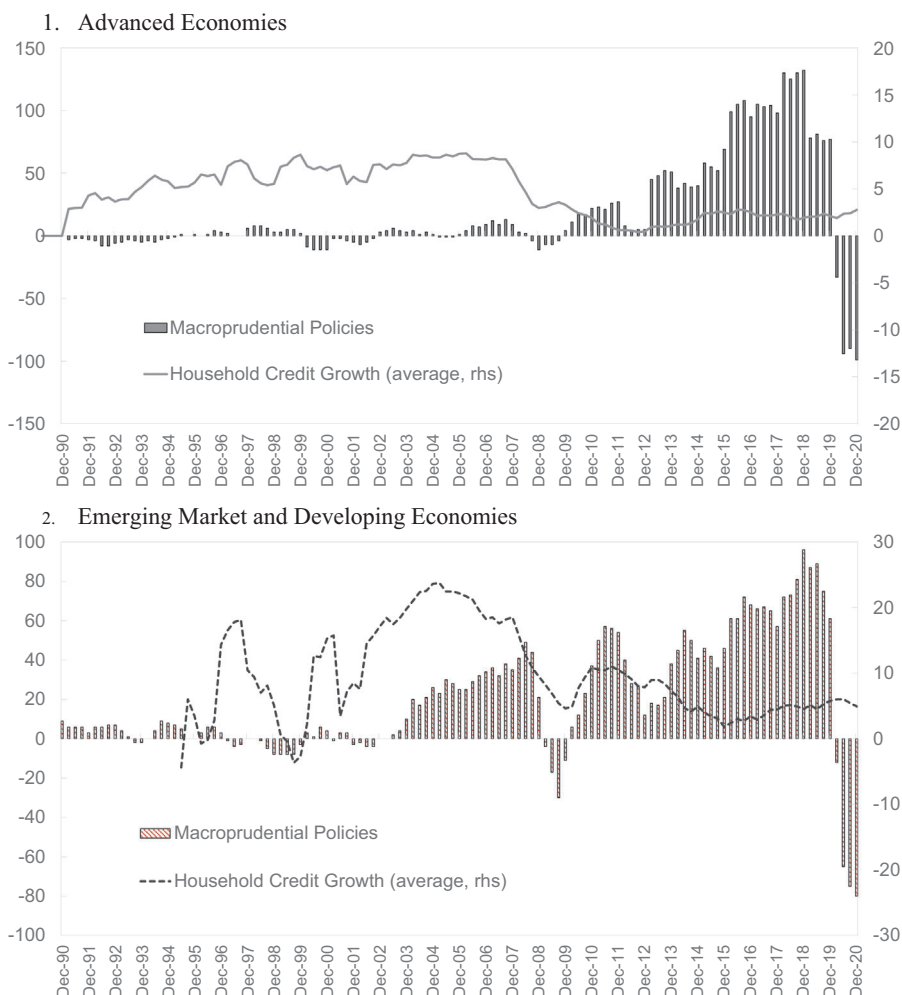


Fig 4. Usage of Macroprudential Policies over Time.

SOURCES: The iMaPP database, BIS, and the authors' calculations.

NOTES: This figure is based on the 27 AEs and 15 EMDEs for which quarterly data of household credit are available at the BIS database. The bars indicate the cumulative sum of the net number of tightening actions of any macroprudential policy instrument over the current and past three quarters and the lines indicate the average household credit growth. AE = advanced economy; EMDE = emerging market and developing economy.

Macroprudential policy tends to be tightened when household credit rises (Figure 4). For example, during the early 2000s, when credit growth was high, the net number of tightening actions across macroprudential instruments rose, especially for EMDEs, while net loosening occurred in crisis times, most pronounced around the onset of the recent COVID pandemic. This suggests that macroprudential authorities

actively take actions in response to credit developments, underscoring the importance of the reverse causality problem for empirical analysis.¹⁶ We address this issue when investigating in detail the effects of changes in LTV limits in the next sections.

2. QUANTIFYING THE EFFECTS OF LTV LIMIT TIGHTENING—METHODOLOGY

2.1 Propensity Score–Based Approach

We now turn to the quantification of the policy effect of tightening of LTV limits, which we want to measure “per unit” of the observed change in the limit, making use of the numerical indicator of LTV limits in the iMaPP database. Such a quantification has been scarce in the literature so far.¹⁷

To help identify the causal effects of LTV regulations, we employ a propensity score–based approach. To address reverse causality, previous studies typically rely on a timing assumption: macroprudential policy does not affect macrofinancial variables (e.g., credit growth) within the same quarter. However, this could be a rather strong assumption, especially for changes in LTV ratio caps that would typically have almost immediate effects on new loans. When lagging the change by one period, the estimated effects are then likely subject to an attenuation bias, that is, a bias toward zero.¹⁸ Therefore, we also employ an inverse propensity-score weighting estimator specifically designed for our purposes. More specifically, we combine a propensity score–based approach with a local projection method to estimate the policy effects for different horizons (Angrist, Jordà, and Kuersteiner 2016, Jordà and Taylor 2016, and Richter, Schularick, and Shim 2019).

The use of propensity score methods is relatively new in the macroeconomics literature, even as it has long been common in biostatistics.¹⁹ The idea of the inverse propensity score weighting estimator is to identify the causal effects of

16. Since consumption or GDP growth are not usually a direct motivation for macroprudential policy actions (Richter, Schularick, and Shim 2019), when estimating side effects on these variables, the reserve causality problem would be less severe but still existent, to the extent that household credit affects their consumption and economic activities.

17. Although not the main focus of this paper, in the Section 4, we show the estimated “per action” effects of various macroprudential instruments, revisiting the standard regression analysis with the dummy-type policy action indicators from the iMaPP database. The results corroborate the findings of Cerutti et al. (2017a) and Kuttner and Shim (2016) of the strong effectiveness of demand-side tools, such as LTVs, compared to other measures, such as capital requirements.

18. This attenuation bias can be present even if the policy variable enters the specification in lagged form; while lagging reduces the reverse causality, contemporaneous effects of policy measures are not then fully captured by the estimate.

19. See Glynn and Quinn (2010) and Jordà and Taylor (2016). As for macro-economic applications, Jordà and Taylor (2016), Angrist et al. (2016), Forbes et al. (2015), and Richter, Schularick, and Shim (2019) apply propensity score–based methods to estimate the effects of fiscal austerity, monetary policy, capital flow management measures, and macroprudential policy, respectively.

TABLE 1
SUMMARY STATISTICS OF THE CHANGE IN THE AVERAGE LTV LIMIT BY GROUP

	Ordered policy action indicator	Mean (ALL)	Standard deviation (ALL)	Number of observations		
				ALL	AE	EMDE
Tightening by no less than 10 ppts and no more than 25 ppts	−20	−17.0	5.5	29	16	13
Tightening by less than 10 ppts	−10	−3.3	2.4	56	36	20
No change (Control Group)	0	0.0	0.0	6,126	3,548	2,578
Easing by less than 10 ppts	10	3.6	2.3	21	11	10
Easing by no less than 10 ppts and no more than 25 ppts	20	14.3	4.6	8	3	5

Sources: The iMaPP database (see Online Appendix A for the original sources) and the authors' calculations.
Notes: The table shows the summary statistics of the change in the average LTV limit for the four treatment groups and for the control group in the estimation sample. Sample period: 1990Q1–2016Q4. Observations with Δ LTV less than or equal to −25 ppts are excluded for the estimation to mitigate the influence of outliers. Also, the sample excludes four observations where LTV limits were changed only for commercial real estate loans (Hong Kong 1994Q1 and 2013Q1, Malaysia 2011Q4, Poland 2014Q2), because the analysis in this paper primarily examines the household sector. AE = advanced economy; EMDE = emerging market and developing economy.

macroprudential policy by penalizing those observations that are more likely to be affected by reverse causality. While there are many variants of the estimator, we use the “augmented inverse propensity-score weighting” (AIPW) estimator, which achieves the smallest asymptotic variance in the class of the “doubly-robust” estimators.²⁰

The AIPW estimation of the effects of changes in LTV limits is conducted in three stages. In the first stage, an ordered logit model—the “treatment model”—is estimated to obtain the propensity score—the probability of changing the LTV limit. The dependent variable is an ordered policy action indicator taking on the values {−20, −10, 0, 10, 20}, which represent the buckets in which the change in the LTV limit (Δ LTV) is grouped into (Table 1), and the regressors are macrovariables that may influence policy actions. In the second stage, outcomes (e.g., credit growth) for each bucket of Δ LTV are predicted using macro-economic variables to correct for unobserved outcomes—the “outcome model.” Then, in the third stage, the average treatment effect on the outcome (e.g., credit growth) is estimated for each treatment group, using (i) the estimated inverse propensity scores to put more (less) weights on the observations that are less (more) likely to be affected by reverse causality; and (ii) the predicted outcomes in lieu of unobserved outcomes in the unrealized states.²¹ To obtain the estimated effect of a 1 ppt change in the LTV limit, we rescale the estimated effect in each treatment group by the average Δ LTV of the group. The AIPW

20. This class of the estimators involve estimating both a treatment model and an outcome model and have a “doubly-robust” property—that is, consistency of the estimated average treatment effect only requires either the treatment model or the outcome model to be correctly specified. See, for example, Robins et al. (1994) and Lunceford and Davidian (2004).

21. Please note that actual outcomes are only observed in a realized state—for example, for countries that tighten their LTV limits, we cannot observe their outcome (e.g., credit growth) in the hypothetical scenario where they did not tighten (i.e., the unrealized state).

estimation is conducted with panel data of 58 countries over the period 1990: Q1 to 2016: Q4.²² Online Appendix B provides further information on the data used for estimation.

The AIPW estimator of the average treatment effect is given as follows:

$$\widehat{ATE}_j^h = \frac{1}{NT} \sum_{t,n} \widehat{TE}_{j,it}^h,$$

where

$$\begin{aligned} \widehat{TE}_{j,it}^h = & \left[\left\{ \left(\frac{D_{j,it}}{\hat{p}_{j,it}} \right) (\Delta^h y_{it} - \hat{m}_{j,it}^h) + \hat{m}_{j,it}^h \right\} \right. \\ & \left. - \left\{ \left(\frac{D_{0,it}}{\hat{p}_{0,it}} \right) (\Delta^h y_{it} - \hat{m}_{0,it}^h) + \hat{m}_{0,it}^h \right\} \right] \cdot \left(\frac{1}{\Delta LTV_j} \right), \end{aligned} \quad (1)$$

h refers the horizon, j refers the treatment (with $j = 0$ indicating the control group—i.e., no change in the average LTV limit), i refers to the country, and t refers to time (quarter). $\Delta^h y_{it}$ refers to the h -horizon change in the outcome variable (e.g., log of real household credit), $D_{j,it}$ is the dummy variable of each treatment, $\hat{p}_{j,it}$ is the estimated propensity score, $\hat{m}_{j,it}^h$ is the predicted h -horizon change in the outcome variable, and ΔLTV_j is the average change in LTV limits in the treatment group j .

Treatment Model: To obtain the propensity scores ($\hat{p}_{j,it}$), we estimate the following ordered logit model:

$$z_{it}^* = \mathbf{X}_{i,t-1}^T \boldsymbol{\beta}^T + e_{it}^T, \quad (2)$$

where z_{it}^* is the latent variable behind the ordered policy action indicator (z_{it}), and $\mathbf{X}_{i,t-1}^T$ includes the lag of real household credit growth, the lag of real GDP growth, the lag of the interest rate, the lag of real house price growth, the lag of the action indicator of other macroprudential policies (the sum of the dummy-type policy-action indicators of all categories except for LTV), time dummies, and country dummies. e_{it}^T is the error term. As specified in Table 1, the ordered policy action indicator z_{it} represents one of the following groups: a tightening of more than or equal to 10 ppts and less than 25 ppts (−20), a tightening of less than 10 ppts (−10), no change (0—the control group), an easing of less than 10 ppts (10), and an easing of more than or equal to 10 ppts and less than 25 ppts (20).

The ordered logit estimation with country- or time dummies may generate inconsistent estimates due to the *incidental parameters problem* under a fixed T or N . However, the bias is small when T (or N) is large, as in our case.²³ Nevertheless, we

22. The estimation sample used for this publication is 1990: Q1 to 2016: Q4, and unchanged from the sample chosen for the Working Paper version of this paper. However, even as the sample period is frozen, we use the latest vintage of the iMaPP database, which includes revisions and ensures the best quality of data over the sample period.

23. See, for example, Wooldridge (2010, p. 612), Dickerson et al. (2011).

consider alternative specifications (such as replacing time dummies with the VIX and quarter dummies) and find that the results are similar. The estimated distributions of the propensity score for treated and control units overlap reasonably well (Online Appendix C).

Outcome Model: The predicted changes in the outcome variable ($\hat{m}_{j,it}^h$) are obtained as the fitted values of the following regression model for each group j and horizon h :

$$\Delta^h y_{it} = \sum_{s=1}^L \alpha_s^h \Delta y_{i,t-s} + \mathbf{X}_{it}^O \boldsymbol{\beta}^{O,h} + e_{it}^{O,h}, \quad (3)$$

where $\Delta^h y_{it} = y_{i,t+h} - y_{it}$, and $\Delta y_{it} = y_{i,t+1} - y_{it}$. A vector \mathbf{X}_{it}^O includes real GDP growth, real short-term interest rate, real house price growth, the action indicator of other macroprudential policies, and VIX, as well as the dummies for AE, EMDE, and quarters.²⁴ The lag length (L) is set at one and $e_{it}^{O,h}$ is the error term. We also consider different specifications and find that household credit results are broadly robust while private consumption results are less robust.

Average Treatment Effect: With the predicted propensity scores ($\hat{p}_{j,it}$) and changes in the outcome variable ($\hat{m}_{j,it}^h$), the AIPW estimate of the average treatment effect is obtained using equation (1) for each treatment group j and for each horizon h . Specifically, we compute $\widehat{TE}_{j,it}^h$ and regress it on a constant term to get \widehat{ATE}_j^h and its standard error clustered by country, as in Jordà and Taylor (2016).²⁵

2.2 Panel Regression Analysis Based on the Timing Assumption

For the purpose of comparison, we also estimate the effect of a 1 ppt LTV tightening using standard panel regression methods with the timing identification assumption of no contemporaneous policy effects, as in previous studies. Specifically, we estimate the following equation:

$$\Delta_4 C_{it} = \rho \Delta_4 C_{i,t-1} + \sum_{s=1}^4 \beta_s \Delta LTV_{i,t-s} + \sum_{l=1}^L \gamma_l \mathbf{X}_{i,t-l} + \alpha_i + \mu_t + \varepsilon_{it}, \quad (4)$$

where i is country and t is the time (quarter). The dependent variable, $\Delta_4 C_{it}$, refers to year-on-year growth rate of total private credit to the household sector (in real terms), which includes both mortgage and consumer credit from all sources.²⁶ When

24. Country and time fixed effects dummies cannot be used due to limited numbers of observations in some treatment groups. Instead, AE and EMDE dummies are used to control some country fixed effects, and quarter dummies and VIX are used to control some time fixed effects.

25. Please note that, if either the treatment model or the outcome model is not correctly specified, these standard errors would miss the uncertainty in the earlier stages of estimation. In such a case, the method by Murphy and Topel (1985) or a bootstrap method would be needed to obtain standard errors that are robust to misspecifications.

26. This baseline choice is justified given that mortgage data are scarcer across countries and time than total household credit data.

considering the side effects, we replace the dependent variable with real private consumption growth.

The main regressor is $\Delta LTV_{i,t-1}$, which is the ppt change in the average LTV limit in country i and quarter $t-1$. Longer lags of ΔLTV_{it} are included to capture prolonged effects of policy changes on credit growth. When examining nonlinear effects, we replace ΔLTV_{it} with the interaction term $\Delta LTV_{it} \cdot Z_{it}$, where Z_{it} is the dummy variable that takes one if ΔLTV_{it} is in a specified range (e.g., $\Delta LTV_{it} \in (-10, 0)$ in Section 3.2; and if the initial LTV level is high in Section 3.3). Time (μ_t) and country (α_i) fixed effects capture time-varying common factors such as global risk aversion and time-invariant country-specific factors.

The lag of ΔC_{it} is also included as a regressor to capture any autonomous dynamics in real credit growth, and a vector X_{it} includes other control variables, such as real GDP growth, domestic real interest rates, real house price growth, and other macroprudential policies.²⁷

3. QUANTIFYING THE EFFECTS OF TIGHTENING OF LTV LIMITS—RESULTS

3.1 *The Effects of a 1 ppt Tightening of LTV Limits*

Our baseline results from the AIPW estimation indicate strong and nonlinear effects of a 1 ppt LTV tightening on household credit.²⁸ For a tightening of less than 10 ppts—the most common change in our sample—the cumulative decline in real household credit growth after four quarters is estimated at 0.51 ppts per 1 ppt LTV tightening, which is sizable. On the other hand, for a larger tightening in the range between 10 ppts and 25 ppts, the per-unit effect is estimated to be smaller, at 0.21 ppts (Panel 1 of Figure 5).²⁹

27. The inclusion of the lagged dependent variable as a regressor could lead to a bias in the fixed-effect estimator (Nickell 1981). However, in our application, the bias is minimal given the long sample period of our sample. Our panel estimation covers on average 71 periods, ranging from a minimum of 19 to a maximum of 104, leading to the bias to reduce to close to zero. We nevertheless conducted a GMM, yielding very similar results.

28. Please note that we report the average treatment effects only for tightening groups, which have relatively more observations. The effects for easing groups are not precisely estimated, mainly due to the small number of easing episodes in the estimation sample. For example, combined with the availability of other variables, only 14 episodes of easing by less than 10 ppts are available for the AIPW estimation on household credit. For the group of larger easing, the AIPW estimation cannot be conducted because the number of episodes is 8, which is less than the number of parameters to be estimated in the outcome model (which is 11).

29. The average effect on household credit growth of tightening LTV measures is estimated at -0.26 ppts (compared to -0.11 ppts in the fixed effects regression), based on the AIPW estimation with three buckets (i.e., the tightening, loosening, and control groups). However, considering the observed nonlinearity, these “linear” models would likely be misspecified. The linear model estimates are broadly comparable with the estimates by Richter, Schularick, and Shim (2019), although caution is needed when comparing the results because the definition of LTV limits differs—their LTV indicator includes loan prohibitions while ours do not. Their estimated effect on household credit growth ranges from -0.58 to

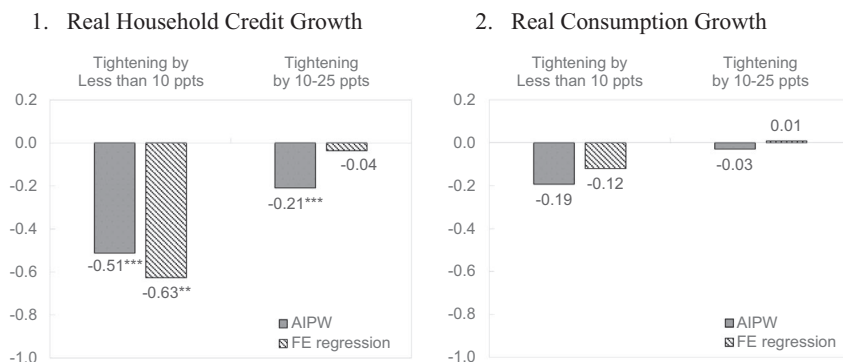


Fig 5. Causal Effects of 1 Percentage Point Tightening in LTV limits.

SOURCES: The iMaPP database, Bloomberg, BIS, OECD, others (see Online Appendix B), and the authors' estimation. NOTES: The figure reports the cumulative effects of a 1 ppt LTV tightening after four quarters, obtained by the augmented inverse propensity-score weighting ("AIPW") estimation and the fixed effects estimation with the timing assumption ("FE regression"). The FE regression uses the interaction terms of ΔLTV with the dummy variables for each bucket (e.g., tightening by less than 10 ppts). Sample period: 1990Q1–2016Q4. To mitigate the influence of outliers, observations with ΔLTV less than -25 ppts or greater than 25 ppts are excluded for the estimation. Also, the sample excludes four observations where LTV limits were changed only for commercial real estate loans (Hong Kong 1994Q1 and 2013Q1, Malaysia 2011Q4, Poland 2014Q2), because the analysis in this paper primarily examines the household sector. Confidence levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors are clustered by country. Baseline results of the fixed effects regressions are presented in Table 2.

A comparison with an estimation using the standard method used in the literature is also instructive. In particular, the fixed effects estimation based on the timing assumption also finds nonlinear effects on household credit, broadly similar to the AIPW estimates in size and statistical significance (Panel 1 of Figure 5, and Table 2).³⁰ The results suggest that, although the typical regression estimates based on the timing assumption may suffer from an attenuation bias, the bias is not empirically severe in this application.

In these exercises, we find that a number of control variables also have strong effects on credit. Credit growth slows in response to increases in real policy rates. Increases in house prices, on the other hand, lead to an increase in credit, as might be expected. We also consistently find that a tightening of other macroprudential tools (i.e., tools other than LTV) puts a break on credit growth.

Importantly, overall, both the AIPW and fixed effect estimates suggest that effects on credit per 1 ppt tightening diminish with the size of the LTV adjustment. This finding of "diminishing returns" from a tightening of LTV ratio caps is novel, even as

—0.18 ppts—the per-action estimate of -4.1 ppts divided by the average LTV change per tightening of 7.1 ppts (with scope adjustments) or 22.5 ppts (without scope adjustments).

30. Jácome and Mitra (2015) also use the timing assumption and report an effect on mortgage credit of 0.07 ppts in four quarters, based on data of five economies.

TABLE 2
THE EFFECTS AND SIDE EFFECTS OF LTV TIGHTENING ON HOUSEHOLD CREDIT AND PRIVATE CONSUMPTION GROWTH

Dependent variable:	(1)		(2)		(3)		(4)		(5)		(6)	
	Household credit (% change, yoy, real)		Household credit (% change, yoy, real)		Household credit (% change, yoy, real)		Private consumption (% change, yoy, real)		Private consumption (% change, yoy, real)		Private consumption (% change, yoy, real)	
Group:	ALL		AE		EMDE		ALL		AE		EMDE	
Household credit (% change, yoy, real, lag = 1q)	0.893*** (0.0191)		0.871*** (0.0459)		0.889*** (0.0138)							
Private consumption (% change, yoy, real, lag = 1q)	-0.0781*** (0.0254)		-0.0795** (0.0348)		-0.0549** (0.0256)		0.825*** (0.0242)		0.824*** (0.0210)		0.772*** (0.0479)	
Short-term interest rate (lag = 1q)	0.127* (0.0671)		-0.0324 (0.0459)		0.316** (0.111)		-0.118*** (0.0330)		-0.0596** (0.0263)		-0.207** (0.0717)	
GDP growth (real, lag = 1q)	0.0438*** (0.0183)		0.0750*** (0.0129)		0.0296 (0.0373)		-0.0889*** (0.0329)		-0.136*** (0.0429)		-0.0464 (0.0588)	
House price growth (real, lag = 1q)	-1.114*** (0.290)		-0.616* (0.314)		-1.611*** (0.478)		0.0215** (0.00894)		0.0295** (0.0108)		0.0326* (0.0173)	
Other macroprudential policies (lag = 1q)	0.0540 (0.133)		-0.00361 (0.133)		0.0897 (0.284)		-0.100 (0.148)		-0.0634 (0.148)		-0.143 (0.233)	
Δ LTV level (lag = 1q, 0 > Δ LTV > -10)	0.355* (0.197)		0.0442 (0.122)		0.868* (0.427)		0.0254 (0.0888)		-0.00314 (0.129)		-0.0406 (0.0728)	
Δ LTV level (lag = 2q, 0 > Δ LTV > -10)	0.219** (0.0852)		0.159 (0.125)		0.0968 (0.161)		0.199 (0.178)		0.276 (0.228)		-0.209* (0.105)	
Δ LTV level (lag = 3q, 0 > Δ LTV > -10)	-0.00221 (0.201)		0.219* (0.120)		-0.547 (0.346)		-0.0873 (0.139)		-0.132 (0.196)		-0.386 (0.267)	
Δ LTV level (lag = 4q, 0 > Δ LTV > -10)	0.0282 (0.0369)		0.00506 (0.0174)		0.110 (0.125)		-0.0176 (0.0235)		-0.0309 (0.0236)		-0.347 (0.0708)	
Δ LTV level (lag = 1q, -10 $\geq \Delta$ LTV \geq -25)												

(Continued)

TABLE 2 (CONTINUED)							
Dependent variable:							
Group:	(1)	(2)		(3)	(5)		(6)
	ALL	Household credit (% change, yoy, real)		EMDE	ALL	AE	EMDE
Δ LTV level (lag = 2q, $-10 \geq \Delta$ LTV ≥ -25)	0.0262 (0.0364)	0.00677 (0.0185)	0.00851 (0.0840)	-0.00818 (0.0241)	0.00619 (0.0273)	-0.0206 (0.0654)	
Δ LTV level (lag = 3q, $-10 \geq \Delta$ LTV ≥ -25)	0.00684 (0.0506)	0.0138 (0.0326)	0.0114 (0.0977)	-0.0142 (0.0305)	-0.0176 (0.0255)	0.0245 (0.0745)	
Δ LTV level (lag = 4q, $-10 \geq \Delta$ LTV ≥ -25)	-0.0256 (0.0333)	-0.0395 (0.0382)	0.0341 (0.0663)	0.00770 (0.0165)	-0.0103 (0.0211)	0.0297 (0.0376)	
Constant	1.426** (0.725)	1.783** (1.021)	0.800 (0.841)	0.545 (0.619)	0.0649 (0.587)	-1.310 (1.158)	
Sum coeff Δ LTV ($0 > \Delta$ LTV > -10)	0.63** (0.05)	0.42 (0.34)	0.51 (0.13)	0.12 (0.71)	0.11 (0.80)	-0.98 (0.21)	
Sum coeff Δ LTV ($-10 \geq \Delta$ LTV ≥ -25)	0.04 (0.78)	-0.01 (0.82)	0.16 (0.57)	-0.01 (0.89)	-0.01 (0.90)	0.05 (0.82)	
Observations	3,518	2,524	994	3,583	2,532	1,051	
R-squared	0.928	0.922	0.936	0.800	0.809	0.818	
TIME FE	Yes	Yes	Yes	Yes	Yes	Yes	
COU FE	Yes	Yes	Yes	Yes	Yes	Yes	
Period	>1999	>2003	>2007	>1999	>1999	>1999	
Group	ALL	ALL	ALL	ALL	AE	EM	
Country no	54	34	20	49	31	18	

SOURCES: The iMaPP database, Bloomberg, BIS, OECD, others (see Online Appendix B), and the authors' estimation.

NOTES: The table reports the effects of a 1 ppt LTV tightening with interaction terms with the dummies for the category of the tightening action (e.g., less than 10 ppt tightening), obtained by the fixed effects estimation with the timing assumption. The row labeled as "Sum coeff Δ LTV level" shows the cumulative decline after four quarters for each category of the tightening action. All specifications include country and time fixed effects, as well as the dummies of moderate and large loosening actions. Sample period: 1990Q1–2016Q4. To mitigate the influence of outliers, observations with Δ LTV less than -25 ppts or greater than 25 ppts are excluded for the estimation. Also, the sample excludes four observations where LTV limits were changed only for commercial real estate loans (Hong Kong 1994Q1 and 2013Q1, Malaysia 2011Q4, Poland 2014Q2), because the analysis in this paper primarily examines the household sector. The p -value of the F -test regarding the sum of the LTV level changes for the four lags being zero is reported. AE = advanced economy; EMDE = emerging market and developing economy.

the precise magnitude of differences in the estimated effects depends to some extent on the choices of thresholds for tightening groups.

A very similar nonlinearity emerges when we examine effects on house price growth instead of the impact on credit. While not shown here, both AIPW and FE estimation methods yield statistically significant estimates of declines in real house price growth by 1 ppt per 1 ppt LTV tightening of less than 10 ppts, but almost no impact for larger tightening measures. This is consistent with the notion that the effects of changes in LTV ratio limits on house prices work through their effects on credit. We return to study potential drivers of the nonlinear effects in Section 3.2.

Across methods, the estimated side effects from a tightening of LTV limits are found to be smaller and less robust. The point estimates of the decline in consumption growth also show a nonlinear pattern but they are not statistically significant (Panel 2 of Figure 5).³¹

Taking this together with the nonlinear effects on credit, a tightening by less than 10 ppts appears to be more efficient than a larger tightening in the sense that it reduces household credit growth more while side effects on consumption growth remain statistically insignificant.

3.2 Possible Reasons for Nonlinear Effects of LTV Tightening on Household Credit

Our results suggest that the size of LTV tightening matters. Effects per 1 ppt tightening on household credit growth diminish with the size of the tightening. Although this nonlinearity may be due to the somewhat smaller number of observations of large LTV tightenings, it could also reflect underlying economic factors. We consider three possible reasons.

One possibility is that the peculiarities related to the introduction of new regulatory LTV limits account for the smaller measured effects of large changes. Quite a few of the introductions of new LTV limits are, according to our measurement, associated with a large reduction in the average LTV limit—and 12 out of 19 episodes in the group of larger tightening in the AIPW estimation are introduction cases. If policy-makers introduced new LTV limits by calibrating them rather neutral relative to the prevailing lending standards (e.g., banks' own policies), then our measurement may exaggerate the actual size of the tightening. For example, if the prevailing lending standards corresponded to an average LTV of 90, and the regulator introduced an LTV cap at 85, the tightening relative to prevailing practice would be 5, not 15 ppts, as implied by our way of measuring (given that we assume 100 as the preintroduction

31. While not considering nonlinear effects with respect to the size of a LTV tightening measure, Richter, Schularick, and Shim (2019, fig. A.6) also estimate the decline in real consumption from a 1 ppt tightening in LTV limits at around 0.05% after four quarters, but with no statistical significance at the 5% confidence level. For comparison, we also considered three buckets (i.e., the tightening, loosening, and control groups), and estimated the average decline of real consumption from LTV tightening measures at 0.04% (AIPW estimation) and 0.02 (fixed effect estimation) after four quarters, but the estimates were not statistically significant at the usual levels. As noted in footnote 25, caution is needed when comparing the results with Richter et al. (2019) because the definition of LTV limits differs—their LTV indicator includes loan prohibitions while ours does not.

TABLE 3
ESTIMATED EFFECTS OF LTV TIGHTENING ON HOUSEHOLD CREDIT GROWTH EXCLUDING THE CASES OF INTRODUCING NEW LTV LIMITS

	AIPW		FE regression	
	Household credit, full sample	Household credit, excluding introduction cases	Household credit, full sample	Household credit, excluding introduction cases
<i>p</i> -value	(0.00)	(0.01)	(0.05)	(0.03)
Tightening by 10-25 ppts	−0.21***	−	−0.04	−0.10
<i>p</i> -value	(0.00)	−	(0.78)	(0.31)
Number of observations				
Tightening by less than 10 ppts	44	37	48	41
Tightening by 10-25 ppts	19	7	21	7

SOURCES: The iMaPP database, Bloomberg, BIS, OECD, others (see Online Appendix B), and the authors' estimation.
NOTES: Based on the sample excluding the LTV introduction cases, the table reports the cumulative effect in real household credit of a 1 ppt LTV tightening after four quarters, obtained by the augmented inverse propensity-score weighting ("AIPW") estimation and the fixed effects estimation with the timing assumption ("FE regression"). The FE regression uses the interaction terms of Δ LTV with the dummy variables for each bucket (e.g., tightening by less than 10 ppts). Sample period: 1990Q1–2016Q4. The LTV introduction cases are detected as a tightening from the country's maximum level of the average LTV limit that is at or higher than 100% and are excluded from the estimation sample. The maximum level of the average LTV limit in each country is considered because some countries set regulatory LTV limits at or higher than 100%. To mitigate the influence of outliers, observations with Δ LTV less than −25 ppts or greater than 25 ppts are excluded for the estimation. Also, the sample excludes four observations where LTV limits were changed only for commercial real estate loans (Hong Kong 1994Q1 and 2013Q1, Malaysia 2011Q4, Poland 2014Q2), because the analysis in this paper primarily examines the household sector. Confidence levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Standard errors are clustered by country.

starting point). Accordingly, we would understate the measured per-unit effects on household credit, potentially contributing to lower estimates for large changes.

However, when we exclude the introduction cases, we still find a nonlinear pattern (Table 3).³² In the case of the AIPW estimation, although a reduction in credit is still statistically significant for the tightening of less than 10 ppts, we cannot estimate the effects for the larger tightening group, because of the small number of observations.³³ The fixed effect estimation, on the other hand, suffers less from the small sample because it entails much fewer parameters to be estimated than the AIPW estimation. These fixed effect estimates still show a similar nonlinear pattern as in the full sample estimation. Therefore, the peculiarities of the introduction cases do not seem to be the reason behind the observed nonlinearity.

Another possibility is that the nonlinearities may reflect policy leakage (e.g., regulatory arbitrage) effects. For example, if the LTV limits only cover domestic bank loans, a strong tightening could incentivize arbitrage, increasing other types of credit

32. Richter, Schularick, and Shim (2019) exclude introductions of new LTV limits when constructing intensity-adjusted LTV action variables.

33. After excluding the introduction cases, the AIPW estimate for the larger tightening group becomes unavailable, because the number of observations in the group reduces to only seven episodes, which is less than the number of parameters to be estimated in the outcome model (which is 11).

or provision of credit by nonbanks and from abroad, offsetting the desired policy effects to some extent (see also IMF-FSB-BIS 2016).

We examine the possibility of policy leakage by estimating the impact of LTV tightening on different types of household credit and comparing the effects on bank versus nonbank credit. To avoid the potential issues associated with LTV introductions discussed earlier, we continue to exclude such introductions. Given the smaller numbers of observations of household credit by type, we can only obtain the fixed effect estimates for LTV tightening of less than 10 ppts. We cannot, therefore, directly examine nonlinear patterns by the size of LTV tightening.

However, we find indirect support for potential policy leakage effects. Estimated policy effects are strongest for those types of household credit falling within the regulatory scope of the regulatory LTV limits, namely, household mortgages extended by banks. By contrast, they are weaker for types of credit not covered by LTV limits (Table 4). Specifically, while a tightening of the LTV ratio is associated with a significant reduction in the provision of household loans by banks, there is no significant cut in credit from nonbanks. If anything, the provision of credit from nonbanks appears to increase upon the tightening of LTV limits, in line with a policy leakage hypothesis.

The third possibility is that nonlinearity could be driven by large tightening applied only to a narrow group of mortgages, such as those applied only to very risky loans. If the larger tightening groups happened to include many cases of narrow-scope tightening, then per-unit effects on overall household credit would be smaller. However, out of the 19 larger tightening episodes used in the AIPW estimation (Table 3), we identify only four episodes that seem to be dominated by relatively narrow groups of mortgages, although we cannot fully assess the size of segmented mortgage markets due to the lack of such data. As a robustness check, we re-estimate policy effects using the sample excluding these four episodes, but still find a nonlinear pattern.³⁴ Therefore, narrow-scope tightenings do not seem to drive the nonlinearity.

In summary, these additional analyses suggest that policy leakage effects could be a factor behind the nonlinear effects, although more data and research are needed to obtain more direct evidence.

3.3 Do Initial LTV Levels Matter?

It is conceivable that a given reduction in the LTV limits could have differential impacts on macrofinancial outcomes depending on whether the starting level of the LTV ratio cap is still relatively loose or already tight. An already-tight LTV limit may indicate that prevailing LTV ratios in the mortgage market were already close to the

34. Using the sample excluding the four seemingly narrow-scoped large tightening episodes (Denmark 2003Q2, Malaysia 2010Q4, Singapore 2012Q4 and 2013Q1), the AIPW estimate for the group of larger tightening is -0.31 with the 1% significance, which is still smaller in the absolute value than the baseline estimate for the tightening group of less than 10 ppts (i.e., -0.51). The fixed effect estimation also remains to show a nonlinear pattern (-0.61 with the 10% significance for the tightening group of less than 10 ppts and -0.06 without significance for the tightening group of 10–25 ppts).

TABLE 4
EFFECTS OF LTV TIGHTENING ON HOUSEHOLD CREDIT GROWTH BY TYPE

	(1) Household mortgages by banks	(2) Other household loans	(3) Household loans by banks	(4) Household loans by nonbank financials	(5) Household mortgages	(6) Household loans excl. mortgages
Tightening by less than 10 ppts	-1.02*** (0.00)	0.06 (0.91)	-0.73** (0.01)	2.15 (0.43)	-0.94** (0.02)	0.04 (0.94)
<i>p</i> -value	0.02	-0.13***	-0.10	-0.81*	-0.09	0.16
Tightening by 10–25 ppts	(0.45)	(0.00)	(0.38)	(0.08)	(0.49)	(0.68)
Number of observations	2,236	2,120	1,838	1,301	2,313	2,065
Tightening by less than 10 ppts	25	24	19	11	27	24
Tightening by 10–25 ppts	1	1	1	1	6	4

Sources: The iMaPP database, Bloomberg, BIS, OECD, others (see Online Appendix B), and the authors' estimation.
NOTES: Based on the sample excluding the LTV introduction cases, the table reports the cumulative effects in the specified variables (e.g., "Household Mortgage Loans by Banks") of a 1 ppt LTV tightening after four quarters, obtained by the fixed effects estimation with the limiting assumption. The fixed effect regression uses the interaction terms of ALTV, with the dummy variables for each bucket (e.g., tightening by less than 10 ppts), but the table reports the estimates for LTV tightening by less than 10 ppts only, because those for larger tightening are not stable due to limited observations (e.g., only one episode of large tightening in the regression for "Household Mortgage Loans by Banks"). Sample period: 1990Q1–2016Q4. The LTV introduction cases are detected as a tightening from the country's maximum level of the average LTV limit that is at or higher than 100% and are excluded from the estimation sample. The maximum level of the average LTV limit in each country is considered because some countries set regulatory LTV limits at or higher than 100%. To mitigate the influence of outliers, observations with ALTV less than -25 ppts or greater than 25 ppts are excluded for the estimation. Also, the sample excludes four observations where LTV limits were changed only for commercial real estate loans (Hong Kong 1994Q1 and 2013Q1, Malaysia 2011Q4, Poland 2014Q2), because the analysis in this paper primarily examines the household sector. The *p*-value of the *F*-test regarding the sum of the LTV level changes for the four lags being zero is reported. Confidence levels: ****p* < 0.01, ***p* < 0.05, **p* < 0.1. Standard errors are clustered by country.

regulatory LTV limit, and thus, a further tightening could have strong effects on credit and consumption by making borrowing constraints binding for many households.³⁵

To examine this hypothesis, we estimate the effects conditioning on the initial level of LTV limits using panel regressions.³⁶ Specifically, in equation (4), we use interaction terms with a dummy variable of “tight” initial LTV levels, which takes one if the lag of LTV level is below the threshold level of 75% (corresponding to the sample median of LTV limits).

Overall, we find no clear relation between policy effects and initial levels of regulatory LTV limits. The estimated effects on household credit are not statistically significant in the case of either tight or loose initial LTV levels.³⁷ The signs of the point estimates often change over different specifications (e.g., different thresholds for tight initial levels, subsamples of AEs and EMDEs, with or without introduction cases). The same applies for policy side effects on private consumption. The inconclusive results may reflect that lower levels of regulatory LTV limits do not necessarily imply that more households face binding borrowing constraints, because the effect depends on prevailing LTV ratios in the mortgage market.

3.4 Further Analysis of Changes in the LTV Limits

Next, we examine whether the effects of the per-unit changes in the LTV limits depend on country characteristics. We use the standard panel regression methods, because an AIPW estimation for such subsamples is not feasible due to the limited number of observations in each treatment group.

We estimate equation (4) with country group dummies. Specifically, we consider dummies of EMDEs and AEs; regions such as Asia, Europe, and the Americas; countries with high exchange rate flexibility, high capital openness, and high financial development; and countries with high debt-to-income ratios among low-income borrowers.³⁸ We also estimate the effects conditioning on a positive credit gap.³⁹

35. Many theoretical and empirical studies highlight the importance of liquidity and borrowing constraints in understanding how household credit and consumption respond to shocks including those from macroprudential and monetary policy actions. For example, see Allen et al. (2020), Cloyne et al. (2019), Gelos et al. (2019), Kaplan et al. (2014, 2018), and Narita (2011).

36. To avoid mechanical effects of LTV levels, here we estimate the effects of a 1% change in the LTV limit, instead of those of a 1 ppt change in the LTV limit. The AIPW estimation for sub-samples by initial LTV levels is not feasible due to the limited number of observations in each treatment group.

37. If initially loose levels of LTV limits tended to induce large tightening actions, then policy effects could be smaller in the case of loose initial LTV limits, considering the smaller estimated policy effects of large tightening actions (Section 3.1). However, the correlation coefficient between the initial LTV level and the size of LTV tightening is modest at -0.32 , and it becomes close to zero when excluding the introduction cases.

38. Using microlevel household surveys, countries are split into high/low indebtedness of low-income borrowers if the average debt-to-income ratio of the bottom 40% households by income is above/below the median.

39. In contrast to the earlier analyses, here we do not include dummy variables to distinguish changes in LTV limits by the size or direction (e.g., tightening by less than 10 ppts).

Looking first at the effect on household credit, our focus is the sum of the ΔLTV coefficients, which encompasses the cumulative effects of the previous 4 lags (Table 5). In terms of regional differences, we find a stronger and more significant impact for countries in the Americas. The policy effects are also found to be stronger when the credit gap is positive (i.e., credit booms), suggesting that macroprudential policies have more traction when the economy is highly leveraged (column 11, Table 5).

Although statistically insignificant, we find a relatively smaller effect on household credit in countries where the capital account is more open (column 8, Table 5). This smaller effect may suggest potential policy leakage across borders—that is, tightening LTV limits on domestic bank credit could induce credit inflows from abroad.⁴⁰

We also assess the impact of ΔLTV on real private consumption (Table 6). The effects on private consumption are generally less statistically significant, in line with the findings by Richter, Schularick, and Shim (2019). While the effects are statistically significant for the Western Hemisphere region, the results are insignificant elsewhere. Interestingly, stronger effects on consumption are found in countries where low-income borrowers are more indebted (column 10, Table 6). This result is consistent with the hypothesis that low-income borrowers have a higher marginal propensity to consume and thus cut consumption more notably in the presence of binding borrowing constraints.

4. REVISITING STANDARD REGRESSION ANALYSIS

In this section, we confirm previous findings obtained by the literature on the effects on credit and asset prices of a broader set of macroprudential tools when using the iMaPP database. We also examine more fully the side effects of this broader range of tools on consumption and output growth, which has not yet been done as comprehensively as here.

Using the iMaPP's dummy-type policy action indicators for various instruments, we estimate panel regressions with fixed effects to assess the effects “per policy action,” which has been the standard approach in the literature.⁴¹ In these regressions, identification of the causal effect of macroprudential policy relies on a timing assumption—macroprudential policies do not affect the dependent variable within the same quarter.⁴²

40. Many studies provide evidence for cross-border policy leakage effects, which suggest cross-border lending increases when domestic macroprudential policy is tightened (e.g., Akinci and Olmstead-Rumsey 2018, Cerutti et al. 2017a, Cerutti and Zhou 2018, Choi et al. 2021).

41. Many empirical studies provide estimated effects per policy action, typically on credit growth and house prices (e.g., Igan and Kang 2011, Elliott et al. 2013, Krznar and Morsink 2014, Kuttner and Shim 2016, Akinci and Olmstead-Rumsey 2018, Cerutti et al. 2017a, IMF 2017, and Poghosyan 2020).

42. These estimates cover the 63 countries (34 AEs and 29 EMDEs) listed in Online Appendix C Table C2, where quarterly data are available between 1991: Q1 and 2016: Q4.

TABLE 5
THE EFFECTS OF ΔLTV LIMITS ON REAL HOUSEHOLD CREDIT GROWTH

Group	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Dependent variable: household credit (% change, yoy, real)										
	ALL	AE	EMDE	ASIA	EUROPE	AMERICAS	Hi FX Regime flexibility	Hi Capital openness	Hi Financial development	Hi DTI Low-income borrowers	Positive credit gap
Household credit (% change, yoy, real, lag = 1q)	0.893*** (0.0190)	0.894*** (0.0188)	0.894*** (0.0188)	0.894*** (0.0191)	0.894*** (0.0190)	0.892*** (0.0192)	0.893*** (0.0190)	0.894*** (0.0189)	0.894*** (0.0190)	0.913*** (0.0253)	0.895*** (0.0193)
Short-term interest rate (lag = 1q)	-0.0790***	-0.0797***	-0.0797***	-0.0788***	-0.0795***	-0.0820***	-0.0788***	-0.0793***	-0.0784***	0.00520	-0.0756***
GDP growth (real, lag = 1q)	(0.0255)	(0.0257)	(0.0257)	(0.0254)	(0.0255)	(0.0260)	(0.0254)	(0.0255)	(0.0251)	(0.0297)	(0.0257)
	0.118*	0.116*	0.116*	0.117*	0.114*	0.114*	0.121*	0.117*	0.119*	-0.0302	0.0979
House price growth (real, lag = 1q)	(0.0673)	(0.0672)	(0.0672)	(0.0675)	(0.0675)	(0.0675)	(0.0673)	(0.0671)	(0.0674)	(0.0551)	(0.0673)
	0.0437**	0.0430**	0.0430**	0.0438**	0.0441**	0.0433**	0.0438**	0.0434**	0.0437**	0.0741***	0.0416**
Other macroprudential policies (lag = 1q)	(0.0185)	(0.0184)	(0.0184)	(0.0185)	(0.0184)	(0.0186)	(0.0184)	(0.0184)	(0.0184)	(0.0249)	(0.0183)
	-1.171***	-1.184***	-1.184***	-1.163***	-1.152***	-1.207***	-1.157***	-1.176***	-1.158***	-0.655*	-1.208***
	(0.333)	(0.333)	(0.333)	(0.328)	(0.329)	(0.330)	(0.321)	(0.334)	(0.326)	(0.311)	(0.343)

(Continued)

Dependent variable: household credit (% change, yoy, real)											
Group	ALL	AE	EMDE	ASIA	EUROPE	AMERICAS	Hi FX Regime flexibility	Hi Capital openness	Hi Financial development	Hi DTI Low-income borrowers	Positive credit gap
Constant	0.632* (0.362)	0.618* (0.341)	0.618* (0.341)	0.603* (0.336)	0.585 (0.354)	0.665* (0.372)	0.473 (0.326)	0.629* (0.355)	0.595* (0.333)	0.450 (0.351)	0.943** (0.373)
Sum coeff ΔLTV (dummy = 0)	0.06	0.14	-0.01	0.04	0.17	-0.04	0.21	0.08	0.00	0.20	-0.12
<i>p</i> -value	(0.62)	(0.52)	(0.92)	(0.82)	(0.20)	(0.68)	(0.12)	(0.71)	(0.98)	(0.47)	(0.46)
Sum coeff ΔLTV (dummy = 1)	-	-0.01	0.14	0.08	-0.09	0.49***	-0.02	0.04	0.12	-0.09	0.20
<i>p</i> -value	-	(0.92)	(0.52)	(0.51)	(0.59)	(0.00)	(0.89)	(0.69)	(0.21)	(0.79)	(0.14)
Difference	-	-0.15	0.15	0.04	-0.27	0.54***	-0.24	-0.04	0.13	-0.29	0.32*
<i>p</i> -value	-	(0.54)	(0.54)	(0.85)	(0.23)	(0.00)	(0.27)	(0.86)	(0.58)	(0.59)	(0.09)
Observations	3,518	3,518	3,518	3,518	3,518	3,518	3,518	3,518	3,518	1,342	3,518
<i>R</i> -squared	0.927	0.927	0.927	0.927	0.927	0.928	0.927	0.927	0.927	0.949	0.928

Sources: The iMaPP database, Bloomberg, BIS, OECD, others (see Online Appendix B), and the authors' estimation. Notes: The table reports the effects of a 1 ppt LTV tightening, obtained by the fixed effects estimation with the timing assumption. The row labeled as "Sum coeff LTV level" shows the cumulative effects after four quarters. All specifications include country and time fixed effects. Period sample: 1990Q1–2016Q4. To mitigate the influence of outliers, observations with ΔLTV less than -25 ppts or greater than 25 ppts are excluded for the estimation. Also, the sample excludes four observations where LTV limits were changed only for commercial real estate loans (Hong Kong 1994Q1 and 2013Q1, Malaysia 2011Q4, Poland 2014Q2), because the analysis in this paper primarily examines the household sector. The *p*-value of the *F*-test regarding the sum of the LTV level changes for the four lags being zero is reported. AE = advanced economy; EMDE = emerging market and developing economy; Hi FX regime flexibility = countries with (de facto) exchange rate regime flexibility, on average, above the median of our sample; Hi capital openness = countries with capital account openness (proxied by Chin-Ito index) above the median of our sample; Hi DTI low-income borrowers = countries with debt-to-income ratio of low-income borrowers (bottom two quintiles) above the median of our sample. Standard errors clustered at the country level are reported in parentheses. Confidence levels: ****p* < 0.01, ***p* < 0.05, **p* < 0.1.

TABLE 6
THE EFFECTS OF ΔLTV LIMITS ON REAL PRIVATE CONSUMPTION GROWTH

Group	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Dependent variable: household credit (% change, yoy, real)										
	ALL	AE	EMDE	ASIA	EUROPE	AMERICAS	Hi FX regime flexibility	Hi capital openness	Hi financial development	Hi DTI low-income borrowers	Positive credit gap
Private consumption (% change, yoy, real, lag = 1q)	0.824*** (0.0243)	0.824*** (0.0243)	0.824*** (0.0243)	0.825*** (0.0243)	0.824*** (0.0245)	0.824*** (0.0244)	0.824*** (0.0245)	0.824*** (0.0243)	0.825*** (0.0243)	0.795*** (0.0246)	0.824*** (0.0244)
Short-term interest rate (lag = 1q)	-0.119***	-0.119***	-0.119***	-0.119***	-0.120***	-0.121***	-0.120***	-0.120***	-0.120***	-0.0735**	-0.115***
GDP growth (real, lag = 1q)	(0.0316)	(0.0317)	(0.0317)	(0.0316)	(0.0316)	(0.0318)	(0.0316)	(0.0318)	(0.0315)	(0.0250)	(0.0302)
	-0.0886***	-0.0886***	-0.0886***	-0.0888***	-0.0889***	-0.0886***	-0.0889***	-0.0886***	-0.0897***	-0.0193	-0.102***
House price growth (real, lag = 1q)	(0.0326)	(0.0327)	(0.0327)	(0.0328)	(0.0326)	(0.0327)	(0.0326)	(0.0325)	(0.0329)	(0.0427)	(0.0322)
	0.0214**	0.0213**	0.0213**	0.0214**	0.0216**	0.0212**	0.0213**	0.0212**	0.0213**	0.0121	0.0189**
Other macroprudential policies (lag = 1q)	(0.00885)	(0.00886)	(0.00886)	(0.00887)	(0.00881)	(0.00892)	(0.00893)	(0.00890)	(0.00886)	(0.0128)	(0.00857)
	-0.109	-0.109	-0.109	-0.108	-0.101	-0.119	-0.112	-0.109	-0.110	0.0910	-0.0940
Constant	(0.151)	(0.149)	(0.149)	(0.152)	(0.151)	(0.148)	(0.149)	(0.148)	(0.148)	(0.201)	(0.150)
	0.840	0.837	0.837	0.842	0.832	0.851	0.795	0.841	0.851	0.833***	0.810
	(0.924)	(0.924)	(0.924)	(0.924)	(0.927)	(0.925)	(0.959)	(0.924)	(0.921)	(0.266)	(0.938)

(Continued)

TABLE 6
(CONTINUED)

Group	Dependent variable: household credit (% change, yoy, real)										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	ALL	AE	EMDE	ASIA	EUROPE	AMERICAS	Hi FX regime flexibility	Hi capital openness	Hi financial development	Hi DTI low-income borrowers	Positive credit gap
Sum coeff Δ LTV (dummy = 0)	0.05	0.05	0.06	0.05	0.11	0.04	0.01	0.07	0.10	-0.09	0.07
p-value	(0.34)	(0.69)	(0.38)	(0.55)	(0.10)	(0.53)	(0.95)	(0.47)	(0.32)	(0.16)	(0.32)
Sum coeff Δ LTV (dummy = 1)	-	0.06	0.05	0.06	-0.02	0.20***	0.08	0.05	0.03	0.20	0.07
p-value	-	(0.38)	(0.69)	(0.40)	(0.82)	(0.01)	(0.25)	(0.51)	(0.67)	(0.16)	(0.37)
Difference	-	0.01	-0.01	0.01	-0.13	0.17	0.07	-0.03	-0.07	0.29	0.01
p-value	-	(0.94)	(0.94)	(0.91)	(0.30)	(0.10)	(0.51)	(0.83)	(0.58)	(0.12)	(0.94)
Observations	3,583	3,583	3,583	3,583	3,583	3,583	3,583	3,583	3,583	1,316	3,583
R-squared	0.799	0.799	0.799	0.799	0.799	0.799	0.799	0.799	0.799	0.813	0.801

Sources: The iMapp database, Bloomberg, BIS, OECD, others (see Online Appendix B), and the authors' estimation. Notes: The table reports the effects of a 1 ppt LTV tightening, obtained by the fixed effects estimation with the timing assumption. The row labeled as "Sum coeff LTV level" shows the cumulative effects after four quarters. All specifications include country and time fixed effects. Period sample: 1990 Q1–2016Q4. To mitigate the influence of outliers, observations with Δ LTV less than -25 ppis or greater than 25 ppis are excluded for the estimation. Also, the sample excludes four observations where LTV limits were changed only for commercial real estate loans (Hong Kong 1994Q1 and 2013Q1, Malaysia 2011Q4, Poland 2014Q2), because the analysis in this paper primarily examines the household sector. The p -value of the F -test regarding the sum of the LTV level changes for the four lags being zero is reported. AE = advanced economy; EMDE = emerging market and developing economy; Hi FX regime flexibility = countries with (de facto) exchange rate regime flexibility, on average, above the median of our sample; Hi capital openness = countries with capital account openness (proxied by Chin-Ito index) above the median of our sample; Hi DTI low-income borrowers = countries with debt-to-income ratio of low-income borrowers (bottom two quintiles) above the median of our sample. Standard errors clustered at the country level are reported in parentheses. Confidence levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE 7

SUMMARY: THE EFFECTS OF MACROPRUDENTIAL POLICIES ON HOUSEHOLD CREDIT AND HOUSE PRICE GROWTH

Dependent variable:	(1)	(2)	(3)	(4)	(5)	(6)
	Real household credit (yoy growth)			Real house prices (yoy growth)		
Group:	ALL	AE	EMDE	ALL	AE	EMED
MaPP index (all tools)	−0.913***	−0.422	−1.385**	−0.108	−0.809**	0.0105
Loan-targeted	−1.424***	−0.969**	−1.936*	−1.035**	−1.622**	−0.659
Demand	−1.348**	−0.701	−2.446	−1.548**	−2.159**	−0.612
Supply—all	−1.095**	−0.0245	−1.801***	0.284	−0.0527	0.0565
Supply—loans	−2.610**	−2.538***	−2.748*	−1.106*	−1.943*	−1.039
Supply—general	−1.102*	0.568	−1.828**	0.472	−0.185	0.202
Supply—capital	−0.782	−0.111	−1.582	0.678	0.458	0.858
dsti	−1.428	−1.237*	−1.789	−1.229	−2.841	1.434
ltv	−1.916**	−0.877	−3.158*	−2.347***	−3.103***	−1.441
tax	−1.477	−1.449	−1.982**	−1.026	−4.188*	1.091
lcg	−1.751	−1.908	−3.227	−1.819	0.158	−2.436
llp	−6.032***	−4.602*	−6.524**	−2.729**	−2.711	−3.654*
loanr	−3.032***	−2.832**	−3.365*	−1.278	−3.253**	−0.357
capital	−1.627*	−0.398	−2.695*	0.462	0.267	0.522
Observations	3,663	2,613	1,050	4,111	2,780	1,331
N (countries)	55	34	21	55	34	21
R-squared (avg)	0.903	0.896	0.919	0.812	0.846	0.786

SOURCES: The iMaPP database, Bloomberg, BIS, OECD, others (see Online Appendix B), and authors' estimation.

Notes: The table reports the cumulative effects of the specified macroprudential tightening after four quarters, obtained by the fixed effects (FE) estimation. Each MaPP index/policy is considered individually. "dsti" = limits to the debt-service-to-income ratio, "ltv" = limits to the loan-to-value ratio, "tax" = tax measures, "lcg" = limits to credit growth, "llp" = loan loss provisions, "loanr" = loan restrictions, and "capital" = capital requirements. "MaPP index" is the sum of dummies for all of 17 categories. "Loan-targeted" group consists of the "Demand" and the "Supply-loans" instruments. "Demand": LTV and DSTI. "Supply-loans": lcg, llp, loanr, limits to the loan to deposit ratio, and limits to foreign currency loans. "Supply-general": reserve requirements, liquidity requirements, and limits to FX positions. "Supply-capital": leverage, countercyclical buffers, conservation buffers, and capital requirements. See Online Appendix A Table A3 for the definitions of instruments. For each country group, *R*-squared is computed as the average of the individual regressions. AE = advanced economy; EMDE = emerging market and developing economy.

Building on previous literature, we estimate the following panel regressions:

$$\Delta_4 C_{i,t} = \rho \Delta_4 C_{i,t-1} + \beta \text{MaPP}_{i,t-1} + \gamma X_{i,t-1} + \alpha_i + \mu_t + \varepsilon_{i,t}, \quad (\text{OA.1})$$

where *i* is country and *t* is time (quarter). The dependent variable, $\Delta_4 C_{i,t}$, refers to the year-on-year growth rate of real household credit, real house prices, private consumption, and real GDP, which are winsorized at 1% to address outliers. The lagged dependent variable ($\Delta_4 C_{i,t-1}$) is included as a regressor to account for persistence. A vector $X_{i,t-1}$ includes lagged macro control variables—that is, real GDP growth, real house price growth, and domestic real short-term interest rates. Time fixed effects (μ_t) capture time-varying common factors such as global risk aversion, while country fixed effects (α_i) capture time-invariant country-specific factors such as institutional characteristics. The main independent variable, $\text{MaPP}_{i,t-1}$, is the policy change indicator for the instrument or the instrument group. This indicator records tightening actions (+), loosening actions (−), and no changes (0), and it is cumulated over the past four quarters to account for potential lagged effects. We consider indices for

TABLE 8

SUMMARY: THE SIDE EFFECTS OF MACROPRUDENTIAL POLICIES ON PRIVATE CONSUMPTION AND REAL GDP GROWTH

Dependent variable:	(1)	(2)	(3)	(4)	(5)	(6)
	Real consumption (yoy growth)			Real GDP (yoy growth)		
Group:	ALL	AE	EMDE	ALL	AE	EMDE
MaPP index (all tools)	-0.326**	-0.290*	-0.306	0.0403	-0.0825	0.0897
Loan-targeted	-1.052***	-0.939**	-0.793*	-0.0421	-0.145	0.0915
Demand	-0.877**	-0.783**	-0.656	-0.129	-0.162	-0.127
Supply—all	-0.0107	0.356	-0.432	0.0955	-0.0997	0.135
Supply—loans	-1.989***	-2.304***	-1.146	0.0365	-0.247	0.287
Supply—general	0.233	0.901*	-0.212	0.134	-0.113	0.162
Supply—capital	-0.159	-0.0196	-0.774	-0.00358	0.0810	-0.193
dsti	-1.522	-1.319	-3.461	-0.0874	-0.179	-0.0417
ltv	-1.034*	-1.001*	0.471	-0.201	-0.249	-0.183
tax	-1.019	-2.147	0.481	-0.0731	-0.513*	0.296
lcg	0.203	-0.255	-0.592	0.648	-1.070***	1.169
llp	-2.673*	-2.807*	-3.067	-0.333	-0.868*	-0.107
loanr	-2.949***	-2.902**	-1.601	0.00853	0.0575	0.165
capital	-0.440	-0.172	-1.027	0.0258	0.0500	-0.107
Observations	3,738	2,631	1,107	4,135	2,790	1,345
N (countries)	50	31	19	55	34	21
R-squared (avg)	0.737	0.788	0.736	0.931	0.943	0.923

SOURCES: The iMaPP database, Bloomberg, BIS, OECD, others (see Online Appendix B), and authors' estimation.

NOTES: The table reports the cumulative effects of the specified macroprudential tightening after four quarters, obtained by the fixed effects (FE) estimation. Each MaPP index/policy is considered individually. "dsti" = limits to the debt-service-to-income ratio, "ltv" = limits to the loan-to-value ratio, "tax" = tax measures, "lcg" = limits to credit growth, "llp" = loan loss provisions, "loanr" = loan restrictions, and "capital" = capital requirements. "MaPP index" is the sum of dummies for all of 17 categories. "Loan-targeted" group consists of the "Demand" and the "Supply-loans" instruments. "Demand": LTV and DSTI. "Supply-loans": lcg, llp, loanr, limits to the loan to deposit ratio, and limits to foreign currency loans. "Supply-general": reserve requirements, liquidity requirements, and limits to FX positions. "Supply-capital": leverage, countercyclical buffers, conservation buffers, and capital requirements. See Online Appendix A Table A3 for the definitions of instruments. For each country group, R-squared is computed as the average of the individual regressions. AE = advanced economy; EMDE = emerging market and developing economy.

instrument groups such as *all*, *loan-targeted*, *demand*, and *supply* measures, which are further subdivided into three categories, including *general*-, *capital*-, and *loan-supply* tools.

Using the comprehensive iMaPP data, we confirm the previous finding that loan-targeted policy actions have significant effects on *household credit growth* (Table 7, columns 1–3). A tightening of any macroprudential measure (captured by the overall MaPP index) is, on average, associated with a decline in household credit growth of 0.9 ppts.⁴³ Looking at subindices, loan-targeted tools (including both demand-side tools, such as LTV and DSTI and supply-side tools, such as limits on certain types of credit) are found to robustly affect household credit growth across all country groups and their effect is larger than that of the average macroprudential tool. Policy effects on *house price growth* are broadly similar to those on *household credit growth*, while they tend to be less statistically significant (Table 7, columns 4–6). These results

43. The unconditional household credit growth averages about 8 ppts per year when all countries are considered. In EMDEs, it averages 11.6 ppts, while in AEs household credit increases yearly by 5.6%. See Online Appendix C Table C3.

corroborate the findings of Cerutti, Claessens, and Laeven (2017a) and Kuttner and Shim (2016), who also find significant effects for demand-side tools.⁴⁴

By contrast, side effects on *private consumption growth* and *real GDP growth* are modest (Table 8). On exception is targeted loan restrictions (“loanr”) that do appear to be negatively associated with both credit and private consumption growth, especially in AEs where the share of consumption financed by credit may be larger than in EMDEs (Table 8, columns 1–3). However, side effects on *real GDP growth* are mostly insignificant across all tools and for both groups of countries (Table 8, columns 4–10). These results are broadly in line with and extend the findings of Richter, Schularick, and Shim (2019), who find small effects on output of changes in LTV caps.⁴⁵

5. CONCLUSIONS

In this paper, we presented a new comprehensive database of macroprudential policies (iMaPP) that combines information from various sources. Exploiting the unique features of this database and using a method that aims to better address endogeneity problems, we found strong and nonlinear effects of reductions in LTV limits.

Advancing on much of the existing literature, we estimate “per-unit” effects of policy action on household credit. We find that the effects per 1 ppt LTV tightening are diminishing with the size of the LTV adjustment, likely due to policy leakage effects. The largest per-unit impact is found for the most popular action in our sample—a tightening of less than 10 ppts—and it indicates that a 1 ppt LTV tightening cumulatively reduces household credit growth by up to 0.5 ppts after 1 year. These results highlight the importance of considering nonlinearity in estimating policy effects in addition to potential reverse causality.

We also made progress toward assessing the side effects of macroprudential policies by investigating their impact on private consumption. We find the effects of a tightening of LTV caps on consumption to be small with point estimates that are not statistically significant.

The new iMaPP database will be updated annually, offering many opportunities for further research. Among the issues that deserve further exploration are the quantification of the per-unit effects of other macroprudential policies and a more comprehensive cost–benefit analysis of macroprudential policies.⁴⁶ Future research should also examine how policy choices depend on the prevailing policy framework, an area that, despite some recent progress, remains underresearched.

44. Other studies, using different data and methods, also show that tighter LTV and DSTI limits reduce household credit growth (e.g., Lim et al. 2011, Arregui et al. 2013, Crowe et al. 2013, Krznar and Morsink 2014, and Jácome and Mitra 2015). The effects of macroprudential policies on house prices are mostly weaker (Table 7, columns 4–6), in line with Kuttner and Shim (2016) and Cerutti et al. (2017a).

45. Further robustness checks, including those addressing a potential Nickell bias (Nickell 1981) using system GMM panel estimates (Arellano and Bover 1995, Blundell and Bond 1998, Roodman 2009), were conducted, but yielded similar results (not shown), in line with the theoretical prediction that such biases are small if T is large. See also footnote 28, above, for the estimation of per-unit effects.

46. See Brandao-Marques et al. (2020) for further progress in this regard.

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Additional supporting information may be found online in the Supporting Information section at the end of the article.

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