Exchange Rates and Domestic Credit—Can Macroprudential Policy Reduce the Link?[1]

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Abstract

This paper examines empirically the role of macroprudential policy in addressing the effects of external shocks on financial stability. A new finding from the baseline results is that an appreciation of the real exchange rate is associated with a subsequent increase in the domestic credit gap, whereas a prior macroprudential policy tightening mitigates this effect. This result is robust to accounting for endogeneity of macroprudential policy. We also examine a feedback effect in which strong domestic credit pulls in additional cross-border funding. We find that tighter macroprudential and monetary policies would further exacerbate cross-border flows, elevating systemic risk, while targeted capital controls can alleviate this policy leakage.

JEL Classification Numbers: E58, F32, F34, G28

Keywords: Macroprudential Policies, Capital Flows, Systemic Risk

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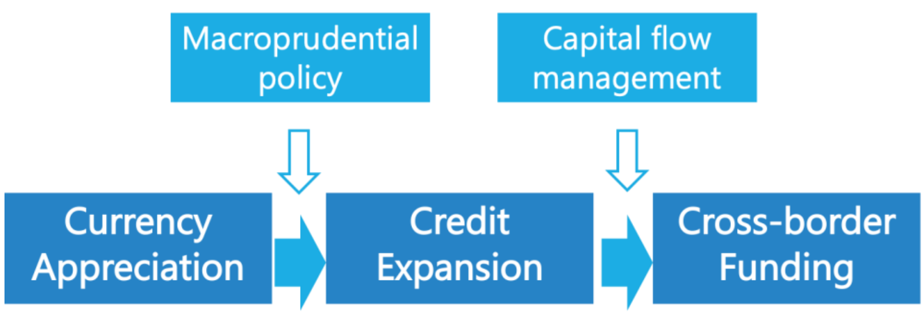
The online appendix is saved at [this location].

I. INTRODUCTION

A recently growing literature explores how movements in exchange rates not only affect macroeconomic outcomes, but can also affect financial conditions and credit developments, which may in turn feed back into the macroeconomic outlook (Blanchard et al., 2015; Shin, 2018; Ghosh et al., 2018; BIS, 2019; Hofmann et al. 2020). The key idea is that a currency appreciation may ease domestic financial conditions, and drive up domestic credit through a number of mutually reinforcing channels. Specifically, an exchange rate appreciation can drive up domestic credit through raising the value of collateral and the net worth of domestic market participants (Krugman, 1999; Céspedes et al., 2004; Bruno and Shin, 2015b). It can also be associated with a reduction in credit spreads and thereby encourage market participants to take greater risks (Hofmann et al., 2020).[2] Through these mechanisms, an appreciation may become expansionary, in contrast to the standard notion that an appreciation should be contractionary, by reducing net exports. Moreover, when an appreciation leads the provision of domestic credit to expand, this can contribute to the build-up of systemic vulnerabilities[3], and potentially require a policy response on the part of macroprudential policymakers.

To help guide the use of macroprudential policy in this context, we study the link between exchange rate movements and domestic credit in a panel of 62 countries over the period of 2000:Q1 to 2016:Q4, and ask to what extent macroprudential policy can attenuate the association between currency movements and domestic credit cycles (left-hand side of Figure 1). We also evaluate a complementary role of targeted controls on inflows, when strong developments in credit in turn lead to increases in cross-border borrowing by banks and corporate firms (right-hand side of Figure 1).

Figure 1. Exchange Rates, Credit, and Capital Flows



Source: Author's descriptions.

[2] Parts of the literature have referred to these mechanisms as the "risk-taking channel" of currency appreciation in the context of cross-border spillovers of monetary policy (Bruno and Shin 2015a; Borio and Zhu 2012; and Hofmann et al., 2020).

[3] Increased credit can also in turn affects local asset prices, or is funded through cross-border borrowing. This can magnify the ultimate effects of exchange rates appreciation, potentially giving rise to a build-up of systemic risk (Gertler et al. 2007; Borio, 2014; Bruno and Shin 2015a,b; IMF 2017; and Baskaya et al. 2017).

Our empirical focus is on macroprudential policy that addresses the time dimension of systemic risk, that is, procyclical risks from interactions between credit developments and the real economy.[4] This builds on existing evidence that supports the notion that macroprudential policies can contain systemic risks, by mitigating potentially excessive increases in credit (Vandenbussche et al., 2015; IMF-FSB-BIS, 2016; Cerutti et al., 2017a; Galati and Moessner, 2018; Alam et al., 2019).

Our analysis deploys a novel database of macroprudential policy actions, the iMaPP database compiled by the IMF (Alam et al., 2019). This database integrates five major existing databases and is the most comprehensive of such databases to date.[5] Another feature of the iMaPP database we exploit is that it distinguishes policy tightening and loosening actions for each of the categories of tools included in the database. Our data sample consists of a large panel of 62 countries[6] over the period 2000:Q1 to 2016:Q4. It covers economies that have taken frequent macroprudential policy actions (e.g., India, Korea, and Russia) and those that have rarely done so (e.g., Chile and Germany), thereby avoiding sample selection biases that may be present in other studies. We thereby improve on some of the existing literature, which often covers relatively small sets of countries, or examines only a subset of macroprudential instruments (e.g., those related to housing markets).

More importantly, our paper differs from the extant literature in that we focus not on the direct effect of macroprudential policy on credit, but on an interaction effect that measures whether macroprudential policy can mitigate the effect of changes in real exchange rates on domestic credit developments. We thereby more squarely address an important issue for small and financially open economies: to what extent is macroprudential policy able to insulate the financial system from the effects of external shocks? In addressing these issues empirically, our paper complements Fendoğlu (2017), who examines the effectiveness of macroprudential policy (including capital flow management-related macroprudential measures) in mitigating the impact of capital inflows on the credit-to-GDP gap, for a small sample of 18 emerging market economies. We focus, in a larger sample, on the effect of changes in exchange rates on credit developments, thereby capturing an important transmission channel of external shocks more broadly (see also IMF, 2017). This has important implications for policymakers in emerging market economies in particular: if macroprudential policy tightening can attenuate the procyclical effects of external shocks, then this would free the hand of monetary policy makers to focus instead on managing the effects of domestic shocks on the economy.

[4] See Galati and Moessner (2013) for the discussion of cross-sectional dimension of systemic risk, which arises through interlinkages of financial intermediaries.
[5] The original iMaPP database covers 17 instruments for a total of 138 countries since 1999 at a monthly frequency. See its coverage comparison with other existing databases in the Appendix I Table 4 of Alam et al. (2019).
[6] The number of countries is mainly constrained by the data availability of domestic credit.

Moreover, our paper sheds light on unintended consequences of macroprudential policy, thereby contributing to the existing literature on such spillovers by Cerutti and Zhou (2018), Avdjiev et al. (2017), Bruno et al. (2017), and Cizel et al. (2019). In particular, we examine a cross-border leakage effect where stricter macroprudential regulation imposed on domestic lending may lead to increased credit provision from abroad (e.g., Ahnert et al., 2021, Reinhardt and Sowerbutts 2015). We embed this analysis of cross-border leakages of macroprudential measures in an empirical framework that examines how strong domestic credit can "pull in" additional capital from abroad.[7] Our focus here is the effect on the type of "other investment flows", which includes direct cross-border borrowing, i.e., loans received by corporates from foreign banks, and cross-border funding of the domestic banking system through loans and deposits received from abroad (Borio, McCauley and McGuire, 2011; Avdjiev, McCauley and McGuire, 2012; Hahm et al., 2013).

In the baseline, first, we find evidence of the linkage between currency appreciation and domestic credit developments, as in the recent empirical literature (Bruno and Shin, 2015a,b; Hahm et al., 2013; Shin, 2018; Hofman et al., 2020). In particular, an appreciation of the local exchange rate vis-à-vis the U.S. dollar is followed by an increase in the credit-to-GDP gap[8] in the next quarter. Second, macroprudential policy is found to have a direct effect on domestic credit developments. A tightening macroprudential policy action leads to a reduction in the credit-to-GDP gap in the next quarter. Third, and most importantly, we examine the interaction effect of macroprudential policy in mitigating the impact of the exchange rate on domestic credit, and find that for a given appreciation of the real exchange rate, the subsequent increase in the credit-to-GDP gap is weaker where macroprudential policies had been tightened in the previous quarter. This is important both from a policy perspective and from the point of view of improving the identification of the causal effects of macroprudential measures.

Endogeneity is a common challenge faced by the literature on the effects of macroprudential policy. In our empirical framework, endogeneity may result from potential reverse causality between macroprudential policy actions and domestic credit, where policy action responds to changes in credit. This would bias the estimated coefficients of macroprudential policy actions, and could conceivably contaminate the coefficient estimate for the interaction term

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[7] Most discussions focus on a causal link that runs from capital inflows to credit, where capital inflows lead to an increase in loanable funds for the domestic banking system, and thereby "push up" the supply of domestic credit. However, many studies acknowledge that there is likely to be a two-way relationship, where strong domestic credit can also "pull-in" additional capital from abroad (Igan and Tan, 2017, Amri, Richey and Willet, 2016, Lane and McQuade, 2014).

[8] The credit-to-GDP gap was proposed by the Basel Committee as an early-warning indicator of financial crises (BCBS 2010; IMF-FSB-BIS 2016), and it captures the time dimension of systemic risk.

between the exchange rate and macroprudential policy. To mitigate the potential endogeneity, we first lag all the independent variables by one-quarter and use the Arellano-Bond difference GMM methodology, thereby following approaches commonly applied in the literature (e.g., Claessens et al., 2013; Cerutti et al., 2017b). In the absence of suitable instruments, we take further steps by constructing "macroprudential policy shocks" that are orthogonal to other covariates. We find that the baseline results are robust after controlling for endogeneity.

We finally doument important evidence of policy leakage that points to the benefit of combining macroprudetial policy and the use of capital controls. First, while tightening macroprudential policy is succesful in reducing domestic credit, it leads to further increases in cross-border flows, as domestic corporates respond by directly borrowing from abroad. Moreover, a complemtary use of targeted capital controls aimed at limiting these types of flows appears to be effective. We find that adopting these controls reduces the extent to which strong domestic credit stimulates capital inflows, thereby limiting the further build-up of systemic risk from direct cross-border borrowing.

The remainder of this paper is organized as follows: Section II describes the baseline data and empirical methodology, and presents the baseline findings. Section III offers a refinement, by constructing macroprudential policy shocks to more fully address the endogeneity of macroprudential policy. Section IV presents an extension focused on how strong domestic credit and the use of macroprudential policy may fuel capital inflows. Section V concludes and discusses policy implications.

II. The Baseline

2.1 Data and Empirical Methodology

Our sample includes 62 economies (35 AEs plus 27 EMEs) as shown in Figure 2. These economies have sufficiently good data at quarterly frequency, not just on macroprudential policy measures, but also on credit and GDP, enabling us to compute credit gaps. The sample covers economies that have taken frequent macroprudential policy actions (e.g., India, Korea, and Russia) and those that have rarely done so (e.g., Chile and Germany). The sample period spans from 2000:Q1 to 2016:Q4, as macroprudential policy has been increasingly used across countries since the early 2000.

In the baseline analysis, we use a dynamic panel model to investigate the determinants of the credit gap. Our baseline setup, which we expand on in further analysis, relates the credit gap (Y) to the real exchange rate movement ($\Delta^4 RER$), the macroprudential policy stance ($MaPP$), their interactions ($MaPP \times \Delta^4 RER$), as well as controls (monetary policy stance (MPS) and forecasted real GDP growth ($\Delta^4 F\_RGDP$):

$$\boldsymbol{Y_{i,t}} = \rho \boldsymbol{Y_{i,t-1}} + \beta_1 \Delta^{\boldsymbol{4}} \boldsymbol{RER_{i,t-1}} + \beta_2 \boldsymbol{MaPP_{i,t-1}} + \beta_3 \boldsymbol{MaPP_{i,t-1}} \times \Delta^{\boldsymbol{4}} \boldsymbol{RER_{i,t-1}} + \theta \boldsymbol{Z}_{i,t-1} + \mu_i + v_{i,t}$$

$$\text{where } E[\mu_i] = E[v_{i,t}] = E[\mu_i v_{i,t}] = 0$$

$$\text{control vector: } Z_{i,t-1} = [MPS_{i,t-1},\ \Delta^4 F\_RGDP_{i,t-1}]$$

The subscripts i and t represent country and quarter respectively; μ_i is a fixed effect that captures time-invariant country characteristics, and $v_{i,t}$ is the error term.[9]

To address endogeneity concerns and avoid the Nickell bias[10] arising in the presence of the lagged dependent variable, we lag all the independent variables by one quarter, and estimate the above equation using the Generalized Method of Moments (GMM) estimator developed by Arellano and Bond (1991). We verify that the Arellano-Bond approach is suitable for our purposes since all further conditions on its use are found to hold.[11] In our estimation, the first-differenced lagged dependent variable is instrumented with its 1–3 lags of its level. We use the forward orthogonal deviation transformation (Arellano and Bover 1995) to mitigate data gap issues in unbalanced panels. We also use two-step covariance estimates to obtain robust standard errors and to correct their downward bias (Windmeijer 2005). All independent and control variables are treated as predetermined rather than strictly exogenous.[12] To remove the effect of outliers, we winsorize the top and bottom 1 percent observations of each variable

[9] The quarterly time-fixed effect, a proxy of the exogenous global push factor, is not considered here. First, potential collinearity arises when the global push factor also affects the domestic drivers of credit developments. Indeed, the inclusion of time-fixed effect makes most coefficients of the domestic drivers statistically insignificant. Second, its inclusion violates the assumption of first order serial correlation in residuals for the dynamic panel model (e.g., p-values of AR(1) test range from 0.35 to 0.9).

[10] The bias of the Least Square Dummy Variables (LSDV) estimator in a dynamic model is generally known as dynamic panel bias or Nickell's bias (Nickell, 1981). If the lagged dependent variable appears as an independent variable, strict exogeneity of the regressors no longer holds. The LSDV is no longer consistent when N tends to infinity and T is fixed. Given our sample period T=68, the theoretical semi-asymptotic bias (assuming $N \rightarrow \infty$) is -0.0391 in the baseline for an autoregressive coefficient of 0.98. The actual bias is supposed to be slightly larger for the N=62 in our sample. Moreover, a smearing effect arises from the endogeneity (the inconsistency of other independent variables due to the endogeneity of the one variable is smeared across all of the least squares estimators), and a large degree of autocorrelation further magnifies the inconsistency (Kiviet 1995).

[11] In addition to the Nickell bias arising in the presence of the lagged dependent variable Y_{t-1}, these are that the independent variables are not strictly exogenous and are correlated with past errors; a statistically significant linear functional relationship holds, which is confirmed by our baseline result; there is unobserved cross-sectional heterogeneity in the credit gap (denoted as μ_i); there is autocorrelation within individual panel's error terms $\vartheta_{i,t}$, which is verified by a AR(1) test in the baseline result (Roodman, 2009).

[12] Predetermined is a weaker restriction than strict exogeneity. The underlying assumption is the current period error term is uncorrelated with current and lagged values of the predetermined variable but maybe correlated with future values.

except the dependent variable and the ordinal variable of macroprudential policy stance. Variables are described in detail below.

Dependent variable (Y): Our domestic credit measure is the credit-to-GDP gap, which is the quarterly credit-to-GDP ratio relative to its long-run trend. Following the method proposed by the BCBS (2010), we calculate the gap from a one-sided HP filter using a long-run smoothing parameter λ=400,000[13]. We adopt a broad definition of credit as total claims on the private non-financial sector from both banks and non-bank financial institutions to capture all domestic sources of debt provided to the private non-financial sector.[14] We use the financial corporations' domestic claims on the private sector from the IMF's International Financial Statistics (IFS) database where available, otherwise, we use depository corporations' (or monetary) domestic claims on the private sector from the same source. For brevity, we refer the credit-to-GDP gap as "credit gap" in the rest of this paper. We consider the simpler 4-quarter change of the credit-to-GDP ratio in a robustness check (see Appendix A.3).

Macroprudential policy stance (MaPP): Our measure of the macroprudential stance is an ordinal indicator variable representing the net number of macroprudential tightening actions (i.e., tightening actions net of loosening actions) that are taken during period t-1 in country i. For example, a value of +3 (-3) represents three policy actions being taken to tighten (loosen) the macroprudential policy stance within the quarter, and a value of 0 represents no action is taken within the quarter. In addition to this measure, we analyze the effects of tightening (T_MaPP) and loosening (L_MaPP) actions separately. The data source is the IMF's iMaPP database (Alam et al., 2019), which is, to the best of our knowledge, the most comprehensive database of macroprudential policies to date (covering 17 instruments[15] for a total of 138 counties over the period 1999–2016 at a monthly frequency). We consider an aggregate measure of the macroprudential policy stance ($iMaPP$) covering 16 instruments,[16] as well as two subgroups: borrower-based tools ($MaPP\_Br$) and financial institutions-based tools

[13] Initially taking the first 24 quarters, then computing the trend and cyclical components recursively (adding one quarter at a time).

[14] The credit data are not fully harmonized across countries as sometimes they are from surveys reported by different entities within a country. For instance, the credit data for Iceland and Taiwan POC are from the central banks. Allowing this heterogeneity is the only way to provide a sufficient sample coverage across country and time for our study.

[15] The 17 macroprudential instruments covered in (Alam et al., 2019) are:
(i) Borrower-based tools: LTV (loan-to-value), DSTI (debt-service-to-income);
(ii) Financial institutions-based tools: CCB (countercyclical capital buffer), LVR (bank leverage), LLP (loan loss provision), Capital (capital requirement), LFC (limits on foreign currency), RR (reserves requirement), Tax, SIFI (systemically important insitutions), Liquidity, LTD (loan-to-deposit), LFX (limit on open FX position), LCG (limits on aggregate credits), Conservation (banks' capital conservation buffers), LoanR (loan restrictions), Other (the rest regulations).

[16] We exclude SIFI (systemically important insitutions) from our analysis, because it is related to the cross-sectional dimension of systemic risk rather than the time dimension.

($MaPP\_FI$). If a tightening macroprudential action has the effect of reducing the credit gap, we would expect $\beta_2 < 0$.

YoY change of the real exchange rate ($\Delta^4 RER$): We use the (lagged) year-over-year log change of the weighted average of the bilateral nominal exchange rate prevailing over the past four quarters[17], which is denoted in national currency relative to the USD, and deflated by the U.S. consumer price index (CPI) against domestic CPI. An appreciating real exchange rate can fuel the build-up in credit through multiple channels as described in section I. By convention, a negative change in $\Delta^4 RER$ represents a real exchange rate appreciation, so we expect $\beta_1 < 0$.

Forecasted YoY growth of real GDP ($\Delta^4 F\_RGDP$): We include the forecasted real GDP growth as a control, in contrast to the widely used actual real GDP growth rates in the existing literature on the effects of macroprudential policy (e.g., Claessens et al., 2013; Akinci and Olmstead-Rumsey 2018; Cerutti et al., 2017a, and Alam et al., 2019).[18] This serves to mitigate a potential endogeneity problem, when "good news" about the economy leads the exchange rate to appreciate and at the same time stimulates credit. By including the growth forecast, we measure the effect of the residual variation in the exchange rate that is orthogonal to these effects. We construct the forecasted quarterly year-over-year real GDP growth by taking a weighted average of the current year's and next year's forecasted growth rates from the Consensus Forecast.[19] Optimism with respect to short-run economic outcomes is expected to drive up both credit demand and supply, so a positive coefficient is expected.

Monetary policy stance (MPS): We use (lagged) central bank policy rates to capture the monetary policy stance. For countries that have implemented unconventional monetary policies during the sample period (Euro Area, U.S., U.K., and Japan), we use the shadow policy rates estimated by Krippner (2016). As a monetary policy tightening is generally found to reduce aggregate demand and increase the cost of borrowing, we expect a negative coefficient.

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[17] The weighted average of variable X is calculated as X_WA= 0.4*X + 0.3*L1.X + 0.2*L2.X + 0.1*L4.X. We also use the simple average and the true year-over-year log change of real exchange rate, and found results to be consistent with using the weighted average (while the significance level and size of coefficients slightly dropped).

[18] A robustness check using the actual GDP growth is reported in Appendix A.1.

[19] The first quarter carries full weight of the forecasted real GDP growth of current year, based on the forecast in January; the second quarter gives 3/4 weight to the current year and 1/4 weight to the next year, based on the forecast in April; the third quarter gives equal weights to the current year and next year, based on the forecast in July; the fourth quarter carries full weight of the next year, based on the forecast in October. For five countries (Iceland, Lebanon, Luxembourg, Malta, and Mongolia) for which consensus forecasts are not available, we apply the same weighted average method but use the realized forward growth rates instead as a "perfect foresight" measure.

A key focus of our investigation is the interaction between macroprudential action and the change in the real exchange rate $MaPP_{i,t-1} \times \Delta^4 RER_{i,t-1}$. If macroprudential action is effective in containing the impact of the real exchange rate appreciation on the credit gap, we expect $\beta_3 > 0$, with this effect thereby attenuating the negative coefficient on the real exchange rate.

2.2 Baseline Finding

In Table 1, we show the baseline results for the aggregated index of macroprudential policy action, which includes all measures ($iMaPP$) and two subgroups of macroprudential policy tools: the borrower-based tools ($MaPP\_Br$), and the financial institutions-based tools ($MaPP\_FI$). The first two columns in each group are based on a measure of net tightening (tightening actions net of loosening actions); whereas the last two columns of each group separate the tightening (T_MaPP) and loosening actions (L_MaPP). We emphasize the following three results:

First, exchange rate movements have a measurable effect on domestic credit developments (column 1). In particular, a 10 percent real exchange rate appreciation is associated with a subsequent increase in the credit gap of 0.5 percentage points of GDP. This is on the same order of magnitude as the median credit gap in the sample (0.33 percent of GDP) and therefore economically meaningful. The size of the effect turns out to be robust across different specifications (with the size of the effect measured as between 0.5–0.6 percentage points) and statistically highly significant (typically at the 1 percent level).

Second, macroprudential policy has a direct effect on domestic credit developments. In particular, a net tightening of macroprudential policy is estimated to decrease the credit gap by 0.875 percentage points of GDP in the next quarter, which again exceeds the median and is roughly 0.08 of the standard deviation of the credit gap. Looking at different groups of macroprudential policy, the effect is stronger for borrower-based than financial institutions-based tools (columns 5 and 9), in line with prior literature (Cerutti et al., 2017a; Fendoglu 2017). Considering tightening and loosening actions separately (column 3), the effect is strong and significant for tightening actions (1.168 percentage points of GDP) but insignificant for loosening actions.

Third, in addition to having a direct impact on the credit gap, macroprudential policy has the effect of weakening the extent to which exchange rate movements impact credit developments. This effect is reflected in the coefficient of the interaction term $MaPP_{i,t-1} \times \Delta^4 RER_{i,t-1}$ which shows a strong and statistically significant effect of macroprudential policy in mitigating the effect of the exchange rate on the credit gap. This mitigating effect holds for both the aggregated and the two subgroups of macroprudential policy, that is for both borrower-based and financial institutions-based policies. In economic terms, a one standard deviation increase in $iMaPP$ is estimated to reduce the sensitivity of the credit gap to real exchange rate movements by 0.82 percentage points of GDP. This effect is again stronger for the borrower-based tools, with a one standard deviation increase in such tools being estimated to reduce the sensitivity of the credit gap to the real exchange rate by 1.4 percentage points of GDP, compared with 0.9 percentage points of GDP for the financial institutions-based tools.

As for the control variables, the estimated coefficients have the expected sign and are highly significant: A one percentage point tightening of monetary policy is estimated to reduce the credit gap by about 0.24–0.29 percentage points of GDP, while expectations of improved macroeconomic conditions increase the credit gap, with a one percent improvement in annual forecasted real GDP growth leading to an increase of the credit gap by about 0.45–0.5 percentage points of GDP.

Furthermore, the instrument lag choice is validated by the p-value of AR(1) and AR(2) at the bottom of Table 1. Results yield small p-values of AR(1) about 2 percent, so the null hypothesis of no first order autocorrelation in first differences is rejected as expected. The instrument lag choice yields all AR(2) p-values above the 10 percent threshold (ranging from 23–37 percent), so the null hypothesis of no first order autocorrelation in levels (AR(2)) is not rejected, suggesting the second lags are appropriate instruments for their current values.

III. USING MACROPRUDENTIAL POLICY SHOCKS

Macroprudential policy actions are not taken in a vacuum, but may be taken in response to macroeconomic and financial developments, which may be the same variables used to assess their effects. In our context, when we want to assess the effect of the policy action on the credit gap, but the credit gap is used as a signal for policy actions by policymakers, this can result in reverse causality. In particular, when high values of the credit gap are likely to prompt a tightening, this could induce a positive correlation that would bias the estimates on the impact of tightening, which are expected negative, up towards zero (attenuation bias).

Already in our baseline, we mitigate the risk of biased estimates due to endogeneity in two ways, with the first commonly applied in the literature (e.g., Claessens et al., 2013; Cerutti et al., 2017a):

- First, we lag the macroprudential indicator and control variables by one-quarter and also include the lagged dependent variable. Estimation is then using the Arellano-Bond difference GMM methodology, which is suitable for independent variables that are not strictly exogenous;
- Second, we focus on the interaction term of $MaPP_{i,t-1} \times \Delta^4 RER_{i,t-1}$. This should suffer less from an endogeneity bias, on the assumption that changes to exchange

rates are not commonly taken into consideration when setting macroprudential policy. The change in the exchange rate then functions as exogenous shifter of the effect of prior macroprudential action, reducing the potential endogeneity problem.[20]

In this section we take a third step and construct macroprudential policy shocks that are orthogonal to both the credit gap and the change in the real exchange rate. Our three-step method to construct these shocks is similar to Brandao-Marques et al. (2020), and also related to the literature that computes policy shocks for monetary (Furceri et al., 2016) and fiscal policy (Auerbach and Gorodnichenko, 2013). An important advantage of the method we use here is that, while the baseline specification assumes that macroprudential policy would not respond to changes in the exchange rate, the shocks we compute are orthogonal to exchange rate changes by construction.

- Step 1: we estimate an ordered probit model of the macroprudential policy indicator variable[21] conditional on observables. As independent variables we use the year-over-year change of the quarterly credit-to-GDP gap, the change of the real exchange rate ($\Delta^4 RER$ as used in the baseline), the quarterly change of net capital inflows to GDP ratio, an indicator of lagged policy actions (the sum of lags one to four of the quarterly macroprudential policy indicator), and a country indicator to capture cross-sectional heterogeneity. The ordered-probit (first stage regression) is shown in Table 2.
- Step 2: we construct the "expected" macroprudential policy stance using the probabilities obtained from the ordered probit regression conditional on the independent variables.
- Step 3: we compute the macroprudential policy shocks as the actual macroprudential indicators minus their expected values. Thus, positive values represent tightening shocks and negative values represent loosening shocks.

When we replace the variable $MaPP_{i,t-1}$in the baseline regression with the macroprudential policy shocks identified above ($MaPP\_Shock\ _{i,t-1}$), the estimation result (Table 3), is roughly consistent with the baseline. In particular, all interaction terms are essentially the same. However, at the margin we find that the base effects of macroprudential action are measured larger and more significant compared to the baseline. This is especially true for the coefficients on the aggregate indicator and the financial institutions-based indicator, suggesting that these coefficients are measured with less bias when using the shocks.

[20] This reasoning is in line with recent econometric theory developed by Bun and Harrison (2019). These authors study inference in models in which there is both an endogenous (x) and an exogenous explanatory variable (w). Their analysis shows that, under fairly general conditions, the interaction term between x and w can be identified consistently using OLS, even if it is difficult to find valid instruments for the base effect of x on y, and the OLS estimator of this effect is inconsistent.

[21] For this exercise we recategorize the macroprudential indicator into five outcomes: tightening by two or more actions (+2), tightening by one action (+1), no change (0), loosening by one action (-1), or loosening by two or more actions (-2). We then use an ordered probit that accounts for these five buckets.

In line with the above, in the first-step regressions (Table 2) we find that the coefficient on the change in the credit gap is sizable and statistically significant in the regressions (1)-(2) explaining the overall and financial-institutions-based indicators, respectively (at the five and one percent levels), while the credit gap is not significant in the regression (3) explaining the use of borrower-based tools. This suggests that the former actions respond more strongly to credit developments, relative to the borrower-based tools, creating a higher risk of attenuation bias in the measurement of policy effects. As regards the first-step results, it is worth noting that the change in the exchange rate does not enter statistically significant in any of the regressions, in line with our prior that macroprudential policy does not tend to react to movements in the exchange rate.

Overall, we find that when using macroprudential policy shocks, the attenuation bias from reserve causality is reduced. This affects mainly the base effect of macroprudential policy, particularly the financial institutions-based tools, but interactions of all types of tools with the change in the exchange rate are not affected substantially when using macroprudential policy shocks in place of the indicator we have in the baseline.

IV. Extension: Policy leakages and cross-border flows

This section extends our analysis to examine feedback effects that run from domestic credit developments, and policy levers that may be deployed to affect them, to specific types of capital inflows (right-hand side of Figure 1).

There is likely a two-way causation between capital flows and credit. On the one hand, where capital inflows are strong for reasons that are exogenous to the economy, this may reduce the cost of domestic credit and thereby "push up" domestic credit supply (Mendoza and Terrones, 2012). On the other hand, domestic factors, including both strong domestic credit demand and policies that temp down domestic credit can also "pull in" cross-border flows. In particular, a tightening of both monetary and macroprudential policy can lead to increases in the so-called "gross other investment inflows", which capture cross-border borrowing by domestic financial institutions and non-financial corporates (Avdjiev, McCauley and McGuire, 2012, Hahm et al., 2013). In this context, our study aims to shine a light on the unintended spillover effects of these domestic policies. We then also examine whether targeted capital controls can reduce gross other investment inflows and thereby reduce the feedback effect.

In this analysis we regress gross other investment inflows ($CFLOW_t$) on the lagged credit gap (Y_{t-1}) and lagged policy variables, as one way of controlling for reserve causality. It is embedded in standard "push-and-pull" capital flow regressions with domestic factors that have been identified in the existing literature as driving capital inflows, including monetary policy (MPS) and growth expectations ($\Delta^4 F\_RGDP$). We include the quarter time-fixed effects (μ_t) to account for global push factors in a "catch-all" manner. In order to control as tightly as possible for time-invariant omitted variables at the country level, we include country-fixed effects (α_i) in addition to the time-fixed effects.

To investigate the unintended spillover effects of domestic policy, we include monetary policy (MPS), macroprudential policy ($MaPP$), and capital controls, with separate regressions for long-standing controls ($FARI$) and episodic controls ($\Delta FARI$). Their interaction terms with the credit gap are included to analyze how policies interact with the credit gap in affecting the level of capital inflows. As explained further below, our main interest is on the interaction effect between measures of capital controls (i.e., the walls and the gates) and the credit gap: do such controls reduce the extent to which high domestic credit stimulates direct borrowing from abroad?

- *The "Walls" Effect of Capital Controls (levels,* $\boldsymbol{FARI}$*) + MaPP*

$$\boldsymbol{CFLOW_{i,t}} = \rho \boldsymbol{CFLOW_{i,t-1}} + \beta_1 \boldsymbol{Y_{i,t-1}} + \beta_2 \boldsymbol{FARI_{i,t-1}} + \beta_3 \boldsymbol{Y_{i,t-1}} \times \boldsymbol{FARI_{i,t-1}}$$
$$+\beta_4 \boldsymbol{MaPP_{i,t-1}} + \beta_5 \boldsymbol{Y_{i,t-1}} \times \boldsymbol{MaPP_{i,t-1}} + +\beta_6 \boldsymbol{MPS_{i,t-1}} + \beta_7 \boldsymbol{Y_{i,t-1}} \times \boldsymbol{MPS_{i,t-1}}$$
$$+\theta_i \, \Delta^4 F\_RGDP_{i,t-1} + \theta_t \mu_t + \theta_i \alpha_i + v_{i,t}$$

- *The "Gates" Effect of Capital Controls (1-quarter change,* $\Delta\boldsymbol{FARI}$*) + MaPP*

$$\boldsymbol{CFLOW_{i,t}} = \rho \boldsymbol{CFLOW_{i,t-1}} + \beta_1 \boldsymbol{Y_{i,t-1}} + \beta_2 \Delta \boldsymbol{FARI_{i,t-1}} + \beta_3 \boldsymbol{Y_{i,t-1}} \times \Delta \boldsymbol{FARI_{i,t-1}}$$
$$+\beta_4 \boldsymbol{MaPP_{i,t-1}} + \beta_5 \boldsymbol{Y_{i,t-1}} \times \boldsymbol{MaPP_{i,t-1}} + +\beta_6 \boldsymbol{MPS_{i,t-1}} + \beta_7 \boldsymbol{Y_{i,t-1}} \times \boldsymbol{MPS_{i,t-1}}$$
$$+\theta_i \, \Delta^4 F\_RGDP_{i,t-1} + \theta_t \mu_t + \theta_i \alpha_i + v_{i,t}$$

We use a simple fixed effect model to estimate the above equations.[22] The main variables are described in detail below:

Capital inflows ($CFLOW$): As our dependent variable, we consider gross other investment inflows[23]. These types of flows are most likely to reflect unintended effects from the domestic policies under investigation, since they have been found to exhibit the most robust

[22] The previously applied dynamic panel (GMM) model in the baseline is inappropriate here, primarily because the assumption of autocorrelation within individual panel's error terms $v_{i,t}$ is not satisfied. The p-values of AR(1) test (ranging from 0.62 to 0.8) invalidate the moment conditions used in the dynamic panel estimation (Roodman, 2009). The trade-off is that we may not fully account for the Nickell bias in the estimation of the effect on the lagged dependent variable. The semi-asymptotic bias (assuming $N \to \infty$) for our sample T=68 is -0.0262 for an autoregressive coefficient equals to 0.72 in the extension section, compared with the bias of -0.0391 in the baseline given its autoregressive coefficient of 0.98. Although we are not able to quantitatively assess the degree of smearing effect arises from the endogeneity for the two cases, it is reasonable to assume that the smearing effect in the feedback regression is smaller and may not be a major concern.

[23] Gross inflows are net purchases of domestic assets by foreign agents while net flows subtract net purchases of foreign assets by domestic agents. Other investment inflows capture all other investments flows than direct investment, portfolio equity and debt, financial derivatives, and reserves. It includes currency and deposits, loans, insurance and pensions, trade credit and advances, other accounts payable, and SDR allocations.

positive association with domestic credit unconditionally (IMF 2017; Igan and Tan 2017). The capital inflows data are expressed in terms of gross other investment inflows within quarter t as a percent of GDP in the previous quarter t-1. The data source is the IMF Financial Flows Analytics (FFA) database.

Push factors: As discussed above, we include quarterly time-fixed effects (μ_t), following Ahnert et al., (2021), to account for global "push" factors that affect all countries equally to the fullest possible extent. Inclusion of these quarter fixed effects should render the variation in "other capital flows" that is left to be explained by the credit gap (and other domestic variables) orthogonal to global push factors that might be driving an impact running from capital flows to increases in domestic credit.[24] This can include changes in global risk aversion, the monetary policy stance in advanced economies, and other variables that may be difficult to observe. Time-fixed effects can help control for all these push factors in a parsimonious way.

Pull factors: In line with the existing literature, we include monetary policy and forecasted real GDP growth as domestic pull factors (with variable definitions and data sources as in baseline). These variables are meant to capture to what extent tight monetary policy and a positive GDP growth outlook attract cross-border flows.

Capital controls (FARI): Different from the majority of previous studies that consider broad-based measures of capital controls and apply indicators on in- and outflow restrictions of all types of flows, we use an index of controls that are targeted at the type of "other investment (banking and corporate) inflows." The relevant "Financial Accounts Restrictiveness Index" (FARI) is compiled based on source data from the IMF's Annual Report on Exchange Arrangements and Exchange Restrictions (AREAER). Following Klein (2012), we distinguish between long-standing capital controls ("walls", $FARI$), measured as the level of the index on "other investment inflows" for each country in a given quarter, and temporary adjustments ("gates", $\Delta FARI$), measured as incremental changes (+1 for net tightening and -1 for net loosening actions) in such controls.

Table 4 has results for a measure of the existence or not of capital account restrictions across the relevant categories (the "walls" effect in the language of Klein (2012), while Table 5 has results for incremental changes in such controls (the "gates" effect). There are four noteworthy findings:

[24] An alternative way to understand this point is to imagine that we were to run a two-step estimation, where other capital inflows are first regressed on a set of time-fixed effects, in order to purge the outcome variable of the effects of global push factors, and where the residual would be used in a second step regression on domestic pull factors, including the credit gap. Basic econometric theory, known as the "Frisch-Waugh-Lovell Theorem" implies that the coefficients of the second step regression would be the same as those shown in our results. See further Davidson and McKinnon (1993, page 19).

First, we find evidence that supports the idea that there is a feedback effect from credit to other investment inflows, which arises when a strong domestic credit cycle leads to greater cross-border borrowing by banks and corporates. As indicated by the coefficients of Y_{t-1}, a 10 percentage points of GDP increase in the domestic credit gap (close to a one-standard deviation change) is associated with a rise of cross-border capital inflows in the subsequent quarter, ranging from 0.9 to 1.3 percentage points of GDP.

Second, we find that a tight monetary policy stance also leads to a higher level of cross-border capital inflows in the subsequent quarter. A 1 percentage point increase in the policy rate is associated with an increase of about 1.7 percentage points in the ratio of other investment inflows to GDP. The coefficients on interactions between monetary policy and credit are not significantly different from zero (coefficients are very close to zero and with relatively large standard errors). This implies that the effect of monetary policy on inflows does not appear to depend on whether the level of the domestic credit gap is high or low.

Third, we find that macroprudential policy tightening is also associated with increases in other investment flows, with the result being significant for borrower-based tools.[25] This is in line with the idea that domestic macroprudential policy tightening is associated with cross-border leakages, that is, tends to further increase borrowing from abroad. An average tightening action of borrower-based tools is found to lead to an increase in cross-border borrowing on the order of 1.3–1.5 percentage points of GDP. This is intuitive as the borrower-based tools restrict borrowing from domestic banks, which can be circumvented by borrowing directly from abroad. On the other hand, financial institution-based tools do not tend to have such strong effects on banks' domestic provision of provision, and they may therefore not give rise to such strong incentives for circumvention. This might explain why we find them to have an insignificant role in affecting other investment flows.

Fourth, and in contrast, targeted long-standing capital controls ("walls") can have sizeable effects in reducing cross-border funding, in particular by affecting the interactions between credit and these other capital inflows. The coefficients of the direct impact of long-standing controls already is negative and significant (at the 10 percent significance level), and these controls also have a significant indirect effect in mitigating the impact of a high credit gap in increasing cross-border funding (again significant at the 10 and sometimes 5 percent level).[26] Hence, this suggests that these long-standing targeted controls are indeed acting as a "wall" when the credit "tide" is up and risks pulling in additional capital from abroad.

As for the episodic controls (Table 5), both the direct effect and indirect effect of capital controls have the expected negative coefficients, although they are not always statistically

[25] Results further improve when using macroprudential policy shocks instead.

[26] These results broadly hold for the random effect model (panel c), although the direct effects of capital controls are smaller and less significant.

significant.[27] However, the interaction effects between episodic controls and the credit gap remain insignificant throughout. The associated standardized coefficients of episodic controls (added in blue) are also much smaller than those found for the long-standing controls. These findings are in line with prior evidence suggesting that the effects of incremental changes in capital controls ("gates") are weaker, relative to the effect of pre-existing controls ("walls").

Overall, this evidence suggests that macroprudential policy and capital controls have a complementary role in mitigating systemic risk, with long-standing and targeted capital controls useful when strong domestic credit pulls in additional capital inflows and this cannot be controlled by macroprudential measures and would be exacerbated by monetary tightening. Our comparison of "walls" and "gates" also suggests that when targeted controls are applied to specific types of flows to complement domestic macroprudential measures, these controls may be more effectively deployed in a precautionary manner, ahead of the occurrence of a surge in such flows.

V. Conclusion & Policy Implications

This paper examines the effectiveness of macroprudential policies in attenuating the impact of real exchange rate movements on domestic credit cycles. Our main result is that macroprudential policy is effective not only in dampening domestic credit, but also reducing the procyclical impact of external shocks on credit–- a novel finding in the literature.

Using dynamic panel regression and a new and comprehensive dataset of macroprudential policy measures that covers a large sample of 62 economies over 2000:Q1–2016:Q4 period, we find robust evidence that exchange rate appreciation is associated with subsequent increases in the credit-to-GDP gap—a well-known early warning indicator of future financial crises. Importantly, we find that tighter macroprudential policies can mitigate this effect, thereby insulating the economy from procyclical effects of external shocks on credit. These findings are robust to employing a range of methods to address endogeneity of the use of macroprudential measures.

We also find evidence supporting the effectiveness of targeted capital inflow controls, especially long-standing ones, in dampening a feedback effect that arises when strong domestic credit leads to increases in borrowing from abroad, and tighter monetary and macroprudential policies would further stoke such cross-border borrowing. This points to the benefits of a potential complementary use of macroprudential policies and capital inflow controls, even as alternatives and tradeoffs should be considered carefully.

[27] When we use "shocks" to capital controls, the statistical significance of the base effect improves in some regressions, pointing to the presence of a reverse causality bias that needs to be addressed.

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Table 1. Baseline Results

	iMaPP				Borrower-based tools				Financial institutions-based tools			
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Y_{t-1}	0.982***	0.989***	0.986***	0.991***	0.988***	0.988***	0.987***	0.988***	0.983***	0.989***	0.987***	0.991***
	(0.020)	(0.018)	(0.020)	(0.018)	(0.020)	(0.019)	(0.019)	(0.017)	(0.021)	(0.019)	(0.021)	(0.020)
$\Delta^4 RER_{t-1}$	-0.050**	-0.054***	-0.053**	-0.052***	-0.053**	-0.060***	-0.054**	-0.056***	-0.051**	-0.053***	-0.054**	-0.050***
	(0.021)	(0.018)	(0.022)	(0.019)	(0.021)	(0.018)	(0.021)	(0.019)	(0.021)	(0.020)	(0.021)	(0.019)
$MaPP_{t-1}$	-0.875*	-0.737			-2.231**	-1.990*			-0.416	-0.365		
	(0.463)	(0.461)			(1.016)	(1.087)			(0.502)	(0.397)		
$MaPP_{t-1} \times \Delta^4 RER_{t-1}$		0.144***				0.253*				0.148***		
		(0.048)				(0.151)				(0.049)		
T_MaPP_{t-1}			-1.168**	-0.852*			-2.000	-1.620			-1.149	-0.810
			(0.524)	(0.503)			(1.395)	(1.597)			(0.728)	(0.624)
$T\_MaPP_{t-1} \times \Delta^4 RER_{t-1}$				0.130**				0.270**				0.112*
				(0.061)				(0.135)				(0.067)
L_MaPP_{t-1}			0.027	-0.630			-3.034**	-2.886			0.504	-0.028
			(0.754)	(0.709)			(1.535)	(1.878)			(1.184)	(0.934)
$L\_MaPP_{t-1} \times \Delta^4 RER_{t-1}$				0.159***				0.224				0.160***
				(0.042)				(0.201)				(0.052)
MPS_{t-1}	-0.269***	-0.246***	-0.264***	-0.245***	-0.290***	-0.273***	-0.285***	-0.290***	-0.262***	-0.240***	-0.263***	-0.249***
	(0.074)	(0.069)	(0.073)	(0.073)	(0.073)	(0.072)	(0.073)	(0.074)	(0.069)	(0.072)	(0.071)	(0.072)
$\Delta^4 F\_RGDP_{t-1}$	0.504***	0.462***	0.503***	0.453***	0.496***	0.453***	0.492***	0.473***	0.465***	0.447***	0.473***	0.457***
	(0.077)	(0.085)	(0.080)	(0.089)	(0.074)	(0.079)	(0.073)	(0.076)	(0.077)	(0.078)	(0.082)	(0.083)
Observations	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842
# of Economies	62	62	62	62	62	62	62	62	62	62	62	62
AB AR(1) test - p value	0.00183	0.000741	0.00157	0.000754	0.00180	0.000833	0.00160	0.000735	0.00279	0.00133	0.00184	0.00121
AB AR(2) test - p value	0.394	0.257	0.264	0.250	0.350	0.255	0.373	0.278	0.390	0.297	0.281	0.253
Hansen test - p value	1	1	1	1	1	1	1	1	1	1	1	1

Source: Authors' calculations.

Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2. Results from Ordered Probit Regression

	iMaPP	MaPP_Br	MaPP_FI
VARIABLES	(1)	(2)	(3)
$\Delta^4 \boldsymbol{Y_{t-1}}$	0.009**	0.006	0.011***
	(0.004)	(0.006)	(0.004)
$\Delta^4 \boldsymbol{RER_{t-1}}$	0.001	0.001	0.001
	(0.001)	(0.002)	(0.001)
$\Delta^4 \boldsymbol{NCFLOW_{t-1}}$	0.000	0.000	0.000
	(0.000)	(0.001)	(0.000)
$\sum_{s=-4}^{-1} \boldsymbol{MaPP_s}$	0.110***	0.029	0.117***
	(0.014)	(0.021)	(0.014)
Constant cut1	-2.076***	-2.882***	-2.165***
	(0.169)	(0.335)	(0.175)
Constant cut2	-1.383***	-2.167***	-1.483***
	(0.163)	(0.308)	(0.168)
Constant cut3	1.490***	2.168***	1.585***
	(0.163)	(0.308)	(0.168)
Constant cut4	2.193***	2.950***	2.345***
	(0.166)	(0.319)	(0.173)
Observations	3,699	3,699	3,699

Source: Authors' calculations.
Note: (1) Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
(2) The constant cut points represent the thresholds of predicted cumulative normal distribution of the dependent variable corresponding to its different categorical values.

Table 3. Robustness—Results with Macroprudential Policy Shocks

	iMaPP				**Borrower-based tools**				**Financial institutions-based tools**			
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Y_{t-1}	0.988***	0.993***	0.989***	0.991***	0.993***	0.990***	0.992***	0.991***	0.989***	0.993***	0.988***	0.989***
	(0.019)	(0.017)	(0.019)	(0.017)	(0.018)	(0.017)	(0.019)	(0.018)	(0.019)	(0.018)	(0.019)	(0.015)
$\Delta^4 RER_{t-1}$	-0.058***	-0.055***	-0.057***	-0.053***	-0.054**	-0.058***	-0.056**	-0.058***	-0.057**	-0.054***	-0.060***	-0.053***
	(0.020)	(0.019)	(0.021)	(0.019)	(0.022)	(0.021)	(0.023)	(0.022)	(0.023)	(0.020)	(0.020)	(0.019)
aPP_Shock_{t-1}	-1.088**	-1.240**			-1.988**	-1.891*			-1.157**	-1.343**		
	(0.490)	(0.529)			(0.884)	(0.986)			(0.562)	(0.576)		
$\iota PP\_Shock_{t-1} \times \Delta^4 RER_{t-1}$		**0.178*** **				**0.240***				**0.192*** **		
		(0.048)				(0.136)				(0.050)		
$T\_MaPP\_Shock_{t-1}$			-2.090***	-1.979**			-1.970	-1.658			-2.870***	-2.522**
			(0.702)	(0.797)			(1.247)	(1.389)			(0.931)	(1.069)
$MaPP\_Shock_{t-1} \times \Delta^4 RER_{t-1}$				**0.215*** **				**0.317***				**0.251** **
				(0.077)				(0.181)				(0.107)
$L\_MaPP\_Shock_{t-1}$			0.914	0.392			-2.371	-1.865			1.784	1.161
			(0.814)	(0.795)			(1.455)	(1.650)			(1.359)	(1.207)
$L\_MaPP\_Shock_{t-1} \times \Delta^4 RER_{t-1}$				**0.126*** **				0.012				**0.116** **
				(0.044)				(0.203)				(0.047)
MPS_{t-1}	-0.315***	-0.276***	-0.269***	-0.250***	-0.312***	-0.301***	-0.315***	-0.289***	-0.287***	-0.263***	-0.249***	-0.221***
	(0.080)	(0.074)	(0.081)	(0.075)	(0.068)	(0.073)	(0.078)	(0.079)	(0.079)	(0.076)	(0.082)	(0.075)
$\Delta^4 F\_RGDP_{t-1}$	0.469***	0.452***	0.450***	0.426***	0.443***	0.459***	0.450***	0.470***	0.485***	0.466***	0.461***	0.432***
	(0.084)	(0.088)	(0.091)	(0.089)	(0.072)	(0.077)	(0.076)	(0.079)	(0.077)	(0.084)	(0.088)	(0.088)
Observations	3,505	3,505	3,505	3,505	3,505	3,505	3,505	3,505	3,505	3,505	3,505	3,505
# of Economies	62	62	62	62	62	62	62	62	62	62	62	62
AB AR(1) test - p value	0.00341	0.000832	0.00135	0.000387	0.00422	0.00210	0.00411	0.00184	0.00348	0.000896	0.000471	0.000253
AB AR(2) test - p value	0.640	0.439	0.311	0.203	0.583	0.434	0.592	0.506	0.625	0.424	0.271	0.162
Hansen test - p value	1	1	1	1	1	1	1	1	1	1	1	1

Source: Authors' calculations.
Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4. Leakages and the "Walls" Effect of Capital Controls (The Fixed Effect Model)

	Baseline	iMaPP				Borrower-based tools				Financial institutions-based tools			
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE
The "Walls" Effect of Capital Controls + MaPP													
$CFLOW_{t-1}$	0.713***	0.713***	0.711***	0.713***	0.713***	0.713***	0.711***	0.713***	0.713***	0.713***	0.711***	0.713***	0.713***
	(0.032)	(0.032)	(0.032)	(0.032)	(0.033)	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)
	0.713	*0.713*	*0.711*	*0.713*	*0.713*	*0.713*	*0.711*	*0.712*	*0.712*	*0.712*	*0.711*	*0.712*	*0.712*
Y_{t-1}	0.091**	0.090**	0.128**	0.088**	0.103*	0.089**	0.126**	0.086**	0.102*	0.091**	0.129**	0.092**	0.104*
	(0.037)	(0.037)	(0.044)	(0.037)	(0.048)	(0.037)	(0.045)	(0.036)	(0.048)	(0.037)	(0.044)	(0.038)	(0.048)
	0.057	*0.057*	*0.081*	*0.055*	*0.065*	*0.056*	*0.080*	*0.054*	*0.065*	*0.058*	*0.082*	*0.058*	*0.066*
$FARI_{t-1}$	-4.933*	-5.004*	-6.116*	-5.014*	-5.218*	-5.161*	-6.251*	-5.298*	-5.378*	-4.907*	-6.034*	-4.919*	-5.127*
	(2.341)	(2.356)	(3.110)	(2.346)	(2.419) h	(2.386)	(3.144)	(2.403)	(2.459)	(2.342)	(3.114)	(2.362)	(2.409)
	-0.045	*-0.046*	*-0.056*	*-0.046*	*-0.048*	*-0.047*	*-0.057*	*-0.048*	*-0.049*	*-0.045*	*-0.055*	*-0.045*	*-0.047*
$MaPP_{t-1}$		0.144	0.130	0.091	0.134	1.474**	1.443**	1.159	1.466**	-0.076	-0.090	-0.048	-0.089
		(0.096)	(0.089)	(0.189)	(0.102)	(0.461)	(0.466)	(0.656)	(0.461)	(0.169)	(0.160)	(0.172)	(0.175)
		0.004	*0.004*	*0.003*	*0.004*	*0.044*	*0.043*	*0.035*	*0.044*	*-0.002*	*-0.003*	*-0.001*	*-0.003*
MPS_{t-1}	0.174**	0.174**	0.170**	0.175**	0.165**	0.176**	0.171**	0.176**	0.167**	0.174**	0.169**	0.173**	0.165**
	(0.074)	(0.074)	(0.071)	(0.074)	(0.071)	(0.074)	(0.072)	(0.075)	(0.072)	(0.074)	(0.071)	(0.073)	(0.071)
	0.047	*0.047*	*0.046*	*0.048*	*0.045*	*0.048*	*0.047*	*0.048*	*0.045*	*0.047*	*0.046*	*0.047*	*0.045*
$Y_{t-1} \times FARI_{t-1}$			-0.110**				-0.107*				-0.111**		
			(0.048)				(0.049)				(0.048)		
			-0.031				*-0.030*				*-0.031*		
$Y_{t-1} \times MaPP_{t-1}$				0.021				0.104				-0.017	
				(0.034)				(0.082)				(0.019)	
				0.006				*0.029*				*-0.005*	
$Y_{t-1} \times MPS_{t-1}$					-0.004				-0.004				-0.004
					(0.005)				(0.005)				(0.005)
					-0.013				*-0.013*				*-0.013*
$\Delta^4 F\_RGDP_{t-1}$	0.570	0.568	0.560	0.566	0.569	0.561	0.554	0.556	0.563	0.571	0.564	0.572	0.573
	(0.366)	(0.365)	(0.358)	(0.365)	(0.360)	(0.364)	(0.357)	(0.363)	(0.359)	(0.366)	(0.359)	(0.366)	(0.361)
	0.074	*0.074*	*0.073*	*0.073*	*0.074*	*0.073*	*0.072*	*0.072*	*0.073*	*0.074*	*0.073*	*0.074*	*0.074*
Constant	0.567	0.601	0.906	0.602	0.727	0.642	0.941	0.700	0.769	0.553	0.862	0.559	0.682
	(2.358)	(2.348)	(2.254)	(2.346)	(2.346)	(2.363)	(2.268)	(2.355)	(2.362)	(2.347)	(2.252)	(2.347)	(2.345)
Time-fixed Effect	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter
Observations	3,603	3,603	3,603	3,603	3,603	3,603	3,603	3,603	3,603	3,603	3,603	3,603	3,603
# of Economies	61	61	61	61	61	61	61	61	61	61	61	61	61
Overall R-square	0.725	0.725	0.726	0.726	0.725	0.726	0.726	0.726	0.726	0.725	0.726	0.725	0.725
Within R-square	0.631	0.631	0.631	0.631	0.631	0.632	0.632	0.632	0.632	0.631	0.631	0.631	0.631

Source: Authors' calculations.
Note: (a) Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
(b) We drop Taiwan POC in the sample due to missing data of financial account restriction index.
(c) The blue italic numbers are standardized coefficients, representing the change of standard deviation in the dependent variable by one standard deviation change in corresponding independent variables.
(d) Both of the capital inflows and capital control measures are based on the gross other investment inflows.

Table 5. Leakages and the "Gates" Effect of Capital Controls (The Fixed Effect Model)

	Baseline	iMaPP				Borrower-based tools				Financial institutions-based tools			
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE
The "Gates" Effect of Capital Controls + MaPP													
$CFLOW_{t-1}$	0.718***	0.719***	0.719***	0.719***	0.718***	0.718***	0.718***	0.718***	0.718***	0.718***	0.718***	0.718***	0.718***
	(0.035)	(0.035)	(0.035)	(0.035)	(0.036)	(0.035)	(0.035)	(0.035)	(0.036)	(0.035)	(0.035)	(0.036)	(0.036)
	0.719	*0.719*	*0.719*	*0.719*	*0.719*	*0.719*	*0.719*	*0.718*	*0.718*	*0.719*	*0.719*	*0.718*	*0.718*
Y_{t-1}	0.097**	0.096**	0.097**	0.095**	0.106**	0.096**	0.096**	0.093**	0.105**	0.097**	0.097**	0.099**	0.107**
	(0.033)	(0.033)	(0.033)	(0.034)	(0.041)	(0.033)	(0.034)	(0.033)	(0.041)	(0.033)	(0.033)	(0.034)	(0.041)
	0.064	*0.064*	*0.064*	*0.063*	*0.071*	*0.064*	*0.064*	*0.062*	*0.070*	*0.065*	*0.065*	*0.066*	*0.071*
$\Delta FARI_{t-1}$	-0.318	-0.352*	-0.338	-0.342*	-0.344*	-0.345*	-0.327	-0.367*	-0.340*	-0.294*	-0.278	-0.307	-0.286*
	(0.184)	(0.180)	(0.207)	(0.173)	(0.171)	(0.175)	(0.205)	(0.174)	(0.167)	(0.159)	(0.183)	(0.169)	(0.150)
	-0.005	*-0.005*	*-0.005*	*-0.005*	*-0.005*	*-0.005*	*-0.005*	*-0.006*	*-0.005*	*-0.004*	*-0.004*	*-0.005*	*-0.004*
$MaPP_{t-1}$		0.111	0.110	0.077	0.101	1.303**	1.306**	0.984	1.294**	-0.087	-0.088	-0.057	-0.099
		(0.070)	(0.069)	(0.129)	(0.074)	(0.519)	(0.517)	(0.590)	(0.512)	(0.156)	(0.152)	(0.147)	(0.164)
		0.003	*0.003*	*0.002*	*0.003*	*0.039*	*0.040*	*0.030*	*0.039*	*-0.003*	*-0.003*	*-0.002*	*-0.003*
MPS_{t-1}	0.161*	0.161*	0.161*	0.162*	0.150*	0.162*	0.162*	0.161*	0.152*	0.161*	0.161*	0.160*	0.150*
	(0.076)	(0.076)	(0.076)	(0.076)	(0.073)	(0.077)	(0.077)	(0.077)	(0.074)	(0.076)	(0.076)	(0.075)	(0.073)
	0.045	*0.045*	*0.045*	*0.045*	*0.042*	*0.045*	*0.046*	*0.045*	*0.043*	*0.045*	*0.045*	*0.045*	*0.042*
$Y_{t-1} \times \Delta FARI_{t-1}$			-0.012				-0.015				-0.013		
			(0.028)				(0.027)				(0.028)		
			-0.002				*-0.002*				*-0.002*		
$Y_{t-1} \times MaPP_{t-1}$				0.015				0.105				-0.023	
				(0.026)				(0.076)				(0.016)	
				0.004				*0.030*				*-0.007*	
$Y_{t-1} \times MPS_{t-1}$					-0.003				-0.003				-0.003
					(0.004)				(0.004)				(0.004)
					-0.010				*-0.010*				*-0.011*
$\Delta^4 F\_RGDP_{t-1}$	0.494	0.491	0.490	0.490	0.493	0.485	0.483	0.478	0.486	0.495	0.494	0.495	0.497
	(0.368)	(0.367)	(0.367)	(0.368)	(0.363)	(0.367)	(0.366)	(0.366)	(0.363)	(0.368)	(0.367)	(0.368)	(0.364)
	0.064	*0.064*	*0.063*	*0.063*	*0.064*	*0.063*	*0.063*	*0.062*	*0.063*	*0.064*	*0.064*	*0.064*	*0.064*
Constant	-2.554	-2.541	-2.539	-2.545	-2.454	-2.534	-2.532	-2.504	-2.448	-2.563	-2.561	-2.551	-2.474
	(1.815)	(1.814)	(1.815)	(1.815)	(1.779)	(1.808)	(1.808)	(1.804)	(1.772)	(1.814)	(1.814)	(1.805)	(1.779)
Time-fixed Effect	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter
Observations	3,783	3,783	3,783	3,783	3,783	3,783	3,783	3,783	3,783	3,783	3,783	3,783	3,783
# of Economies	61	61	61	61	61	61	61	61	61	61	61	61	61
Overall R-square	0.724	0.724	0.724	0.724	0.724	0.725	0.725	0.725	0.725	0.724	0.724	0.724	0.724
Within R-square	0.633	0.633	0.633	0.633	0.633	0.634	0.634	0.634	0.634	0.633	0.633	0.633	0.633

Source: Authors' calculations.

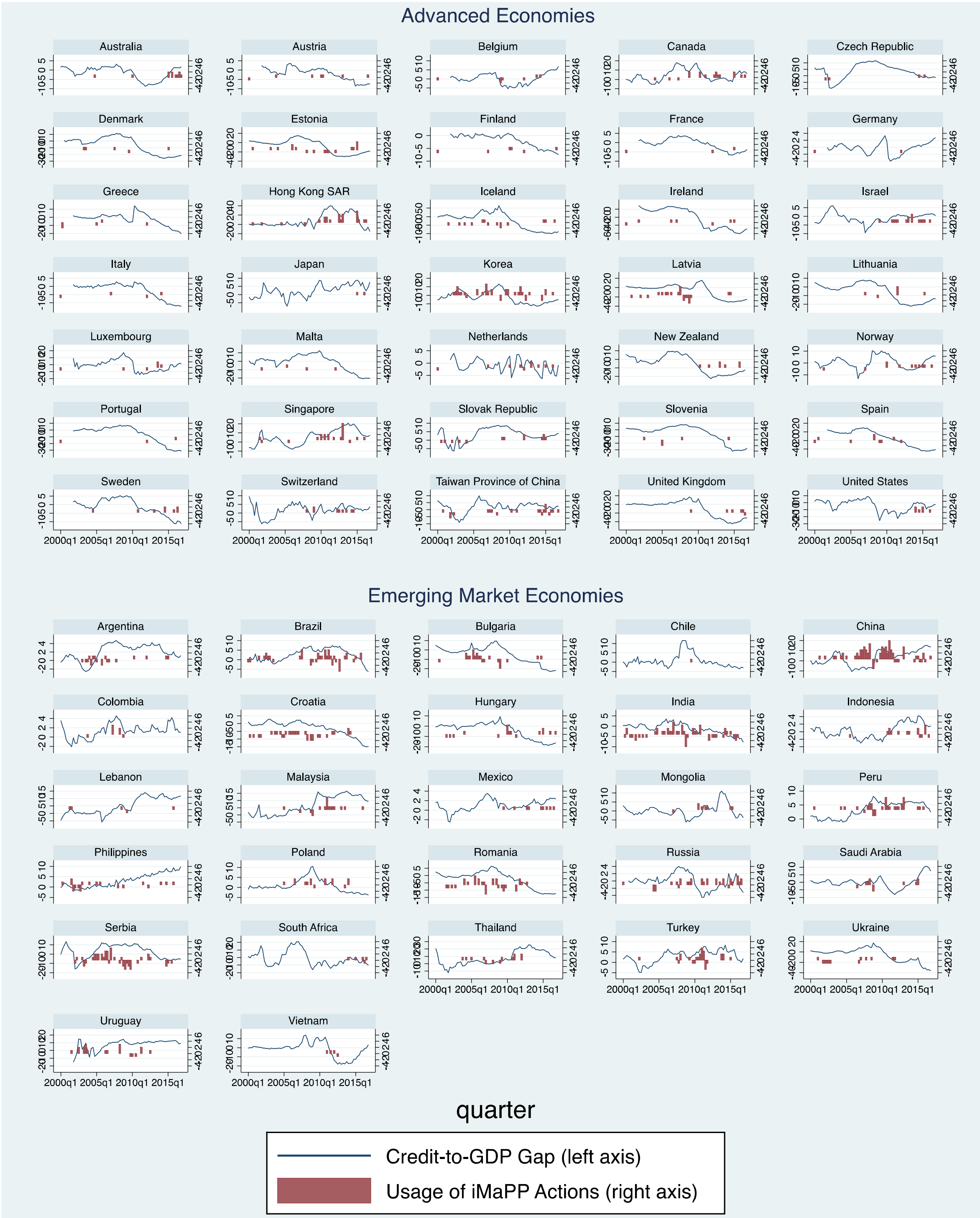
Note: (a) Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

(b) We drop Taiwan POC in the sample due to missing data of financial account restriction index.

(c) The blue italic numbers are standardized coefficients, representing the change of standard deviation in the dependent variable by one standard deviation change in corresponding independent variables.

(d) Both of the capital inflows and capital control measures are based on the gross other investment inflows.

Figure 2. Credit-to-GDP Gap and Usage of iMaPP Actions
(2000: Q1–2016: Q4, by Economy)



Source: Authors' calculations.
Note: Country classification is based on the latest WEO.