Exchange Rates and Domestic Credit – Can Macroprudential Policy Reduce the Link? [*]

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Abstract

This paper examines empirically the role of macroprudential policy in addressing the effects of external shocks on financial stability. The baseline results show that an appreciation of the local exchange rate is associated with a subsequent increase in the domestic credit gap, while a prior tightening of macroprudential policies dampens this effect. These results are robust to accounting for endogeneity of macroprudential policy and changes in exchange rates. We also examine a feedback effect where strong domestic credit pulls in additional cross-border funding, potentially further increasing systemic risk, and find that targeted capital controls can alleviate this effect.

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I Introduction

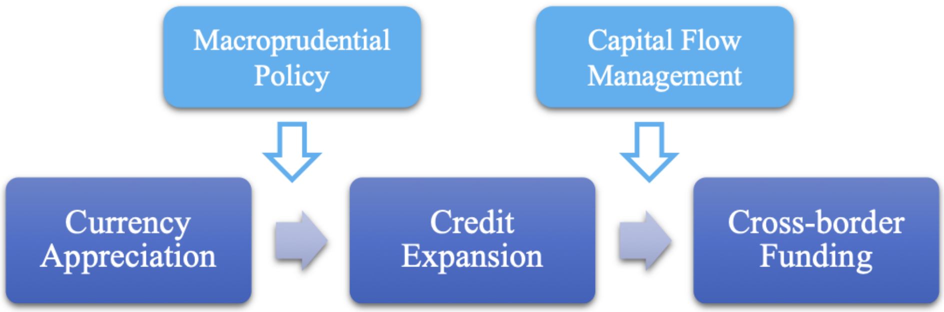
A recently growing literature explores how movements in exchange rates not only affect macroeconomic outcomes, but can also affect financial conditions and credit developments, which may in turn feed back into the macroeconomic outlook (Blanchard et al., 2016; Shin, 2018; Ghosh et al., 2018; Hofmann et al., 2020; BIS, 2019). The main idea is that a currency appreciation would tend to 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 collateral values and 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 encourage market participants to take greater risks (Hofmann et al., 2020).[1] Through these mechanisms, an appreciation may become expansionary, in contrast to the standard notion in the earlier literature where an appreciation is held to be contractionary by reducing net exports. Moreover, when an appreciation leads the domestic provision of credit to expand, this can contribute to systemic risk[2], and potentially require a policy response on the part of macroprudential policymakers.

To help guide the use of macroprudential policy, in this paper, 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 effects of currency movements on 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).

[1]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; Hofmann et al., 2020).

[2]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; Baskaya et al., 2017).

Figure 1: Exchange Rates, Credit, and Capital Flows



Source: Author's descriptions.

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.[3] Most existing studies find evidence supporting 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). However, due to the lack of detailed cross-country information on macroprudential policy or credit developments, the existing literature primarily covers a small set of countries (that have frequently taken macroprudential policy actions as in Fendoğlu (2017)), often over a fairly short period of time (e.g., post global financial crisis), or examining a small subset of macroprudential instruments (e.g., Vandenbussche et al. (2015) and Kuttner and Shim (2016) examine housing-related tools only). Comprehensive evaluations across a large set of economics and over a long period are still limited.

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.[4] Another feature

[3]See Galati and Moessner (2013) for the discussion of cross-sectional dimension of systemic risk, which arises through interlinkages of financial intermediaries.

[4]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

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[5] over the period 2000 to 2016, with observations at the quarterly frequency for 16 categories of macroprudential instruments. 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.

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 impact 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? For this type of economy, Gourinchas and Obstfeld (2012) find that a rapid increase in leverage and a sharp real currency appreciation emerge consistently as the two most robust and significant predictors of financial crises. Moreover, traditional monetary policy adjustment through increases in short-term interest rates would not be suitable to contain the financial expansion that precedes these crises. Instead, macroprudential policy tightening may be needed to address systemic risks, by reducing the link between the exchange rate and domestic credit.

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)).

et al. (2019). Our paper excludes the instument of SIFI because it is related to the cross-sectional dimension of systemic risk.

[5]The number of countries is mainly constrained by the data availability of domestic credit.

If macroprudential policy tightening can attenuate the procyclical effects of external shocks, then this would free the hand of monetary policy makers to focus more squarely on managing the effects of domestic shocks on the economy.

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); 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 (Borio et al., 2011; Avdjiev et al., 2012; Hahm et al., 2013).[6] Our focus here is the effect on the category 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.

In the baseline, first, we find evidence of the linkage between currency appreciation and domestic credit developments, as in the recent empirical literature (Hofmann et al., 2020; Bruno and Shin, 2015a,b; Hahm et al., 2013; Shin, 2018). In particular, an appreciation of the local exchange rate vis-is the U.S. dollar is followed by an increase in the credit-to-GDP gap[7] 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

[6]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 et al., 2016; Lane and Mcquade, 2014).

[7]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.

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 (i) potentially omitted effects of economic fundamentals, which may drive both credit and exchange rate changes; (ii) potential reverse causality between macroprudential policy actions and domestic credit, where policy action responds to changes in credit. These two issues would bias the estimated coefficients of exchange rate movement and macroprudential policy actions, respectively, and could conceivably contaminate the coefficient estimate for the interaction term 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 "exchange rate shocks" and "macroprudential policy shocks" that are orthogonal to other covariates. We find that the baseline results are robust after controlling for endogeneity.

In an extension, we find that increases in domestic credit are associated with increases in other investment flows. Moreover, where macroprudential policy is tightened, this leads to further increases in cross-border flows. We interpret this as evidence of policy leakage, where domestic corporates respond to macroprudential tightening by directly borrowing from abroad. On the other hand, targeted capital controls that aim to limit these types of flows appear to be effective. We find that adopting these controls reduces the size of other investment flows that are being pulled in by strong domestic credit, or the tightening of macroprudential policy, 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 presents the baseline empirical methodology, data, and discusses the baseline findings. Section III addresses the endogeneity problem. Section IV presents the extension on how domestic credit may fuel capital inflows. Section V concludes and discusses policy implications.

II The Baseline

II.1 Empirical Methodology

In the main part of the paper, we use a dynamic panel framework to investigate the determinants of the credit gap. Our baseline set-up, which we expand on in further analysis, relates the credit gap (denoted Y) to changes in the real exchange rate, macroprudential policy actions, their interactions, as well as controls:

$$\mathbf{Y}_{i,t} = \rho \mathbf{Y}_{i,t-1} + \beta_1 \Delta^4 \mathbf{RER}_{i,t-1} + \beta_2 \mathbf{MaPP}_{i,t-1} + \beta_3 \mathbf{MaPP}_{i,t-1} \times \Delta^4 \mathbf{RER}_{i,t-1} + \theta \mathbf{Z}_{i,t-1} + \mu_i + v_{i,t}$$

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

$$\text{control variable } \mathbf{Z}_{i,t-1} = \left[MPS_{i,t-1}, \Delta^4 F\_RGDP_{i,t-1}\right]$$

The subscripts i and t represent country and time (quarter) respectively; μ_i is a fixed effect that captures time-invariant country characteristics, and $v_{i,t}$ is the error term.[8] $\Delta^4 RER_{i,t-1}$ is the year-over-year log change of real exchange rate, which is lagged by one quarter. $MaPP_{i,t-1}$ an ordinal indicator variable representing the number of macroprudential policy actions by direction (tightening actions net of loosening actions) that are taken during

[8]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).

period $t-1$ in country i. In addition to this measure, we analyze the effects of tightening (T_MaPP) and loosening (L_MaPP) actions separately.

$Z_{i,t-1}$ is a vector of control variables, which includes the monetary policy stance (MPS) and the consensus forecast of year-over-year real GDP growth ($\Delta^4 F\_RGDP_{i,t-1}$), again lagged by one quarter. In contrast to the actual real GDP growth rates widely used in the existing literature (e.g., Claessens et al., 2013; Akinci and Olmstead-Rumsey, 2018; Cerutti et al., 2017a; Alam et al., 2019), we use the forecasted real GDP growth rates rather than the more widely used actual GDP growth rates to mitigate a potential endogeneity concerns stemming from both credit and the exchange rate being driven by good news about the economy. By including the growth forecast, we measure the effect of the residual variation in the exchange rate that is orthogonal to these effects. A robustness check using the actual GDP growth is reported in Appendix A.1.

The aggregate measure $MaPP_{i,t-1}$ is a lagged ordinal indicator variable representing the number of macroprudential policy actions by direction (tightening actions net of loosening actions) which 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.

If a tightening macroprudential action has the effect of reducing the credit gap, we would expect $\beta_2 < 0$ in the equation above. Moreover, by construction, a negative change in $\Delta^4 RER_{i,t-1}$ represents a real exchange rate appreciation. If an appreciation is associated with an increase in the credit gap, we would therefore expect $\beta_1 < 0$.

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.

We estimate the above equation using the Generalized Method of Moments (GMM) estimator developed by Arellano and Bond (1991), to address endogeneity concerns and avoid the Nickell bias[9] arising in the presence of the lagged dependent variable. In addition, we verify that the Arellano-Bond approach is suitable for our purposes since all further conditions on its use are found to hold.[10]

We lag the $MaPP$ variables by one-quarter, which is consistent with the approach in the previous literature (Akinci and Olmstead-Rumsey, 2018; Cerutti et al., 2017a; Fendoğlu, 2017). We finally lag the real exchange rate, as well as all other independent and control variables to mitigate endogeneity concerns.

In our estimation, the first-differenced lagged dependent variable is instrumented with its 1-3 lags of its level. All dependent and control variables are treated as predetermined.[11] 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).

II.2 Data

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

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[9]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 goes to infinity) 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).

[10]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 $\theta_{i,t}$, which is verified by a AR(1) test in the baseline result (Roodman, 2009).

[11]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.

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. To remove the effect of outliers, we winsorize the top and bottom 1 percent observations of each variable except the dependent variable and the ordinal variable $MaPP$. See Table 1 for the description of variables, and Table 2 for the summary of statistics.

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.[12] Credit is broadly defined in this paper as total claims on the private non-financial sector from both banks and non-bank financial institutions to capture all domestic sources of debt funds for the private sector.[13] 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 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 ($MaPP$): The data source for macroprudential policy actions 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 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 ($iMaPP$) as well

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

[13] 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.

as two subgroups: borrower-based tools ($MaPP\_Br$) and financial institutions-based tools ($MaPP\_FI$). Moreover, the iMaPP database allows us to distinguish policy adjustments that amount to a tightening and those that lead to a loosening of macroprudential constraints. We exclude from our index tools used to conrol risks from systemically important insitutions (SIFIs), resulting in an index that aggreagates 16 out of the 17 categorires in the iMaPP database, because that category of tools is related to the cross-sectional dimension of systemic risk rather than its time dimension. Details of the individual macroprudential instruments covered in each subgroup are available in Table 3 .

YoY change of 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[14], 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 $\Delta^4 RER_{i,t-1}$ represents a real exchange rate appreciation, so we expect a negative coefficient.

Forecasted YoY growth of real GDP ($\Delta^4 F\_RGDP$): We include the consensus forecast of future GDP growth as a control, since 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. 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.[15] For five countries (Iceland, Lebanon, Luxembourg, Malta, and Mongolia) for which consensus forecasts are not available, we apply the same

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[14]The weighted average of variable X is calculated as $X_{WA} = 0.4 \times X + 0.3 \times l1.X + 0.2 \times l2.X + 0.1 \times l3.X$. 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).

[15]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.

weighted average method but use the realized forward growth rates instead as a "perfect foresight" measure. Optimism with respect to short-run economic outcomes is expected to drive up both credit demand and supply, so a positive coefficient is expected. We use the actual GDP growth rates in a robustness check.

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 so-called 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.

II.3 Baseline Finding

Table 4 presents the baseline regression results on the effects of the exchange rate and macroprudential policy on the credit gap.

We show results for the aggregated index of macroprudential policy action, which includes all measures ($iMaPP$) and two subgroups of macroprudential policy, the borrower-based tools ($MaPP\_Br$) and the financial institutions-based tools ($MaPP\_FI$). The first two columns shown for each group are based on a measure of net tightening (tightening actions net of loosening actions); and the last two columns of each group separate the tightening (T) and loosening actions (L). We emphasize four 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; Fendoğlu, 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 loosing 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 interactions 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.

Fourth and the most importantly, we find evidence that a prior relaxation of macroprudential policy can have a strong and significant effect in increasing the extent to which an exchange rate shock affects the credit gap. This is evident from looking at the interaction terms for tightening and loosing actions separately (column 4). Columns 8 and 12 reveal that this effects appears to be driven by loosening of financial institutions-based tools rather than borrower-based ones.

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.

The coefficients of the lagged dependent variable appear close to unity, but a Fisher-type panel unit root test confirms that the credit gap variable is stationary (in at least one panel).[16] Also, the large autoregressive coefficient is partially due to the setting of forward orthogonal deviation transformation in the GMM estimation that is needed to account for unbalanced panels.

Furthermore, the instrument lag choice is validated by the p-value of AR(1) and AR(2) at the bottom of Table 4. 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 Addressing Endogeneity

A common challenge faced by the literature on the effects of macroprudential policy is the endogeneity problem that arises when an independent variable is correlated with the error term. In our empirical framework, endogeneity can arise in two ways. First, it can be related to potentially omitted variables, such as omitted economic fundamentals that may drive both credit and the exchange rate, thereby inducing biased coefficient estimates. Second, endo-

[16]See Phillips and Moon (2000) for their review of recent econometric methods of panel unit root tests and their discussion on the inadequacy of those tests.

geneity can arise from potential reverse causality between macroprudential policy actions and domestic credit (Galati and Moessner, 2018; Alam et al., 2019). 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).

We mitigate the risk of biased estimates due to endogeneity in three ways, with the first being commonly applied in the literature (e.g., Claessens et al., 2013; Cerutti et al., 2017a):

- First, already in our baseline set up, 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.[17]

- Third, and finally, in the absence of suitable instruments, we construct more "exogenous" exchange rate shocks and macroprudential policy shocks, and include them in the main regression. The advantage of this approach is that, by construction, these

[17] 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.

shocks are orthogonal both to the credit gap and other covariates. We discuss the detailed methodology and result below.

III.1 Distilling Exchange Rate Shocks

We first focus on potential omitted economic fundamentals that are not included in the baseline regression, but may simultaneously be driving both the real exchange rate and credit developments. For instance, it is conceivable that, in addition to growth expectations, inflation and current account developments affect the exchange rate, and these might also affect the credit gap. This would result in an omitted variable bias in that the coefficient on the exchange rate would reflect both the causal effect and the correlation induced through the omitted fundamentals, thereby overstating the former. While this need not be the case, this bias could conceivably then also affect the interaction term between the exchange rate and macroprudential policy.

To address this concern, we attempt to distill more "exogenous" exchange rate shocks for use in our main regressions. The proposed two-stage procedure we use here is similar in spirit to those used in Auerbach and Gorodnichenko (2013) for fiscal policy, Furceri et al. (2016) for monetary policy, and Ahnert et al. (2021) for macroprudential policy. First, we "purge" the impact of domestic fundamental factors on the real exchange rate by running a fixed-effect regression of the exchange rate movement on those domestic fundamentals,[18] and in the second stage we use the residuals from this regression to replace $\Delta^4 RER_{i,t}$ in our baseline dynamic panel regression with those (lagged) "purged shocks." The first stage regressions have the following forms:

$$\Delta^4 \mathbf{RER}_{i,t} = \beta_1 \Delta^4 \mathbf{Inflation}_{i,t} + \beta_2 \Delta^4 \mathbf{F\_RGDP}_{i,t} + \beta_3 \Delta^4 \mathbf{CA\_Deficit}_{i,t} + \eta_i + e_{i,t}$$

[18] Results of the second stage regressions are robust given different combinations of domestic fundamental variables considered in the first stage (we have also tried the quarterly change of domestic demand), and they all yield very low explanatory power in the first stage fixed-effect regression (overall R-square less than 0.05).

Where $\Delta^4 Inflation$ is the year-over-year change of the CPI, $\Delta^4 F\_RGDP_{i,t-1}$ is the year-over-year real GDP growth, and $\Delta^4 CA\_Deficit$ is the year-over-year change in the current account deficit (positive values entail a greater deficit while negative values a move towards surplus). Data availability means that the size of the sample is slightly reduced from 62 to 60 economies when the current account variable is included in the first-step regression.

Table 5 shows the results of the first stage. Unsurprisingly, while individual coefficients on some of the fundamentals come out significant, the overall explanatory power of the first-stage regressions is low (R-square is about 0.04), in line with the well-known notion that exchange rate movements are difficult to explain, and are therefore to a considerable extent "random" (Rossi, 2013). On the other hand we find that both growth forecasts and inflation developments are significant in explaining movements in exchange rates, with the effect of the GDP forecast overall the strongest.

Table 6 shows the results of the second stage that uses "exchange rate shocks," obtained as the residuals from the first-stage regressions, instead of the actual change in the real exchange rate, as a determinant of the credit gap. We find that, while there is a very slight drop in the size of the coefficient on the exchange rate, relative to the baseline, the results on this effect continue to hold strongly. Moreover, the ability of macroprudential policy to affect the credit gap directly and indirectly, by reducing the impact of the exchange rate shocks on credit, continues to hold.[19]

As an alternative, we have attempted to instrument the domestic real exchange rate fluctuations in the baseline regression using a standard instrumental variable approach, with potential instruments in the first stage regression including time fixed effects, average changes in real exchange rates against the dollar in countries within the same region, as well as

[19] A commonly used technique is to bring the omitted variable into the main regression. The disadvantage is that this does not generate information on the drivers of the exchange rate changes. Moreover, it accounts less well for the endogeneity at issue here. Empirically, we find that although the coefficient of forecasted growth is significant in the baseline, when we include inflation and changes in the current account in the main regressions these variables are not significant in affecting the credit gap. The results on other variables are largely unchanged (results not shown).

changes in commodity prices from Gruss and Kebhaj (2019). We found, however, that the results throughout suffered from a "weak instruments" problem, in that the correlation of the predicted values with the actual change in real exchange rates is low, then reducing also the correlation with credit developments that we investigate in the second stage. We take away from these exercises that country-specific variation in exchange rate movements account for an important part of the co-movement between exchange rates and the credit gap, and that an approach that retains this information is better able to address the endogeneity problem.

Overall, we conclude that it is crucial to control for endogeneity induced by omitted economic fundamentals in our context. We find that including GDP forecasts in the specification is already a quite powerful way of doing so. Constructing more explicit exchange rate shocks can also be useful, but does not lead to major changes in the estimates when compared to a baseline that already controls for growth expectations.

III.2 Using Policy Shocks

As set out above, when estimating the effects of macroprudential policy on credit, a further endogeneity concern arises from reverse causality – when macroprudential policy is not random, but reacts to credit developments. When we want to measure the effects of policy action on credit developments, this can lead the estimated coefficients to be biased down towards zero–the so-called attenuation bias (see also Alam et al., 2019). While our baseline regression already attempts to mitigate this bias, by using the lag of the macroprudential indicator, we here go a step further, by using macroprudential policy "shocks."

Specifically, the identification of macroprudential policy shocks follows a three-step method closely following Brandao-Marques et al. (2020), and related again also to the literature that computes policy shocks for monetary (Furceri et al., 2016) and fiscal policy (Auerbach and Gorodnichenko, 2013):

- Step 1: we estimate an ordered probit model of the macroprudential policy indicator variable[20] 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 ($\Delta^4 RER_{i,t}$ as used in the baseline), the quarterly change of net capital inflows (as percent of GDP), 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 7.

- Step 2: we compute the "expected" macroprudential policy stances 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.

By construction, the shocks are orthogonal to credit developments, as measured by the past changes in the credit gap, helping to reduce endogeneity. In addition, importantly, they are orthogonal to exchange rate changes, thereby mitigating concerns that macroprudential policy might respond to exchange rate movements.

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, shown in Table 8, 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 holds in particular for the coefficients on the aggregate indicator and the financial institutions-based indicator, suggesting that these coefficients are measured with less bias when using

[20] 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.

the shocks.

When inspecting the first-step regressions (Table 7), consistent with this, we find that the coefficient on the change in the credit gap is sizable and statistically significant at the five and one per cent levels in the regressions explaining the overall and financial-institutions-based indicators, respectively, while the credit gap is not significant in the regressions 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 greater potential for attenuation bias in the measurement of policy effects. As regards the first-step results, it is worth noting also 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, and in particular the financial-institutions-based tools while 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: The Feedback Effect from Credit to Capital Inflows

Up until now, we have focused on the role of exchange rate shocks in driving domestic credit developments and the ability of macroprudential policy to attenuate these effects. This section extends our analysis to examine feedback effects that run from credit developments, and policy levers that may be deployed to affect them, to specific types of capital inflows (right-hand side of Figure 1). More specifically, we hypothesize that both strong domestic credit demand and policies that tamp down domestic credit–potentially both monetary and macroprudential policy–can lead to increases in the so-called "gross other investment inflows" that capture cross-border borrowing by domestic financial institutions and non-financial corporates. Evidence in favor of this hypothesis would shine a light on the unintended spillover effects of these domestic policies. In this context, we also examine whether targeted capital controls can reduce gross other investment inflows and thereby reduce the feedback effect.

As noted already, there is likely to be a two-way causation between capital flows and credit. On the one hand, strong credit demand may cause domestic financial institutions and corporates to "pull in" capital from abroad, as domestic funding sources are exhausted or constrained (Avdjiev et al., 2012; Hahm et al., 2013). On the other hand, where capital inflows are strong for other reasons, this may reduce the cost of domestic credit and thereby "push up" domestic credit supply (Mendoza and Terrones, 2012). In general equilibrium, both these effects would lead us to observe a positive correlation between measures of credit and measures of capital inflows.

In this analysis we regress gross other inflows on the lagged credit gap as one way of controlling for reverse causality. In addition, and importantly, we include quarter time-fixed effects to account separately for all global push factors. 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 lead to common variations in capital inflows driving changes in the supply of domestic credit.[21] Despite this, it is difficult to rule out that the coefficient on the credit gap still captures a "push" effect that runs from inflows to credit to some extent.[22] We therefore prefer to interpret the coefficient on the credit gap in the analysis below as measuring a conditional

[21]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 MacKinnon, 1993, page 19).

[22]For instance, it is possible that different countries have different sensitivity to global push factors. Our regression set-up cannot control for that, since our data are at the country level, and therefore do not offer sufficient degrees of freedom to include interactions between time- and country-fixed effects.

correlation that is consistent with a feedback relationship, rather than measuring a causal effect.

This analysis is embedded in otherwise 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 and growth expectations, as well as the "catch-all" push factor discussed above. The main variables we use are the following:

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 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. The benefit of these time fixed effects is that they control all global factors that are common across countries in each period. 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 all these push factors in a parsimonious way.

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

[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.

Capital controls ($FARI$): Different from the majority of previous studies that consider broad-based measures of capital controls and apply indicators on inflow 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" we investigate in the regression. 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"), measured as the level of the index on "other investment inflows" for each country in a given quarter, and temporary adjustments ("gates"), measured as incremental changes (+1 for net tightening and -1 for net loosening actions) in such controls.

Cerdeiro and Komaromi (2021) point out the difficulty of identifying empirically the effects of capital controls. On the one hand, these controls do not vary much over time, reducing the power of standard fixed effects regressions. On the other hand, their level could be correlated with a number of country-specific factors, exposing a random effects regression to potential omitted variables. They argue in favor of an identification through interaction effects, by showing in a simple model that capital controls not only affect the unconditional mean of flows, but importantly the sensitivity of flows to various push and pull factors; a point that is usually not exploited in regressions that neglect interactions and assume an additive linear effect of capital controls on the level of capital flows.

Our regressions equations are given below. We regress gross other capital flows on the credit gap (Y), lagged by one quarter, and we also include lagged policy variables, with separate regressions for long-standing controls and episodic controls. As explained further just 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, $FARI$) + $MaPP$

$$\mathbf{CFLOW}_{i,t} = \rho\mathbf{CFLOW}_{i,t-1} + \beta_1\mathbf{Y}_{i,t-1} + \beta_2\mathbf{FARI}_{i,t-1} + \beta_3\mathbf{Y}_{i,t-1} \times \mathbf{FARI}_{i,t-1}$$
$$+ \beta_4\mathbf{MAPP}_{i,t-1} + \beta_5 Y_{i,t-1} \times \mathbf{MAPP}_{i,t-1} + \beta_6\mathbf{MPS}_{L,t-1} + \beta_7\mathbf{Y}_{l,t-1} \times \mathbf{MPS}_{i,t-1}$$
$$+ \theta\Delta^4\mathbf{F\_RGDP}_{i,t} + \theta_t\mu_t + \theta_i\alpha_i + v_{i,t}$$

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

$$\mathbf{CFLOW}_{i,t} = \rho\mathbf{CFLOW}_{i,t-1} + \beta_1\mathbf{Y}_{i,t-1} + \beta_2\mathbf{\Delta FARI}_{i,t-1} + \beta_3\mathbf{Y}_{i,t-1} \times \mathbf{\Delta FARI}_{i,t-1}$$
$$+ \beta_4\mathbf{MAPP}_{i,t-1} + \beta_5 Y_{i,t-1} \times \mathbf{MAPP}_{i,t-1} + \beta_6\mathbf{MPS}_{L,t-1} + \beta_7\mathbf{Y}_{l,t-1} \times \mathbf{MPS}_{i,t-1}$$
$$+ \theta\Delta^4\mathbf{F\_RGDP}_{i,t} + \theta_t\mu_t + \theta_i\alpha_i + v_{i,t}$$

Following Cerdeiro and Komaromi (2021), we include country-fixed effects, in addition to the time-fixed effects, in order to control as tightly as possible for time-invariant omitted variables at the country level.[24] For identification, our main interest is then on the interaction effect $\mathbf{Y}_{i,t-1} \times \mathbf{\Delta FARI}_{i,t-1}$ between the credit gap and measures of capital controls: do such controls reduce the extent to which high domestic credit stimulates direct borrowing from abroad? In addition, we include interaction terms to analyze how other policies interact with the credit gap (the domestic pull factor of interest) in affecting the level of other investment flows.

The estimation here does not use the previously applied dynamic panel (GMM) model, in the main since the assumption of autocorrelation within individual panel's error terms $v_{i,t}$ is not satisfied in the estimated equation of the feedback effect. The p-value of AR(1) test

[24]The random effect regression is considered in a robustness check (see Appendix B). It is based on the de-meaned variables (of capital inflows and monetary policy stance) and shock variables (of macroprudential policy shocks, and the shock of capital inflow control for the "gate" effect only). The average credit-to-GDP ratio over sample period by country is considered as an additional country-specific "pull" factor in the random effects regressions.

ranges from 0.62 to 0.8, implying strong evidence against the first order serial correlation of residuals in differences, and thus would invalidate the moment conditions used in the dynamic panel estimation (Roodman, 2009).[25]

Throughout, we include all three policy variables: monetary policy, macroprudential policy, and capital controls. Table 9 has results for a measure of the breadth of capital account restrictions across the relevant category (the "walls" effect in the language of Klein (2012)), while Table 10 has results for incremental changes in such controls (the "gates" effect). Appendix Table B1 reports results for macroprudential policy shocks that account for reverse causality (as above in section III.2); and Appendix Table B2 reports the finding of a robustness check applying the random effects approach.

There are four noteworthy findings: First, we find evidence that supports the idea of a feedback effect from credit to other investment inflows that 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.

[25]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 goes infinity) 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.

Third, we find that macroprudential policy tightening is also associated with increases in other investment flows, at the margin, with the result being significant for borrower-based tools and in particular when using macroprudential policy shocks (see Appendix Table B1). 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 the domestic credit provision, potentially reducing 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, it appears 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 10), both the direct effect and indirect effect of capital controls have the expected negative coefficients, although they are not always statistically significant. When we use "shocks" to capital controls, the statistical significance of the base effect improves in some regressions (see Appendix Table B2), pointing to the presence

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

of a reverse causality bias that needs to be addressed.[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 and Policy Implication

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 in 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 the period of 2000:Q1-–2016:Q4 period, we find robust evidence that exchange rate appreciation is associated

[27] Shocks to capital controls are computed in a manner analogous to the way we construct macroprudential policy shocks, by first running an ordered probit of the gates measure of controls on the same set of variables used to compute macroprudential shocks, including the credit gap, the change in the real exchange rate and the net capital inflow, and then using the difference between the actual value on the indicator and its conditional expectation.

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 change in the exchange rate or 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.

Appendix

The online appendix is saved at [this location]

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Table 1: Description of Variables

Variable	Symbol	Description	Sources
Dependent variable:			
Credit-to-GDP gap	Y	Calculated by one-sided HP filter using smoothing parameter=400,000. [(a) (b)]	IMF IFS; BIS; central banks
Independent variables:			
Macroprudential policy stance	$MaPP$ T_MaPP L_MaPP	+1 represents a tightening action is taken within the quarter; -1 represents a loosening action is taken within the quarter; 0 represents no policy action is taken within the quarter. $iMaPP$ is the aggregated measure, $MaPP\_Br$ is the borrower-based measure, and $MaPP\_FI$ is the financial institutions-based measure.	IMF iMaPP Database (Alam et al. 2019)
YoY change of real exchange rate	$\Delta^4 RER$	Year-over-year log change of weighted average [(c)] of quarterly bilateral nominal exchange rate in NC per USD, deflated by U.S. CPI against domestic CPI. Positive value = depreciation against USD.	IMF IFS
Control variables:			
Monetary policy stance	MPS	Central bank policy rates. Use shadow policy rates for Euro Area, U.S., U.K., and Japan.	IMF IFS, BIS, and Krippner (2016)
Forecasted YoY real GDP growth	$\Delta^4 F\_RGDP$	Year-over-year quarterly log change of forecasted real GDP growth. It is a weighted average of current year's and next year's forecasted growth rates [(d)]	Consensus Forecast, IMF WEO
Other variables:			
YoY real GDP growth	$\Delta^4 RGDP$	Year-over-year log change of quarterly real GDP.	IMF WEO
Capital inflows	$CFLOW$	Gross other investment inflows within quarter t (as percent of GDP in t-1).	IMF FFA
Capital control	$FARI$	Financial account restriction index on gross other investment inflows, range 0-1 (higher values indicate more restrictive system).	IMF AREAER

Note:
(a) Initially take the first 24 quarters, then compute the trend and cyclical components recursively (add one quarter at a time).
(b) We use the financial corporations' domestic claims on private sector from IMF IFS where available, otherwise, we use depository corporations (or monetary) domestic claims on private sector from IMF IFS. The credit data of Iceland and Taiwan POC are from the central banks.
(c) The weighted average of variable X is calculated as $X_{WA} = 0.4 \times X + 0.3 \times l1.X + 0.2 \times l2.X + 0.1 \times l3.X$
(d) 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.

Table 2: Summary of Statistics

Variable	**Mean**	**Median**	**St. Dev**	**Min**	**P25**	**P75**	**Max**	**Obs**
Y	-1.227	0.330	11.785	-99.806	-2.970	3.572	63.642	4,112
$\Delta^4 RER$	-0.502	-0.675	9.198	-23.474	-6.246	4.844	25.934	4,152
MPS	4.172	3.301	5.159	-5.728	1.000	6.000	26.000	4,083
$\Delta^4 RGDP$	3.256	3.199	3.676	-8.781	1.319	5.504	12.296	4,212
$\Delta^4 F\_RGDP$	3.169	3.071	2.292	-3.517	1.776	4.613	9.225	4,216
$CFLOW$	5.514	1.502	25.824	-60.800	-1.702	6.632	169.592	4,120
$FARI$	0.181	0.167	0.166	0	0.083	0.250	0.750	3,904
$\Delta FARI$	-0.001	0.000	0.025	-0.333	-0.250	0.250	0.500	3,903
$iMaPP$	0.095	0.000	0.626	-4	0	0	6	4,216
$MaPP\_Br$	0.020	0.000	0.229	-2	0	0	3	4,216
$MaPP\_FI$	0.070	0.000	0.543	-4	0	0	5	4,216

Source: Authors' calculations.
Note: We winsorize the top and bottom 1% observations of each variable except the dependent variable Y and the categorical variable MaPP.

Table 3: Definition of Macroprudential Policy Instruments

	Instrument	Definition
1	CCB	A requirement for banks to maintain a countercyclical capital buffer. Implementations at 0% are not considered as a tightening in dummy-type indicators.
2	LVR	A limit on leverage of banks, calculated by dividing a measure of capital by the bank's non-risk-weighted exposures.
3	LLP	Loan loss provision requirements, which include dynamic provisioning and sectoral provisions (e.g. for impaired or housing loans).
4	Capital	Capital requirements for banks, which include risk weights, systemic risk buffers, and minimum capital requirements. Countercyclical capital buffers and capital conservation buffers are captured in their sheets respectively and thus not included here.
5	LTV	Limits to the loan-to-value ratios, mostly targeted at housing loans, but also includes those targeted at automobile loans.
6	DSTI	Limits to the debt-service-to-income ratio and the loan-to-income ratio, which restrict the size of debt services or debt relative to income.
7	LFC	Limits on foreign currency (FC) lending, and rules or recommendations on FC loans.
8	RR	Reserve requirements (on domestic or foreign currency) for macroprudential purposes.
9	Tax	Taxes and levies applied to specified transactions, assets, or liabilities, which include stamp duties, capital gain taxes, and levies on banks' noncore funding.
10	SIFI	Identification of and additional buffer requirements for global and domestic systemically important financial institutions.
11	Liquidity	Regulations for liquidity and funding risks, including minimum requirements for liquidity coverage ratios, liquid asset ratios, net stable funding ratios, and core funding ratios.
12	LTD	Limits to the loan-to-deposit (LTD) ratio and penalties for high LTD ratios.
13	LFX	Limits on net or gross open foreign exchange (FX) positions, limits on FX exposures and FX funding, and currency mismatch regulations.
14	LCG	Limits on growth or the volume of aggregate credit, the household-sector credit, or the corporate-sector credit by banks, and penalties for high credit growth.
15	Conservation	Requirements for banks' capital conservation buffers.
16	LoanR	Loan restrictions, that are more tailored than those captured in "14.LCG". They include loan limits and prohibitions, which may be targeted at loan characteristics (e.g., the maturity, the size, the LTV ratio and the type of interest rate of loans), bank characteristics (e.g., mortgage banks), and other factors. Restrictions on foreign currency lending are captured in "7.LFC". External debt restrictions for banks are captured in "13. LFX".
17	Other	Explicitly macroprudential regulation not included elsewhere, such as stress testing, restrictions on profit distribution and limits on foreign investment.

Source: Alam et al. (2019). We exclude SIFI from the original database to construct the aggregated macroprudential policy measure because this paper focuses on the time dimension of systemic risk.

Table 4: Baseline Result

	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: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5: "Purging" the Real Exchange Rate

	(1)	(2)
	FE	FE
$\Delta^4 \boldsymbol{Inflation_t}$	0.252***	0.260***
	(0.061)	(0.069)
$\Delta^4 \boldsymbol{F\_RGDP_t}$	-2.111***	-2.215***
	(0.166)	(0.182)
$\Delta^4 \boldsymbol{CA\_Deficit_t}$		-0.000
		(0.000)
Constant	5.259***	5.211***
	(0.612)	(0.650)
Observations	4,151	3,804
R-squared	0.041	0.040
# of Economies	62	60

Source: Authors' calculations.
Note: (a) Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
(b) FE regression (1): $\Delta^4\mathbf{RER}_{i,t} = \beta_1\Delta^4\mathbf{Inflation}_{i,t} + \beta_2\Delta^4\mathbf{RGDP}_{i,t} + \eta_i + e_{i,t}$
(c) FE regression (2): $\Delta^4\mathbf{RER}_{i,t} = \beta_1\Delta^4\mathbf{Inflation}_{i,t} + \beta_2\Delta^4\mathbf{RGDP}_{i,t} + \beta_3\Delta^4\mathbf{CA\_Deficit}_{i,t} + \eta_i + e_{i,t}$

Table 6: Robustness — Results with "Purged" Exchange Rate Shocks

	Baseline				$\Delta^4 RER$ = Residuals from FE Regression (1)				$\Delta^4 RER$ = Residuals from FE Regression (2)			
	iMaPP				iMaPP				iMaPP			
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$\Delta^4 RER_{t-1}$	-0.050**	-0.054***	-0.053**	-0.052***	-0.044**	-0.054***	-0.044**	-0.046**	-0.033*	-0.043**	-0.033*	-0.039**
	(0.021)	(0.018)	(0.022)	(0.019)	(0.022)	(0.021)	(0.022)	(0.020)	(0.020)	(0.019)	(0.019)	(0.018)
$MaPP_{t-1}$	-0.875*	-0.737			-0.866**	-1.351***			-0.940*	-1.461***		
	(0.463)	(0.461)			(0.414)	(0.512)			(0.481)	(0.424)		
$MaPP_{t-1} \times \Delta^4 RER_{t-1}$		**0.144*** **				**0.206** **				**0.188** **		
		(0.048)				(0.085)				(0.078)		
T_MaPP_{t-1}			-1.168**	-0.852*			-1.161**	-1.566***			-1.476***	-2.034***
			(0.524)	(0.503)			(0.471)	(0.592)			(0.515)	(0.618)
$T\_MaPP_{t-1} \times \Delta^4 RER_{t-1}$				**0.130** **				**0.168** **				**0.157** **
				(0.061)				(0.067)				(0.071)
L_MaPP_{t-1}			0.027	-0.630			0.009	-0.711			0.293	-0.570
			(0.754)	(0.709)			(0.832)	(0.927)			(0.898)	(0.786)
$L\_MaPP_{t-1} \times \Delta^4 RER_{t-1}$				**0.159*** **				0.257				**0.281* **
				(0.042)				(0.159)				(0.149)
Observations	3,842	3,842	3,842	3,842	3,841	3,841	3,841	3,841	3,544	3,544	3,544	3,544
# of Economies	62	62	62	62	62	62	62	62	60	60	60	60

Source: Authors' calculations.
Note: (a) Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
(b) FE regression (1): $\Delta^4 \mathbf{RER}_{i,t} = \beta_1 \Delta^4 \mathbf{Inflation}_{i,t} + \beta_2 \Delta^4 \mathbf{RGDP}_{i,t} + \eta_i + e_{i,t}$
(c) FE regression (2): $\Delta^4 \mathbf{RER}_{i,t} = \beta_1 \Delta^4 \mathbf{Inflation}_{i,t} + \beta_2 \Delta^4 \mathbf{RGDP}_{i,t} + \beta_3 \Delta^4 \mathbf{CA\_Deficit}_{i,t} + \eta_i + e_{i,t}$

Table 7: Results from Ordered Probit Regression

	iMaPP	MaPP_Br	MaPP_FI
VARIABLES	(1)	(2)	(3)
$\Delta^4 Y_{t-1}$	0.009**	0.006	0.011***
	(0.004)	(0.006)	(0.004)
$\Delta^4 RER_{t-1}$	0.001	0.001	0.001
	(0.001)	(0.002)	(0.001)
$\Delta^4 NCFLOW_{t-1}$	0.000	0.000	0.000
	(0.000)	(0.001)	(0.000)
$\sum_{s=-4}^{-1} MaPP_s$	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: (a) Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
(b) The constant cut points represent the threshods of predicted cumulative normal distribution of the dependent variable corresponding to its different categorical values.

Table 8: 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)
$MaPP\_Shock_{t-1}$	-1.088**	-1.240**			-1.988**	-1.891*			-1.157**	-1.343**		
	(0.490)	(0.529)			(0.884)	(0.986)			(0.562)	(0.576)		
$MaPP\_Shock_{t-1} \times \Delta^4 RER_{t-1}$		**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)
$T\_MaPP\_Shock_{t-1} \times \Delta^4 RER_{t-1}$				**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 9: 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
<u>The "Walls" Effect of Capital Controls + MaPP</u>													
$\boldsymbol{CFLOW_{t-1}}$	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*
$\boldsymbol{Y_{t-1}}$	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*
$\boldsymbol{FARI_{t-1}}$	-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*
$\boldsymbol{MaPP_{t-1}}$		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*
$\boldsymbol{MPS_{t-1}}$	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*
$\boldsymbol{Y_{t-1} \times FARI_{t-1}}$			-0.110**				-0.107*				-0.111**		
			(0.048)				(0.049)				(0.048)		
			-0.031				*-0.030*				*-0.031*		
$\boldsymbol{Y_{t-1} \times MaPP_{t-1}}$				0.021				0.104				-0.017	
				(0.034)				(0.082)				(0.019)	
				0.006				*0.029*				*-0.005*	
$\boldsymbol{Y_{t-1} \times MPS_{t-1}}$					-0.004				-0.004				-0.004
					(0.005)				(0.005)				(0.005)
					-0.013				*-0.013*				*-0.013*
$\boldsymbol{\Delta^4 F\_RGDP_{t-1}}$	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) Robust 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 10: 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.

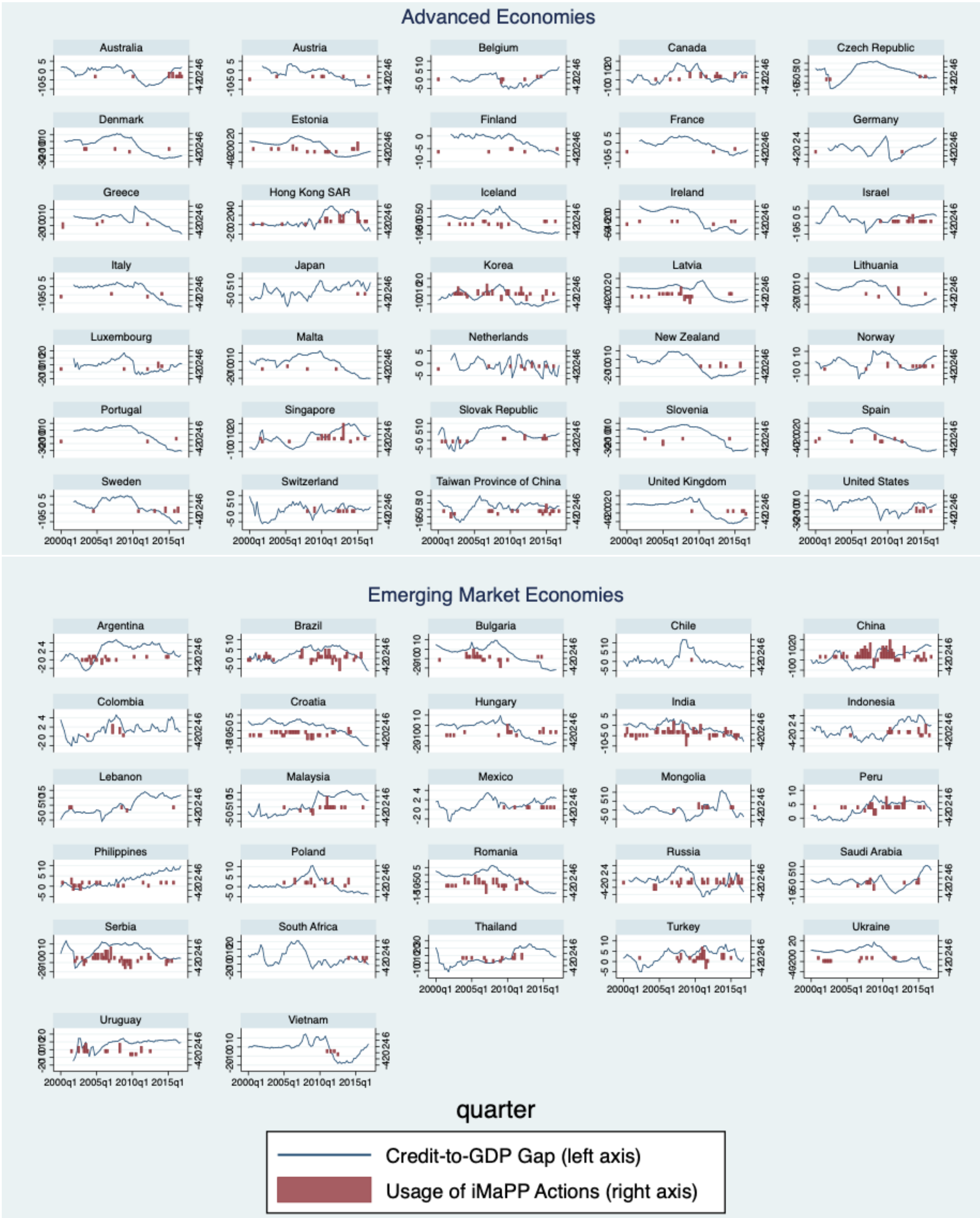
Note: (a) Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

(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
(2000Q1–2016Q4, by Economy)



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