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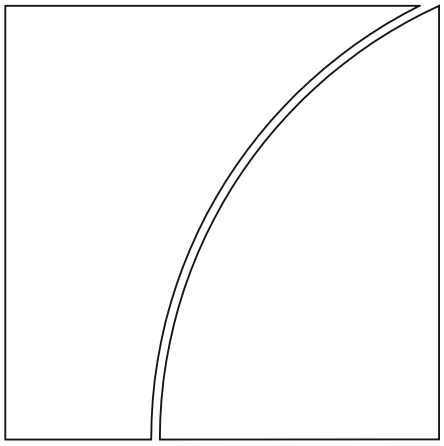

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Monetary and Economic Department

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Keywords: capital account openness, capital flows, capital controls, macroprudential policy, banking crises, currency crises

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The effectiveness of macroprudential policies and capital controls against volatile capital inflows

Jon Frost, Hiro Ito and René van Stralen[1]

Abstract

This paper compares the effectiveness of macroprudential policies (MaPs) and capital controls (CCs) in influencing the volume and composition of capital inflows, and the probability of banking and currency crises. We distinguish between foreign exchange (FX)-based MaPs, which may be similar to some types of CCs, and non-FX-based MaPs. Using a panel of 83 countries over the period 2000-17, and a propensity score matching model to control for selection bias, we find that capital inflow volumes are lower where FX-based MaPs have been activated. The imposition of CCs does not have a significant effect on the volume or composition of capital inflows. Further, we find that the activation of MaPs is associated with a lower probability of banking crises and surges in capital inflows in the following three years.

Key words: capital account openness, capital flows, capital controls, macroprudential policy, banking crises, currency crises

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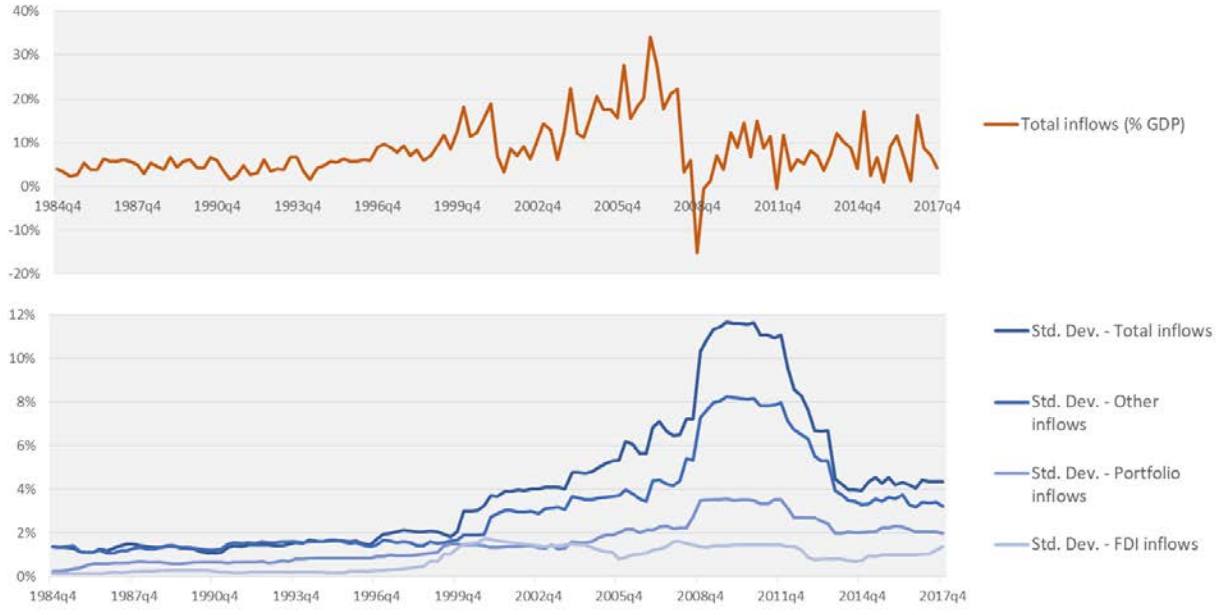
1. Introduction

Volatile capital inflows can pose serious challenges to economies. In the past few years, after a period of relatively strong capital inflows, countries like Turkey, Argentina, and South Africa were all buffeted by rapid outflows. Whereas foreign capital can be an important source of growth and investment for countries, it is notoriously "fickle." Just before the global financial crisis (GFC) of 2008-9, cross-border flows, as measured by the IMF's balance of payments (BOP) statistics, reach record heights, only to reverse sharply during the GFC (see Figure 1). Subsequently, supported by low interest rates, portfolio flows – especially from advanced economies (AEs) into emerging market economies (EMEs) – were quite strong over 2010-2012 and 2014-2015. These reversed sharply during the so-called "Taper Tantrum" of 2013 (Eichengreen and Gupta, 2015) and again in 2015 as commodity prices fell and risk sentiment once again flared (Eichengreen, Gupta and Masetti, 2017). In the first few months of 2020, as a result of the Covid-19 pandemic, capital flows to EMEs have reversed even more sharply, even for countries that had borrowed from abroad primarily in their domestic currency (Hofmann, Shim and Shin, 2020).

Figure 1: Global capital inflow volatility

Capital inflows measured in % of GDP; volatility as the standard deviation over a 5-year moving window



Source: IMF, authors' calculations

Following strong inflows, such capital flow reversals can lead to greater volatility of exchange rates and output growth. At the same time, there is a growing body of evidence linking strong capital inflows to credit bubbles (Mendoza and Terrones, 2012; Calderon and Kubota, 2014;

Frost and van Tilburg, 2014; Magud et al., 2014). When inflows suddenly stop, this can lead to sharp exchange rate and asset price adjustments, difficulty rolling over debt liabilities (denominated in foreign currencies) and a higher conditional probability of currency and banking crises (Calvo, 1998; Mendoza, 2006).

Forbes and Warnock (2012) provide a useful taxonomy of four types of capital flow episodes. *Surges* in inflows can inflate credit and asset price bubbles and make economies more vulnerable to currency and banking crises; *sudden stops*, *capital flight* and *retrenchment* are generally linked to the materialization of such crises.[2] Similarly, both banking and currency crises have been linked to surges in capital inflows. The IMF, national authorities and academia have written much on the optimal policy response to such flows (see, e.g. Edwards, 2007; Qureshi et al., 2011; IMF, 2012; Beirne and Friedrich, 2014; Bruno, Shim and Shin, 2017). Many countries, particularly EMEs, use capital controls to try to influence capital inflows and outflows. In addition to the volume of such flows, they may try to influence the composition, e.g. to encourage a smaller share banking flows or longer maturities of debt inflows (De Gregorio et al., 2000). Yet countries can also use macroprudential policies (MaPs), which target specific imbalances in the financial system. Qureshi et al. note that there is some overlap between MaPs and capital controls, and the IMF proposes the catch-all category "capital flow management" (CFM) measures to describe both measures. In particular, foreign exchange (FX)-based MaPs, such as limits on FX lending and FX-based reserve requirements, may be considered a category of CFM.[3] The relative effectiveness of these measures is the subject of debate, with far-reaching policy implications (IMF-FSB-BIS, 2016).

With a decade of available annual data since the GFC, we can start looking back at the effectiveness of MaPs and capital controls in a global cross-country panel. In this paper, we ask the following research question: are macroprudential policies or capital restrictions effective in reducing or changing the composition of gross capital inflows, the probability of capital inflow surges, and the probability of currency and banking crises? We formulate the null hypothesis that neither a new MaP, nor a change to a country's capital account openness (subsequently called a "policy action") have a significant negative impact on the volume or composition of capital

[2] Broadly, surges are an abrupt increase in gross inflows by non-residents, while sudden stops are a decline in such inflows. Meanwhile, capital flight is an abrupt increase in capital outflows by residents, and retrenchment is a similar decrease in such outflows. Recently, Cavallo, Izquierdo and León-Díaz (2019) look at the role of retrenchment in dampening the impact of sudden stops.

[3] For a rich body of further evidence and policy guidance, see Ghosh, Qureshi and Ostry (2018).

inflows, nor on surges, currency crises or banking crises. We expect that this null hypothesis can be rejected for foreign exchange (FX)-based MaPs, but perhaps not for other MaPs or for capital controls. Moreover, we would expect that there could be different effects in AEs and EMEs, and between countries based on the strength of their institutions.

One means of testing this impact would be a simple panel model of the volume and composition of inflows, or a logit model on the probability of surges, banking crises and currency crises. Yet because the choice of whether to institute MaPs or capital controls is likely correlated with capital flows and crises, there is potential for endogeneity. In particular, because countries will be using MaPs and CCs when inflows are high, there may be an attenuation bias, or underestimation of effects. Our empirical approach thus involves a two-stage estimation comparable to Glick, Guo and Hutchison (2006) and Forbes, Fratzscher and Straub (2015).[4] In the first stage, we regress the likelihood that a country will activate a MaP, or that it will open or close the capital account, against various macroeconomic and institutional factors. In the second stage, we use a propensity score matching (PSM) methodology to compare countries that took such a policy action to a similar country that did not (while both are facing the same sort of economic conditions). The difference between these groups can be interpreted as a "treatment effect" of the specific policy measure on the probability of surges and crises.

Our dataset comprises 83 AEs and EMEs for which consistent data on macroeconomic fundamentals, capital account openness and MaPs is available. MaP data are from Cerutti, Claessens and Laeven (CCL, 2017), as updated in 2018. In robustness checks, we use an alternative dataset on prudential policies (PPs) from Boar, Gambacorta, Lombardo and Pereira da Silva (BGLP, 2017). Data on capital controls are from Fernandez et al. (2015), as updated through 2017. Following Lane and Milesi-Ferretti (2017), we exclude financial centers, in which capital flows may be driven by non-fundamental factors, and we exclude extreme values of capital flows. We consider the time period 2000-17, on an annual basis.

Our results indicate the following. Capital inflow volumes tend to be lower in the year in which FX-based MaPs have been activated, and the following year. The composition of flows shifts, with a lower share of portfolio inflows, but no significant changes in the share of foreign direct investment (FDI). Other (primarily banking) inflows are lower in the year of activation.

[4] Cerutti and Zhou (2018) use a two-stage approach on bilateral banking claims between countries, using gravity factors, to estimate the impact of MaPs and CCs on cross-border and local affiliate lending. They find evidence of the circumvention of MaPs through direct cross-border lending by banks.

Changes in CCs do not have a significant effect on either the volume or composition of capital inflows. Further, we find that both FX-based and non-FX-based MaPs are associated with a lower probability of capital inflow surges and banking crises in the following three years.

The findings are broadly consistent with several other studies. For instance, the results on greater effectiveness of MaPs relative to CCs in influencing key macroeconomic outcomes are consistent with Forbes, Fratzscher and Straub (2015). The finding that FX-based MaPs can reduce the volume of capital inflows is consistent with a finding by Ahnert et al. (2018) – although they find that the decline in bank borrowing in FX is partially offset by higher FX debt issuance by firms. The finding that FX-based MaPs can leader to a lower probability of surges is consistent with one of the tests of Forbes and Warnock (2012) for the impact of FX-based measures on the probability of capital flow surges. This is also in line with Aizenman, Chinn and Ito (2020), who find that MaPs can help peripheral economies (re)gain monetary independence from center economies. The insight that annual changes in capital inflow controls do not have a significant impact on flows or their composition at annual frequency is consistent with Klein (2012).

The rest of this paper is paper is structured as follows. Section 2 describes our empirical approach and data sources. Section 3 contains the results from our first stage estimations on the probability of policy actions. Section 4 reports second stage estimations on the effects of capital controls and MaPs, as well as a number of robustness checks. The last section summarizes the main conclusions.

2. Empirical approach and data

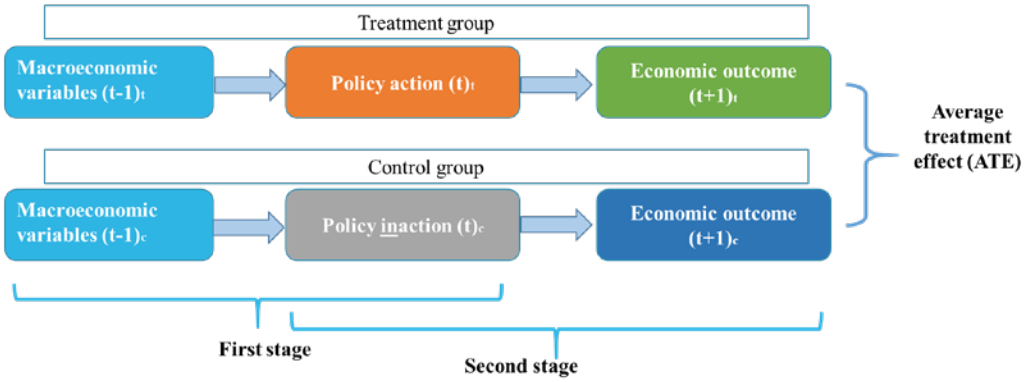
The objective of this paper is to study how policy interventions influence the volume and composition of gross capital inflows, capital flow surges, and the probability that a country experiences a banking or currency crisis. The main challenge is to disentangle such policy effects from the development of macroeconomic fundamentals, both domestic and external. Therefore, we estimate regressions for 5 different (sets of) dependent variables: (i) the volume of gross capital inflows to gross domestic product (GDP); (ii) the composition of such flows, measured by the share of FDI, portfolio and other investment inflows to GDP; (iii) the probability of a capital inflow surge in the following three years; (iv) the probability of a banking crisis in the following three years; and (v) the probability of a currency crisis in the following three years. These are estimated against a rich set of macroeconomic controls (growth, bank resilience, reserves, financial development, recent crises) and our policy variables.

A key challenge in estimating policy effectiveness is that there may be interaction between gross flows and the policy variables, such that direct estimates would be subject to endogeneity. Concretely, a country may activate a MaP such as restrictions on FX lending, or a capital control such as a tax on portfolio inflows, precisely because there are strong inflows. A simple panel or logit estimation with policy actions as independent variables may suffer from attenuation bias: it may find that these policies are ineffective, or even increase the probability of a capital flow surge, or a banking or currency crisis, merely because such policies were taken in a period in which the conditional probability of these events was already higher. To address this issue, we choose to use a propensity score matching (PSM) methodology.

2.1. The propensity score matching (PSM) approach

PSM was introduced in research on medicine (Rosenbaum and Rubin, 1983) and it has since become popular in macroeconomics and other fields as a means of addressing endogeneity. By matching a qualitatively similar treatment and control group, it simulates the effect of a randomized experiment in non-random, observed data. For studying capital controls across different countries, PSM has been applied e.g. by Glick, Guo and Hutchison (2006), and for capital controls and MaPs jointly by Forbes, Fratzscher and Straub (2015). In our study, the PSM methodology entails comparing countries that imposed CCs or MaPs (treatment group) to countries that had a similar probability or instituting these policies but did not do so (control group). Estimations of the probability that a country will take these policy measures in year *t*, based on macroeconomic controls in *t-1*, are the first stage in the estimation. In a second stage, the outcomes, in year *t* and subsequent years, in countries with similar predicted scores in the treatment and control group are compared with one another. This allows a comparison of the effectiveness of policies in preventing systemic risk, i.e. the probability of capital flows surges, banking and currency crises. Figure 2 gives a conceptual overview of the approach.

Figure 2: Overview of timing of first and second-stage estimation



Formally, the first stage can be expressed as:

$$p(x_{i,t}) \equiv Pr(\Omega_{i,t} = 1|x_{i,t-1}) = \Phi(\alpha_1 + \beta_1 x_{i,t-1} + \varepsilon_{i,t}) \quad (1)$$

where $p(.)$ is the propensity score, defined as the probability that the dummy variable $\Omega_{i,t}$ is equal to one. This dummy variable in turn denotes policy action in country *i* in year *t*. The probability of policy action is estimated based on $x_{i,t-1}$, a vector of macroeconomic control variables, lagged by one year. Finally, $\Phi(.)$ is the cumulative distribution function of the standard normal distribution; α_1 and β_1 are estimated coefficients; and $\varepsilon_{i,t}$ is an error term.

The second stage in a PSM estimation involves matching individual observations (i.e. country-year combinations) where a policy action was taken to the country-year observation with observations with similar estimated probabilities of policy action, but where policy action was not taken in year *t*. The mean difference in the outcome of interest between the two in subsequent years is called the average treatment effect (ATE). Observations are matched with nearest neighbor matching in the baseline, and other methods such as augmented inverse propensity weights (AIPW) in the robustness checks. In many applications of PSM, the outcome being measured is a continuous variable (such as a patient's blood pressure, or a country's GDP growth). In our study, the outcomes of interest are continuous variables for the volume and composition of flows in years *t* and *t+1*. In these cases, the ATE can be interpreted like a coefficient in a linear regression. In addition, we look at outcomes in terms of dummy variables indicating whether country *i* had a capital flow surge, banking or currency crisis in years *t+1*, *t+2* or *t+3*. In these cases, we are

comparing the occurrence of these events in different country-year observations for the treatment and control group. The ATE can be interpreted as a change in probability of these outcomes based on a linear estimation.

In order for the PSM estimation to yield unbiased estimates, it is important that the potential outcomes are independent of treatment conditional on vector $x_{i,t-1}$. In other words, there should not be (too) strong of a correlation between the explanatory variables for a policy action and the probability of a surge or a crisis. Moreover, the first stage should involve a strong enough goodness of fit that observations can be accurately matched with similar observations, but not so strong so as to perfectly divide the group into treated observations with high probability of treatment and non-treated observations with low probability. Of course, given the use of country-level observations and the substantial diversity between different countries over time, it is very difficult to control for all possible dimensions to make for qualitatively similar treatment and control groups. Nonetheless, by showing statistical regularities among outcomes after policy actions at the macro level, we will show that this approach can yield useful insights into aggregate policy effectiveness.

2.2. Selection of variables

In our first stage, we consider the probability that countries implement new FX-based or non-FX-based MaPs, or impose new CCs. Here, there is relatively little literature to draw from. The *extent* of CCs is estimated in a number of existing studies (e.g. Alesina, Grilli and Milesi-Ferretti, 1994; Grilli and Milesi-Ferretti, 1995; Brooks, 2004). These studies use various institutional and political variables such as the ideology of the government (right-wing vs. left-wing), central bank independence and the exchange rate regime, as well as macroeconomic variables like log GDP, the size of the banking sector, trade openness (generally defined as imports plus exports over GDP), the current account and inflation. However, we focus on the *activation* of such policies, and that may result from different factors. The activation of CCs or MaPs could result from yet other factors, which may relate more to financial sector vulnerabilities and financial system resilience. In the absence of strong guidance from the literature to select the variables, we consider the following candidates:[5]

[5] We have also considered GDP per capita, inflation, and right-wing vs. left-wing governments, but these have not yielded consistent effects.

- *Output growth*: As one baseline indicator of heterogeneity across different countries and years, we consider growth in GDP per capita. Countries experiencing high growth may not face the need to intervene financial markets with MaPs because the economy is robust. Conversely, faster growing countries may be more likely to activate MaPs to contain growing systemic risks. Hence, the predicted sign can be ambiguous. The change in GDP per capita may be an internationally comparable means of gauging recent output growth across countries with very different demographic trends (data source: Penn World Tables).[6]
- *Credit growth*: MaPs and CCs may be changed in response to movements in the financial cycle. As exemplified by Kaminsky and Reinhart (1999), a number of studies have shown that credit grows fast before periods of financial instability. We use the year-on-year change in the ratio of private domestic credit to GDP (data source: World Bank GFDD).[7]
- *Banking sector z-score*: Countries may be especially likely to impose MaPs or CCs when the banking sector faces a higher probability of default. The resilience of the banking sector can be captured by the distance to default (z-score), defined as the aggregate banking sector's return on assets (ROA) plus equity over assets, divided by the standard deviation of ROA (data source: World Bank GFDD). Lower z-score indicates that the country of concern has a higher probability of bank default. Conceptually, a higher z-score (i.e., lower probability of bank default) should indicate a lower probability of imposing MaPs or CCs.
- *Changes in reserves to GDP:* Many countries accumulate international reserves as a means of providing insurance against external shocks. Authorities may implement MaPs (in particular FX-based MaPs) or CCs in parallel to central bank purchases of reserves. In this case, it can be argued that reserve holdings play a complementary role to MaPs and CCs. Conversely, MaPs and reserve holdings could be substitutes. In that case, countries may actively implement MaPs instead of accumulating reserves, as an alternative buffer to external shocks.
- *Financial development*: More financially developed countries may be more likely to use MaPs actively because the (opportunity) cost of financial instability would be higher. We use the financial development index by Sahay et al. (2015), which is based on measures of depth, access and efficiency of financial institutions and markets.

[6] As an alternative business cycle measure, we have considered the output gap (IMF-IFS) and cumulative growth over time. The results are qualitatively similar, but result in fewer observations.
[7] As an alternative (available for fewer countries), we have considered the credit gap, or the deviation of the credit-to-GDP ratio from its long-term trend (data source: BIS). However, neither turned out to be significant.

- *Banking and currency crises:* A country may be more likely to restrict financial account transactions if it has a recent experience of a banking or currency crisis. It may also impose new MaPs to preemptively prevent financial sector vulnerabilities because of the past experience. Based on dummy variables for banking and currency crises in the past 5 years, we construct decay variables which give a diminishing effect of the crisis over time, falling by 20% in each subsequent each year (data source: Laeven and Valencia, 2018).[8]

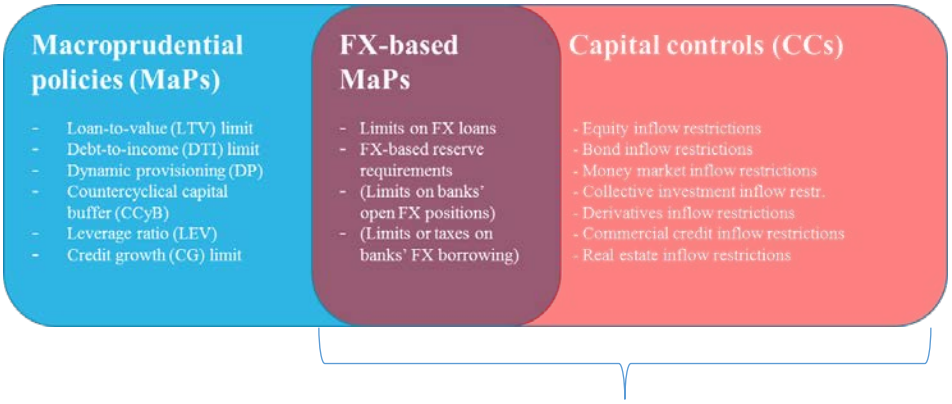
2.3. Data description

Our dataset consists of a cross-country panel of 83 countries (both AEs and EMEs) on an annual basis over the period 2000-17. The data on MaPs come from Cerutti, Claessens and Laeven (2017; CCL), as recently updated, who use an IMF survey to compile annual data for 160 countries on 12 categories of macroprudential measures over the period 2000-17. The data are coded such that the use of any particular measure in a given year is a "1." The overall macroprudential index counts the number of types of MaPs that a country uses in each year. When including controls and excluding financial centers (Lane and Milesi-Ferretti, (2017), and those for which other co-variates are unavailable, we have a sample of 83 countries. For robustness checks, data on broader prudential policies (PPs) are from Boar, Gambacorta, Lombardo and Pereira da Silva (BGLP, 2017), which is quite similar in structure and country coverage to Cerutti, Correa, Fiorentino and Segalla (CCFS, 2017). These datasets, which focus on policy measures like bank capital controls for 64 countries, is coded such that a tightening is "1," while a loosening is "-1."

MaPs can be classified into FX-based measures and non-FX-based measures. In particular, FX-based reserve requirements (from CCL and BGLP) and limits on FX lending (from CCL) are considered FX-based, while all other measures in CCL and BGLP are considered non-FX based. Ideally, we would also like to include limits on open FX positions (as used in Turkey and Brazil) or on FX funding (as in the Korean Macroprudential Stability Levy) in the measure of FX-based MaPs, but these are not available in CCL and BGLP. Figure 3 provides a visual classification of the different measures.

[8] This means the effect of the crisis depreciates by 20% over the course of five years. For a similar approach, see Aizenman, Cheung and Ito (2014).

Figure 3: Classification of FX-based MaPs and CCs



The use of CCs is measured with the index of Fernández et al. (2015). This index, based on public information sources including legislation and the IMF's *Annual Report on Exchange Arrangements and Exchange Restrictions (AREAER),* measures the *de jure* openness of a country's capital account to different categories of inflows and outflow. It is available for 100 countries over the period 1995-2016. The source of other variables and descriptive statistics are detailed in tables A1 and A2 in Annex A.

3. Empirical results from first-stage estimations

In this section, we first conduct the first stage estimations for the determinants of FX-based and non-FX-based MaPs (section 3.1) and for capital controls (section 3.2).

3.1. Determinants of macroprudential policy activation

We first implement the logit estimation to examine what factors contribute to the implementation of MaPs. The dependent variable is the activation of FX-based and non-FX-based MaPs. That means that the variable takes the value of one when the respective MaPs index *increases*. As described before, to mitigate endogeneity, all the explanatory variables are lagged by one year. The marginal effects of the explanatory variables are reported in Table 1. (Throughout the paper, results for FX-based measures are denoted with a grey background).

Countries that are experiencing high output growth are less likely to implement either FX-related or non-FX-related MaP. EMEs in particular are more likely to avoid enacting FX-based MaP when they are experiencing high output growth. The findings can also be interpreted as that

countries that experienced economic slowdown tend to impose more of FX- or non-FX-based MaP, as has happened in the aftermath of the GFC. The economic magnitude of this negative effect of output growth is greater for FX-based MaPs.

Policy makers are more willing to impose FX-based MaPs when *credit* growth last year was high. This tendency is stronger among AEs. Interestingly, however, higher credit growth does not lead to the imposition of non-FX-based MaPs. Instead, non-FX-based MaPs are more likely to be enacted for countries with more developed financial markets. FX-based MaPs do not respond to the level of financial development.

Countries with less resilient banking sectors (as measured by the z-score) could be expected to be more likely to take MaPs; we do indeed find a negative coefficient for FX-baesd MaPs, but it is not statistically significant. Thus, while distance to default of the banking sector is in principle a plausible driver of policy action, we do not find differences between economies when controlling for other variables.

Table 1: Imposition of new macroprudential measures

VARIABLES	(1) FX MaP Activation	(2) FX MaP AEs Only	(3) FX MaP EMEs Only	(4) non-FX MaP Activation
Output growth	-5.453***	-3.608	-5.540**	-2.475***
	(2.051)	(4.633)	(2.201)	(0.925)
Credit growth	7.864***	13.395***	5.254**	-0.035
	(1.952)	(3.869)	(2.374)	(1.584)
Z-score	-5.292	-2.579	-5.402	0.096
	(3.720)	(18.351)	(3.295)	(1.107)
Financial development	-0.619	0.242	1.228	1.649***
	(0.919)	(3.979)	(1.764)	(0.340)
Reserves flows to GDP	9.428**	1.581	8.244*	5.977**
	(4.363)	(25.599)	(4.590)	(2.436)
Banking crisis effect	-0.579	0.974	-1.195	-1.268**
	(1.291)	(2.380)	(1.572)	(0.539)
Currency crisis effect	-2.231		-2.229	-0.431
	(1.464)		(1.406)	(0.530)
Observations	1,346	353	985	1,348
Countries	83	24	59	83
r2	0.0686	0.184	0.0603	0.0324

The dependent variable is a dummy variable that takes the value of one when the relevant policy is strengthened. The original data come from Claessens, Cerutti and Laeven (2017). Standard errors in parentheses. *** denotes significance at the 99% confidence level; ** denotes the 95% level and * the 90% confidence level. The constant term is included in the estimation, but omitted from presentation to preserve space.[9]

[9] The table reports no estimate for the currency crisis effect for FX-based MaPs in AEs because there is no occurrence of currency crisis for this subsample of countries during the sample period.

Countries that are accumulating foreign exchange reserves tend to implement both FX-based and non-FX-based MaPs to a greater extent. This seems to imply that both types of measures are largely complementary to building up a war chest of buffers.

We do not find significant differences between countries' propensity to take FX-based in the 5 years after a banking or currency crisis (based on our lagged "decay" variable), but we do find countries that have experienced banking crisis in the last five years less likely to take non-FX-based MaPs. This finding suggests that countries that experienced banking crisis in the past tend to have more resilient banking sectors (due to government interventions and structural reforms); they may find it less necessary to impose (non-FX-based) MaPs.

3.2. Determinants of the impositions of capital controls

We next examine the determinants of the imposition of CCs on inflows. Table 2 presents the results. As was the case with the imposition of the FX-based MaPs, AEs with higher credit growth are more likely to impose CCs on capital inflows, potentially reflecting periods of high credit growth driven by foreign inflows. For the full sample, the probability of adopting a new capital control is higher if the country has had a currency crisis in the past 5 years. Apparently, the experience of a past currency crisis has made policy makers realize the potential cost of experiencing further financial instability that can be exacerbated by external factors.

The probability of new controls is not driven significantly by reserve flows to GDP, banking crises or financial development. While the z-score has a negative coefficient, this is again not statistically significant. This suggests that banking sector resilience is not a core driver of such policy actions.

Overall, as shown in Table 2, the results from the estimations on the activation of capital controls are generally weak. It should be recalled that we are not testing for the absolute level nor stringency of capital controls, but only for changes when capital controls are strengthened. Based on the theory of the trilemma – countries can achieve only two of the three open macro policy goals of monetary independence, exchange rate stability, and financial openness to the full extent – changing the stringency of capital controls may involve changes in the other two policies, which could make the estimation of the *level* of capital controls more structural (see e.g. Aizenmann, Chinn and Ito, 2016). However, we only attempt to examine the determinants of the strengthening of capital controls which only reflects partial picture of capital controls policy. That may explain the weakness of the estimations.

Table 2: Logit regressions for the changes in overall capital controls

VARIABLES	(1) New controls	(2) AEs Only	(3) EMEs Only
Output growth	1.392	0.251	1.516
	(0.993)	(1.734)	(1.117)
Credit growth	1.651	4.837**	0.154
	(1.658)	(1.909)	(2.023)
Z-score	-1.999	-4.172	-1.624
	(1.374)	(3.528)	(1.506)
Financial development	-0.628	-1.325	1.091
	(0.418)	(0.962)	(0.685)
Reserves flows to GDP	0.271	-4.046	-0.733
	(2.766)	(4.229)	(3.175)
Banking crisis effect	-0.095	-1.550	0.544
	(0.451)	(1.029)	(0.591)
Currency crisis effect	0.738*	1.154	0.638
	(0.423)	(0.997)	(0.498)
Observations	1,196	317	879
Countries	83	24	59
R-squared	0.0140	0.0536	0.0137

Notes: The dependent variable is a dummy variable that takes the value of one when the relevant policy is strengthened. The original data come from Fernandez et al. (2015). Standard errors in parentheses. *** denotes significance at the 99% confidence level; ** denotes the 95% level and * the 90% confidence level. The constant term is included in the estimation, but omitted from presentation to preserve space.

4. Empirical results from second-stage regressions

Now that we identified the factors that contribute to the probabilities of imposing FX-based and non-FX-based MaPs as well as capital controls on inflows, we move on to our second stage, in which we estimate the *impact* of these policy actions on a number of macroeconomic and financial conditions in subsequent years.

To do this, we compare treatment (policy action taken) and control groups (no action), based on PSM with nearest neighbor matching. To illustrate how this works, we are essentially comparing countries like Indonesia in 2013, which instituted an FX-based MaP, with Jamaica in 2013, which had a similar propensity score but did not take a measure. Similarly, in 2009, both Turkey and Serbia had similar probabilities of taking non-FX-based measures; Turkey did so while Serbia did not.

In what follows, we first examine the impact on the volume and composition of gross capital inflows (section 4.1), and then on the probability of capital flow surges, banking crises, and currency crises (section 4.2). Finally, section 4.3 contains a number of robustness checks.

4.1. Effect on the volume and composition of capital inflows

Table 3 reports the analysis of how an increase in the MaP measure impacts the volume of capital inflows in the year of policy action, and in the following year. We find that FX-based MaP tightening leads to a significantly lower volume of inflows in the year of activation (column 1). In economic terms, a newly added implementation of FX-based MaP lowers inflows by about 4.7% of GDP. The impact is economically significant considering mean inflows in our sample of 7% of GDP and a standard deviation of 10%. By contrast, there is no effect in the following year. Non-FX-based MaPs are associated with inflows that are 1.4% and 1.8% of GDP less in the year of activation and subsequent year (column 2), respectively. There is no significant effect on inflows of newly implemented capital controls (columns 2 or 3). This finding is consistent with the popular use of MaPs by some EMEs to deal with the risk of massive capital inflows causing financial instability.

Table 3: PSM estimations for impact of policy variables on the volume of capital inflows

VARIABLES	(1) FX-based MaP activation (CCL)		(2) Non-FX-based MaP activation (CCL)		(3) Capital inflow controls	
	Current year	Next year	Current year	Next year	Current year	Next year
Average treatment effect (ATE)	-0.047*	0.004	-0.014*	-0.018*	0.007	0.007
	(0.025)	(0.014)	(0.008)	(0.010)	(0.020)	(0.022)
Observations	1,346	1,342	1,348	1,344	1,196	1,196
P-value	0.064	0.801	0.090	0.058	0.716	0.766

Notes: Dependent variable taken from IMF IFS. Standard errors in parentheses. *** denotes significance at the 99% confidence level; ** denotes the 95% level and * the 90% confidence level.

This result is qualitatively similar with Forbes and Warnock (2012), who find that capital controls have generally been ineffective in countering surges, while FX measures are linked to a somewhat lower probability of inflow surges – at least in one estimation, though the authors point out that this is not robust to sensitivity tests in their case. Our results are also consistent with Klein and Shambaugh (2015), who find relatively little effectiveness of (small changes in) capital

account openness on overall monetary autonomy. It could be that especially even a one-year horizon, such new measures are likely to be circumvented and thus become less effective.

Next, we ask: do the policy actions of interest contribute to changing the composition of capital inflows? For FDI, we do not find any significant effects of FX-based policies (Table 4, panel (a), column 1), non-FX-based MaPs (columns 2), or of capital controls (columns 3). Yet for portfolio inflows, we do find a statistically significant impact (panel (b), column 1); portfolio inflows to GDP are 1.7% lower in the year of FX-based MaP activation, and 0.9% lower in the following year. Portfolio inflows are also lower, by 0.7%, in the year that non-FX-based MaPs are activated (panel (b), column 2). Finally, other inflows decline by 1.4% in the year of activation of FX-based MaPs (panel (c), column 1). None of the components of inflows respond significantly to new CCs. Overall, thus, activation of MaPs and CCs do not appear to shift inflows to longer-term investment, such as FDI, but especially FX-based MaPs do seem to reduce the portfolio share of inflows. CCs again appear ineffective.

Table 4: PSM estimations for impact of policy variables on the shares of different types of capital inflows

	(1) FX-based MaP activation (CCL)		(2) Non-FX-based MaP activation (CCL)		(3) Capital inflow controls	
	Current year	Next year	Current year	Next year	Current year	Current year
(a) FDI inflows						
Average treatment effect (ATE)	-0.015	0.008	-0.004	-0.002	0.001	-0.001
	(0.020)	(0.008)	(0.004)	(0.004)	(0.003)	(0.003)
Observations	1,338	1,335	1,340	1,337	1,188	1,189
P-value	0.438	0.292	0.336	0.585	0.792	0.847
(b) Portfolio inflows						
Average treatment effect (ATE)	-0.017***	-0.009***	-0.007**	-0.003	-0.001	0.011
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.011)
Observations	1,303	1,302	1,305	1,304	1,155	1,157
P-value	0.000	0.001	0.046	0.403	0.765	0.881
(c) Other flows						
Average treatment effect (ATE)	-0.014*	0.004	-0.007	-0.010	0.011	-0.000
	(0.008)	(0.008)	(0.006)	(0.007)	(0.019)	(0.021)
Observations	1,345	1,342	1,347	1,344	1,195	1,196
P-value	0.071	0.620	0.251	0.111	0.579	0.981

Notes: Dependent variable taken from IMF IFS. Standard errors in parentheses. *** denotes significance at the 99% confidence level; ** denotes the 95% level and * the 90% confidence level.

4.2. Effect on the probability of surges, banking crises and currency crises

The treatment effect of capital controls and MaPs can also be measured for capital flow surges, as defined by Forbes and Warnock (2012), and banking and currency crises.[10] Because these events – in particular banking and currency crises – occur relatively rarely, we consider the three-year horizon following the imposition of new measures. In other words, we consider the probability that a crisis occurred in a treatment country in the three years following policy action relative to the control group in the three years after non-action. Of course, given the limited number of observations, the results should still be interpreted with some caution.

Table 5: PSM estimations for impact of policy action on surges in the following 3 years

VARIABLES	(1) FX-based MaP activation (CCL)	(2) Non-FX-based MaP activation (CCL)	(5) Capital inflow controls
Average treatment effect (ATE)	-0.271***	-0.160***	0.071
	(0.033)	(0.051)	(0.075)
Observations	688	688	647
P-value	0.000	0.002	0.342

Notes: Dependent variable taken from Forbes and Warnock (2012), as updated. Standard errors in parentheses. *** denotes significance at the 99% confidence level; ** denotes the 95% level and * the 90% confidence level.

Table 5 shows the results for capital flow surges in the three years after policy activation, with columns (1) and (2) again covering FX-based and non-FX-based MaPs, and column (3) covering the imposition of CCs. Because the second-stage model is linear, the coefficient can be interpreted directly. Overall, we find a significantly lower probability of surges in the next three years (by about 27%, at the 99% confidence level) following activation of FX-based MaPs, and also (by about 16%, at 95% confidence) after non-FX-based MaPs. Not surprisingly, the mitigating effect of FX-based MaPs on surges is greater than that of non-FX-based MaPs. This example again underscores the importance of controlling for selection bias: without any control, the conditional probability of a surge in the next three years for countries that did not use FX-based MaPs is 35%, while that of countries that with FX-based MaPs was 40%. Because the latter are likely countries

[10] Forbes and Warnock (2012) define "surge" as gross inflows that are more than 2 standard deviations above the 5-year average. We convert their quarterly data to annual data, defining a "1" if a country has experienced a surge in any quarter during the year. The identification of banking or currency crisis is based on Laeven and Valencia's (2018) database on the occurrences of currency, banking and sovereign crises.

that faced higher risks of surges to start with, a naïve comparison would have meant that the effects of the policy are not visible. Meanwhile, we do not find any significant effect of new capital inflow controls.

Table 6 shows the results for the probability of banking crises. Here, we find that the activation of both FX-based and non-FX-based MaPs is associated with a lower probability of banking crises in the following three years (columns 1 and 2). The average FX-based measure is associated with a 2.4% lower probability of a banking crisis, at the 95% confidence level. The imposition of non-FX-based MaPs and capital inflow controls is not associated with any change in the probability of banking crisis in the following three years.

Table 6: PSM estimations for impact of policy action on banking crises over 3 years

VARIABLES	(1) FX-based MaP activation (CCL)	(2) Non-FX-based MaP activation (CCL)	(3) Capital inflow controls
Average treatment effect (ATE)	-0.024* (0.014)	-0.003 (0.023)	0.034 (0.022)
Observations	1,215	1,217	1,144
P-value	0.082	0.886	0.125

Notes: Dependent variable taken from Laeven and Valencia (2018). Standard errors in parentheses. *** denotes significance at the 99% confidence level; ** denotes the 95% level and * the 90% confidence level.

Finally, Table 7 considers the impact of measures on currency crises, as defined by a sudden depreciation of the local currency or depletion of international reserves (Laeven and Valencia, 2018). Here, we do not find any significant effects of MaP activation or the imposition of new capital inflow controls on the probability of crises. This latter result contrasts somewhat with Glick, Guo and Hutchison (2006) and Frost and Saiki (2014), who find that countries with liberalized capital accounts have a lower likelihood of currency crises.[11]

[11] The lack of significance may reflect that currency crises relate more to monetary vulnerabilities (e.g. high inflation coupled with a fixed or managed exchange rate) than to the financial vulnerabilities targeted by macroprudential policies. We have also tried a variable combining banking and currency crises, and do not find significant results.

Table 7: PSM estimations for impact of policy action on currency crises over 3 years

VARIABLES	(1) FX-based MaP activation (CCL)	(2) Non-FX-based MaP activation (CCL)	(3) Capital inflow controls
Average treatment effect (ATE)	-0.004 (0.018)	-0.020 (0.019)	0.010 (0.020)
Observations	1,215	1,217	1,144
P-value	0.821	0.303	0.606

Notes: Dependent variable taken from Laeven and Valencia (2018). Standard errors in parentheses. *** denotes significance at the 99% confidence level; ** denotes the 95% level and * the 90% confidence level.

Overall, there are at least two notable insights. First, while both domestic and external factors can contribute to external imbalances and to banking and currency crises, it appears that both FX-based MaPs (targeted at currency mismatches) and non-FX-based MaPs (targeted at domestic imbalances) can play a role in mitigating risks. Perhaps intuitively, FX-based MaPs have a stronger estimated coefficient for the probability of inflow surges than non-FX-based MaPs, but the impact on crisis probability is quantitatively similar. Second, the imposition of new capital controls does not seem to help reduce the risk of capital flow surges in the next years. This seems to signal that changes in capital controls, on average across countries, have not led to large changes in the outcomes that they often seek to target.

4.3. Robustness checks

In addition to the baseline results, we have performed a number of robustness checks. First, we have tried the alternative BGLP data set on broader prudential policies (PPs) instead of MaPs (see Annex B). We can also split these prudential policies into FX-based and non-FX-based measures. Tables 8 and 9 present results. Here, we find that FX-based PPs are associated with capital inflows that are 5.4% of GDP lower in the current year, and 2.9% lower in the following year. In the current year (i.e. year of activation), this is driven especially by changes in other (primarily banking) inflows. In the next year, the impact on portfolio flows seems to dominate.

Table 8: PSM estimations for impact of PPs on volume and composition of inflows

	Total capital inflows			
VARIABLES	(1) FX-based PP activation (BGLP)		(2) Non-FX-based PP activation (BGLP)	
	Current year	Next year	Current year	Next year
Average treatment effect (ATE)	-0.054***	-0.029***	0.006	0.000
	(0.013)	(0.010)	(0.015)	(0.021)
Observations	751	751	751	751
P-value	0.000	0.004	0.718	0.984
	FDI inflows			
VARIABLES	(1) FX-based PP activation (BGLP)		(2) Non-FX-based PP activation (BGLP)	
	Current year	Next year	Current year	Next year
Average treatment effect (ATE)	-0.016***	-0.004	-0.007	-0.008
	(0.003)	(0.007)	(0.007)	(0.008)
Observations	751	751	751	751
P-value	0.000	0.556	0.329	0.331
	Portfolio Investment inflows			
VARIABLES	(1) FX-based PP activation (BGLP)		(2) Non-FX-based PP activation (BGLP)	
	Current year	Next year	Current year	Next year
Average treatment effect (ATE)	-0.014**	-0.013***	0.001	0.007
	(0.006)	(0.004)	(0.008)	(0.008)
Observations	746	746	746	746
P-value	0.024	0.004	0.886	0.398
	Other inflows			
VARIABLES	(1) FX-based PP activation (BGLP)		(2) Non-FX-based PP activation (BGLP)	
	Current year	Next year	Current year	Next year
Average treatment effect (ATE)	-0.026***	-0.013	0.007	-0.002
	(0.007)	(0.008)	(0.008)	(0.012)
Observations	750	751	750	751
P-value	0.000	0.131	0.392	0.864

Notes: Dependent variable taken from IMF IFS. Standard errors in parentheses. *** denotes significance at the 99% confidence level; ** denotes the 95% level and * the 90% confidence level.

Here, we find a significantly lower probability of banking crises (Table 9), to the tune of 6.4% for FX-based PPs and 5.4% for non-FX-based PPs. The probability of currency crises is 2.7% lower after FX-based measures, and 4.9% higher in the three years after non-FX based PPs. We do not find statistically significant effects for the probability of surges.

Table 9: Impact of PPs on surges, banking and currency crises over 3 years

	Surges		Banking crises		Currency crises	
VARIABLES	(1) FX-based PP activation (BGLP)	(2) Non-FX-based PP activation (BGLP)	(3) FX-based PP activation (BGLP)	(4) Non-FX-based PP activation (BGLP)	(5) FX-based PP activation (BGLP)	(6) Non-FX-based PP activation (BGLP)
Average treatment effect (ATE)	-0.064 (0.175)	-0.026 (0.061)	-0.064*** (0.009)	-0.054*** (0.012)	-0.027*** (0.006)	0.049* (0.025)
Observations	608	608	734	734	734	734
P-value	0.714	0.665	0.000	0.000	0.000	0.054

Notes: Dependent variable taken from Laeven and Valencia (2018). Standard errors in parentheses. *** denotes significance at the 99% confidence level; ** denotes the 95% level and * the 90% confidence level.

Second, inspired by Wang and Wu (2019) and Aizenman, Chinn and Ito (2020), we have investigated whether results are different in periods when the Fed, as the issuer of the world's major reserve currency, was conducting expansionary monetary policy, versus other periods. Results (available upon request) show that FX-based and non-FX-based MaPs, but also new capital controls, each have a significant impact during periods of years of monetary expansion. In economic terms, the effects are 12.2% of GDP, 9.6%, and 5.7%, respectively. Measures are not effective during periods in which monetary policy is not expansionary.

Finally, we have tried several alternative approaches. This includes a different selection of variables in the first stage, which sometimes leads to a larger or smaller number of observations. We have also tried alternative PSM weighting methods, such as augmented inverse propensity weight (AIPW) matching. The results from these alternative PSM weighting methods are broadly consistent, but statistical significance is often weaker. We have not found any significant impact of policy measures on the real effective exchange rate in the following year. Finally, for countries with a fixed exchange rate (as defined by Ilzetzki, Reinhart and Rogoff, 2019), we have confirmed that both FX-based and non-FX-based MaPs reduce inflows, while capital controls do not.

5. Conclusion

Since the global financial crisis, authorities around the world have experimented with new and old policy approaches to deal with volatile capital flows and the risks of systemic crises. As the experience with these policies grows – and policy changes are recorded in a consistent and systematic manner across countries – it is becoming easier to compare policy effectiveness in a panel setting. However, due to selection bias, it is necessary to control for the probability that countries will take specific actions, and to estimate effects accordingly. Propensity score matching is one mean of doing this. While these estimations, and cross-country panels more generally, necessarily have their limitations, they may shed light on broad trends that are not visible for individual country studies or for more narrow samples.

In this paper, we have applied a PSM methodology to data for 83 countries over the period 2000-17 to estimate the relative effectiveness of FX-based and non-FX-based MaPs and CCs. Table 10 summarizes our results visually. We find that capital inflow volumes are lower after FX-based MaPs have been activated, by about 4.7% of GDP. Changes in CCs do not have a significant effect on the volume of capital inflows. FX-based MaPs are associated with a lower portfolio share of inflows. We also find that FX-based and non-FX-based MaPs are linked to a lower probability of capital flow surges, and FX-based MaPs are linked to a lower probability of banking crisis in the following three years. We do not find significant effects on the probability of currency crises.

Table 10: Overview of empirical findings

	FX-based MaPs		Non-FX-based MaPs		Capital controls	
	Current year	Next year	Current year	Next year	Current year	Next year
Volume of capital inflows	-		-	-		
Volume of FDI inflows						
Volume of portfolio inflows	-	-	-			
Volume of other inflows	-					
Capital flow surges	-		-			
Banking crisis	-					
Currency crisis						

Overall, our results indicate that macroprudential policies – especially those that target FX mismatches – may be more effective at responding to volatile capital inflows than capital controls that discriminate on the basis of residency. This complements results by Forbes and Warnock (2012) and Forbes, Fratzscher and Straub (2015), with alternative measures and the more recent period.

Further work could help to illuminate these relationships further. In particular, more disaggregated data for one or a few jurisdictions could help to understand how exactly FX-based MaPs influence the behavior of market participants and macrofinancial outcomes. Studies may look at higher-frequency data during the current Covid-19 shock to assess how countries that have used different policy tools have fared. These types of studies can be a valuable complement to the assessment of average trends at a global level.

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Annex A: Construction of the dataset

Data on macroprudential policies come from Cerutti, Claessens and Laeven (2017); and data on capital controls from Fernandez et al. (2015). Data on gross capital flows come from the IMF's BOP statistics, while macroeconomic variables come from the IMF's International Financial Statistics (IFS) and from the BIS. Log GDP per capita at purchasing power parity (PPP) is from the Penn World Tables (PWT). Other macroeconomic variables come, where possible, from World Bank sources, which generally have broad coverage and (fairly) few errors or extreme values. Nonetheless, we remove observations of extreme capital inflows or outflows (larger than 100% of GDP), and observations of credit growth larger than 50% or smaller than -50% (such as Argentina after the crisis, and Slovenia in 2004). Banking and currency crisis data are taken from Laeven and Valencia (2013).[12] When used as an independent variable, we follow Aizenman, Cheung and Ito (2014) and measure crises with a "decay" function, such that the effect of the crisis declines over the following 5 years.[13] The exchange rate stability index is from Aizenman, Chinn and Ito (2016), while the exchange rate regime is from Ilzetzki, Reinhart and Rogoff (2019). In addition, following Lane and Milesi-Ferretti (LMF, 2017), we exclude financial centers (FCs). As LMF point out, flows in FCs may be driven by decisions of corporates regarding firm structure, and by "round-tripping" of portfolio investment through investment funds. Yet compared to LMF, we use a "narrow" definition of FCs. Our definition follows LMF except for the UK and Belgium, which are borderline cases and which we do not classify as FCs.[14] Tables A1 and A2 give descriptive statistics of our main dataset and the data used in robustness checks.

[12] These data have been updated with author calculations.
[13] Specifically, the crisis variable is defined as "1' in the year in which the crisis occurs, "0.8" in the next year, "0.6" in the year after that, and so on.
[14] These two countries have an international investment position (IIP) to GDP ratio that is lower than the other financial centers defined by LMF.

Table A1: Descriptive statistics of the main variables

Variable	Observations	Mean	Std. Dev.	Min	Max
Non-FX Macroprudential index (CCL)	1,530	1.98	1.68	0.00	8.00
FX Macroprudential index (“”)	1,528	0.31	0.56	0.00	2.00
Output growth (1-year, in log per capita) (WEO)	1,672	0.01	0.10	-0.49	0.41
Credit growth (1-year, in %) (WB WDI)	1,524	0.01	0.06	-0.33	0.33
Financial development index (Sahay et al.)	1,530	0.40	0.23	0.05	0.95
Banking sector z-score (WB GFDD)	1,506	0.13	0.08	0.00	0.45
Reserves flows (in % of GDP) (IMF)	1,611	0.01	0.04	-0.30	0.26
Capital controls on inflows (Fernandez et al.)	1,445	0.37	0.32	0.00	1.00
Gross capital inflows (in % of GDP) (IMF)	1,601	0.07	0.10	-0.89	0.72
Direct investment inflows (in % of GDP) (IMF)	1,602	0.03	0.05	-0.41	0.55
Portfolio investment inflows (in % of GDP) (IMF)	1,553	0.02	0.05	-0.40	1.00
Other investment inflows (in % of GDP) (IMF)	1,609	0.02	0.15	-3.18	2.16
Banking crisis (Laeven and Valencia)	1,520	0.02	0.13	0.00	1.00
Currency crisis (Laeven and Valencia)	1,520	0.02	0.14	0.00	1.00
Banking crisis 5 years decay	1,700	0.06	0.19	0.00	1.00
Currency crisis 5 years decay	1,700	0.07	0.21	0.00	1.00
Capital flow surges (Forbes and Warnock)	855	0.21	0.41	0.00	1.00

Table A2: Descriptive statistics of additional variables

Variable	Observations	Mean	Std. Dev.	Min	Max
Non-FX Macroprudential index (BGLP)	900	0.22	0.92	-3.00	9.00
FX Macroprudential index (BGLP)	900	0.03	0.92	-5.00	10.00

Note: dataset includes observations for 1999-2018, to allow for estimations over the full sample period of 2000-17.

Annex B: Use of alternative dataset on prudential policies

As an alternative to the CCL data on MaPs, we have also considered a dataset on broader prudential policies (PPs), a groups that includes various institution-specific measures on bank capital requirements, etc. The data come from Boar, Gambacorta, Lombardo and Pereira da Silva (BGLP, 2017). Results from the first stage are presented in Table B1. Similar to the estimations for MaPs (CCL data), we find that FX-based PPs are less frequent when the output growth is lower. Again, we find a positive link with accumulation of FX reserves, implying that FX-based PPs and reserve accumulation are largely complements. Across the sample, there is relatively little impact from recent banking or currency crises.

Table B1: Logit regressions for the activation of prudential policies (BGLP)

VARIABLES	(1) FX MaP Activation	(2) FX MaP AEs Only	(3) FX MaP EMEs Only	(4) non-FX MaP Activation
Output growth	-4.797***	-9.891***	-2.513	-0.543
	(1.601)	(3.077)	(1.935)	(1.150)
Credit growth	-1.233	-7.580**	2.138	1.362
	(1.924)	(3.093)	(2.497)	(1.752)
Z-score	-2.140	-2.490	-1.610	-3.580**
	(3.144)	(5.311)	(3.827)	(1.492)
Financial development	-0.493	-0.281	1.047	0.041
	(0.874)	(1.322)	(1.446)	(0.535)
Reserves to GDP	10.951***	19.764*	9.255***	3.393
	(2.989)	(10.248)	(3.423)	(2.960)
Banking crisis effect	0.125	-0.337	1.011	0.117
	(0.832)	(0.378)	(1.216)	(0.390)
Currency crisis effect	-1.967		-0.995	-0.824
	(1.416)		(1.256)	(1.097)
Observations	751	331	412	751
Countries	49	24	25	49
r2	0.0597	0.165	0.0432	0.0125

Dependent variable taken from Cerutti, Correa, Fiorentino and Segalla (2017). Standard errors in parentheses. *** denotes significance at the 99% confidence level; ** denotes the 95% level and * the 90% confidence level.

In the second step, we look at the impact of FX-based and non-FX based PPs on the variables of interest. These are reported in Tables 8 and 9 of the main text.

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