Comments on: Effectiveness of macroprudential and capital flow measures in Asia and the Pacific

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This paper by Valentina Bruno, Ilhyock Shim and Hyun Song Shin asks some important questions: what are the effects of capital flow management (CFM) policies? How do bond and bank CFMs affect the composition of capital flows? And what are the effects of domestic macroprudential policies on credit? These questions are important because a growing number of countries are deploying capital controls and macroprudential policies, and, if these policies are to be used wisely, then we need to understand how they impact our economies.

The authors conclude that bank and bond inflow CFMs do affect inflows, and they provide evidence that suggests that sectoral-type policies affect the composition of capital flows: after 2009 they find that controls on bond flows stimulate bank capital flows, and prior to 2007, they find that controls on bank flows affected bond flows. Perhaps somewhat surprisingly, the authors also find that tighter domestic macroprudential measures, in the single regression in which they are statistically significant (Table 3, column 1), increase the growth in external claims on domestic banks.

My remarks on this paper reflect a particular audience niche. As a central bank policymaker, I want to use research to provide quantitative guidance for decision-making. In the rest of this discussion I focus on two areas relevant for policymakers: the treatment effects of policy, with some discussion of the econometric measurement of these effects and the design of policy; and the metric used to assess or guide policy.

Treatments and doses

The essence of panel data analysis of the type used in this paper is to use cross-sectional and time variation to understand the effect of a "treatment" on dependent variables. These methods can be used to understand the effects of implemented treatments, but not hypothetical treatments. In the current context, we can think of CFM or macroprudential policies as the treatments of interest, and we are interested in how these treatments impact the macroeconomy (in particular capital flows and credit growth). Of course, the cross-sectional focus also requires us to account for other characteristics that may vary across time and across countries, which in turn may confound our assessment of the treatments.

Experimental data represent the ideal for assessing treatment effects, since a treatment is then randomised across treated and untreated groups, ensuring that the treatment is uncorrelated with any other factor that might influence the outcome. Yet with macroeconomic policies it is never possible to live up to this experimental ideal. As is well understood, macro policies are not randomly distributed on economies. Rather policies are implemented as policymakers respond to the circumstances that they face, including both the political and policy

frameworks within which they operate. Forbes et al (2015) note that countries that adjust their CFMs tend to have different characteristics than other countries. Macroeconomic and econometric policy analysis, then, needs to disentangle the effects of policy from the effects of these other macroeconomic drivers. As Bruno, Shim and Shin note at the beginning of their empirical analysis, they conduct panel regressions without country fixed effects, which thus assumes away differences between countries, and assumes the countries are all representative and drawn from the same data-generating process. While the authors are commendably upfront about this assumption, it does seem of questionable validity.

To assess the effect of capital controls, a counterfactual case needs to be developed indicating what would have occurred in the policy's absence. Yet if the treatment (the capital control or macroprudential policy) is not applied randomly to countries, then the effect of the treatment may be misestimated. Forbes et al (2015) use a propensity score matching approach to identify an untreated-group that forms the basis of the counterfactual, thus enabling one to correct for selection bias, to more accurately assess the impact of the treatment. Forbes et al's general conclusion is that most CFMs do not significantly affect target variables. It would be interesting to know whether the conclusions that Bruno, Shim and Shin reach about the effects of CFMs are robust to this alternative methodology.

From a policymaker's perspective, one of the frustrating aspects of the paper is that it does not precisely specify the type and quantity of the policy treatment. The analysis specifies a dummy variable which: (i) takes a value of +1 when a CFM or macroprudential policy is introduced or tightened; (ii) takes a value −1 when a policy is relaxed; and (ii) is otherwise 0. The treatment regressor is then the cumulation of these actions within a quarter. However, it is not clear that the CFM and macroprudential policies put into practice in different countries are really the same, and the analysis does not allow us to discriminate between policy variants.[1] In medicine, the type and dosage of drugs matter for outcomes – in conjunction with the underlying physiology of the patient – and the effects of CFM policy treatments are undoubtedly driven by the same general considerations.

CFM and macroprudential policies also have multidimensional properties or attributes, and these attributes may influence the efficacy of the policies that are implemented. For example, do tax-based restrictions have the same implications as quantity restrictions? Are the effects of, say, a tax on capital flows linear in the size of the tax, or do larger taxes create larger incentives for avoidance? Do caveats within a given policy matter for outcomes? Questions of policy design are not very well addressed by this kind of cross-country analysis unless the policies are specified in much finer detail. This criticism is by no means unique, and has been directed at previous papers in this literature (see Jinjarak et al (2013) and Straetmans et al (2013) for example).

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[1]   This is one of the "apples-to-oranges" problems discussed by Magud et al (2011).

Normative evaluations of CFMs

The paper by Bruno, Shim and Shin is empirical in nature, trying to establish the effect of CFM and macroprudential policies. It is worth straying into normative questions about these policies since one might be tempted to assume that, if CFM policies can affect capital flows or the composition of flows, then it will be desirable to implement them.[2]

The theoretical case for and against capital controls is not yet settled. As Straetmans et al (2013) eloquently articulate, the theory of the second best means that distortions such as capital controls may be welfare-enhancing if they mitigate welfare losses that arise from pre-existing distortions, such as incomplete markets, imperfect competition, asymmetric information or price stickiness. A variety of theoretical papers, such as Costinot et al (2014), Farhi and Werning (2012), Korinek (2011) and Jeanne and Korinek (2010), examine these issues and illustrate why controls may be beneficial.

Of course, the relative importance of the various theoretical mechanisms, the benefits of controls, and indeed the costs of deploying capital controls need to be evaluated empirically. It is by no means clear that an ability to affect capital flows for relatively short horizons of between one and four quarters is useful for thinking about whether such policies should be deployed.

In evaluating policies we need to consider not only the immediate circumstances, but the circumstances that may prevail in future, and thus any transition in the policies implemented. For example, Chile, a poster-child for capital controls, ultimately decided that the implementation costs of their capital control regime outweighed the macroeconomic benefits (Jeanne (2012); Gallego et al (2000)). Thus we need to consider not only the introduction of such policies but the possibility that they will later be removed. The Swiss National Bank's ceiling on the Swiss franc, and its subsequent removal, provide a more recent example.

We also need to consider the system-wide properties of the controls. Forbes (2007) finds that Chilean capital controls increased financial constraints for small traded-goods firms. These presumably undesirable consequences need to be weighed against the benefits of reduced capital flows. Likewise, the empirical results of Bruno, Shin and Shim suggest that, prior to 2007, controls on bank flows affected bond capital flows, but it is possible that controls on bank flows make banks safer at the cost of increased vulnerability in bond markets. Furthermore, the fact that the estimated effects of capital controls pre- and post-2007 and 2009 have different consequences suggests that we need to develop a deeper structural understanding of financial markets if we are to understand the consequences of CFM policies.

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[2] This discussion is a variant of the third "apples-to-oranges" problem identified by Magud et al (2011): how do we measure success?

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