Are Capital Inflows Expansionary or Contractionary? Theory, Policy Implications and Some Evidence,” By Olivier Blanchard, Jonathan D. Ostry, Atish R. Ghosh and Marcos Chamon

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Late-substitution, so I may not be able to do the paper justice.

1. Motivation “Policy Implications” (I think related to the origins of “currency war” complaints from Ems)
2. Model “Theory” (Portfolio Balance model)
3. Empirical “Some Evidence” (a bit related to a paper by Olivier (and other co-authors) that I commented on in our meeting last year).

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|  | Olivier J. Blanchard, and Irineu E. de Carvalho Filho, Gustavo Adler, [*Can Foreign Exchange Intervention Stem Exchange Rate Pressures from Global Capital Flow Shocks?*](http://www.nber.org/confer/2015/SI2015/IFM/Adler.pdf) |

The three aspects correspond to the three phrases in the subtitle: Theory, Policy Implications and Some Evidence,”

Start with the motivation. The basic question is an excellent one: whether or when capital inflows are expansionary or contractionary. But I am stopped by how they go on to characterize the two possibilities.

[abstract “The workhorse open-economy macro model suggests that capital inflows are contractionary because they appreciate the currency and reduce net exports. Emerging market policy makers however believe that inflows lead to credit booms and rising output, and the evidence appears to go their way. To reconcile theory and reality, we extend the set of assets included in the Mundell-Fleming model to include both bonds and non-bonds.”]

First few sentences:

“Are capital inflows expansionary or contractionary? [T]here is a striking schizophrenia: Standard models, along Mundell Fleming lines or more modern incarnations, give one answer: For a given monetary policy rate, inflows lead to an appreciation, and thus a contraction in net exports and, in turn, a contraction in output… Emerging market policy makers however have a completely different view. They see capital flows as leading to credit booms and an increase in output…”

I would like to hear which EM policy-makers they are talking about (preferably with citations). The last big surge of capital inflows came 3-6 years ago. Many EM policy-makers, particularly in Brazil, at that time had a complaint that sounds very different [complained that the capital inflows were making their life complicated, attributing them to easy US monetary policy and coining the phrase “currency wars.”]

Brazilian Finance Minister Guido Mantega in September 2010 coined the phrase “currency wars” as a complaint about depreciation of the dollar and other currencies against the real:
“We’re in the midst of an international currency war, a general weakening of currency. This threatens us because it takes away our competitiveness” (9/27/2010).

Brazilian President Dilma Rousseff In April 2012, continued the currency war accusation, criticizing QE as a “monetary tsunami” that had detrimental effects on others via the exchange rate.

This sounds like the position that OB et al attribute not to EM policy-makers, but to the Mundell-Fleming model. Meanwhile, the Mundell-Fleming model is perfectly capable of giving the opposite effect, that capital inflows lead to an expansion of output in the receiving country. So I feel a need to try to elucidate before we get past the first three paragraphs.

Let’s start with the obvious point that capital inflows are endogenous variable. It would help to be a little more explicit about what is the exogenous cause. If the exogenous cause were a boom in the EMs, say a manufacturing boom in Asia or a commodity boom in other EMs [empirically: procyclical capital flows;; theory: collateral models], then of course it would be wrong to talk about causality running from capital flows to output rather than the other direction. So let’s be more explicit than the authors are that the exogenous cause is a reduction in US interest rates (though it could also be something else like a shift to “risk on” mode as measured by a fall in the VIX). All of the above happened in 2007-08 and again in 2010.

Now let’s do Mundell-Fleming, if you don’t mind. In my classes I still use it as the jumping-off point, spending the first two weeks on it.

TB
KA
IS
LM or Taylor rule+div.coin.

Many would argue that the LM curve is obsolete. But a partial answer to that is that you could update the Mundell-Fleming model by replacing the LM curve with another upward sloping relationship: the Taylor rule (when combined with the “divine coincidence” that stabilizing GDP and inflation are the same thing, from a seminal 2005 paper of Olivier’s [with Jordi Gali].)

Consider, first, the simplest case: because of perfect capital mobility, k approaches infinity and the domestic interest rate is tied to the world interest rate: i=i\*.

There are two stark polar cases: if the currency floats, then it must appreciate, leading to a trade deficit, leftward shift of IS, and a fall in output. This is the case that BOGC attribute to Mundell-Fleming. But there is the opposite polar case of a fixed exchange rate. Keep in mind that during the first 30 years of the Mundell-Fleming model, virtually no developing countries were floating; and even today, no EM countries are floating cleanly. What is the effect of a capital inflow if the exchange rate is fixed? A monetary inflow shifts the LM curve rightward, producing a rise in output. This seems to be the case that BOGC seem to say is inconsistent with the basic Mundell-Fleming model.

There is a third choice: half-appreciation and half-inflows. Then the IS and LM curves meet in the middle. If the EM policy-makers calibrate it right, then there need be neither a rise nor a fall in output. This is in fact roughly what Brazil and most other major EM countries did five years ago. It is one reason why my sympathy for some of the EM critics was limited.

I think the best characterization of the Brazilians is that they were unhappy not because capital inflows made their output too high or too low, but because it hurt their trade balance, a secondary objective. Incidentally, they should have been following a tighter fiscal policy at that time, which would have allowed the achievement of both of those objectives and would have left then in a far better situation today, after the boom turned to bust. [That’s another reason why I have limited sympathy.]

Now consider the more realistic case where k is finite, due to some combination of transactions costs, capital controls, default risk, or currency risk. BP=0 has some slope. The same three choices exist. But now there is also a fourth choice: sterilized intervention, which can keep the economy at point A, at least for awhile.

So if we take as given that the motivation of the paper is whether capital inflows are expansionary or contractionary, then it seems to me that the focus should be seeing to what extent countries can and do use the tools of foreign exchange intervention, sterilization, and capital controls.

We have certainly learned of things over the last few decades that take us well beyond the basic Mundell-Fleming model. At the top of my list, in the EM context, would be the possibility that currency depreciation can be contractionary rather than expansionary, via non-TB channels…

I haven‘t left much time to discuss the authors’ portfolio balance model and their empirical work.

The key attribute of the PB model is to distinguish between bonds and non-bond assets, which includes FDI, equities, and bank credit, all of which have risk characteristics that make them imperfect substitutes for bonds.

Empirical: 19 countries, 2000- . Testing the effects bond vs. non-bond flows and the policies used to react to them, such as foreign exchange intervention, which in my view is where the action should be (perhaps along with macroprudential policies). They conclude that their results support their theory “Exogenous bond flows appear to have on average small negative effects on output, while exogenous non-bond flows appear to have a positive effect.”

I began my comments by mentioning the likelihood that one reason for the capital inflows of 2007-08 and 2010 was a response to rapid growth in EMs. We could call it the global China+commodities boom. From an econometric viewpoint, such factors would render the capital inflows endogenous. The authors realize the need to deal with an endogeneity problem: their argument “applies to exogenous flows, i.e., not to flows triggered by developments in the country itself.” Their solution is “when looking at each country, we use global flows to all emerging market countries as instruments, on the assumption that these are unlikely to be correlated with developments in any particular emerging market country.” I don’t accept that the aggregate capital flows to EMs can be viewed as exogenous from the viewpoint of an individual EM, in the key sense that it would have to be uncorrelated with the error term in the regression. It is not.

I am much more sympathetic with the part where they go on to consider the effect of policy responses like fx intervention and they use the US treasury bill rate and the VIX as instrumental variables for the capital inflow variable. I think these instruments do get at the endogeneity problem on a time series dimension. But they don’t help on a cross-section dimension. One wants to know whether the countries that foreigners found particularly attractive in these booms but tried to dampen the currency appreciation and its effects via foreign exchange intervention were able to do so successfully (sterilized fx intervention in particular). I can think of ways to ways to address the endogeneity problem on this dimension. But now I am in danger of fully repeating my comments from one year ago on another interesting paper co-authored by Olivier. (Olivier Blanchard, Irineu de Carvalho Filho, Gustavo Adler, “[*Can Foreign Exchange Intervention Stem Exchange Rate Pressures from Global Capital Flow Shocks?*](http://www.nber.org/confer/2015/SI2015/IFM/Adler.pdf)*”*) So I will stop.