Discussion of "Global Imbalances and Taxing Capital Flows"

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1. Introduction

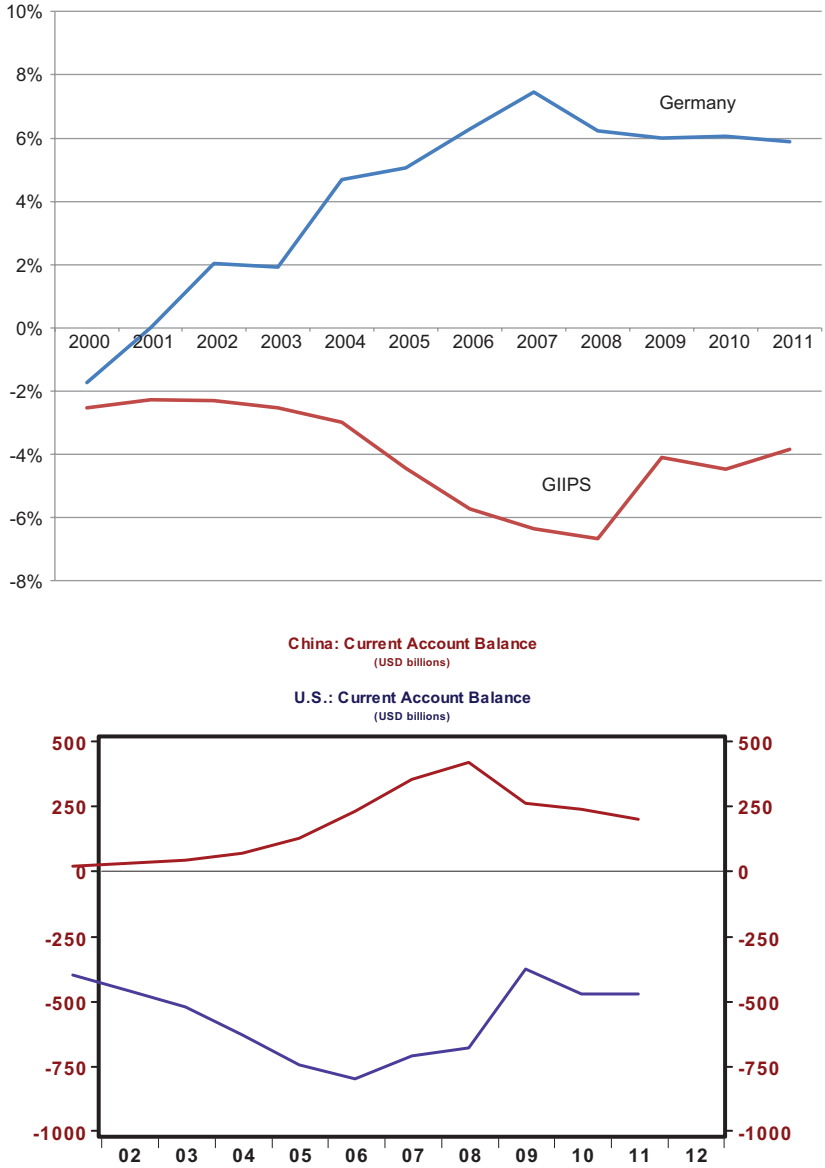
In the theoretical model of Goodhart, Peiris, and Tsomocos (this issue) (henceforth GPT), some countries have large current account (CA) surpluses. The accompanying financial outflows from CA-surplus countries can lead to an unsustainable accumulation of debt in CA-deficit countries. The somewhat inevitable debt restructuring is costly, and both surplus and deficit countries would be better off had capital controls (i.e., restrictions on financial account transactions) been in place.

I will focus on practical aspects of the paper. I will organize my comments around three main questions: Is this an important topic? Is the paper's focus on debt flows reasonable? Are capital controls the appropriate tool?

2. Is This an Important Topic?

The answer to this is an unequivocal yes. CA imbalances, though they have narrowed somewhat recently, are quite large (see figure 1). Within the euro zone, in 2011 Germany had a current account surplus amounting to 6 percent of GDP, while the GIIPS countries (Greece, Ireland, Italy, Portugal, and Spain) averaged a 4 percent deficit. At the same time, two of the largest countries in the world (China and the United States) had a surplus and deficit of similar (large) magnitudes. And, historically, capital mobility—at the heart of the GPT model—is associated with crises: Reinhart and Rogoff (2009) show an increased incidence of banking crises during periods of high capital mobility. So the feature GPT insert into their

Figure 1. Current Account Imbalances



Source: Haver Analytics.
Notes: The top panel is shown as a percent of GDP, with "GIIPS" being a GDP-weighted average of current account balances in Greece, Ireland, Italy, Portugal, and Spain. The bottom panel is shown in USD millions.

Table 1. Forbes and Warnock (2013) Debt- and Equity-Led Capital Flow Episodes

	Surge	**Stop**	**Flight**	**Retrenchment**
	% of Episodes that Are Debt Led			
	82%	**80%**	**71%**	**72%**
High Income	81	83	79	75
Medium Income	81	83	63	76
Lower Income	84	68	64	56
North America	67	69	74	72
Western Europe	89	87	81	77
Asia	80	79	67	68
Eastern Europe	88	71	64	82
Latin America	81	85	74	67
Other	33	54	42	29

model—large current account imbalances—and the resulting capital mobility leading to crises are both prominent in the real world. The underlying phenomena the authors are tackling are relevant, evident, and can be associated with substantial pain. This is an important topic.

3. Is the Focus on Debt Flows Warranted?

The flows that cause damage in the GPT model are debt flows. There is no equity in the model, and thus no equity flows, but in reality equity and foreign direct investment (FDI) flows both exist and can be important. Does this focus on debt flows limit the model's relevance? No, because extreme capital flow episodes are rarely driven by equity flows. As table 1 (reproduced from Forbes and Warnock 2013) shows, most extreme capital flow episodes are "debt led": changes in debt flows exceed changes in FDI and equity flows. In 80 percent of all surges and stops, debt flows dominate equity flows. The paper's focus on debt flows, while it ignores equity flows, is consistent with what is observed during extreme capital flow episodes.

4. Are Capital Controls Appropriate?

Perhaps the most controversial aspect of the paper is its implicit assumption that capital controls are the appropriate tools to address the large flows seen in the model. But any policy response should take into consideration the source of the problem; in this paper the source of the imbalances is not modeled. A takeaway from the paper is that in a world in which current account imbalances and their associated capital flows can lead to defaults, capital controls can improve everyone's welfare. Perhaps. But the appropriate policy response should take into consideration the source of the problem.

There are many strong arguments in favor of capital controls.[1] I am persuaded by Dani Rodrik's eloquent argument that essentially says that in a second-best world, first-best solutions are not necessarily appropriate. This line of reasoning seems very compelling. I'll note, parenthetically for now, that in Rodrik's argument real exchange rate appreciation (not default, as in the GPT model) is what causes the pain of large capital inflows. Anton Korinek also has a nice series of papers on prudential capital controls. The basic idea is that capital flows might be optimal for each individual, but negative externalities are not internalized, so there is room for a Pigouvian tax. The pain in Korinek papers tends to come from balance sheet effects.

While the arguments by Rodrik and Korinek are convincing, the case for capital controls is far from self-evident.[2] We can take a minor point first. The negative aspect of capital flows in Rodrik's work tends to come through real exchange rate appreciation. And we hear this time and time again: A primary reason emerging economies want to implement capital controls is to limit exchange rate appreciation. But De Gregorio, Edwards, and Valdés (2000) suggest that the capital controls Chile implemented in the 1990s—often held up as a success—had no persistent effect on the real value of the peso. The controls did alter the reported composition of capital inflows, away from the type of flows that were taxed, toward other type of flows, but had no impact on the overall amount of inflows into the

[1] See, for example, Rodrik (2007), Rodrik and Subramanian (2009), and Korinek (2011).

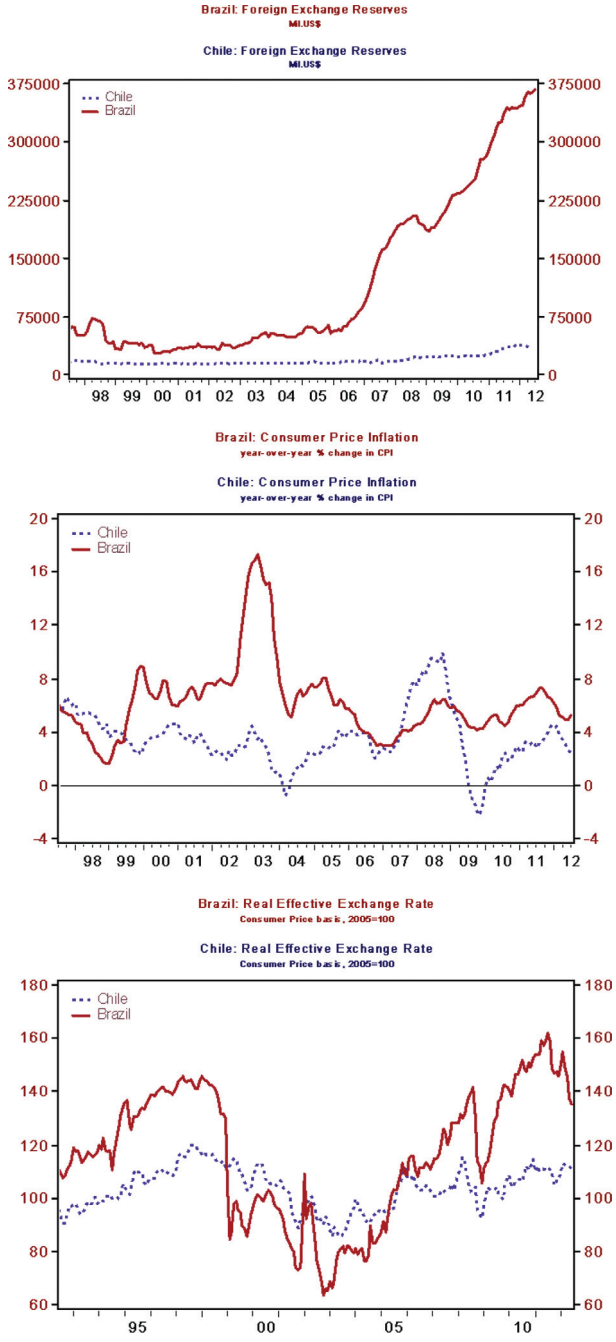
[2] Some of the following is based on Warnock (2011).

country. (Parenthetically, it is never clear if "reported" is equivalent to "actual"; financial engineers are paid to get around controls and, given enough time, they surely will. We might find evidence of a changed composition in *reported* flows data; were actual flows to mimic those reported, we might also believe that the composition really changed.)

More recently, De Gregorio (2011) has criticized emerging-market economies' (EMEs') other favorite response to capital inflows, which is to resist exchange rate appreciation by accumulating foreign-currency reserves. Reserve accumulation is intended to prevent the nominal appreciation of a currency. But, as De Gregorio correctly points out, it is not the nominal exchange rate that measures a country's international competiveness, but the real exchange rate (the nominal exchange rate adjusted for inflation differentials). In almost every case, countries that limit nominal exchange rate appreciation through reserve accumulation experience a real appreciation anyway, because the reserve accumulation is inflationary. So they end up in the same place—with a real appreciation—and have only traded off nominal exchange rate appreciation for higher inflation. Figure 2 illustrates (but does not prove) De Gregorio's point. Brazil accumulated vast reserves; in Chile reserve accumulation was much more muted. Perhaps owing at least in part to large reserve accumulation, Brazil ran high inflation; Chile did not. And, not surprisingly, real exchange rate appreciation—the very thing Brazil's reserve accumulation (and capital controls) was designed to avoid—occurred in Brazil but not in Chile. One must wonder whether there might be a different source of Brazil's problems.

Another important point that to date has not been adequately addressed by advocates of capital controls is flow diversion (De Gregorio 2011). Can we really make the case for capital controls in a two-country setting without considering third-country effects? If country B implements effective capital controls, flows will be diverted to other countries. But why, if B does not welcome these flows, should its neighbors feel differently? If diverted flows went to country A, why would A abstain from implementing controls? And, if the diverted flows then went to U or P, might those countries follow suit? If the controls are effective, will we end up in a world in which every country erects barriers to capital flows?

Figure 2. A Tale of Two Countries



Source: International Monetary Fund/Haver Analytics.

The lack of work on third-country effects of capital controls is disappointing. The International Monetary Fund (IMF) (2011b) mentions this (briefly, on pages 40–42 of a ninety-five-page report), but one gets the impression that it was an afterthought. The prospect of flow diversion implies that the bar for blessing the implementation of capital controls should be high. For the current study, I strongly suggest an extension—perhaps in a separate paper—that includes a multi-country model. This is not a topic that should be left to two-country models.

Another weakness in the case for capital controls is that the supporting empirical evidence is less than compelling. The IMF's October 2010 Global Financial Stability Report reported:

> Th(e) self-reinforcing cycle between flows and returns exacerbates market movements on the upside and on the downside, with important implications for financial stability. Higher returns and lower volatilities resulting from elevated foreign inflows can lead to perceptions of higher risk-adjusted returns and an underpricing of risk. By the same token, if flows to emerging markets reverse suddenly, a self-reinforcing cycle of outflows and lower risk-adjusted returns could follow, potentially resulting in a deep market sell-off.

The IMF then goes on to suggest that in the face of such potentially destabilizing capital flows, the imposition of capital controls might indeed be a reasonable policy response. This sounds right. Momentum trading can drive asset prices further and further way from anything justified by fundamentals until some bad event leads to a reassessment of risk, at which point the capital flows reverse themselves and emerging economies become cut off from global capital markets. If this is the way the world works, perhaps capital controls should be implemented early in such a cycle. But is it right? I don't think so. The IMF bases its assessment of the trading behavior of international investors on bilateral flow data, which show that global investors chase past returns. This is in line with seminal research on international capital flows from the 1990s, which found a positive correlation between U.S. flows into a country's equity market and past returns in that market, a correlation that is at the heart of most analysis of returns chasing by international macroeconomists. But can we really say anything about trading behavior by observing

bilateral flows? Recent research suggests that evidence on trading behavior that arises from flow data should be reassessed (Curcuru et al. 2011). The results are striking. For one large and important group of global investors (U.S. investors), a flows-based assessment of trading behavior—the type of assessment at the heart of many policymakers' views about global capital flows—is almost entirely incorrect. In fact, at the one-, two-, and three-month horizons, U.S. investors don't chase returns, but rather sell past winners. This is stabilizing.

Here is another point on the empirical facts: We seem to know that when interest rates in the United States are low, we will see a surge in capital inflows that can destabilize EME markets. This is at the heart of references to a "currency war" in response to the Federal Reserve's quantitative easing policies. The empirical literature on capital flows backs up this view, as study after study has found that low interest rates in the United States lead to higher flows to emerging markets. But Forbes and Warnock (2012) suggest that this consensus should be reassessed. Episodes of surges of capital inflows are no more likely when U.S. rates are low. If the Federal Reserve and other central banks spur global growth, or if global money supply increases sharply (something that, in the aftermath of the global financial crisis, has not yet occurred), then yes, one would expect more surge episodes. But low U.S. interest rates aren't enough. The following is a case to make the point: In the darkest days of the Global Financial Crisis, U.S. rates plummeted and there were no surges, just retrenchment.

There is another problem with the empirical evidence: Should the focus be on net or gross inflows? The focus is almost always on net inflows. The IMF, in its April 2011 Global Financial Stability Report, asks whether net capital flows are reliable or fickle (and argues that they are fickle). Many researchers and policymakers assess net inflows, but in their minds (and words) they are really focusing on foreigners' flows. The problem is that net inflows consist of distinct components: inflows and outflows from foreigners as well as inflows and outflows from domestic investors. When focusing on net inflows, one never knows who—foreigners or locals—is behind the flows. For example, a country experiences increasing net inflows (and an appreciating currency?) and assumes, naturally, that foreigners are behind the "surge" in inflows. But perhaps locals,

knowing that prospects in the local economy are good, are repatriating funds or shipping fewer funds to foreign markets. Looking at net inflows, and assuming any increase is due to increased inflows by foreigners, the policymaker might conclude that a tax on inflows is the right response. But suppose the "surge" is due to locals keeping money home, rather than foreigners wanting to invest in the country. In that case, policy would be addressing something (a surge in foreign inflows) that did not exist. Another example is as follows: Net inflows can plummet either because foreigners rapidly exit the market, as is often presumed, or because knowledgeable locals, perhaps knowing that domestic returns are about to decrease, lead the rush to the exit. If such a "sudden stop" is troublesome, policymakers may well impose capital controls on foreigners to prevent a large buildup of positions that they might later liquidate (en masse). But if the sudden stop was due to locals' decision to leave (capital flight?), are capital controls on foreigners really the right policy? Or should the conditions that prompted the locals' flight be addressed? How often might this confusion matter? That is unknowable to me, as it requires peering into the minds of policymakers. But Rothenberg and Warnock (2011) shows that almost half (twenty-four of fifty-five) of previously defined "sudden stops" of net capital inflows are actually episodes of "sudden flight." In those cases, the fall in net inflows reflected the actions of locals, not foreigners.

5. Concluding Thoughts

Recall the main takeaway of the GPT paper. Some countries have large current account (CA) surpluses, and the accompanying financial outflows can lead to an unsustainable accumulation of debt in CA-deficit countries. The somewhat inevitable debt restructuring is costly. Both surplus and deficit countries would be better off had capital controls (i.e., restrictions on financial account transactions) been in place.

Clearly, the authors are working on an important topic. The underlying phenomena the authors are tackling are relevant, evident, and can be associated with substantial pain. And the focus on debt flows seems right to me. I am less comfortable with the focus on capital controls. I fully agree that capital flows are scary, but the paper puts a lot of faith in capital controls. If it is debt flows we

have in mind, why not properly regulate and supervise the financial sectors that are very likely intermediating these flows?[3] That said, while I am uneasy with the focus on capital controls given the existing evidence, I find the work of Rodrik and Korinek very compelling and am happy to be further educated on this point.

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[3] I acknowledge that I am not sure how to think about the unregulated, more shadowy corners of our financial sectors. Some discussion on this is warranted.

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