

WHY DID FINANCIAL GLOBALIZATION DISAPPOINT?

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I. Introduction

A little over a decade ago, just before the Asian financial crisis of 1997 hit the headlines, there was an emerging consensus among leading macroeconomists that it was time for developing countries to embrace the liberalization of their capital accounts. In a famous speech during the IMF's Annual Meetings in 1997, Stanley Fischer put forth the case in favor of financial globalization and advocated an amendment to the IMF's articles the purpose of which "would be to enable the Fund to promote the orderly liberalization of capital movements" (Fischer 1997). Yes, there were risks associated with opening up to capital flows, but Fischer was convinced that these were more than offset by the potential benefits. Rudiger Dornbusch, having written so eloquently and convincingly on the usefulness of financial transactions taxes just a short while ago (Dornbusch 1996), now declared capital controls "an idea whose time is past" (Dornbusch 1998). He wrote: "The correct answer to the question of capital mobility is that it ought to be unrestricted" (Dornbusch 1998, 20).

At the time that these ideas were being floated, there was little systematic evidence that the theoretical benefits of capital flows would in fact be realized. One could look at the reduction in financing costs that accessing international markets enabled, or the competitive gains from foreign bank presence—as Fischer (1997) did—and conclude that the gains were already visible. Or one could look at the still-fresh Mexican peso crisis of 1994-95 and the Asian financial crisis which was brewing to conclude that the risks were too big to take on. Nonetheless, so strong were the theoretical priors that one could presume, as Fischer did (2003, 14), that the evidence in favor of capital-account would cumulate over time, just as with the evidence on the benefits of trade liberalization a couple of decades earlier.²

As Fischer had prophesied, there has been an explosion in empirical studies on the consequences of financial globalization. But far from clinching the case for capital-account liberalization, these studies paint quite a mixed and paradoxical picture.³ Kose, Prasad, Rogoff, and Wei (2006, hereafter KPRW), who provide perhaps the most detailed and careful review of the literature, conclude that the cross-country evidence on the growth benefits of capital-account openness is inconclusive and lacks robustness. They

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² Of course, the Asian financial crisis forced the IMF to abandon efforts to amend its Articles of Agreement to promote capital-account liberalization. But in some of its recent bilateral trade agreements, the US did succeed in getting its trading partners (for example, Chile and Singapore) to commit irrevocably to it.

³ We do not evaluate here the impact of financial globalization on financial crises, except to note the recent assessment by Reinhart and Rogoff (2008, 7): "Periods of high international capital mobility have repeatedly produced international banking crises, not only famously as they did in the 1990s, but historically."

argue that one should look for the gains not in enhanced access to finance for domestic investment, but in indirect benefits that are hard to detect with macroeconomic data and techniques (an argument which we will evaluate below). In another paper, Kose, Prasad and Terrones (2003) find that consumption volatility actually *rose* (relative to output volatility) in emerging market economies during the current era of financial globalization—a finding that flatly contradicts theoretical expectations. Perhaps most paradoxical of all are the findings of Prasad, Rajan, and Subramanian (2007, hereafter PRS) and Gourinchas and Jeanne (2007), which throw cold water on the presumed complementarity between foreign capital and economic growth: it appears that countries that grow more rapidly are those that rely less and not more on foreign capital; and in turn foreign capital tends to go to countries that experience not high, but low productivity growth.

What these findings reveal are the shortcomings of the mental model that dominated thinking about capital flows a decade ago. This model had two key premises. First, it presumed that low savings and weak financial markets at home were first-order constraints on economic growth and development. Thus greater access to investible funds from abroad and improved financial intermediation would provide a powerful boost to domestic investment and growth along with better consumption smoothing. Second, while it recognized the potential of adverse interactions between lenders' incentives abroad and borrowers' incentives at home, it assumed that sufficiently vigilant prudential regulation and supervision could ameliorate the attendant risks sufficiently. Indeed, given the presumed importance of access to international finance, this model required that policy makers give very high priority to the implementation of appropriate regulatory structures.

In brief, the argument was this: (1) Developing nations need foreign capital to grow. (2) But foreign capital can be risky if they do not pursue prudent macroeconomic policies and appropriate prudential regulation. (3) So developing countries must become ever more vigilant on those fronts as they open themselves up to capital flows. This syllogism remains at the core of the case for financial globalization (e.g. Mishkin 2006), even though, as we shall see, some newer arguments have begun to take a different tack (e.g. KPRW). But the syllogism relies heavily on a premise that is by no means self-evident. Certainly the results of PRS (2007) and Gourinchas and Jeanne (2007) are at variance with the presupposition that poor nations need foreign finance in order to develop.

To make sense of what is going on, we need a different mental model. We must begin by taking note of the fact that developing countries live in a second-best world, which means that they suffer from multiple distortions and constraints. While some nations may be severely constrained by inadequate access to finance, others—and perhaps a majority—are constrained primarily by inadequate investment demand, due either to low social returns or to low private appropriability. As we shall argue below, targeting the external finance problem when the “binding constraint” lies with investment demand can be not only ineffective, it can actually backfire. In particular, capital inflows exacerbate the investment constraint through the real exchange rate channel: the increase in the real exchange rate which accompanies capital inflows reduces the real profitability of investment in tradables and lowers the private sector's willingness to invest. The result is that while capital inflows definitely boost consumption, their effect on investment and

growth is indeterminate, and could even be negative.⁴ The flat investment profile that most emerging market economies have seen since the early 1990s—compared to their experience prior to financial globalization—can be understood in these terms. The exceptions are countries such as China, India, or Chile that have managed to prevent real exchange rate appreciation for a sustained period of time thanks in part to their reliance on capital controls.

Furthermore, government capacities are limited. Priorities have to be selected carefully since not all distortions can be removed simultaneously. The emphasis on strengthening financial regulation and governance, demanding as it is even in advanced countries, is particularly challenging in countries that are struggling with problems of underdevelopment. Confronting this challenge, and paying up the implied opportunity costs, makes a lot of sense if what one gets in exchange is a big boost in growth, as would be the case when the binding constraint on growth is access to external finance. But otherwise, exhortations on prudential regulation serve little purpose other than reveal the professional limitation of every specialist: insistence that the government undertake all the complementary reforms that would ensure the success of the specialist's policy recommendation, and indifference to the trade-offs that might arise from the needs of more urgent reforms elsewhere.⁵

Our paper proceeds as follows. In the next section we review some of the newer arguments in favor of financial globalization. First we look at the arguments of Henry (2007), who provides a critique of the existing literature and points to the research on stock-market liberalizations and to micro-studies that purportedly provide much stronger and more robust evidence on the benefits of capital-account liberalization. We also examine the collateral-benefits argument due to KPRW. We next analyze the argument laid out in Mishkin's (2006) recent book, *The Next Great Globalization*. Then we review briefly some of the micro-based research. In section III, we lay out a simple framework that distinguishes investment- and saving-constrained economies and explains how they respond differently to capital-account liberalization. Finally, we offer some concluding thoughts in section IV.

II. The New Arguments

Figures 1 and 2 present the simple correlation between economic growth and financial globalization (hereafter FG, measured in de facto terms, i.e. as the sum of gross foreign assets and liabilities as a share of GDP). In Figure 1, the period covered is 1970-2004, with Panel A showing the relationship in terms of the *level* of FG and Panel B showing it in terms of the *change* in the level of FG. Figure 2 repeats this exercise for the period 1985-2004. The absence of any apparent relationship between FG and growth is, of

⁴ See Bresser-Pereira and Gala (2007) for a similar argument.

⁵ This point brings to mind the complaint that Larry Summers voiced while discussing a paper on China's banking problems: "Like experts in many fields who give policy advice, the authors show a preference for first-best, textbook approaches to the problems in their field, while leaving other messy objectives acknowledged but assigned to others. In this way, they are much like those public finance economists who oppose tax expenditures on principle, because they prefer direct expenditure programs, but do not really analyze the various difficulties with such programs; or like trade economists who know that the losers from trade surges need to be protected but regard this as not a problem for trade policy." (Summers, 2006)

course, the key piece of evidence that has elicited a lot of analysis and that is the focus of the re-evaluation in KPRW.

But the cause of financial globalization (FG) has been taken up recently by a number of newer studies. These studies offer a range of responses to the earlier, and generally unfavorable (for FG) evidence. Some say we have been looking at the wrong places; others say we have not looked hard enough; and yet others say that we should just do our homework and be patient. We now offer some comments on each of these arguments, focusing on the work of Henry (2007), KPRW (2006), Mishkin (2006) and on recent micro evidence.

Looking in the wrong places, version I: Henry (2007)

Henry (2007) argues that the failure of existing studies to detect a positive impact of financial globalization (FG) on growth stems from three factors:⁶ first, the studies look for permanent growth effects whereas in the basic Solow growth model permanent decreases in the cost of capital and hence increase in the ratio of investment to GDP only have a *temporary* effect on growth. Second, much of the empirical work does not distinguish between effects of FG on developing and developed countries.⁷ And third, that FG indicators are measured with considerable error. He then suggests that studies that address these deficiencies provide a little more favorable evidence for the positive effects of FG.

How persuasive are these reservations and arguments? It is not clear to us that Henry's criticism has much bite. Consider Henry's first objection, namely that cross-country regressions cannot pick up the positive effects of capital-account liberalization because the neoclassical growth model predicts that a reduction in the interest rate faced by investors produces only *temporary* growth effects. However, the neoclassical growth model is hardly the only framework that motivates growth regressions. Endogenous growth models enable policies to have long-run growth effects, which is why running growth regressions with measures of different kinds of policies (trade policies, fiscal policies, and so on) on the right-hand side has been such a popular research strategy during the last couple of decades. Moreover, if KPRW are right in suggesting that the most important effects of FG are the indirect or collateral effects, then the standard cross-country framework is exactly the right framework to use because these indirect effects (institutional and financial development etc.) are permanent not transitory ones.

Perhaps more importantly, even though the neoclassical growth model predicts only temporary growth effects, it does predict a permanent rise in the investment share of GDP. Consider the shock analyzed by Henry, in which capital-account liberalization induces a fall in the interest rate facing investors. This results in a higher capital-output ratio in the steady state, to support a higher GDP per (effective) worker. While the growth rate of capital returns to its original level (given by the sum of labor-force growth

⁶ Before we review these new arguments, it should be noted that at least two additional papers that have appeared since PBH's survey—Gourinchas and Jeanne (2007) and Prasad, Rajan, and Subramanian (2007)—come closer to the conclusion that foreign capital has a *negative effect* on long run growth. So, if anything, the weight of at least the macro-economic evidence has shifted toward a less favorable view of FG.

⁷ Gourinchas and Jeanne (2007) focus only on developing countries while Prasad et. al. (2007) distinguish between developed and developing countries.

and technological progress), the investment-GDP ratio becomes *permanently* higher.⁸ This is important, because it gives us a clear strategy to address the Henry critique: check directly to see whether capital-account liberalization results in higher investment ratios. We are not aware of studies that have systematically demonstrated any such link. In fact, the evidence available either suggests no relationship between financial integration and investment rates (Schularick and Steger 2007) or a negative relationship (Gourinchas and Jeanne, 2007).⁹

Moreover, even if one accepts Henry's contention that "the statistically significant portion of that impact occurs in the immediate-to-near aftermath of liberalization," which he calculates as typically less than 5 years, it is not clear why we should not (and he does not) look at the available panel evidence on growth over 5-year horizons (or less). Indeed, in the summary of the evidence presented in Table 4A in KPRW, there are 13 studies that have tested the effects of FG over the shorter horizons that PBH favors: of the 13, 9 are in the "mixed evidence" category, 1 each in the "no effect/mixed evidence" and "no effect" categories, and 2 in the "no effects category." Moreover, the five-year panel estimations in PRS suggest a mildly negative (although statistically insignificant) effect of FG on growth.

With respect to Henry's (2007) claim that FG indicators used in the literature are afflicted by measurement error, we would point out that many of the variables in the cross-country literature mentioned above are also measured with error. But not all have suffered the same fate as the FG indicator. Clearly, cross-country growth regressions have many problems, and this is not the place to discuss their relative merits. But the cross-country framework has generated reasonable evidence of economically and statistically significant effects with respect to human capital, macroeconomic stability, and exchange rate undervaluation, even though all of these areas are also subject to measurement error. Moreover, while it is true that *de jure* measures of financial globalization are likely to be prone to measurement error, this is less likely of the *de facto* measures based on actual inflows and outflows of capital (KPRW). However, even *de facto* measures do not evidence in favor of positive effects from FG (see Figures 1 and 2).

While the macro-based literature on FG provides little support for the benign view, the evidence on opening domestic equity markets to foreign participation has been invoked as being more positive. The evidence takes the form of comparing the variable of interest—stock prices, cost of capital, investment, and growth—before and after equity market reforms within a country, which are dated by the first clear policy attempt at allowing foreign equity participation. This framework has the advantage of focusing on short-term effects and on within-country-over-time variation in the data, and of exploiting the potentially large shocks that could more easily allow some of the effects to be detected in the data. Here Henry provides a good summary of the evidence, as well as a discussion that highlights its limitations.

⁸ This point is obscured in Henry's (2007) account because Henry focuses on the growth rate of the capital stock (I/K , assuming no depreciation), which is of course different from the investment rate (I/Y). In the neoclassical growth model, capital-account liberalization has long-run effects on the latter, but not the former. This can be seen by writing $I/Y = (I/K)(K/Y)$ and noting that in the post-liberalization steady state I/K is constant while K/Y is higher.

⁹ Schularick and Steger (2007) find a positive relationship between financial integration and investment rates during the earlier era of globalization (1880-1913), but no such relationship in the data for 1980-2002. They interpret this as being the result of much larger net capital flows under the classical gold standard.

These limitations include: the conflation of equity market reforms with a host of other ones (trade reforms, privatization, macro-stabilization etc.) many of which coincided with the former; the problem of reverse causality because reforms could have been easier to carry out precisely because of the favorable overall environment; the relatively small sample size due to the fact that equity market reforms were enacted in few emerging market countries (less than 20); the discrepancy between the relatively small impact on the cost of capital and the substantial impact on private investment and especially economic growth—of about 1 percent a year or more, which is inconsistent with the theoretical predictions from a simple growth model which caps the effect at no more than one-third of one percent.

Looking in the wrong places, version II: KPRW (2006)

KPRW accept the weakness of the macro evidence in favor of FG, but they surmise that this is due to the fact that researchers have been looking in the wrong places. They argue that the effects of FG operate not so much through the cost of capital and investment, but indirectly through macroeconomic discipline, and financial and institutional development. This is the “collateral benefits” argument. Moreover, their claim is that there are threshold effects, with FG more likely to have positive effects the higher the level of financial and institutional development and the greater the macroeconomic discipline. As KPRW acknowledge, this argument is largely speculative at this point, as the evidence is largely suggestive and preliminary.

The KPRW argument is not self-evident to us. One can just as easily come up with arguments where FG weaken macroeconomic and financial discipline and undermine institutional development. For example, access to international finance often enables profligate governments to operate on soft budget constraints for longer periods of time than they would have been able to do otherwise. Turkey during the 1990s presents a case in point. Having opened up to FG in the late 1980s, the Turkish government found a ready source of cheap finance (external borrowing intermediated through domestic commercial banks) with which to sustain a growing fiscal imbalance. Without FG, Turkey would have been forced to put its fiscal house in order a lot sooner than in 2001, and in a much less costly manner. Another counter-argument, pertaining to institutional development, is that FG enables important domestic stakeholders to favor “exit” over “voice.” Why demand and invest in domestic institutional reforms if you can shift your wealth abroad?

We make two further points about the KPRW, one empirical and the other normative. The empirical point is that if the collateral-benefits argument were valid, it would be relatively easy to pick it up in the data. As KPRW argue, their view of FG implies that a conditional correlation framework will typically fail to detect the influence of FG because the indirect effects will be soaked up or captured by the other channels through which FG works. But then this implies that the right approach—or at least first step—is to look for evidence of an *unconditional* kind. If FG provides important spillovers in terms of macroeconomic and other aspects of governance, then there should be an unconditional positive correlation between FG and growth. But as Figures 1 and 2 clearly show, the evidence is not kind to this view. To brush this evidence aside, while holding on to the collateral-benefits view, would require that we assume growth

prospects were far worse in those countries that chose to embrace FG (for reasons unrelated to FG and its effects).

The claim that the effects of FG are gradual and felt over long periods and hence difficult to detect in the cross-country framework is also problematic. The same is also true of the effects of education (educational capability in the population takes a long time to accumulate), macroeconomic stability (a government's reputation for macro-stability is hard-earned and over long periods), or undervalued exchange rates (a sufficient period of time is required for resources to get allocated toward tradable sectors). But, as we mentioned above, in all these cases cross-country growth regressions have generated significant correlations. We doubt that there is anything specific to FG that makes it inherently harder to detect its long run effects.

The normative point is that even if we grant the collateral-benefits argument, the policy implications would be far from clear. The best way to achieve a particular policy objective—whether it is macroeconomic stability or institutional development—is to do so directly, not through reforms in other areas which may also incidentally serve that objective. In order to make an argument in favor of FG on account of its collateral benefits, one needs to not only demonstrate the presence of those benefits, but also to demonstrate that FG is a particularly effective way—among all possible reform strategies—of achieving those benefits. That in turn requires that FG both have sufficiently strong first-order effects on the channels in question and that its administrative and other costs be small (compared to other reforms in the feasible set). KPRW do not present an argument along those lines, and we doubt that it could be constructed in sufficiently general terms to yield a presumption in favor of FG.

The check is in the mail: Mishkin (2006)

Mishkin's book *The Next Great Globalization* presents an exceptionally well-written and clearly argued case in favor of the benefits of financial globalization. We scrutinize it here because it is the best example of a certain type of argument in the literature: The gains from financial globalization are huge, and if we have not reaped them yet, it is only because we have not undertaken the complementary reforms yet; do those reforms, and there will be big benefits around the corner. We can call this the “check is in the mail” argument after Ricardo Hausmann (Hausmann et al. 2005).

Mishkin views a sound financial system as the sine qua non of economic growth. Without appropriate financial intermediation, savers cannot channel their resources to investors and capital does not get allocated efficiently. Hence the potential gains of financial globalization are too large to pass up. Mishkin does recognize that international financial integration is incomplete; that international financial markets work imperfectly; that capital flows can create all sorts of mischief when financial institutions take excessive risks; that capital-account liberalization can misfire when done badly; and that there are no one-size fits all policies when it comes to prudential regulation. In fact, much of his book is about financial globalization gone bad. He devotes considerable space to the financial crises in Mexico, South Korea, and Argentina, and to the difficulties of undertaking financial reform. Nonetheless, the appropriate reaction to these complications is not to delay liberalization or throw sands in the wheels of international finance, but to ensure that the requisite complementary reforms are also undertaken.

Essentially Mishkin presents a more recent version of the Fischer argument we summarized in the introduction, updated in light of the intervening financial crises in East Asia and elsewhere.

The case for financial globalization as laid out in this book relies critically on three premises. First, improved finance is key to unleashing economic growth in developing economies. Second, integration with *international* financial markets (financial globalization) is especially effective and potent in making finance work for development. And third, the complementary reforms required to make financial globalization work are not just worth it (the first two premises), but that they are doable in the relevant context of developing economies. We have our doubts on all three fronts.

With respect to the first premise, while finance may be the binding constraint in some settings, we are not sure that it represents the sole or most important constraint in many others, as we shall argue further below. Interestingly, in presenting his case Mishkin draws largely on a-priori reasoning, rather than actual historical evidence. It would be hard to argue that improved finance was among the key drivers of economic growth in Britain or other early industrializers. It would be even harder to argue that the take-off of countries like South Korea (in the early 1960s) or China (since the later 1970s) was due to financial liberalization and improved financial intermediation. Mishkin's discussion on China (pp. 41-42) acknowledges as much, pointing out that at some point China's institutional shortcomings will indeed become a binding constraint—which is hard to disagree with. It is also true that the cross-country evidence shows a strong association between economic growth and measures of financial depth (Demirgüç-Kunt and Levine 2007). But this literature has not sorted out the causality issues convincingly, nor has it demonstrated a direct link between policy reforms in the financial sector and overall growth (see also below).¹⁰

On the second premise, Mishkin (2006) argues that financial globalization could raise total factor productivity in countries in a number of very broad ways by imposing capital market discipline on governments, breaking up local monopolies, and broadly promoting a whole range of institutional improvements that KPRW refer to as the “collateral benefits” of financial globalization. For example, if foreign investors can take controlling stakes in domestic financial firms and bring in state-of-the-art financial intermediation practices, domestic financial efficiency would be improved across-the-board.

We would argue that *international* finance not only greatly extends and complicates the range of institutional reforms needed, it also works differently with respect to a crucial variable: the real exchange rate. Improved domestic financial intermediation, which helps mobilize domestic saving for investment purposes, tends to depreciate the real exchange rate, as it closes the ex ante gap between desired investment and saving. That is good for investment in tradables and for economic growth. But improved access to foreign finance has the opposite effect on the real exchange rate—it appreciates it—with adverse effects on growth. Once again, we will return to this issue below, because it is critical to our understanding of how financial globalization works (or doesn't work).

¹⁰ One of the more persuasive papers in this literature is due to Rajan and Zingales (1998). But this paper establishes an effect on relative growth (finance-intensive sectors grow more rapidly than other sectors in countries where there is greater financial depth) and not on the average level of growth.

Finally, we have doubts with respect to the theoretical consistency and practical feasibility of the regulatory and other reforms needed to support financial globalization in the kinds of environments faced by developing nations. Mishkin (2006) acknowledges that there are prerequisites—akin to the threshold effects suggested by KPRW—to reaping the benefits of FG. These prerequisites include: developing strong property rights, strengthening the legal system, reducing corruption, improving the quality of financial information, improving corporate governance, and getting the government out of the business of directing credit! Mishkin and KPRW’s premise is that financial globalization will deliver these threshold benefits. But there is, of course, a tension, even contradiction, in implicitly calling for greater FG to deliver the broader collateral benefits that are in turn prerequisites for FG reform to be successful in the first place.

Leave aside this tension and consider the feasibility of implementing the supporting reforms. Even advanced countries have a hard time putting in place the kind of finely tuned and calibrated prudential regulations that would rein in excessive risk-taking by financial intermediaries that have been set free—a point that needs no belaboring in the current sub-prime mortgage crisis. The challenge for developing countries, with weak administrative capacities and low human capital, is many times larger. Meeting these prerequisites is of course the heart of the challenge of development. If poor countries could develop strong property rights, strengthen the legal system, reduce corruption, improve the quality of financial information, improve corporate governance, and get the government out of the business of directing credit, they would no longer be poor, and FG would certainly be a clearly dispensable side-show. So, FG is either ineffective or irrelevant.

Indeed, Mishkin’s book makes for very sobering reading on this score. During the 1990s, Argentine economic policy was driven by the single-minded goal of achieving complete financial globalization. The convertibility law, which pegged the peso to the dollar, was intended both as a restraint on monetary policy and as a strategy for reducing transaction costs in international finance. Most importantly, Argentina put in place “one of the most innovative bank regulation and supervisory regimes in the world” (Mishkin 2006, 109), going further than advanced countries in a number of respects. Prior to 2000, some twenty troubled banks were closed. Entry of foreign banks was encouraged. Capital requirements were stricter than those in the Basel Accord. By 1998, the World Bank ranked Argentina second only to Singapore among emerging markets in terms of the quality of its regulatory environment (Mishkin 2006, 112). None of this prevented the currency crisis and spectacular crash of 2001-2002.

The lesson from the Argentine crisis? No matter how much you do, there is still more left to do—and then there is always bad luck. This bottom line lays bare the fatal flaw of those arguments that stress the importance of undertaking complementary reforms in support of financial globalization: in practice, the list turns out to be an open list, typically ending with “so on.” It does not leave much room for optimism with regard to the likelihood that countries will be able to complete their (as yet not fully specified) homework.

What do the micro-studies really show?

In the face of disappointing and mixed evidence from macro studies, proponents of FG have also increasingly turned to exploring micro-economic data. One key advantage, of course, is degrees of freedom; another is the ability to exploit the variation within countries across sectors or firms, thereby controlling for shocks or reforms that are common to sectors and firms (see Forbes 2007a and b, Desai et al. 2004).

The theory is that if “financial globalization enhances efficiency, then imposing capital controls should diminish efficiency in at least two important ways. First, capital controls may reduce the supply of capital, thereby raising the cost of borrowing and tightening the financing constraints faced by domestic firms. Second, by reducing the supply of capital, capital controls can decrease competition and market discipline, permitting firms that might not survive if their competitors had access to credit to flourish behind closed borders.” The key finding of Forbes (2007a) is that in Chile small publicly traded firms did better than larger firms before 1991 and after 1998 which is the case for developed countries that have liberal capital flows. However, during *El Encaje*, the period of capital account controls, this behavior is reversed with the investment growth of small firms dropping below that of large firms.

At first sight, these studies seem to suggest that capital controls must have adverse effects on overall investment through the cost-of-finance channel—and that is how they are often interpreted. Our skepticism about this firm-level evidence is both empirical and conceptual. First, we would note that the evidence is itself mixed as Henry (2007) makes clear. Moreover, other micro-evidence on the impact of capital flows that uses data on many more countries (and not just on one country as in the case of Forbes (2007)) goes the other way. For example, in Prasad et al. (2007), the question posed was whether greater capital flows do in fact relieve financing constraints. The evidence strongly suggested that in countries that were more open to various forms of capital flows, sectors that were more dependent on finance (defined as in Rajan and Zingales (1998)) actually grew *slower* in countries with less-than-average level of financial development. Thus, foreign financing seemed to worsen rather than improve access to finance.

But the more important limitation of the micro-approach is that in some ways it cannot capture a fundamental aspect of capital flows. The micro-approach attempts to measure the effect of a treatment (foreign financing) by comparing the treated group (say, small publicly traded firms in Forbes 2007) with a control group (large firms). Even if there were significant effects of the treatment, the experiment is not designed to capture a key externality associated with capital flows, namely that firms in traded good sectors (both actual and potential) will be worse off as a result of the treatment. In other words, what the cross-section (across firms) evidence cannot address is the counterfactual question of what would have happened to aggregate investment in the absence of the controls, especially once the induced real exchange rate changes are factored in. It is entirely possible for aggregate investment to be higher in the equilibrium with restricted capital mobility (and therefore a more competitive real exchange rate) than in the equilibrium with full capital mobility, even though some firms are in effect facing higher costs of finance in the latter equilibrium. Our argument (see below) is that this is especially likely in investment-constrained economies.

Indeed, there is evidence to suggest (within a micro-approach) that foreign capital has just this effect. In Rajan and Subramanian (2006), there is strong evidence that in countries that receive more aid, sectors that are more exportable grow slower. This

finding is extended to private flows in PRS. If the key characteristic of developing countries is that they are investment-constrained, then checking for the effect of capital flows on investment opportunities is essential before assessing the overall impact of FG.

III. Investment- versus Saving-Constrained Economies

General considerations

Poor economies are poor because there are many things that are wrong with them. Addressing underdevelopment by trying to fix all market and government failures simultaneously—in effect wishing that we can turn, say, Ethiopia, into a Netherlands through coordinated policy reforms across the board—is a fool’s errand. The lesson of economic history is that development happens differently: though a sequence of relatively small-scale changes or reforms that tackle the most binding constraints to growth over time (Rodrik 2005). The policy implication in turn is that successful reform depends on the correct diagnosis of the binding constraints of the moment. How this can be done is discussed in Hausmann et al. (2007) using the heuristic device of a decision tree.

At the very top of the decision tree is the following question: is private investment in the economy held back primarily by lack of access to finance or by low perceived returns? In other words, is investment low despite the existence of many privately profitable projects because entrepreneurs cannot finance these projects at reasonable cost? Or is it low because there is plenty of credit but entrepreneurs do not see many profitable investment opportunities?

One might think that this would be a distinction that is hard to make in practice for real-world economies. But economies that suffer from these two sets of constraints exhibit distinct symptoms and different co-movements in macro variables. Regardless of whether an economy is saving- or investment-constrained, the private investment rate will be low. But in a saving-constrained economy, real interest rates will be high, borrowers will be chasing after lenders, and any (exogenous) increase in resource transfers from abroad will finance mainly investment rather than consumption. If you ask entrepreneurs what they would invest in if you gave them \$50 million, you would hear in response a long list of projects. In economies constrained by investment-demand, by contrast, real interest rates will be low, banks will be sitting on top of mountains of liquidity, and it will be lenders who are running after borrowers. When you query entrepreneurs about investment ideas in such economies, your question will be met by a long silence, followed by the riposte: “do I have to invest the \$50 million here?” Any resource windfall will be eaten up by consumption rather than investment.

To see that this is not just a theoretical possibility, consider the case of El Salvador. In 1994, El Salvador held its first democratic elections after a long period of civil war. At the time, investment stood at 18 percent (of GDP). Over the following decade (1994-2004), remittances from abroad increased by 4 percent of GDP, from 12 to 16 percent. Investment meanwhile fell to 16 percent. Where did the increase in remittances go? Into consumption, as saving fell one-for-one, from 16 percent to 12 percent of GDP. In view of the large increase in remittances, El Salvador clearly was not a saving-constrained economy. Indeed, banks were flush with liquidity and had to expand

internationally in order to find clients to lend to. By all indications, the problem was that the perceived return to domestic investment was very low (Hausmann and Rodrik 2005).

El Salvador is a straightforward case where it is easy to diagnose that the binding constraint lies with investment demand rather than saving. What about elsewhere? In principle, we would need an in-depth diagnostic exercise to determine the nature of the constraint in different national settings, but we offer here a simple test that we think is quite suggestive. As we indicated above, domestic investment should be highly sensitive to the availability of resource inflows only when an economy is saving-constrained. If we can isolate a reasonably exogenous determinant of such inflows, we could then check whether domestic investment moves alongside changes in this measure in the direction suggested by the saving constraint. Since capital inflows to emerging markets respond to monetary conditions in the U.S., and since those are plausibly exogenous to developing countries, we could use U.S. interest rates as our proxy for the strength of resource inflows to such countries.¹¹ The higher U.S. interest rates, the smaller the volume of capital inflows; and if the saving constraint binds, domestic investment in emerging market economies ought to be correspondingly lower.

Table 1 shows the correlation between U.S. real interest rates and investment rates for a sample of emerging markets. Two sets of correlation are presented, one for the 1985-2006 period and the other for the 1990-2006 period, to allow readers to pick their own preferred date for the onset of financial globalization (1985 or 1990). But either way, the numbers tell a striking story. The vast majority of countries exhibit *positive* correlations, some of which are quite large, indicating that their investment rates tend to fall when U.S. interest rates are low and external liquidity is plentiful. This is the exact opposite of what one would expect to find in the presence of a saving constraint.¹² And interestingly, the only two countries for which the correlation is negative and sizable are China and India, countries that have shielded themselves from financial globalization.¹³ We will offer further interpretation for these findings below.

¹¹ See for example Calvo, Lederman, and Reinhart (1996) on the role of US interest rates as a determinant of capital flows to developing countries.

¹² These results do not seem to be due to changes in relative prices: the pattern of correlation we get is quite similar when we compute the investment effort by taking the ratio of investment to GDP at constant local currency units. Nor can we explain it easily through other channels by which US real interest rates affect economic activity in emerging markets. When US interest rates are high, the demand effect is negative, and that should exert a depressing effect on investment in developing countries.

¹³ In principle, the fact of being more closed to capital means that interest rate changes in the US should have little impact on the availability of savings in these countries, leading to a weak or no correlation. One interpretation of the strong negative correlation would then be that it reflects the more traditional demand channel of higher interest rates in the US reducing the demand for exports and hence leading to lower investment.

Table 1: Correlation between U.S. real interest rates and domestic investment (as a % of GDP)

	1990-2006	1985-2006
Indonesia	0.48	0.43
Philippines	0.60	0.22
Thailand	0.16	0.03
Turkey	0.64	0.26
Argentina (1993-2006)	0.23	..
Bolivia	0.67	0.28
Brazil	0.28	0.40
Chile (last year 2005)	0.59	0.04
Colombia	0.10	0.03
Mexico	0.05	-0.08
Peru	0.69	0.43
Malaysia	0.43	0.22
South Korea	0.33	0.00
Uruguay	0.58	0.10
India (last year 2005)	-0.56	-0.67
China	-0.57	-0.66

Source: US real interest rates are T-bill rates adjusted by CPI inflation (both from IMF, International Financial Statistics database). Investment figures come from the World Bank, World Development Indicators database.

That countries might be predominantly constrained by investment demand, as suggested by these results, should not come as a big surprise to anyone who has followed the recent literature on economic growth and development. The heavy focus on institutions as a determinant of long-run development patterns (as in North 1990, Acemoglu et al. 2001, Rodrik et. al. 2004) is grounded in the view that investment and entrepreneurship are severely hampered in low-income environments by the imperfect appropriability of the social returns to investment. From this perspective, poor property-rights protection, weak contract enforcement, and fear of expropriation are the root causes of underdevelopment. Each one of these keeps investment demand lower than what would have been the case under better institutions. Of course, a poor contracting environment affects adversely also the functioning of credit markets and financial intermediation, but the literature on institutions has tended to emphasize the investment-demand channel rather than the finance/saving channel. One can interpret Acemoglu and Johnson (2005), who find that the protection of property rights is significantly more important to growth than the quality of contracting, as directly reinforcing this emphasis.

Appropriability of investment returns is hampered not just by poor institutions, but also by market imperfections. Here too there is a long tradition in development economics which emphasizes the learning and other externalities associated with investments in non-traditional, tradable industries. In the presence of such externalities, investment in tradables is too low and the pace of structural transformation too slow. Recent papers have refined these views, linking the pace of economic growth to the capacity to produce new goods through “cost-discovery” (Hausmann and Rodrik 2003; Hausmann et al. 2007; Hausmann and Klinger 2007). In common with the institutionalist perspective, these models identify inadequate investment demand as the relevant binding

constraint. But they focus on investment demand for a specific type of economic activity, namely non-tradable tradables, which are presumed to be particularly important in driving economic growth.

A stylized framework

We now sketch a very stylized framework to illustrate more clearly the difference between investment- and saving-constrained economies and to analyze the manner in which they each respond to openness to capital inflows.

Consider a world in which agency problems render external finance more expensive than internal cash flow as a source of investment funds. In such a world, firms will not resort to external finance as long as they have adequate cash flow or internal funds in relation to their investment needs. Indeed, the bulk of investment in developing countries is self-financed, through retained earnings or the family savings of owners/partners (see Beck, Demirgüç-Kunt, and Maksimovic 2008). In such a setting, firms will be of two types.

- Type I: Profitability of investment projects is high relative to current cash flow, leaving firms in need of external finance for the undertaking of incremental investments;
- Type II: Profitability of investment projects is low relative to cash flow, leaving firms not in need of external finance.

We call economies in which firms are preponderantly of Type I as “saving-constrained” and economies characterized by Type II firms as “investment-constrained.” The key difference is that financial conditions, and in particular market interest rates, will not be a major determinant of the volume of aggregate investment in the second case. The investment demand schedule will be virtually vertical in investment-constrained economies.

Now consider opening an economy where interest rates are high relative to the rest of the world to capital flows. In a saving-constrained economy, capital-account liberalization works in the conventional fashion (Figure 3a): a reduction in domestic interest rates and the increase in the availability of external finance spurs domestic investment, as firms travel down their investment demand schedule.¹⁴ Consumers meanwhile face a change in intertemporal relative prices, inducing them to consume more and save less. The increase in domestic investment and reduction in saving are financed by capital inflows. The economy grows more rapidly as a consequence of the boost to investment. This is the standard textbook story on capital-account liberalization.

But in an investment-constrained economy, the investment demand schedule is vertical, so the effect of liberalization is purely to boost consumption. Investment is unaffected because the equilibrium level of investment is determined primarily by the perceived returns, which are presumed to be low. Foreign savings simply substitute for domestic savings, with no net effect on investment or growth. (Remember the El Salvador story.)

¹⁴ The investment demand schedule here is strictly speaking that for tradables because that is key for long-run growth.

But there is more to the story because capital inflows also appreciate the real exchange rate (see PRS and Figure 4 below for evidence). In theory, the impact of real exchange rate appreciation on aggregate investment is ambiguous. Appreciation is good news for producers of non-tradables, especially if capital goods are mostly imported. But it is bad news for producers of tradables. But from a growth standpoint, the second effect clearly dominates. This is shown in Figure 3b, where the inflows of foreign savings shifts the investment demand schedule for tradables leftwards because of the appreciation associated with the inflows. The new equilibrium (at C) is to the left of the old (autarky) one (at B), implying that investment declines consequent upon opening up to foreign capital.

There is strong and robust evidence indicating that real exchange rate overvaluation is bad for growth, while undervaluation is good (Rodrik 2007; Johnson, Ostry and Subramanian, 2007 and Figure 5 below).¹⁵ This evidence indicates that the operative channel is the size of the traded sector, and of industry in particular. Real exchange rate appreciation reduces the relative profitability of industry and slows down its rate of growth. That this is associated with adverse effects on *aggregate* economic growth indicates that tradables are in some sense “special” in developing countries.

Rodrik (2007) presents two arguments for this. One is that tradables (and industry in particular) is much more demanding than non-tradables of the institutional environment. A barber needs little more than a few implements and a stool to operate. But a garment firm depends on the reliable delivery of a whole range of public inputs and services (licenses, certification, transport links, public safety, enforcement of contracts with suppliers, and so on). Therefore weak institutions impose a larger “tax” on tradables than they do on non-tradables. The effect of appreciation is to exacerbate this distortion and—in a model of endogenous growth—reduce growth further. The second argument is that tradables are subject to a much greater extent to the learning and other externalities that prevail in low-income environments. This creates a similar second-best role for the real exchange rate. For either or both of these reasons, a real appreciation of the currency is bad news for those components of investment that matter most to economic growth (see Rodrik 2007 for an explicit model). Note that this reasoning may also shed light on the pattern of correlations displayed in Table 1.

A final point bears emphasis. How does our distinction between saving and investment-constrained economies fit with the macro literature on finance and growth, which has argued that financial depth and the quality of the financial system have important effects on economic growth (see Demirgüç-Kunt and Levine 2007 for a recent review of this literature). One of the key benefits of financial development is to augment domestic savings. Does this evidence then not indicate that developing economies are, to use our terminology, predominantly saving-constrained rather than investment-constrained?

Leaving aside questions about causality and endogeneity, which we do not believe this literature has fully resolved, it is not entirely clear that these results can clearly

¹⁵ Johnson, Ostry and Subramanian (2007) show that not only is the average level of overvaluation significantly greater for slow-growing sub-Saharan African countries relative to the sustained growth performers but that consecutive spells of overvaluation are longer in duration and the degree of overvaluation during these spells significantly greater.

distinguish between these two regimes. To see why, consider an improvement in financial intermediation, which raises domestic saving and enhances access of firms to domestic finance. In investment-constrained economies, the direct effect of this on aggregate investment will be nil or small. But an increase in economy-wide saving does have an effect on the real exchange rate, which is the crucial intermediating variable in our account: it leads to a depreciation of the home currency in real terms. (A rise in domestic saving relative to investment reduces net capital inflows.) Therefore improved financial intermediation will in fact boost domestic investment and growth—not necessarily through the cost-of-finance channel, but through the profitability channel.

This is shown in Figure 6 (building upon Figures 3a and 3b). An increase in savings (rightward shift of the supply of investible resources schedule) in an investment constrained economy will not lead to an increase in investment, merely to a reduction in foreign savings (i.e. the equilibrium will remain at B but inflows will be lower by the amount AC). However, the reduction in foreign savings will induce a real depreciation (shown as a rightward shift of the investment demand schedule) so that in the new equilibrium investment is greater (the equilibrium shifts from B to D) despite an unchanged cost of finance (which continues to be the world real interest rate).

The key point is that there is a crucial difference between domestic and foreign finance: improvements in the former depreciate the real exchange rate, while improvements in the latter appreciate it. Indeed, the evidence in PRS suggests that for any given level of investment, the more that is financed by domestic savings the greater the long-run growth.

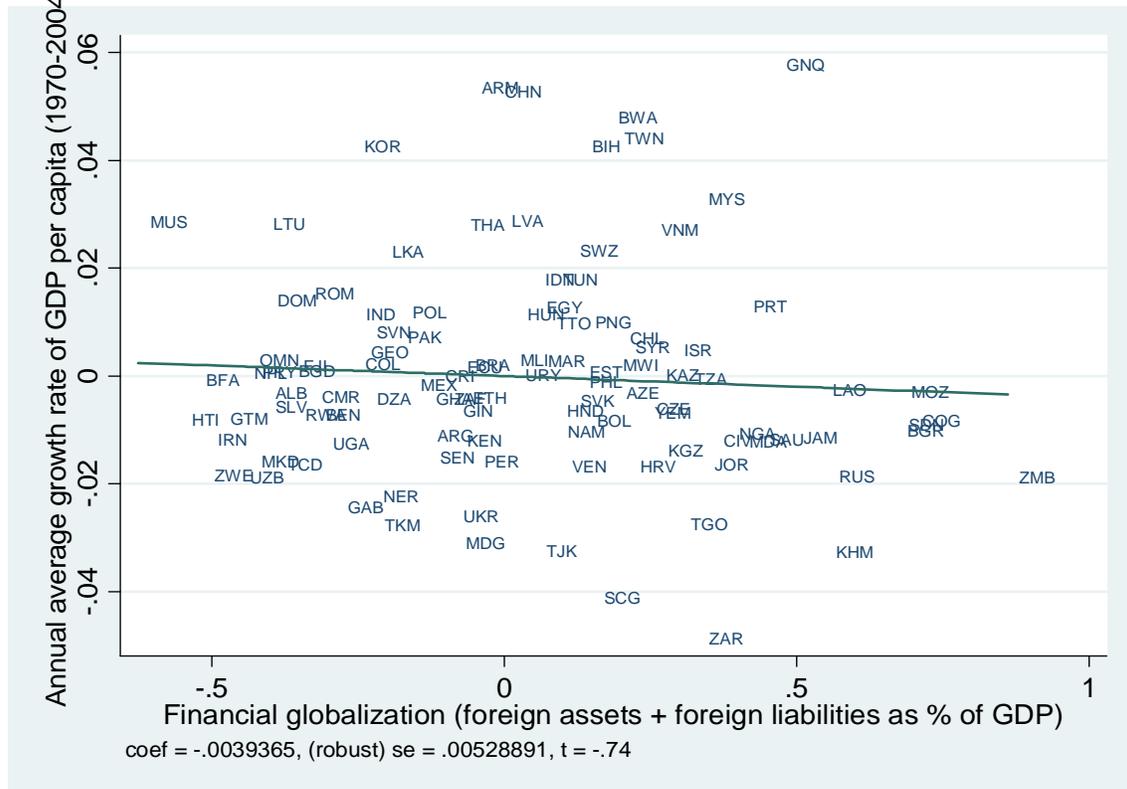
We have one final remark on the composition of capital flows. A lot of the literature makes a distinction between the effects of foreign direct investment (FDI) and other flows on growth, as well as between the effects of debt-related flows and others, on the likelihood and severity of financial crises. These distinctions are important and need to be made depending on context. On crises, which we have not explored in this paper, debt instruments may well be more risky than other flows. On growth, the distinction to make is between the real transaction and its financing counterpart. FDI is first and foremost a real transaction, involving the transfer of technology and skills, and we would agree that this is highly desirable for developing countries. But this real transfer may or may not be accompanied by a capital inflow. Our macroeconomic concern becomes relevant when, in fact, there is an inflow, especially a large one; in such an event, distinctions between types of inflows become less important as long as they have similar effects on the exchange rate.

IV. Concluding remarks

In the wake of the sub-prime financial crisis, the claims that recent financial engineering has generated large gains are sounding less plausible, and it is becoming clear that domestic finance will come under closer scrutiny. On the international front, even leaving financial crises aside, it seems increasingly clear that the benefits of financial globalization are hard to find. Financial globalization has not generated increased investment or higher growth in emerging markets. Countries that have grown most rapidly have been those that rely less on capital inflows. Financial globalization has not led to better smoothing of consumption or reduced volatility. If you want to make an

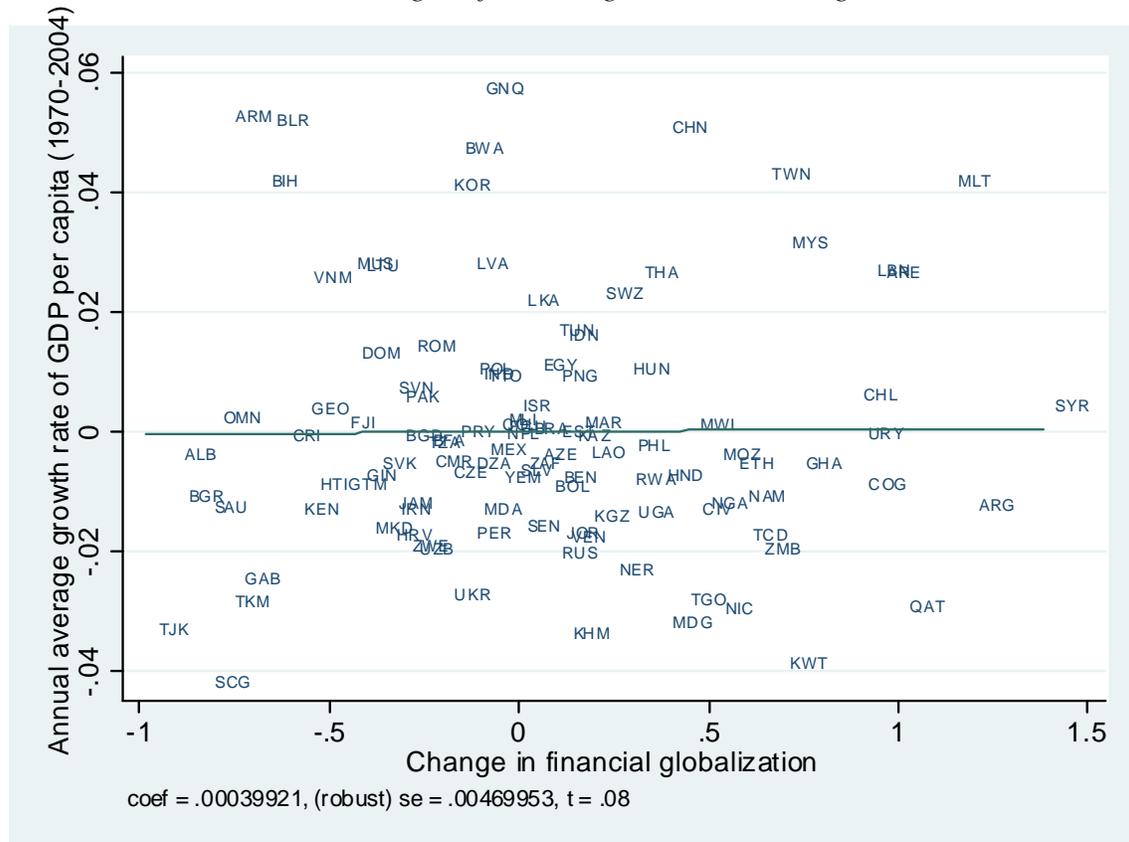
evidence-based case for financial globalization today, you are forced to resort to fairly indirect, speculative, and, in our view, ultimately unpersuasive, arguments.

It is time for a new paradigm on financial globalization, and one that recognizes that more is not necessarily better. As long as the world economy remains politically divided among different sovereign and regulatory authorities, global finance is condemned to suffer from deformations far worse than those of domestic finance. Depending on context and country, the appropriate role of policy will be as often to stem the tide of capital flows as to encourage them. Policymakers who view their challenges exclusively from the latter perspective will get it badly wrong.

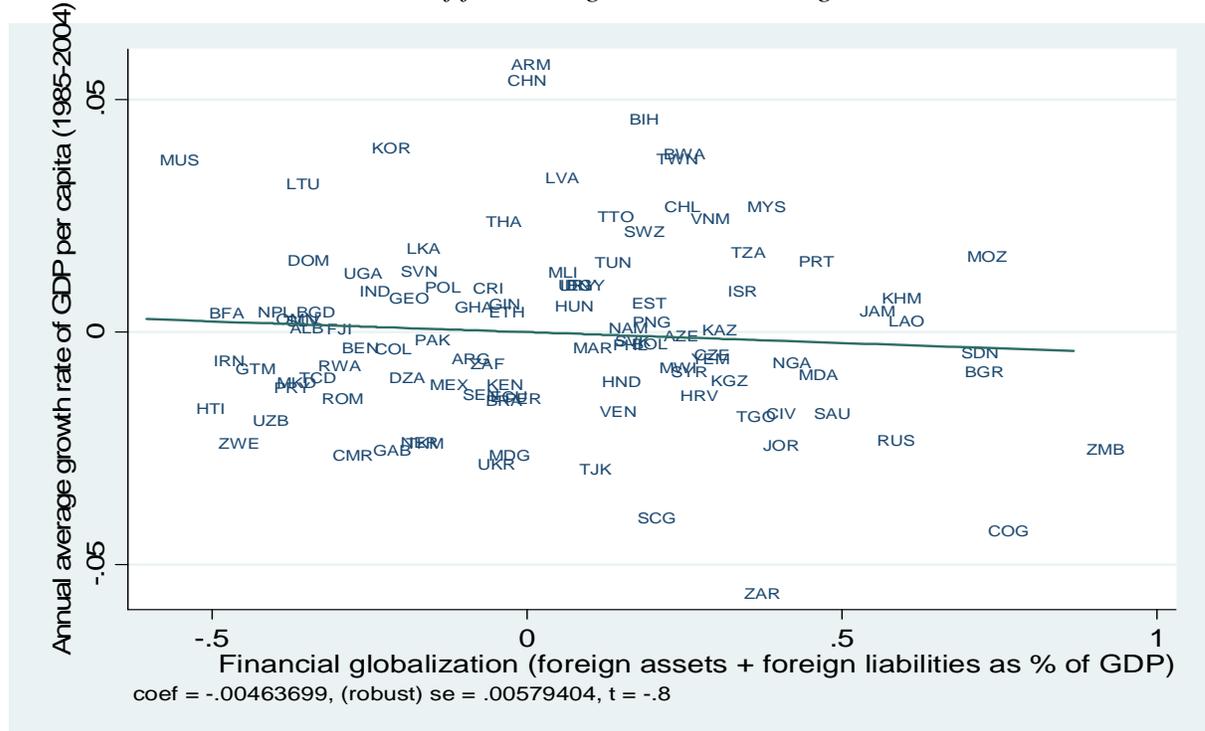
Figure 1: Financial globalization and growth (1970-2004)*Panel A: Level of financial globalization and growth*

The relationship shown above is conditioned on size (measured as the log of population). The sample of 105 countries excludes OECD countries and, for presentational reasons, 14 developing countries that have very high levels of financial globalization. Including these countries, and not conditioning on size, does not affect the relationship shown above. Growth rate is from the Penn World Tables, v 6.2 and the financial globalization measure is due to Lane and Milesi-Ferreti (2006).

Panel B: Change in financial globalization and growth

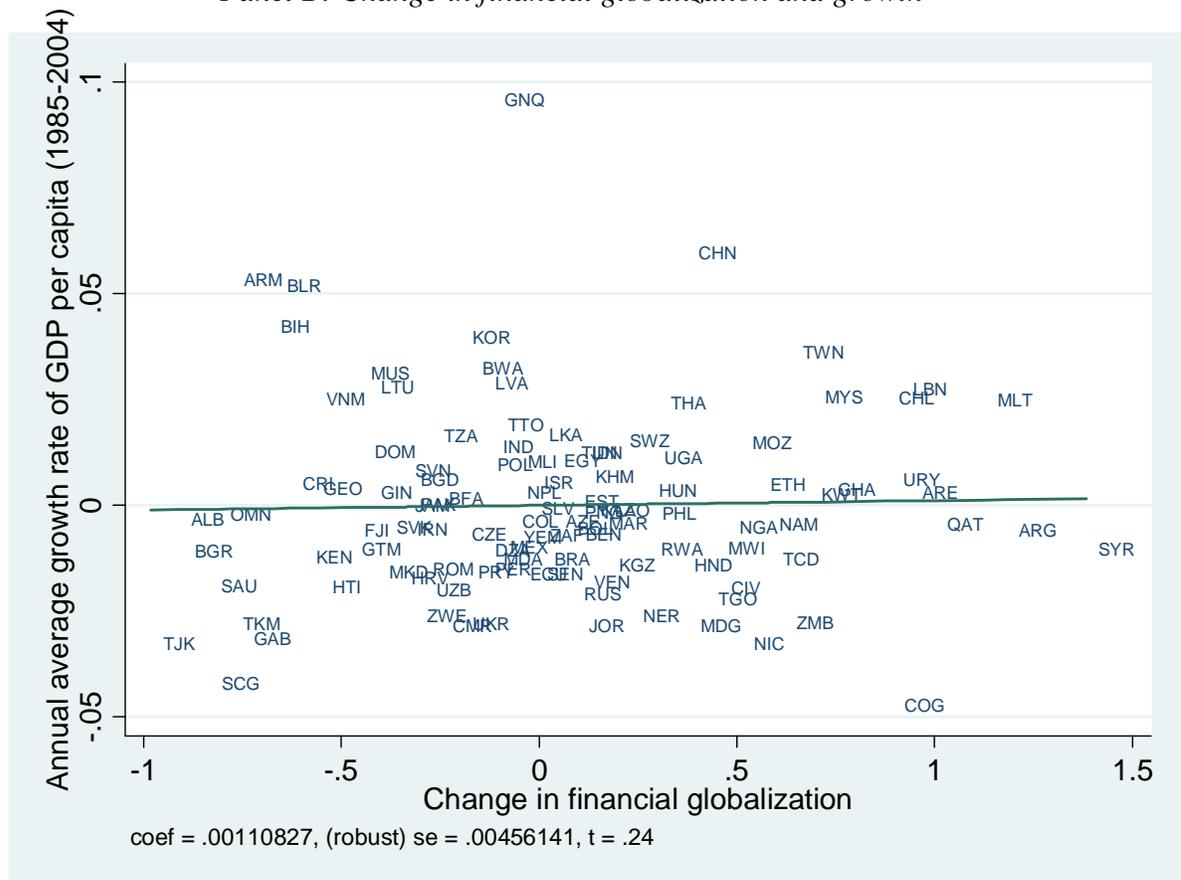


The relationship shown above is conditioned on size (measured as the log of population). Change in financial globalization is measured as the difference between the average level of financial globalization for 2000-2004 (or for the 5 years closest to these dates) and the average level of financial globalization for 1970-1974 (or for the 5 years closest to these dates). The sample of 110 countries excludes OECD countries and, for presentational reasons, 9 developing countries that have very high changes in the levels of financial globalization. Including these countries, and not conditioning on size, does not affect the relationship shown above. Growth rate is from the Penn World Tables, v 6.2 and the financial globalization measure is due to Lane and Milesi-Ferreti (2006).

Figure 2: Financial globalization and growth (1985-2004)*Panel A: Level of financial globalization and growth*

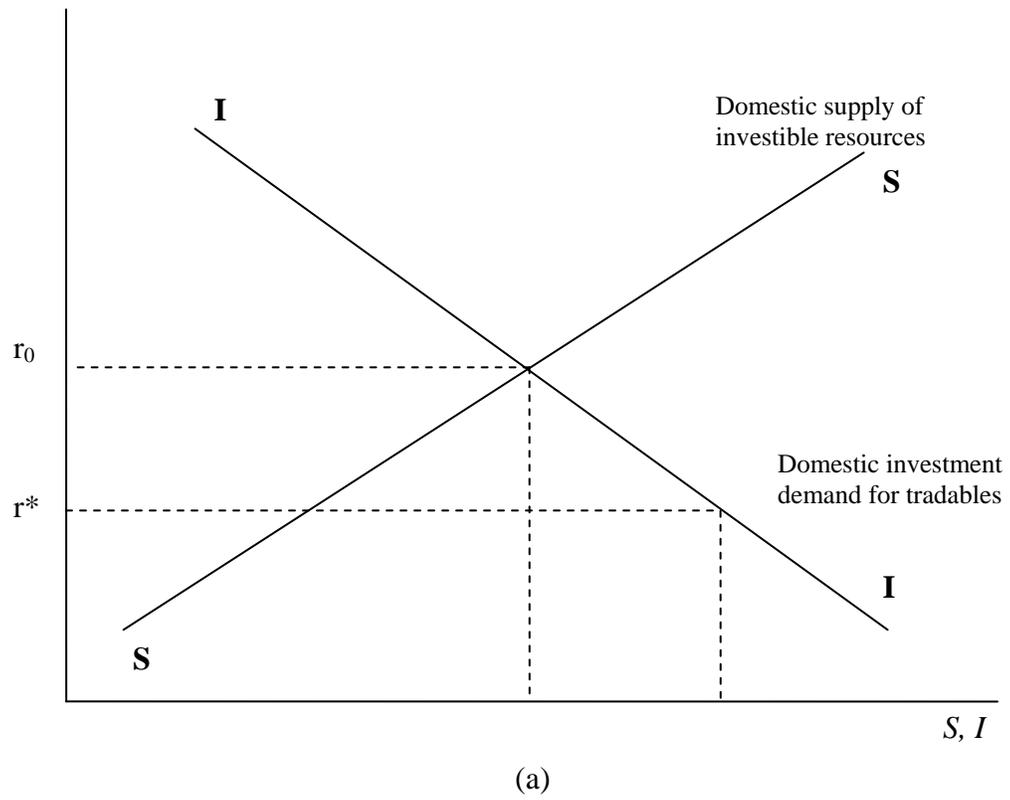
The relationship shown above is conditioned on size (measured as the log of population). The sample of 110 countries excludes OECD countries and, for presentational reasons, 9 developing countries that have very high levels of financial globalization. Including these countries, and not conditioning on size, does not affect the relationship shown above. Growth rate is from the Penn World Tables, v 6.2 and the financial globalization measure is due to Lane and Milesi-Ferreti (2006).

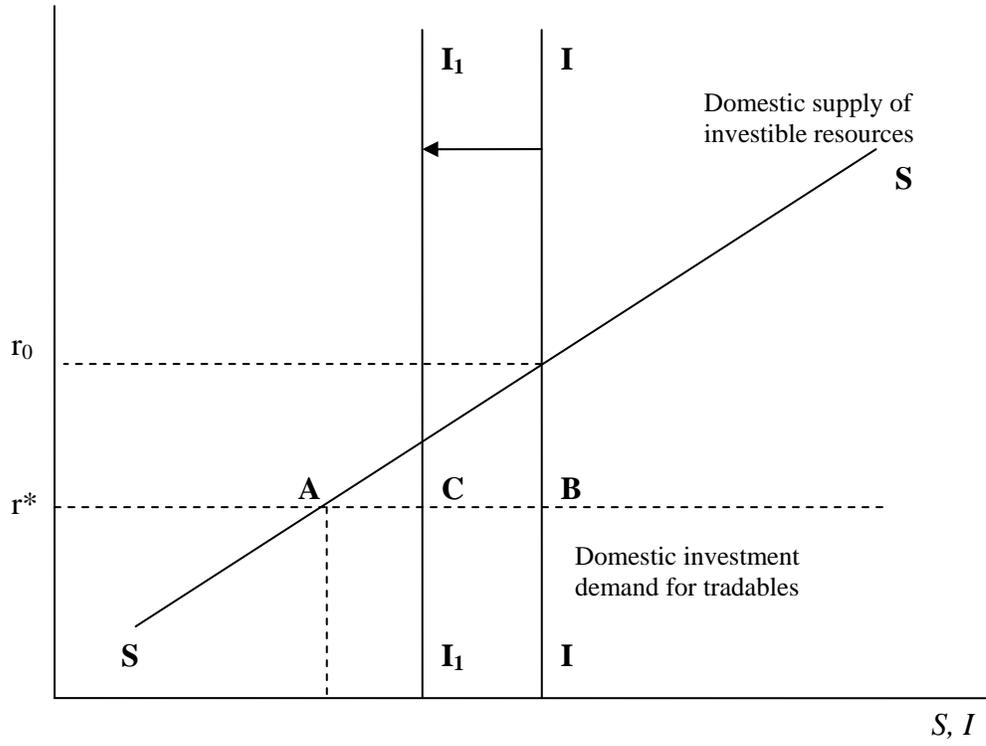
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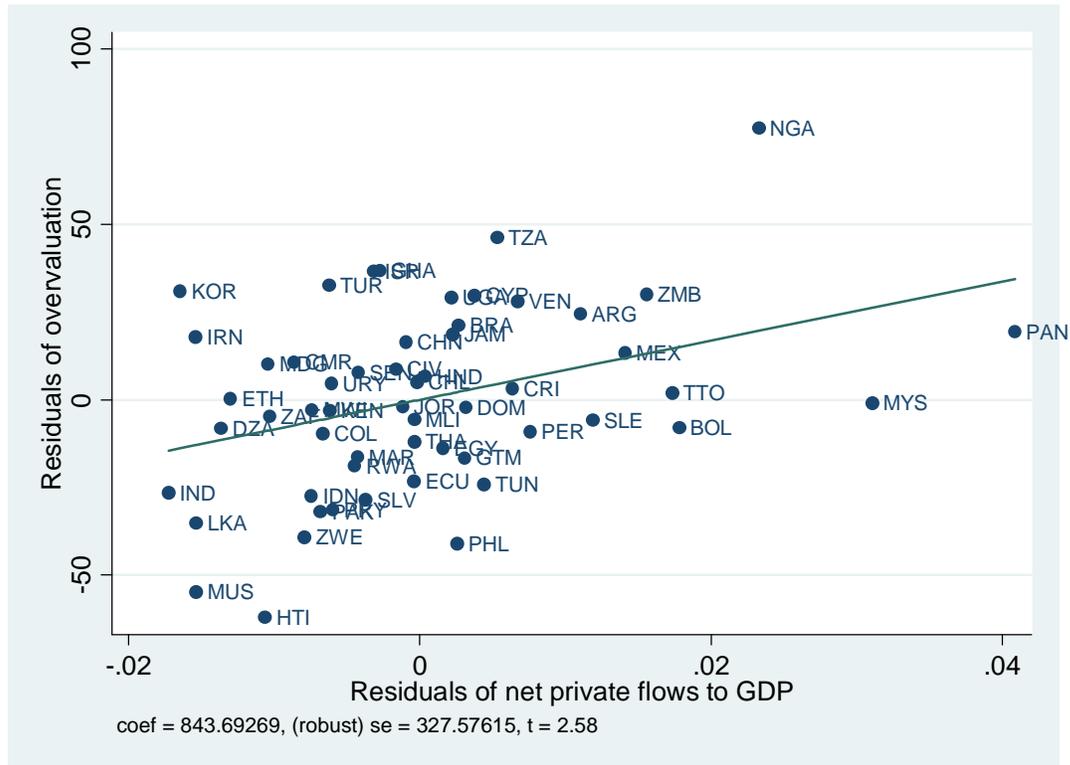
Figure 3: Savings and Investment-Constrained Economies and the Effect of Financial Globalization



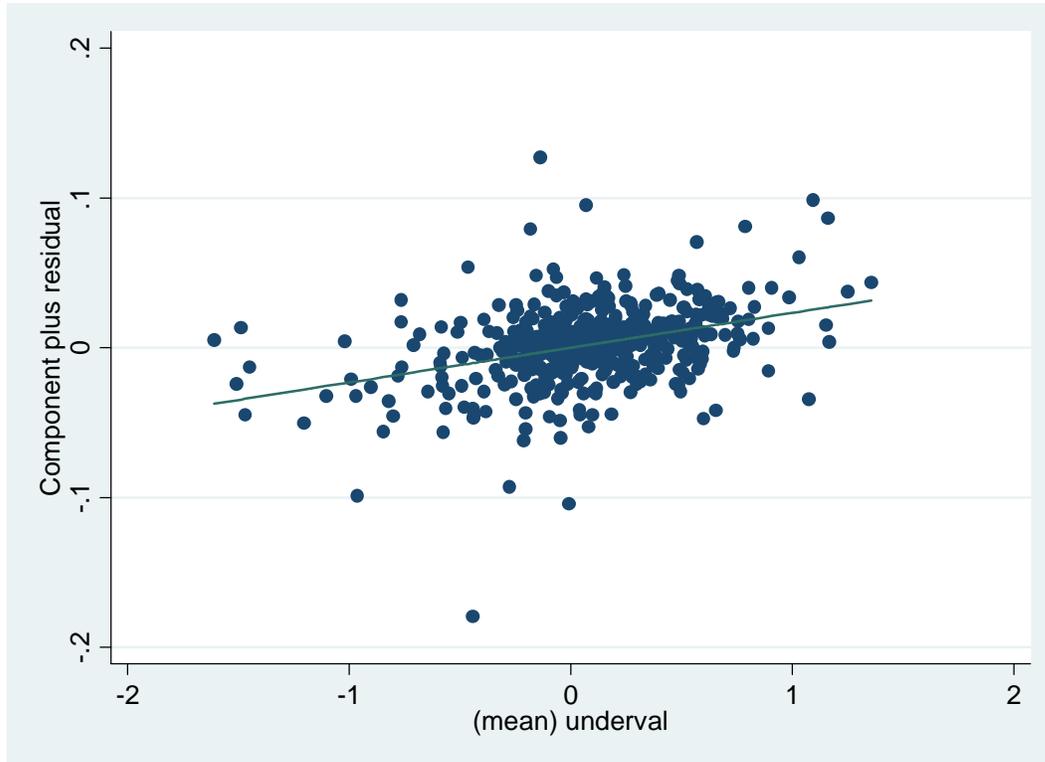


(b)

Figure 4: Overvaluation and Capital Flows, 1970-2004

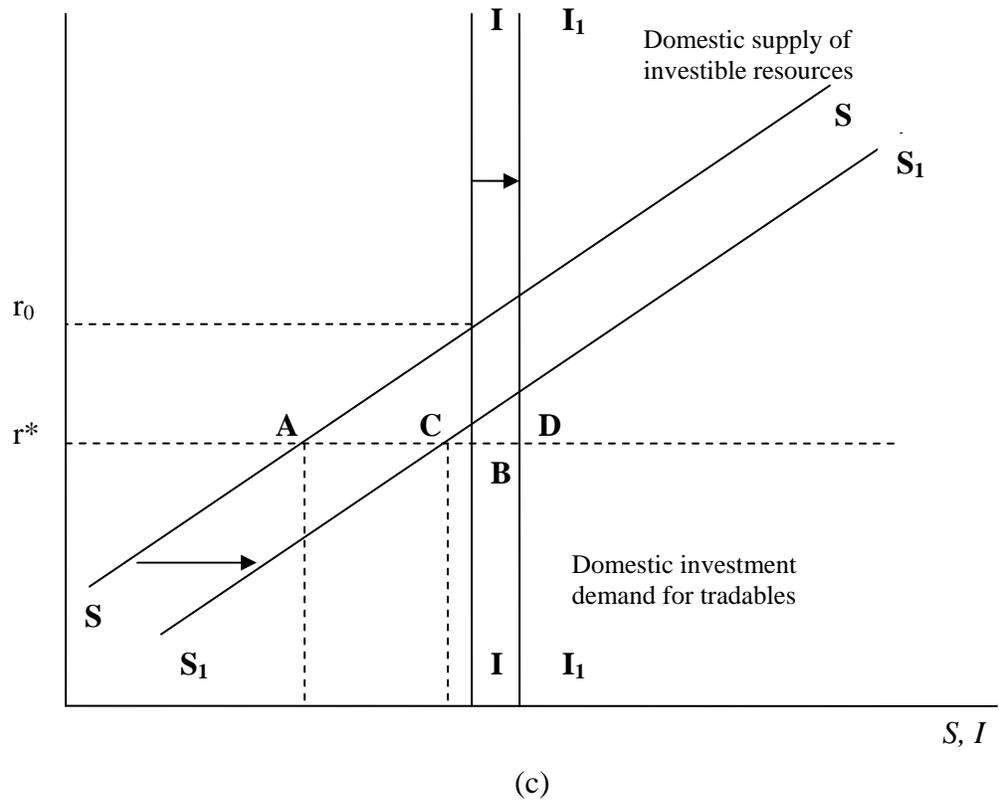


Partial relationship between a measure of overvaluation of the real exchange rate and net private flows, comprising portfolio equity, debt, and FDI, (controlling for demographics and a dummy for oil exporting countries). Reproduced from Prasad, Rajan and Subramanian, 2007.

Figure 5: Economic Growth and Undervaluation of the Real Exchange Rate

Partial relationship between a measure of undervaluation of the real exchange rate and growth rate of per-capita GDP (controlling for initial income and country and period fixed effects). Data are for developing countries and cover a panel of five-year averages from 1980-84 through 2000-04. Based on Rodrik (2007).

Figure 6: Effect of Increase in Domestic Savings



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