

Comment on Caballero, 'Structural Volatility in Latin America'

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(Forthcoming, Brookings Press)

Panel Meeting, Economia, Journal of the Latin American Economic Association

New York University, May 12-13, 2000.

I applaud the approach of this paper. It does not suffer the common habit of some finance experts writing on the subject of volatility, who, unable to explain why market prices go up or down, are content to identify time periods or places when volatility is high. The approach taken by the author is to summarize earlier theoretical work of his, and then to try to relate it to recent events in three case studies (Argentina, Mexico and Chile). This is a difficult challenge for any theorist. I will concentrate my comments on the case-study application, which takes up most of the paper.

I agree with the opening observation that desirable economic reforms will not necessarily lead to a decline in fluctuations. They may in fact lead to an *increase* in volatility, particularly in the case of financial liberalization. (The paper skirts around the hypothesis that in the presence of an underdeveloped domestic financial system, international financial integration can be on net harmful.) The United States during its historical phase of high-growth industrialization suffered regular financial and economic collapses, which involved much sharper falls in output than we would be prepared to tolerate today.

I agree also with the recurrent theme that international financial markets in practice fail one of their prime assignments, smoothing fluctuations, and that this failure is acute in the case of emerging markets. But I have some quibbles. Two of the pieces of evidence that Caballero has to offer are (1) the Figure [25] showing that Chile's GDP is sensitive to the world price of copper, and more sensitive than Norway's GDP is to the price of oil or Australia's GDP to the price of coal; and (2) the "small size of Latin American current account deficits relative to the neoclassical benchmark." My smaller quibbles consist of two questions concerning the first piece of evidence: Isn't copper a substantially higher share of Chile's economy than oil is of Norway's or coal of Australia's? And shouldn't one look for evidence of smoothing in countries' consumption, rather than their GDP? But no matter, I think the point is correct: financial markets do not allow developing countries to smooth out fluctuations in their living standards as they should.

I think the situation is worse than this however. Not only are net capital inflows inadequately countercyclical, they are actually procyclical. It is not just that current account deficits are small in Latin America (item 2 above), they move in a direction that destabilizes consumption relative to output rather than the reverse. The money floods in to finance current account deficits in boom times, and rushes out during recessions. Worse still, the evidence is mounting that these variations in capital flows are the *origins* of the economic fluctuations, at least in a proximate sense. One could readily attribute the crises that surfaced in Mexico in 1982 and 1994 to bad macro policies (particularly in the first crisis), to bad luck in terms of local political instability (particularly in the second case), and to increases in US interest rates (in both cases). But the crises that surfaced in East Asia in 1997 had no such evident proximate causes, even if one subscribes to the view that their deeper origins lay in structural flaws in their economies.

My final point on this issue is that the small size and procyclicality of net capital inflows is not just a problem of developing markets. It is also a phenomenon of industrialized countries with highly developed financial markets. That trade balances go into deficits in expansions and surpluses in recessions is one of the most robust empirical regularities of open-economy macroeconomics. So one must begin with the evident failure of the theoretical paradigm -- "the neoclassical benchmark" -- that models current accounts as the outcome of intertemporal optimization.

Who is to blame for these crises? It has become customary to speak of three generation models of speculative attacks. The first-generation models of speculative attacks point to overly expansionary macroeconomic policies. But countries have "solved the fiscal problem" (a claim of Caballero's that would be easier to sustain fully in East Asia than in South America). The third-generation speculative attack models blame banks and other "crony capitalists" who partake of moral hazard. I agree with Caballero's implication that this part of the story, but not the whole story: "If not only misbehavior and corruption, what else?" This leaves the villains of the second-generation models: speculators and other participants in international financial markets that are prone to multiple equilibria. Herd behavior and panics can bring about a crisis even without a change in fundamentals.

When capital inflows turn sharply to capital outflows, it is difficult to disentangle the roles of domestic vs. foreign residents. The author offers one small piece of evidence that could be interpreted as pointing the finger at foreign residents: that equity prices for large companies, counterintuitively, are more volatile in emerging markets than are prices for small companies. I would offer a piece of evidence that seems to me a bit stronger, although still very far from conclusive, and that goes the other way. At the time of the December 1994 Mexican peso crisis, the price of a basket of stocks in Mexico City turned down sooner and more strongly than the New York-traded price of the country fund that consisted of the identical basket of stocks.¹

¹ However the country fund statistics suggest that the subsequent contagion to the rest of Latin America does indeed come via New York. Frankel and Schmukler (1996, 1998).

A key external influence on the financial fortunes of emerging market countries is the U.S. interest rate. Caballero notes that interest rates in Argentina, Brazil, and Mexico -- and even Chile -- are more volatile than the US corporate interest rate. But, again, it is worse than that, in the sense that the Federal Reserve is often the source of the fluctuations. When the US central bank raises the Fed Funds rate, interest rates tend to rise worldwide, and more so in Latin America than within the United States. This factor is one that the author neglects in his description of the origins of the Mexican peso crisis.²

Other factors that belong on the list, beyond the familiar macroeconomic questions regarding exit from an overvalued exchange rate target, are political. They are: the Chiapas uprising in January 1994 and assassinations of PRI leaders, which scared investors; the election timetable which encouraged the authorities to sterilize reserve outflows (or more-than-sterilize, in the case of M2); and the change in personnel with the new administration in December 1994, which may have led to the so-called “botched devaluation.”

Let us now turn to the implications for policy, divided into long-run structural solutions and short or medium run macro policy. Caballero tentatively places in the structural category, in addition to the usual proposals to strengthen the international financial architecture (banking supervision, corporate governance, etc.), the issue of Chile-style capital controls. I agree with the appraisal that under certain conditions, such penalties on short-term inflows into emerging markets can play a useful role in changing the composition of capital inflows, even if they have little effect on the total inflow, and even if financial markets increasingly find ways around them with the passage of time. But I would like to offer my own version of their role. It is that they be used, not permanently, nor in a continuous role that diminishes gradually as financial markets develop, but rather episodically. By this I mean that they should be used temporarily, at a certain stage in the boom-bust cycle. When the first lines of response against large short-term capital inflows -- sterilized intervention and appreciation -- are not helping, and yet it is still not clear if the inflow is going to finance high-return investment, it may be time to try a year or two of Chile-style penalties. It is a way of “playing for time.” In this view, capital controls are a tool of short-term macro policy, not a structural solution. In making such recommendations, one must be wary of the danger that a sanctioning of capital controls will encourage their indiscriminate use, for example, by policy-makers seeking protection from the international consequences of their own unsound policies.

Finally we come to contingent rules, which Caballero favors for fiscal policy, labor contracts, and the exchange rate. In the case of the exchange rate, he presumably has in mind an escape clause that specifies what kind of external will trigger a devaluation. As he recognizes, such contingent rules must be simple and easily verifiable if they are to work. Unfortunately, my judgment is that this makes the proposal impractical in most cases.

² It is striking that flexible exchange rate countries, such as Mexico more recently, have interest rates that can be even more sensitive to the US Fed funds rate than do countries, such as Argentina or Panama, that use currency boards or dollarization to tie their monetary fates rigidly to the US (Table 1 in Frankel, 1999.)

The idea is to pre-specify that changes will be linked, in particular, to exogenous changes in the terms of trade. I do think that Chile should issue bonds indexed to the price of copper and Mexico to the price of oil, and I am not sure why this long-standing proposal has met with so little successful implementation.³ It would be an efficient means of risk-sharing. It would make “private sector burdensharing” automatic: in the event of a collapse in the terms of trade it would avoid the excruciating difficulties of collective twisting of arms of bankers or bondholders to allow “voluntary” rollovers or stretch-outs.

But the lack of success is easier to understand in the case of most other countries, where a single mineral commodity with an exogenously-determined and easily-verified price does not occupy such a large share of exports as it does in Chile. It is also easier to understand in the case of budget rules, labor contracts, and exchange rate regimes, where the typical player has a lower level of sophisticated understanding than a finance specialist. Even such highly skilled economists as we ourselves cannot agree on the causes of recent crises, whether they are due to exogenous shifts in the terms of trade, domestic policy failure, or other causes. We cannot even agree on the causes after the fact, let alone in the heat of the moment. Was the 1982 international debt crisis in part caused by a fall in international commodity prices? Then why did it originate in Mexico, a commodity exporter? Was the 1997 East Asia crisis in part caused by a fall in international prices of semi-conductors and other manufactured products? If so, what are the corresponding products that increased in relative price? These are not questions that I know the answer to. But that is the point. If we don't know the answers, then contingent rules are not easily verifiable. There is never a single agreed-upon measure of the terms of trade. And every country that devalues, regardless the cause, exhibits a “deterioration in its terms of trade” by most measures. The central bank when it devalues will always point to circumstances beyond its control, and many in the markets will always consider the devaluation a failure of the government's will to abide by its commitment.

The author is on the right track: we need more research into what sorts of rules and contracts are verifiable by the person in the street. But we need to avoid the temptation of thinking that because we have written down an exogenous disturbance term in a theoretical model, that the model and disturbance are necessarily knowable and observable in the real world.

³ E.g., Lessard and Williamson, (1985).

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