Comments on Glick and Hutchison

"Banking and Currency Crises:

How Common are Twins?"

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I think of there as being four broad objectives for the emerging quantitative work on currency crises. The goals of this research program can be summarized in a series of questions:

1. What are the *determinants* of crises?
2. Can one *predict* crises or construct “early warning systems”?
3. Is there joint causality across countries, i.e., *contagion*? What are the channels?
4. Is there *joint causality* between banking and currency crises?

In this fine paper, Glick and Hutchison focus on the last issue, the problem of joint causality. This is an important issue, and the stakes are high for their work. A finding of joint causality between banking and currency crises means has strong implications for our understanding of the causes of both, and, more importantly, for policy actions to prevent future crises. They also ask an important question which has thus far not been directly addressed in the literature, namely “does country aggregation matter?” The extant literature dis-aggregates by time and sometimes the degree of capital mobility, but most papers either use OECD or emerging market data, but not both. Glick and Hutchison emerge from their extensive empirical analysis with two key conclusions. First, there is in fact joint causality; more specifically banking crises tend to cause currency crises. Second, emerging markets are different. Both of their findings are plausible and sensible! This adds to the appeal of their paper, though it makes the job of the discussant that bit more demanding.

One important methodological issue rears its head at the outset. Can one do an investigation of the fourth issue without taking a strong stand on the first three issues? Glick and Hutchison are clear about their assumptions vis-à-vis the rest of the research program. In particular, they assume that a) know determinants of banking and currency crises, b) crises are predictable; c) contagion is irrelevant. Fair enough. But one immediately asks: how sensitive are the results to these assumptions? If one disagrees with the assumptions, does it make suspect the conclusions that follow?

It is worth exploring this issue a little more deeply, since there is no consensus in the area at large. There is much disagreement about the determinants of crises; few "fundamentals" such as loose monetary or fiscal policy are present in most crises. There is even more dispute about the efficacy of early warning systems. My view is that mechanistic systems do not have a good *ex ante* track record. That conclusion is consistent with the evidence presented here: all the predictions of crises are very low in Glick and Hutchison. But there is certainly much dispute. Finally, no one disagrees that there are clusters of crises in 1982, 1993, 1995 and 1997. But the interpretation of this clustering is far from clear. Personally, I believe that there is contagion, and it is simply not the case that common external shocks cause whole regions to plunge into crisis simultaneously for the same reason. Thus, I think of it as a mistake to ignore foreign effects in general. But even if one believes that what looks like contagion is actually a series of common external shocks (such as US or German interest rates and/or OECD growth), can one really discuss crises without analyzing these external phenomena?

I have a number of smaller issues that I fully imagine the authors will handle in future research. The authors place a good deal of emphasis on their country dis-aggregation scheme, appropriately so in my view. Still, it would be interesting to add more economic meat to the scheme itself, which currently seems somewhat arbitrary. One could imagine dis-aggregating countries in many different ways: why is this appropriate? In particular, distinguishing between "Emerging Markets" and "Developing Countries" on well-specified economic criteria seems like a goal for future work. There may be better ways to dis-aggregate groups of countries, and establishing the sensitivity of the aggregation scheme is also a worthwhile objective.

Another issue is that the currency crisis construction scheme is multilateral. This is a novel approach. Most crises affect countries in fixed bilateral rate regimes; one thinks of the ERM crisis which is essentially a crisis which centered on Germany. Similarly the Mexican and Asia crises also centered on exchange rates which were formally or informally pegged to the American dollar. In this regard, it is also interesting to note that the multilateral scheme leads a number of countries (Cameroon, Equatorial Guinea, Grenada, Guinea-Bissau, and Swaziland) to register currency crises while they were within currency unions.

A related issue is that the scheme which builds the measures of currency crises measures outliers vis-à-vis *country-specific* distributions. Do we really believe that each country should have approximately the same number of crises? Pooling across countries allows one to give Argentina a disproportionate share of crises, while allowing stable countries like the Netherlands to register long periods of tranquility. Glick and Hutchison may also want to relax other aspects their methodology. For instance, they may want to focus on predictions other than one year in advance. I also recommend that they shy away from focusing on the real exchange rate as a determinant of currency crises. It is inherently difficult to measure, and since their definition of a currency crises almost entails depreciation, over-valuation is likely to precede crises.

Still, these are small issues. Essentially, they point to the enormous potential for future research. Glick and Hutchison have advanced the research program in the area, and I look forward to more of their work in the future.