

Comments on “Is the Crisis Problem Growing More Severe?”

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Bordo, Eichengreen, Klingebiel and Martinez-Peria have set themselves a difficult task, namely to answer the question that their title asks. Since their title is a question, I assumed that they followed the standard in economics, namely to answer it in the negative. And indeed, that is mostly true; they seem to believe that the crisis problem is not growing more severe for the most part. This seems to be a reasonable answer, and I have no reason to disagree with them.

The Crisis Crisis

Let me say at the outset that the question asked in this research is an important one. That is, the paper is well motivated. The crisis literature is in crisis. Understandably enough, there is an enormous interest in financial crises from the policy domain. Crises have important effects on the economies (especially of developing countries). Governments fall and rise because of the vagaries of capital flows. So it is scarcely surprising that so much work has been done in the area. Yet the huge amount of recent research has left us with remarkably few concrete results. As a profession, we simply do not have a very good understanding of what causes crises (especially currency crises). We are therefore unable to provide policy-makers with good crisis prevention techniques, early warning systems, and so forth.

Now this state of affairs may not be a surprise to some, and it may be inevitable. For theoretical reasons, some crises may be unpredictable, as we discovered in the “second generation” literature. But more disturbingly, looking back on the 1990s, each wave of crises in the 1990s seems to have generated a lagging wave of theoretical papers meant to explain what just happened and why we collectively missed the now-obvious symptoms. Even more distressingly, I feel that a fair look at the evidence indicates that we cannot predict the onset of crises even *ex post* using models and data that were unavailable at the time. Theory is ahead of empirics in this area of economics, but both are in terrible shape.

Faced with this miserable state of affairs, researchers have gone to lengths to broaden the data sample being examined. We started with individual country case studies of currency crises. Next came post-war panels of industrial countries. Then we added developing countries. Banking and twin crises were added to the list next, allowing even more observations to be modeled. The logical end to this broadening of the data set is deepening it by going further back in time, and this is precisely what Bordo et. al. do.

I applaud their efforts for two reasons. First, we should certainly try to exploit all available data to try to understand financial crises. As the authors state, using more of the past is surely better than using less. Second, the historical data must surely be the final

¹ I should note that I am a past, present, and (hopefully) co-author of Eichengreen ... as is his other discussant, the editor, ...

frontier. If we don't appreciably add to our stock of knowledge with this data, perhaps it would be best if we collectively took a breather. A break from statistical analysis might add the perspective necessary to figure our way out of the current morass.² In this somewhat pessimistic conclusion, I also agree with the authors.

The Information

The authors interpret their findings as saying that there's nothing new under the sun; like the poor, crises have always been with us, and will continue to be with us. They then claim that the frequency of crises has risen, that crises are more costly, and that twin crises are more prevalent. The exceptional period is the interwar period, hardly a typical experience. They do this based on a huge amount of empirical work, covering four historical periods covering over fifty countries and 120 years of data, dis-aggregated over three types of crises, and covering the cost (in terms of foregone output) of crises as well as their incidence.

I interpret their results somewhat differently. Suppose I was to claim that the compelling lesson from Bordo et al's look at the historical data is that there is remarkably little to be learned from the past about financial crises. Would this really be at gross odds with the data? The crisis frequencies in Table 1 hardly point to a noticeable trend in crisis incidence (and this impression would be strengthened with some standard errors). Ditto the average recovery times of Table 3 or the output losses of Table 4. In fact the heterogeneity in crises makes the standard errors in Tables 3 through 8 so large as to make any strong statements about long-term trends infeasible.

The authors confirm that it is difficult to determine the causes of crises and therefore to construct early warning systems. Fair enough. But they also argue that countries with universal banking experience more crises, and that pegged exchange rates and capital mobility are dangerous since they add to the severity of crises. These are all reasonable points, and I am sympathetic to many of them. However, the evidence presented does not obviously corroborate the conclusions drawn by the authors. When I look at Table 11, I see mostly statistically insignificant coefficients for universal banking, pegged exchange rates and capital mobility. The same is true of Table 14. It is true that some positive results appear, but they seem to be fragile. Thus, while I agree with the main thrust of the authors' conclusions, they should be careful not to overstate the results of their extensive econometric analysis.

There is a special reason to be cautious of the author's conclusions of the costs of crises. When we think of the American recession of 1990-91, we think of the Gulf War and the associated shock to the price of oil, in unfortunate conjunction with tight monetary policy intended to lower inflation. Similarly, we refer to the Volcker recession of the early 1980s, the shock of German Unification and its consequences for German interest rates and its EMS partners, to the bursting of the Japanese asset price bubble in 1989, ..., and so forth. In each case, we think of business cycles as having causes that we can usually point to. But none of these structural effects is controlled for in the

² It would also improve the quality of life for editors of international journals.

regression analysis that Bordo et al do. That is, the econometric results from Table 4 on don't control for the standard determinants of recessions (monetary shocks, the oil price, productivity effects, and so forth). Since the latter are quite plausibly correlated with crises, one has to think of the paper's findings on the cost of crises as being preliminary.

One final point on methodology. What is the argument for running regressions (on e.g., crisis determination or cost) over different periods of time? If one learns that e.g., inflation was more important as a crisis determinant in one period than in another, where does that leave us? And usually the results don't look different. A better way to proceed might be to pool all the data, and then test for sub-sample stability. That is, the authors should consider proceeding on the hypothesis that the crisis problem is not growing more severe, confirm this (or not), and then ask what is it about the behavior of the crisis determinants that's responsible. I assume they'd conclude that growing capital mobility is the primary culprit, but that remains to be determined. It might make for easier exposition.

Where do We Stand?

Current data show us remarkably little about the causes of crises and therefore about crisis prediction and prevention. The same is basically true of pre-WWII data. So the first message for policy makers from this paper is basically negative. We simply don't currently have the ability to determine what causes crises. This negative conclusion carries with it a host of negative corollaries. If we can't understand the determinants of crises, we can't predict them with mechanical early warning systems. Preventing crises is that much harder.

One of the real contributions of this paper is that it pushes us away from trying to explain crises and towards the study of what causes crises to be more or less costly. This is surely a worthwhile goal, and I applaud it without reservation. Even if we can't prevent the disease, knowing how to reduce its impact is an important objective. The reorientation of the literature has only begun with this paper, and I look forward to more work in the area.³

What do we know about crises? They are not all alike. They never have been. The heterogeneous nature of financial crises is their defining characteristic. It explains why it's so hard to model them empirically, and why new theoretical models seem always to be needed ex post to explain the latest outbreak. This may be cold comfort indeed for policy makers, but at least they can appreciate the opportunity this represents for researchers.

³ So long as it's not submitted to *JIE*.

Smaller Comments

These are mostly editorial, and are meant to improve the exposition of the paper. I won't present them, and so have written them quickly and informally.

You should summarize the main messages of the paper in both the introduction and conclusion.

Perhaps you should drop the interwar period (and only mention it in footnotes). Is it really a useful data point? It might be better to compare the gold standard era with the modern era and Bretton Woods, since they were similar in many ways. Simply drop the interwar period, since it's confusing, short and detracts from the analysis.

Your paper has far too many tables, most of which are unnecessary and distract from the paper. More is less here. It's very hard for a bureaucrat to look at one of your tables and get the big picture (and that's the audience you're shooting for). Besides you can always make a much longer version available on the web, and refer to it in the text.

I'd highly recommend using graphs to portray the descriptive statistics in Tables 1-8. Let me be explicit: use grouped bar graphs in multiple rows. Industrial countries get a cluster of 4 bar graphs next to each other; from left to right, they represent the frequency of banking crises during the gold standard, interwar, Bretton Woods and Modern eras. Repeat for currency, twin and all crises, for a total of four clusters. The next row is identical, except it portrays developing countries. Different tables represent different crisis metrics; frequency, duration, output loss and so forth.

Speaking of tables, why aren't there explicit chi-squared tests of independence used? It's important to support your argument, and I bet the confidence intervals are wide.

Why shift from frequencies to numbers of crises in Table 2? It's harder to interpret.

In Tables 1-2 you have time eras on rows; then you switch to using them in columns in Table 3. Poor presentation! Be consistent!

Your regression analysis is grossly over-represented. Most of your regressions fit badly, have few observations and mostly insignificant coefficients. So is it really necessary to show them? For instance, why have both parts of the pooled table of Table 9? Why not record one and discuss the results of the other in passing (or a footnote)? Ditto Table 10. Why record all the sub-period estimates in Table 11.a through 11.c? The reader stares at a sea of insignificant coefficients. Why not just go with the pooled results with regime-specific controls, and discuss the sub-periods as necessary? In fact, what is the point of the sub-sampling? And why bother with Table 11.a and 11.b at all? Where's the Table 11.c. regression with the data pooled from all eras?

In Table 9 pooled, are those t-statistics or standard errors? (Is there a mistake with the asterisk on crisis coefficients?) And why do some pooled regressions end in 1971 (e.g., Table 13.b) why others go through the end of the sample?

Is it “gold” (Table 9) or “pre-1914” (Table 10)? Be consistent! And it’s “output loss” not “output growth” (which can mistakenly imply that the crisis leads to a permanent change in the growth rate).

Incidentally, please lighten the line delimiters in the regression tables, or eliminate them altogether. And align table labels with tables (they’re currently on different pages).

Are the coefficients for the logits etc, slopes, derivatives, coefficients, or what?

That is: *clean up your tables!* I’m a sympathetic reader, but not everyone else will be.