Comment

Alan M. Taylor, University of California, Davis, NBER, and Centre for Economic Policy Research

I

It is a pleasure to comment on the paper by Qian, Reinhart, and Rogoff, which builds and expands on the many rich insights and the treasure trove of data developed by Reinhart and Rogoff (2009) in the course of writing their book *This Time Is Different: Eight Centuries of Financial Folly*. That book is a hard act to follow: it is no exaggeration to describe it as the most influential work in macroeconomic history of recent years, and fortuitously timed, it has deservedly won wide acclaim on its way to becoming a best seller.

Still, as the authors recognize, the project of understanding macroeconomic crises, their causes, and consequences is even now very much a work in progress. In contrast to what went before, the sudden wave of research in empirical and theoretical macroeconomics since the global financial crisis erupted in 2008 attests to a subject matter unduly neglected. Fortunately, such complacency is no longer an option, and a better understanding will have to rely on both inductive and deductive approaches to macroeconomics in which economic history will have a very important role to play (Eichengreen 2009).

The prime lesson of *This Time Is Different* and of this paper is that a retreat into introspection is not the only tool we have at hand to understand crises. Rather, with a record of such events stretching back over many centuries, we have an excellent historical laboratory in which to search for empirical regularities, stylized facts, and other clues that might lead to better-informed theoretical models and improved policy choices in the future. Indeed, this is an arena of scholarship in which economic history matters more than ever: crises of this sort are undoubtedly “rare events,” and so to gain any sort of traction with an empirical
investigation one needs many years of data and many sample countries to generate any sort of statistical power.

The novel contribution of this paper is to try to address a question left tantalizingly open by the book, perhaps because that very question is so hard to tackle that answers, if not elusive, can be at best only preliminary and somewhat speculative. As of a couple of years ago, this might have been cast as the question of whether countries can in fact ever “graduate” from being crisis-prone “emerging” economies to an “advanced” economy status in which, one might have presumed, they would be immune from such problems forever. Now, of course, our innocence is lost, and we realize that not even so-called advanced economies are immune from crises and that some countries apparently cured of the crisis problem can end up suffering a relapse. Thus, perhaps the notion of graduation is a mirage? Multiple advanced countries suffered very costly banking crises in 2008; some countries are now facing escalating sovereign debt, with markets pricing in nontrivial default probabilities; and as central bankers have embraced quantitative easing strategies, some fear that the current deflationary trends may yet quickly reverse and, even in a slump, return us to 1970s-style stagflation.

In our current conjuncture, this paper, which provides a retrospective examination of the crisis record and some exploration of the capacity of countries to graduate from a “serial crisis” mode to a more stable macroeconomic environment, is welcome as a valuable and pioneering contribution to our understanding. In these comments I will endeavor to summarize and comment on the main results and, along the way, note some unanswered questions and suggest potentially useful directions for what promises to be a very broad research agenda in the years ahead.

II

The paper starts by introducing the definitions of various kinds of crises and reviewing the time line of such events, focusing mainly on the last 150–200 years. Here the authors draw on the exhaustive crisis classification work undertaken in the book This Time Is Different. These definitions and classifications, in turn, build on earlier antecedents due to Kaminsky and Reinhart (1999), Bordo et al. (2001), and others.

The definition of a default or sovereign crisis is at once narrow and broad. It is narrow in the sense that only cases of external debt crisis are considered here. It would be instructive, though much more work, to expand this to generate a complete and accurate list of all defaults. As Reinhart and Rogoff have noted in their book, there is a rich and
understudied history of internal debt that shows that sovereigns have defaulted on domestic liabilities on many occasions, and the correlation with external default is not perfect. The distinction has perhaps mattered less in cases in which empirical macroeconomists have been focusing on emerging-market sovereign crises (e.g., the literature from the 1980s to the 2000s). For those cases, especially in a world of so-called original sin, the most significant debt burdens are those issued in foreign currency. However, after the global financial crisis, the specter of sovereign risk now hovers over advanced countries, where debt is issued mainly in local currency and may be held pari passu by domestic and foreign creditors alike. Thus, going forward, great interest attaches to the lessons that history offers about default in such supposedly advanced countries.

Yet at the same time, the default definition used in this paper is broad. It includes almost any deviation from the contractual terms, whether full-blown repudiation or some less consequential partial default. This problem applies to domestic and external defaults. In the space available, one cannot review a raft of cases, and indeed there has already been intense discussion—from working papers, to the letters page of the Financial Times, to the blogosphere—as to what really counts as a default. Nonetheless, the authors’ binary classification probably makes sense as a first cut at this problem, and it will be a task of future research to try to settle such disputes, explore more granular default definitions, and explore the robustness of the results under different definitions.

In terms of the failure of a nominal anchor, the preferred classification here is an inflation crisis, defined as inflation above 20%. Elsewhere, an alternative approach has been to rely on a currency crisis indicator, defining a crisis for the exchange rate as a steep depreciation event. In most, if not all, cases these two signals will agree. Since inflation targets have become the de facto if not de jure policy target in most advanced, and many emerging, economies, this way of defining the loss of a nominal anchor, as a breach of an inflation threshold, might be the preferred one.

Finally, to define a banking crisis the authors follow established convention and look for cases in which the banking sector is disrupted by widespread failures, bankruptcies or capital losses, or suspensions or else can limp through only with substantial official assistance. As with the default measure, this reduces a continuum of outcomes to a binary variable. And again some of the classifications are judgment calls. However, a binary classification is the right place to start any analysis, even if it is not the final word. In the future, researchers might want to examine more carefully the intensity of different episodes of banking distress to come up with a more subtle measure of banking crises.
With these definitions in hand some of the central results in the paper are presented in the form of time lines showing the occurrences of each type of crisis in each year in their figures 4–6, where the data are broken down into high-income (advanced) and middle-/low-income (emerging/developing) countries. In these figures some key results appear, some well known, others less so. The following struck me as noteworthy historical facts that every macroeconomist should know:

- Since the 1970s no rich country has defaulted, and such a period of 30–40 years without relapse is unusual in history.
- Severe global recessions are correlated with poorer countries’ defaults: the 1870s, the 1930s, and the 1980s.
- The classical gold standard era 1870–1914 stands out as the only period when inflation crises were largely averted in both rich and poor countries alike for an extended period of time.
- In the rich countries, since the end of the U.S. Civil War, extensive inflation crises have struck only three times: in each of the two world wars, but only once in peacetime, in the 1970s.
- In poorer countries, the widespread inflation crises of the 1980s have abated, but the current experience of more stable prices has not endured very long.
- There are virtually no financial crises in situations in which the financial system is small or weak (e.g., poorer countries in the nineteenth century).
- There are no financial crises in situations in which the financial system is repressed or tightly regulated (e.g., rich countries in the 1945–70 period).
- There was no sign of widespread inflation crisis in the 1930s, but banking and default crises were at their most intense in all of recorded history (something that is echoed in today’s tensions between price stability objectives and the use of easy money to relieve nominal debt overhang or perform bank rescues).

The other very interesting set of results appear in their figures 7–9: these show the frequency distribution for the length of observed periods of “tranquil time” (i.e., time between crises). These frequency distributions show some similarity across the different types of crisis, with a large share of events loaded in the 10–20 year range. Thus, we are led to look at 20 years without reversal as indicative of a country having nearly graduated, although given the “short” span of data at hand and given the
rare-event nature of the problem, we have very few cases of countries getting out to 30, 40, or 50 years without a crisis, whereupon we might think the odds of a reversal to be much lower. It is also evident that while richer countries have a lower propensity for inflation and default crises in the first 20 years after an event, the same cannot be said of banking crises, earning them the moniker of “equal opportunity menace.” Even before the current financial crisis it was true that advanced countries have had a much harder time escaping from this particular type of crisis.

Figures 11–13 look at the issue a different way, by showing years since the last crisis as of 2010. For default it is clear that poorer countries (median 14) are completely different from rich countries (median 105). For inflation crises the gap between the distributions is not as striking but still large (median 15 vs. 59). Prior to the 2008 financial crisis, the same might have been true for banking crises, but by now the two groups are almost indistinguishable on that metric.

There is considerable scope for further research here. One suggestion would be to simply apply standard tests for equality of distributions (e.g., Kolmogorov-Smirnov) to judge whether there are shifts across the two groups of countries. And then, partitioning the data by time period, one could ask the same question for different subperiods. Clearly, the slicing cannot be taken too far, given the small sample size. Another possibility is to look at conditioning the tranquil time on other country characteristics: in essence, moving to a formal duration model. Beyond the scope of this paper, this is surely a likely target for future papers by these or other authors.

One interesting conditioning variable could be macroeconomic volatility, measured in various ways, and perhaps instrumented by terms of trade volatility and/or institutions. Section VII of the paper touches on this topic, but the question provides enough of a challenge for a paper in its own right. It is indeed striking that for both rich and poorer countries, macroeconomic volatility has fallen since the 1950s, but for both sets of countries the incidence of banking crises has gotten worse. This poses a challenge for “real-side” explanations of banking crises and offers prima facie evidence that other factors matter—for example, changes in the structure of finance over the last 50 years due to, say, “innovation” or regulation/supervision.

III

This excellent paper builds on the very timely work in This Time Is Different. As the economics profession digests the lessons of the global
financial crisis, the stage is set for a renaissance of work in macroeconomic and financial history. To conclude these comments, I suggest two areas worthy of further consideration. Given recent events, I focus on the case of banking crises, which represents the major locus of attention in the current macrofinance debate.

First, I am sure the empirical finance and growth literature will now be revisited again. It was already the case even before the crisis that the linkages between financial liberalization and growth were subject to dispute and uncertainty (e.g., Kose et al. 2009; Rodrik and Subramanian 2009). In addition, we might also expect some further empirical exploration of the links between financial intensity and the volatility of growth (e.g., Ranciere, Tornell, and Westermann 2008) and the trade-offs associated with capital controls (e.g., Eichengreen and Leblang 2003). Do larger or more liberalized financial systems really deliver higher growth? Do they also create more volatility? Is there a trade-off here, and is it linear? Is there a case to be made that beyond a certain point the risks start to outweigh the gains? The raw data for advanced countries say that, despite highly repressed financial systems in the 1950s and 1960s, the immediate postwar period was one of unusually high growth and rapid capital formation. But more formal analysis of these and other episodes is certainly needed before any causal inference can be drawn. In particular, we need to devise careful methods to solve the identification problem associated with measuring the incremental costs of crises; all recessions have costs, and so we need to think in terms of the excess cost when a recession turns into a recession-plus-crisis. The construction of an appropriate counterfactual is not simple, and the same applies to any thought experiment involving counterfactual policies: for example, if policies were implemented to avert crises ex ante, would these also bring about costs in cases of a “false alarm”? I would also expect some interesting lines of research here to explore the frequency and intensity of banking crises under different exchange rate regimes and also under different capital mobility regimes, using historical variation in the solutions to the trilemma in history (Obstfeld and Taylor 2004). Clearly the debate over financial openness in general, and the specific dispute over how much blame for the current crisis lies with “global imbalances,” suggest that these areas of research might be very helpful for policy makers.

Second, can we say more about the workings of the banking sector in different times and places, and can such evidence help illuminate our understanding? One of my interests here is in the role that bank credit plays in the causes and consequences of crises. In recent research based
on a new long-run credit data set (Schularick and Taylor 2009), it is apparent that over the very long run the banking sectors of advanced economies have experienced profound structural changes. Figure 1 displays summary data from 14 countries for the years 1870–2008 on the ratio of loans to deposits (or assets to deposits) as a measure of the funding leverage of banks. This leverage measure was high and stable until 1929 in an era of financial instability, then fell dramatically in the 1930s and 1940s. It climbed back slowly only through the 1970s in the crisis-free postwar era of financial repression. After 1970 this ratio continued to climb past its previous peak, and we entered another era of financial instability. In addition to this correlation between leverage levels and instability, we also found that sharp increases in the growth rate of credit were significant predictors of future crises. Thus, linking the classification of banking crises used in this paper with measures of banking sector evolution could enhance our understanding further as to changes in the conditional probability of crises over the entire sample.

Endnotes

1. To take just one example: did the British have the right to call and refinance debt in the war conversion loan of 1932 and similar conversions in the nineteenth century? This is an example of a disputed domestic default. See M. R. Weale, “‘Default’ on UK Government Debt Was No Such Thing,” Financial Times, letters, March 8, 2010.

2. For example, in Schularick and Taylor (2009), we elected not to classify the German giro bank failures of 1977 as a banking crisis, since we felt that the failures were not very costly, widespread, or of macroeconomic significance.
References