

simple relationship between wars and the real price of oil. Most importantly, there is no evidence that the rise in oil prices attracted trend-following speculative traders who bought oil futures because oil prices were rising and, in turn, caused oil prices to rise because they were buying oil futures. In fact, recent research flatly rejects this interpretation using several complementary methodologies, removing one of the linchpins in the causal chain the authors construct (see, e.g., Kilian and Dan Murphy 2010).

Similar concerns apply to the authors' efforts to establish the existence of a recurring cycle back to the 1860s. The notion that the current oil price cycle mirrors a similar cycle in the 1860s seems farfetched. Clearly, that earlier oil price cycle, even if we accept these oil price data at face value, predated the emergence of a petroleum society in the West and arose in the absence of all the institutional characteristics underlying the post-1970 cycles described in this book. Moreover, it appears that coal was the most important fuel of the previous round of globalization from the end of the nineteenth century until World War I, rather than oil, as claimed in the book, and that globalization occurred long after the 1860s.

Finally, the analysis of how oil market developments feed back to the U.S. economy is simplistic at best. The authors chose to ignore extensive research on this question available at the time their book was written. For example, the authors make no effort to account for the fact that business-cycle-driven oil price increases have different effects on oil-importing economies than oil price increases driven by geopolitical events.

This short list of examples illustrates that the chain of causality that the book aims to establish is not nearly as tight as the authors imply. Nevertheless, the book is thought provoking and timely. The authors are at their best when they highlight the perspective of Middle Eastern oil-producing countries and describe the social and political change wrought by the gyrations of the oil market. This is an unusual perspective in the literature on oil and the economy, but important if we wish to understand both sides of the market and of the political debate over the price of oil.

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*This Time Is Different: Eight Centuries of Financial Folly.* By Carmen M. Reinhart and Kenneth S. Rogoff. Princeton and Oxford: Princeton University Press, 2009. Pp. xlv, 463. \$35.00. ISBN 978–0–691–14216–6.

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Every once in a while, a work comes along whose key points ought to be part of the information set of every literate economist. Carmen M. Reinhart and Kenneth S. Rogoff's *This Time is Different: Eight Centuries of Financial Folly* is such a work. It describes and analyzes a long international history of several types of financial crises. (Among important historical antecedents discussed in the book is Charles Kindleberger 2005.) *This Time is Different* is broader in scope and more comprehensive in data than previous studies. There is a renewed interest in crisis research (e.g., Robert J. Barro and José F. Ursúa 2009, which focuses on stock market crashes and depressions). While the analysis does not produce structural inferences, the weight of the data,

historical examples, and suggestive evidence leave a compelling impression.

The recent economic and financial crisis, on which Reinhart and Rogoff have a not-inconsiderable amount to say, enhances interest in the book. But its main contributions are the analyses of historical episodes, event study synopses, data development, and interpretation. It surveys several types of (sometimes interrelated) crises: inflation, currency crashes, currency debasement, bursting asset price bubbles, banking, external debt, and domestic debt. One common modern scenario is real estate bubbles bursting, leading to banking crises, losses in the financial sector, deep economic contraction, and sharp deterioration of public finances, followed by a slow, protracted recovery that is far worse when geographically widespread.

Left out is a compelling economic theory—that would be asking too much, although the authors do interpret and conjecture. They present useful individual event data and summary statistics from averages to histograms to conditional and unconditional probabilities, e.g., on debt ratios on the eve of defaults or the depth and duration of crises and their aftermath. And the history, while depressing in the story it tells, should fascinate all economists.

Reinhart and Rogoff define crises by quantitative thresholds on inflation, currency crashes, or currency debasement; and by events such as banking crises, external default, or domestic default. They use a threshold of a 20 percent per year inflation rate as an inflation crisis marker and an exchange rate depreciation of 15 percent per year to mark an exchange rate crisis. (Barro and Ursúa 2009 use a 25 percent decline in equity values to delineate a stock market crash, a consumption decline of 25 percent for a major, and 10 percent for a “minor,” depression).

The volume covers an array of topics, including currency reforms and conversions (part of every hyperinflation episode in the sample), debt intolerance, cycles of sovereign default on external debt, default and inflation, domestic debt and default, the incidence of expropriation of residents and nonresidents, inflation and currency crashes, currency debasement, banking crises, the recent financial crisis, and comparisons to the Great Depression.

The authors present a global database offering a variety of data on events, bailouts, currency

devaluations, real GDP declines—a long history, including a wide range of data quality and attempts to improve it. It focuses on government accounts, national accounts, global imbalances, banking data, real housing prices, and new data on public deficits and debt, important additions to the minimal data in Kindleberger (2005).<sup>1</sup>

In a financial crisis history of emerging Europe from 1300 to 1800, we learn that chronic default plagued France, Spain, the early Italian city-states, Portugal, Prussia, Austria, and Russia (Turkey and Egypt too). And from 1800 to well after World War II, Greece (prophetically) continuously defaulted.

Domestic debt crises typically occur against the backdrop of much worse economic conditions than the average external default, although usually they do not involve powerful external creditors. Reinhart and Rogoff suggest this is why so many of them are little noticed externally (e.g., Argentina’s external defaults in 1980 and 2001 attracted great international attention, yet its large-scale 1989 default, which did not involve a new external default, drew scant mention).

Among the most important contributions is a cross-country international catalog of historical defaults on domestic public debt spanning two centuries and sixty-six countries. Reinhart and Rogoff demonstrate that domestic public debt in developed, developed for its era, and emerging markets has been extremely significant and helps resolve a host of puzzles pertaining to episodes of high inflation and default.

The authors emphasize debt intolerance, a vicious cycle involving loss of market confidence, rapidly increasing interest rates on external government debt, and political resistance to repaying foreign creditors. External borrowing can substitute for tougher decisions on spending and taxation. Default often occurs at levels of debt well below 60 percent of GNP. Indeed, only one in six defaults or restructurings in middle-income countries occurred when the debt–GNP ratio got above 100 percent (Mexico in 1982 was 42 percent; Argentina in 2001, the largest ever at \$95

<sup>1</sup>They conclude the “. . . lack of transparency is endemic in government debt, but the difficulty of finding basic historical data on central government debt is almost comical” (p. xxxi).

billion, just over 50 percent). Failure to take debt intolerance into account leads to an underestimate of how easily unexpected shocks can lead to a loss of market confidence or of the will to repay, even when the ability to do so is not in question (as in current strategic defaults on underwater mortgages), and therefore to yet another debt collapse.

Historically, the cases in which countries have escaped high external debt burdens through growth or repayment have been the exception. (It would have been potentially quite useful, e.g., for survivor bias, for the authors to survey such episodes and explore any lessons learned by comparison.) Most large reductions in external debt have been by restructuring or default. Reinhart and Rogoff cite the failure to recognize the difficulty in escaping a situation of high debt intolerance simply through growth even at low ratios of debt to GNP as a fundamental flaw in standard private sector and governmental analyses of debt crises.

Why have so few countries managed to avoid default on central government debt to foreign creditors, whereas far more countries have been serial defaulters? Why do so many countries default on, or restructure, their external debts at seemingly low debt thresholds? In the 250 episodes of default on external debt in the database, severe domestic debt issues were found in well over half. Domestic debt has been a major factor in many governments' incentive to allow inflation. Reinhart and Rogoff correctly note that calculating the seigniorage maximizing inflation rate on the monetary base may be misleading. For example, in 1920s Germany, domestic debt was almost triple the size of the monetary base; in 1990 Brazil, almost twenty. Thus default via inflation appears to be an important component of the domestic default calculus.<sup>2</sup>

<sup>2</sup>This is an issue most advanced economies are likely to face as they return to more normal levels of resource utilization. It is worth noting that the current maturity structure of the U.S. federal debt averages about four years; so the gain from attempts to inflate away the debt are quite modest at the moment but could become substantial if the maturity structure lengthens. Recall more than half is now held externally. With all the concern about external debt being more mobile, perhaps more prone to rapid disruptive flight, it is also worth recalling the benefit of externally held public debt: the substitution of government bonds for tangible capital occurs on foreign, not domestic, portfolios (Peter A. Diamond 1965).

In the period 1800–1939, the incidence of external default was higher than for domestic residents (20 percent versus 12 percent). The widespread adoption of fiat money appears also to have led to inflation becoming an expedient form of expropriation. As a result, the incidence of expropriating domestic residents increased after World War II.

A common feature of the run-up to banking crises is a “capital flow bonanza”—sustained large capital inflows (Reinhart and Vincent Reinhart 2008). The probability of a banking crisis in a three year window in the 1960–2007 period rises 5.2 percent, conditional on a capital flow bonanza. In the post–World War II period prior to the current crisis, there were five severe bank crises (Spain 1977, Norway 1987, Finland 1991, Sweden 1991, Japan 1992) and thirteen milder bank-centered financial crises.<sup>3</sup>

Reinhart and Rogoff turn their attention to the U.S. subprime mortgage meltdown and the subsequent recession. They present an historical comparison, highlight a series of “this time is different” syndrome statements that are embarrassing in hindsight, and discuss early warning signs.<sup>4</sup> They focus on large current account imbalances, especially the massive U.S. deficit. Others debate its relevance, focusing on other factors such as loose monetary policy (John B. Taylor 2009), the immense government social engineering of housing or financial firm leverage or, in my own popular writing, these plus several other factors and their combustible interaction (Michael J. Boskin 2008). This is a useful discussion of how the “subprime” crisis fits into the Reinhart and Rogoff framework, rather than a thorough analytical discussion of the causes of the crisis.

The authors provide a useful set of data on the aftermaths of severe crises.<sup>5</sup> Averages for the already-completed episodes: real housing prices fell 35.5 percent over six years; real equity prices

<sup>3</sup>Including the U.S. Savings and Loan crisis, the resolution of which I helped design and oversee.

<sup>4</sup>I spare the policy officials and academics reprising them here.

<sup>5</sup>The recent crisis for the United States, United Kingdom, Austria, Hungary, Iceland, and Ireland, plus Japan 1992, Spain 1977, Norway in 1987, Sweden and Finland in 1991, plus several episodes from the 1997–98 Asian crisis, Colombia 1998, and Argentina in 2001.

fell 55.9 percent over 3.4 years; real GDP fell 9.3 percent over 1.9 years; and unemployment rose seven percentage points (less than one-third the United States in the Great Depression. In the recent recession, U.S. real PCE fell 2.4 percent, less than one-quarter the cut-off for a “minor” depression, and real GDP fell a bit over 4 percent over one year). The contraction was much larger for emerging economies than for developed ones, which the authors attribute to abrupt reversals in the availability of foreign credit. Fiscal deficits peaked at 11–12 percent of GDP in the three worst deteriorations following banking crises (Finland and Sweden 1991 and Argentina 2001) and in the three years following the crisis, real public debt almost doubled, quite similar to the United States and United Kingdom currently.

Reinhart and Rogoff compare these severe postwar crises with the Great Depression in several countries. The duration averaged 4.1 years, peak to trough, for the fall in real per capita output in the Depression versus 1.7 years in the postwar crises, and it correspondingly took an average of ten years for output per capita to return to its previous level compared to 4.4 years. Interestingly, real public debt grew more slowly in the aftermath of the Depression episodes than the severe postwar crises prior to the current one, a postwar history that is being continued in many countries today.<sup>6</sup>

The authors end their valuable volume with reflections on early warning, graduation, policy responses and foibles of human nature. Following previous works by Reinhart and her coauthors, they rank indicators from best to worst (best being lowest noise to signal—false alarm versus accurate prediction—ratio). For both banking crises and currency crashes, real exchange rates are best (for banking crises closely followed by real housing prices) and sovereign ratings close to worst, with current account balances in between. The authors conclude that “The fading memories of borrowers and lenders, policy makers and academics and the public at large do not seem to improve over time, so the policy lessons on how to ‘avoid’ the next blow-up are at best limited” (p. 287) and “The ability of governments and

investors to delude themselves . . . seems to have remained a constant” (p. 292), hence the title of the book.

The book includes useful data appendices on macro time series, public debt, dates of banking crises, and historical event summaries. (Appendix A.4 is a 34-page gem of synopses of crises, 1800 to 2008, from Argentina to Zimbabwe.) The data sets developed by Reinhart and Rogoff promise to be quite useful but thus far have been used for historical summary statistics and reduced-form event studies (for example, on the real GDP growth effects in the aftermath of high-public-debt episodes (Reinhart and Rogoff 2010).

There are some topics which deserved greater attention, including, where possible, an analysis of the size of the primary surpluses that might have been necessary to avert restructuring or default. (To get back to a pre-current episode safety zone will require a decade of primary surpluses of roughly 4 percent of GDP for the United States, 3 percent for the Eurozone, and 7 percent for Japan). The tax bias toward debt that increases leverage; analysis of when, how, and exactly who would do what before “it’s too late,” especially for current account imbalances (enhanced IMF surveillance won’t get far); and what difference it makes how public debt is reduced all deserve more attention.<sup>7</sup> Is there anything in these crisis episodes that sheds light on the propriety of Bagehot’s rule to lend freely (to solvent borrowers with good collateral) at a penalty rate? Or Keynesian fiscal stimulus (the authors correctly note its extensive use only in the recent crisis in many countries and in Japan post-1992)? Perhaps Reinhart and Rogoff will turn their considerable

<sup>7</sup> We are now in the midst of a great debate about the timing and structure of fiscal exit strategies, with the United States pursuing a longer Keynesian stimulus with massive long-term debt costs and most European countries, over U.S. objection, trying to consolidate more rapidly. Can there be a pro-growth fiscal consolidation (Francesco Giavazzi and Marco Pagano 1990)? How big are those long-run costs (Harald Uhlig 2010)? Does this sweeping history provide any guidance? More attention to crises averted or minimized and more reporting and evaluation of the policy responses to crises and potential crises (e.g., separating the automatic stabilizers prevalent in modern advanced economies from discretionary fiscal actions; tax versus spending changes; interest rate and/or lender of last resort roles of central banks).

<sup>6</sup> Mexico, Indonesia, Argentina, Brazil, Italy, France, Germany, Canada, the United States, and Japan.

talent to a companion volume or a *JEL* survey article on the history of monetary, fiscal, and regulatory responses to financial crises. How much impact was there on the depth and duration of the recession, the speed and strength of the recovery and, especially as it tends to be downplayed in contemporaneous discussion, the long-run costs from the increased public debt and future taxes, permanent expansion of government programs, and moral hazard?

The authors resist giving too much structural interpretation to their analysis. Most would agree with their conclusion that “. . . highly leveraged economies, particularly those in which continual rollover of short-term debt is sustained only by confidence in relatively illiquid underlying assets, seldom survive” (p. 292). While awaiting convincing identifying assumptions to isolate exogenous movements in government debt to allow structural estimation of the effects of public debt on growth, its absence is not an excuse to ignore markers that are associated with extremely serious problems. Prudent policy would attempt to avoid getting into such situations and take sensible steps to limit them. For example, whatever the merits of short-run Keynesian stimulus, the medium- and long-term public debt projections for most advanced economies, including the United States, are well into such territory now. But how to operationalize these insights into sensible policy that balances the risk of crises with the risk of stifling growth remains elusive.

The study is well suited to upper-level undergraduate and graduate courses and, in addition to professional economists, to economically and numerically literate noneconomists interested in public, international, development, monetary, financial, and macro economics and economic history.

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## H Public Economics

*Fiscal Federalism: Principles and Practices of Multiorder Governance*. By Robin Boadway and Anwar Shah. Cambridge and New York: Cambridge University Press, 2009. Pp. viii, 620. \$140.00, cloth; \$49.99, paper. ISBN 978-0-521-51821-5, cloth; 978-0-521-73211-6, pbk.  
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The number of federal, federal-like, and decentralized unitary countries has expanded dramatically during the past half century.<sup>1</sup> This “silent revolution” now encompasses two-thirds of the world’s population (p. vii). Spurred by the development of, and interest in, decentralized government and governance, the academic literature on the topic has grown exponentially and none more so than that on fiscal federalism. While many books make valuable contributions, a current comprehensive analytic introduction to fiscal federalism has been missing. Fortunately, Boadway and Shah have stepped forward to fill that gap. Both are well-known contributors to the fiscal

<sup>1</sup> See Ronald L. Watts (2008). Related to that growth has been a similar increase in the number of democratic governments. See, for example, the trends reported by the Polity IV Project (Monty G. Marshall 2009).