G. Page West III and Robert M. Whaples, eds., *The Economic Crisis in Retrospect: Explanations by Great Economists* (Cheltenham, UK, and Northampton, MA: Edward Elgar, 2013), pp. vii, 192. $110.00. ISBN 978-1-78254-532-3.

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This awkwardly titled book consists of an introductory essay and seven papers derived from talks given to undergraduates at Wake Forest University in 2011–12 about what can be learned about the recent economic crisis from the economics of the past. Five contributors concentrate on specific economists—Perry Mehrling on Walter Bagehot, Robert Prasch on Thorstein Veblen, Bradley Bateman on John Maynard Keynes, Richard Langlois on Joseph Schumpeter, and Bruce Caldwell on Friedrich Hayek—and two take a more general approach: Peter Temin on "Insights from the Great Depression," and Thomas Sargent on some implications of recent (and not so recent) rounds in what he portrays as an ongoing struggle for ascendency in monetary theory and policy between the "real bills" doctrine and the quantity theory of money. All of the talks were evidently thoughtful, provocative, and non-technical—hence, highly accessible—and their audience was surely well served. But whether they amount to a book that provides, as its editors hope, "excellent material for economics and business students" who need to broaden their reading beyond today's standard syllabus is a more complicated question.

To begin with, though the quantity theory's place in the analysis of economic instability gets some attention in Bateman's account of Keynes's work before 1932, and in Sargent's paper, particularly in his negative assessment of the "Chicago plan" for 100% money and Friedman's k% money growth rule, it receives no systematic treatment here to parallel those accorded other approaches. There is no paper on Irving Fisher, Ralph Hawtrey, or Gustav Cassel, nor, quite extraordinarily, given this book's overarching concern with what earlier work might have to say about the latest crisis, is Friedman and Anna Schwartz's (1963) *Monetary History of the United States* so much as mentioned anywhere in its pages. In a series of talks delivered over a period of several months, such omissions might barely have been noticed, but in a book, they are glaring. A second issue is more subtle. Some chapters read as if they are the texts of talks "as delivered," rather than papers subsequently revised and rewritten prior to publication. No author acknowledges any comments or advice from colleagues, referees, or editors, and references to other literature are in some cases sparse (median for the volume, six). Talks being what they are, missed opportunities to make points, as well as errors of commission and omission, not all of them minor, are inevitable. A few of the more serious examples that have found their way into cold print will be highlighted below.

All this being said, Mehrling's account of how, after 2007, the Fed adapted Bagehot's (1873) principles to today's very different financial system and became the "dealer of last resort" in the US—the "lender of last resort" to much of the rest of the world—and "saved our bacon" is easier going than his recent book on this topic (Mehrling 2011). It can be recommended to student readers, not least because, as Mehrling remarks, we still suffer from "an inability to talk about [this process]. But we need to talk about it." Bateman's Keynes—in a piece again deriving from a recent book (Backhouse and Bateman 2011)—and Caldwell's Hayek both emerge as real political economists, not the demon "central planner" and the grim apostle of the "market" who so often substitute for them in today's debates. Students can learn important lessons from these two chapters about just how close to one another Keynes and Hayek were on certain topics, and about how and why they differed on others, and they will also learn a lot about how economists' minds can change, and how their ideas can and cannot be applied to the world in which we live.

I am marginally less enthusiastic about Langlois on Schumpeter and Prasch on Veblen, even though these are perhaps the two most finished essays in the collection, and even though their subjects are much neglected in the standard syllabus. My problem is that in each case the author could have forged tighter connections between his subjects' ideas in general and their relevance to the specific crisis that links this book's individual chapters. Langlois deals authoritatively with Schumpeter's vision of a cycle driven by innovation as integral to capitalist growth, about the essentially accommodative and secondary role played by the financial system in this process, and about his overall scepticism about the ability of economic policy to improve economic performance, but he writes as if Schumpeter produced nothing dealing specifically with the Great Depression. He did, however: a chapter entitled "Depressions" in that often maligned Harvard volume *The Economics of the Recovery Program* (Brown 1934), where his opposition to the deployment of expansionary monetary measures was unyielding. Langlois's readers might well miss just how similar were Schumpeter's views to those of Hayek on what ought not to have been done about the Depression while it was in progress.

Veblen died just before what his intellectual successor as a critic of capitalist excess John Kenneth Galbraith called *The Great Crash 1929* (Galbraith 1954), so he didn't have much to say that is directly pertinent to a market economy in major crisis. Prasch nevertheless sticks to Veblen, and ignores Galbraith altogether (the editors do mention Galbraith as having been on their "short list" of possible subjects), thus missing a valuable opportunity to explore more thoroughly the relevance to recent events of the particular and important strain of American institutionalism Veblen founded. What would Galbraith have made of the current resonance of the title he chose in 1954 for his Chapter 3, in which he explained the mysteries of leverage: "In Goldman, Sachs we Trust"? And how would he—or Veblen, for that matter—have dealt with the fact that what Mehrling calls a "daily text" of the "tribe" that inhabits today's financial markets, namely the *Financial Times,* still distributes a glossy supplement devoted to conspicuous consumption (reference intended) under the shameless title *How to Spend It?*

The role of an inevitably fading collective memory in ensuring that a crisis such as the Great Depression could (almost) repeat itself after eight decades is one of Temin's central themes. More optimistically, another is that such events provide opportunities for genuine reform rarely found in a world where "all politics is local," a provocative if unintended counter to Mehrling's more pessimistic reflections on the difficulty of even discussing such matters. So far so good, and Temin's suggestion that nostalgia for an earlier time and simpler economic ideas helped set the intellectual stage for the crises that began in 1929 and 2007 is also worth considering. I have serious doubts, however, that in either instance the earlier time in question was, as he claims, the eighteenth century, which he describes as a period of "minimal government, free competition in industry and stable exchange rates." This century saw the Mississippi and South Sea bubbles near its beginning, and the American and French revolutions near its end, with lots in between. Moreover, 1797 was the year of the Bank of England's "temporary" suspension of gold convertibility—barely a footnote in political and economic history, to be sure—which provoked the most sustained and rapid advances in the theory of money, prices, and the exchanges ever seen. Among other things, these reduced the intellectual status of the monetary economics of David Hume—Temin's main example of simpler economic ideas—to that of a primitive, though remarkably sound, prototype. I can't imagine that Temin would have let his arguments about the influence of nostalgia for the eighteenth century on the twentieth stand as starkly as they do here (twice), had a colleague or editor suggested that he think a little more about them.

Finally we come to Thomas Sargent, whose main point is that monetary policymakers neglected warnings about mounting moral hazard and increasingly fragile asset markets in the run-up to the recent crisis, and that such matters remain important. This argument, and its corollary—that the financial system would have been and would still be more stable without a lender of last resort and deposit insurance, but with more unregulated competition—needs to be taken seriously, not least by those of us who share Perry Mehrling's enthusiasm for the Bagehotian tradition. But Sargent's student readers should judge his position solely on its own logical and empirical merits, because the way in which he deploys earlier economic thought in support of his analysis does not set a good example for them. One instance must suffice to illustrate this assertion.

Sargent's monetary economics is based on what he insists is a modern variation of the "real bills" doctrine, and he claims Adam Smith (1776) as its founding hero, citing Smith's well-known advice to Scottish (*pace* Sargent, *not* English) bankers, to confine their business to lending only on the security of good-quality, short-term, commercial bills. Sargent presents this advice as sanctioning the issue of banknotes "backed not by gold, but by things that are going to be convertible into gold soon," as "predicting that these self-liquidating private loans will be as good as gold," and, hence, as supporting an eighteenth-century version of the kind of unregulated banking system that he recommends for today's world—a point here in Temin's defense, perhaps? Serious historians of monetary economics know, however, that Smith doubted that even competitive bankers could be relied on to follow his advice, and believed that a paper money system would always be riskier than one using only gold. They know too that he unequivocally supported legal restrictions that required banknotes to be instantly convertible on demand into gold, and also put a floor on the denominations in which they could be issued sufficiently high that their circulation would effectively be restricted to a financially sophisticated merchant community. Finally, they also know that Smith defended such interference with freedom in banking as follows: "The obligations of building party walls, in order to prevent the communication of fire, is a violation of natural liberty, exactly of the same kind with the regulations of the banking trade which I here propose" (1776, p. 324). Sargent mentions none of these qualifications.

The editors' introduction to this volume suggests, "Beyond the technical training they receive in their respective disciplines, it is important for future leaders [in business and society] to appreciate the deeper insights gained from reading the great economists." Few graduate programs teach the history of economic thought nowadays, and the pages of many professional journals are closed to papers in the area. This decline in the specialized study of their discipline's history has not stopped economists from deploying that history in discussions of current issues, but it is leading to a decline in the scholarly standards they adopt as they do so. Most contributors to this volume nevertheless provide, give or take some mostly minor slips, exactly the kind of well-informed guidance to older literature that undergraduates need, but other, slacker, tendencies are also sometimes evident. It's a shame that West and Whaples did not put up a little editorial resistance to them.

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Carl Hampus Lyttkens, *Economic Analysis of Institutional Change in Ancient Greece: Politics, Taxation and Rational Behavior* (London and New York: Routledge, 2013), pp. xiii, 188, $140. ISBN 978-0-415-63016-0.

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Carl Lyttkens's book was written, he tells the reader, with the two-fold goal to persuade his fellow economists that the ancient Greek world is an under-explored realm for economic analysis and to persuade his fellow students of Greek antiquity that modern economic analysis can be fruitfully employed in their analyses. To make his case, Lyttkens opens with a claim guaranteed to create awe and wonder. It needs to be quoted:

> In 487/6 BC the Athenians decided that the archon—the most important state official in Athens—was going to be appointed by drawing lots among the candidates. This was an extraordinary idea: think about running France, the United States or Iran by drawing lot among the candidates from president. In Athens, this innovative method of appointment was later extended to many other posts. (p. 1)

Space and my competence being scarce, I'll focus on this aspect of the argument—the "extraordinary" idea of election by lot as a topic to intrigue modern economists, and then touch on Lyttkens's concerns with economic rationality.

Lyttkens avows his methodological presuppositions in a most transparent manner. He writes in the context of "New Institutionalist Economics" set forth by Douglas North in association with many able and energetic fellow economists. The tool set includes an appreciation of the principle of unintended consequences, the use of