

found in depictions of their Western counterparts. To some extent, the relative weakness of the sections on the Qing dynasty are a product of the comparatively poorer access to archives in China. But the unfortunate consequence is that the Western agents of empire appear as the sole engine of change in the late nineteenth century. In Wu's narrative framework, Qing and early Republican figures appear to be largely reactive, doing little more than simply importing these new ideas about coal and industrial production wholesale from the West.

None of this is to discount the importance of Wu's larger findings regarding the revolutionary transformation in ideas about natural resources and their connections to state power in the late nineteenth century. Indeed, the formation of a nexus between mining, industrial production, and national prestige continues to resonate in China today. Like its counterparts in the Republic, and the early People's Republic, the Chinese Communist Party continues to defend its legitimacy in terms of industrial production and the achievement of economic production benchmarks. Wu reminds us that this shift was not a product of 1950s big-push planning, as one might have previously suspected, when Chinese Communist Party planners measured national success in terms of tons of iron mined, coal coked, and steel produced. Instead, this monumental change was a product of ideas imported in the late nineteenth century by a set of reformers desperate to save the Qing empire.

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Hubris: Why Economists Failed to Predict the Crisis and How to Avoid the Next One. *By Meghnad Desai.* New Haven and London: Yale University Press, 2015. xii + 287 pp. Figures, bibliography, notes, index. Cloth, \$28.00. ISBN: 978-0-300-21607-3.

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Reviewed by Marcel Boumans

This is the kind of book I would encourage any first-year undergraduate student in economics to read. Why? Because it surveys in a clear and comprehensive way the basic ideas, the “building blocks,” that economists have used in the past and continue to use to explain crises and cycles. But I would also emphasize that the book is not about what its

title says it is. That is, it is not hubris that caused economists to fail to predict the crisis. Moreover, the book is not about how economists can avoid the next crisis, for the same reason: the economy is simply too idiosyncratic to get a grip on.

A more appropriate title for this book would be *Ignorance: Why Economists Fail to Explain Crises and Thus Do Not Know How to Avoid the Next One*. The author, Meghnad Desai, outlines a history of economic ideas that is characterized by a long struggle with crises and cycles—but not for a lack of ideas. Desai finds sufficient—and in his view, satisfactory—ideas in the works of Karl Marx, Knut Wicksell, and Joseph Schumpeter. But although these ideas were supported by empirical evidence, they lacked “the support of modern statistical methods” and “the logical way of thinking about cycles” required to analyze crises and cycles (p. 76).

Both these “modern statistical methods” and this “logical way of thinking” were developed within a new discipline, called econometrics, that emerged in the early 1930s. Econometrics is well known for having provided these statistical methods, but Desai emphasizes that the creator of econometrics, Ragnar Frisch, also “made a profound contribution to our understanding of the nature of cycles” (p. 110). Frisch compared the economy to a rocking horse. If left alone, the horse would be stationary, but if hit with a stick repeatedly (though not necessarily regularly), it would rock.

Desai blames both mainstream economists and the Keynesians for suffering from overconfidence, both in their ideas and in their methods of understanding crises. In his criticism, however, he gets the history wrong, particularly in relation to econometrics and the rocking horse metaphor. He often conflates Keynesianism and econometrics. This confusion can mainly be attributed to Lawrence Klein, the author of *The Keynesian Revolution* (1947), who became known for developing macroeconometric models of the U.S. economy, based on the theoretical framework set out in John Maynard Keynes’s *The General Theory*. But Klein was not the first to build such a model of the U.S. economy. The Dutch economist Jan Tinbergen did so in the late 1930s, not using Keynes’s theory but instead testing the many different business cycle theories available at that time. This work by Tinbergen became famous because of Keynes’s critique of this new econometric modeling method.

A second historical misapprehension in *Hubris* is that Desai assumed that the Klein-Goldberger model of the U.S. economy did not explain the U.S. cycles. Frank and Irma Adelman carried out the first computer simulation of the Klein-Goldberger model, to see whether it reproduced the cycles; contrary to Desai’s conclusion, it did (p. 152). The Adelmans showed that the economy depicted by the model was indeed like Frisch’s rocking horse. When left alone it would not show cyclical

behavior, but when the Adelmans added repeated shocks to the economy, it mimicked the characteristics of the observed U.S. cycles.

Because of this astonishing result, this characteristics test became the dominant test device for macroeconomic models in the following decades—although it came to be called calibration by the new classical economists. The quality of a model is thus judged not by the accuracy of its forecast, but by how well it mimics the characteristics of the fluctuations (p. 195). This kind of testing is closely related to the design of the rocking horse model. It was also the manner by which Frisch had tested his own original model.

The largest part of this book is a “journey” through the history of economic ideas, while only the final (seventh) chapter addresses “the search for an answer.” Because it is an answer to the question of “why things turned out the way they did,” this is a search for an explanation. Desai proposes an explanation that is built on (a) the idea of long cycles as put forward by Kondratieff/Schumpeter, supplemented by (b) the Marx-Goodwin notion of a cycle in the income shares of wages and profits, and (c) the Wicksell-Hayek theory of short cycles caused by the gap between the market rate of interest and the natural rate (p. 240). I must admit that when I arrived at his proposed explanation, I was surprised. Not by the author’s selection of the favorite economists of the past—in his historical journey he is clear about which are and are not his favorites—but by the type of explanation he is offering. It is a classical economics, nineteenth-century type of explanation in terms of trends. I had expected a twentieth-century, rocking horse type of explanation, which consists of two parts: a propagation mechanism and additional impulses. The propagation mechanism consists of the behavioral and institutional relationships of an economy. These relationships are represented by a macroeconomic model. Because these relationships are not trends, holding only for a certain period—say, of five to ten years—such a model needs to be updated regularly. But a macroeconomic model is not sufficient to explain crises; therefore, the external impulses are needed—exogenous shocks, which can be caused by any historical contingency (e.g., a war, a terroristic attack, a tsunami). This kind of explanation would better suit Desai’s underlying message, expressed throughout his journey and most explicitly on page 238: “There are no permanent laws in economics. Only historically contingent truths.” Such a message would be the most appropriate response to the hubris of many economists.

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Model the World into Numbers (2005) and Science Outside the Laboratory (2015).

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Mainstream Growth Economists and Capital Theorists: A Survey. *By Marin Muzhani*. Montreal: McGill-Queen's University Press, 2014. x + 558 pp. Bibliography, notes, index. Cloth, \$120.00; paper, \$44.95. ISBN: cloth, 978-0-7735-4365-2; paper, 978-0-7735-4366-9.

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Reviewed by Geoffrey M. Hodgson

The author declares that “the primary objective of this book is to introduce, in the form of a survey of the economic literature, a straightforward account of modern growth theory and capital theory from the 1930s to the present, along with a critical examination of each” (p. 5). The book thus sets out on a large-scale assessment of the field. To a large degree it is successful, especially when dealing with developments in modeling, but there are some important omissions and weaknesses. As a survey of the development of models of economic growth, it is a very useful text.

Chapter 1 looks at the classical economists, particularly Adam Smith, David Ricardo, and Karl Marx. Chapter 2 examines the “neo-Keynesian” theory of growth inspired by Roy Harrod and Evsey Domar. Chapter 3 outlines the theory of distribution and growth developed by Nicholas Kaldor and Luigi Pasinetti. Chapters 4 and 5 discuss the neoclassical theory of growth developed by Robert Solow and several others. Chapter 6 looks at the Cambridge capital controversies, between Joan Robinson and others in the United Kingdom on the one side, and Paul Samuelson and others in the United States on the other side. Chapter 7 looks at theories of economic growth and development. Finally, chapters 8, 9, and 10 contain a detailed examination of endogenous growth theory, bringing us up to the cutting-edge mainstream growth theory of today.

The book's emphasis is very much on mathematical models. For those looking at the development of models of growth and capital since the 1930s, this is a very useful volume, executed with great care and attention to detail. But for those looking further, it may be a disappointment.

While the book is driven by the technical details of the modeling, relatively little attention is given to the empirical shortcomings of even the most sophisticated growth models. The failures of standard growth models to give accurate predictions of growth have dogged all theory in this genre. In repeated cases, large residuals typically remain