BOOK REVIEW:

“BANKS AND FINANCE IN MODERN MACROECONOMICS”

BY BRUNA INGRAO AND CLAUDIO SARDONI

REVIEWED BY

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Review of Ingraio and Sardoni, *Banks and finance in modern macroeconomics*, 2019, Edward Elgar.

Juan Acosta

Bruna Ingraio and Claudio Sardoni have put together an ambitious survey of mainstream macroeconomics' views on banks and financial markets, from the early 20th century to the post 2007-8 crisis. Their book is, above all, an effort to understand when and why mainstream macroeconomics went wrong and how it thus became unable to foresee or even understand the 2007-8 crisis. Their argument, they hope, will convince economists (and economics students) to part ways with mainstream macroeconomics and embrace a different view, one that the authors claim to want to restore: "a view of financial markets and banks as primary actors in the functioning, or malfunctioning, of market economies, [that captures] a more realistic vision of the 'visible hands' at work, rather than the impersonal operation of 'invisible' and impersonal market forces" (p. 3). This book was clearly not written for historians of economics—at least not as the main target audience—but the authors do make an interesting and original argument that historians should engage with, and that will be useful as a benchmark for thicker work on this topic.

The authors' main argument is that between the 1930s and the 1980s mainstream macroeconomists gave little to no importance to banks and financial markets in their models. Banks in particular played a key role in the theories of the business cycle and the price level presented by economists like Wicksell, Fisher, Schumpeter, and Robertson up to the 1930s. But an inflection point came with Keynes' *General Theory* and two crucial choices: (i) his focus on liquidity preference and its effect on the economy as a whole, and (ii) the adoption of an equilibrium methodology instead of the sequential analysis of his *Treatise on Money*. These choices hid the importance of the plumbing behind the quantity of money in an economy, and the authors thus blame Keynes for putting macroeconomics on a path where there was little room for complex characterizations of the behavior of banks and financial institutions. The architects of the postwar Neoclassical Synthesis (the authors focus on Hicks, Patinkin, and Modigliani) further strengthened macroeconomics' neglect of banks and finance, as well as its commitment to general equilibrium analysis. Tobin's attempts to bring back banks and other intermediaries (inspired by Gurley and Shaw) were unsuccessful, and Friedman's monetarism pushed attention further away from anything but the supply of money. New Classical macroeconomists (the authors focus on Lucas) renewed macroeconomics' vows with equilibrium modeling and kept the focus on the money supply. Kydland and Prescott's RBC model, however, changed course and completely ignored the monetary side of the economy.

The authors recognize that the 1980s brought a renewed interest in the complexities of banks and financial markets, but they don't see this as a major development. They discuss in particular the work of Stiglitz and Bernanke on imperfections, credit rationing, and the financial accelerator, as well as Shiller and others' reactions to Fama's Efficient Market Hypothesis. However, they present this work as a slight improvement that doesn't fully capture the complexity of banks and financial markets. The final pages of the book are used to close the authors' pitch for moving away from mainstream macroeconomics by stressing the limitations of general equilibrium modeling and the focus on perfect competition as the benchmark for macroeconomic thinking:

these are seen as dead ends, incapable of incorporating the inherently dynamic and disruptive nature of banks and finance. Instead, the authors argue for a more comprehensive analysis, less reliant on models, that they see as a continuation of the Classical Political Economy tradition.

Whatever one thinks of their ultimate goal, Ingraio and Sardoni's effort to make sense of this topic for such a long period is a welcome addition to the literature—in particular to histories of monetary economics that dealt with previous periods (Arnon 2011; Laidler 1991, 1999) and to histories of macroeconomics that have for the most part neglected banks and finance (Snowdon and Vane 2005; De Vroey 2016; Backhouse and Boianovsky 2013). However, their historiographical choices should not be accepted uncritically. In particular, I think there's much to be gained from challenging their narrow focus on theoretical and academic work. The authors' disclaimer—that their analysis isn't exhaustive and that they instead aim to "critically explore the threads and issues in the evolution of ideas that [they] regard as most relevant" (p. 2)—is welcome, but it isn't enough to stop the reader from wondering how much of their story would change if other characters, settings, and types of work had also been considered. For example, recent work on the construction of large-scale macroeconomic models during the 1960s—including Modigliani's collaboration with the Federal Reserve Board on their model—shows that banks and other financial institutions were in fact taken into account during these years (Acosta and Rubin 2019). Similarly, if the work of Brunner and Meltzer had been included, it would be difficult to make the case that monetarism neglected banks and financial intermediaries. I thus suspect that taking into account the applied work done in and outside academia will result in a more nuanced periodization than the one presented in the book.

More generally, however, the main problem with the authors' narrow focus on academic macroeconomists is that it's not clear why we should care so much about them—and about their difficulties understanding banks and financial markets, or to foresee the 2007-8 crisis. The authors point out that, given the scope of their book, they "cannot deal in any detail with economic history, changing economic policies, or the sociology of research in academic communities" (p. 2). However, this is precisely what would be needed to argue that these debates between academics had any important consequences whatsoever. Whether these macroeconomists' institutional connections or funding mattered—and can, for example, help explain why some modeling choices and conclusions were widely accepted in the profession and beyond—is not explored. The authors also pay very little attention to the evolution of banking and financial institutions and to policy discussions, providing no evidence of the contemporary pertinence of academics' work or its impact beyond academia. Lastly, the authors completely neglect the evolution of economic analysis at central banks and other key institutions for macroeconomic policy, again failing to provide any evidence of the influence of academic debates beyond academia. In the case of the United States, for example, recent work on the relationship between the Federal Reserve and academics shows that they have different interests and priorities, and have gone through varying degrees of closeness in the past decades (Backhouse and Cherrier 2019; Claveau and Dion 2018; Rancan 2020). It would thus be a mistake to assume that academic debates have always and exactly determined contemporary policymakers' views on banks, money, or the economy in general. Instead, to unravel the complex relationship between academic macroeconomists' debates and policy—and the extent to which the latter has been helped or hindered by the former—we need a broader understanding of

macroeconomics' history that goes beyond the narrow focus on academia and includes central banks, think tanks, and other similar institutions.

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