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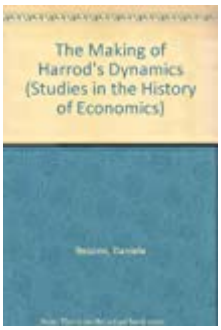

Daniele Besomi. *The Making of Harrod's Dynamics.* New York: St. Martin's Press, 1999. xii + 289 pp. $75.00 (cloth), ISBN 978-0-312-21908-6.

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Movie-going economists might recall the line in *Apollo 13* that NASA learned more from this failed mission than it had from all its previous successes. In part, Daniele Besomi tries to achieve something of the same in Roy Harrod's failure to inspire economists to adopt a dynamic approach to their discipline.

There is little doubt that Harrod's adventure into dynamics was a failure, in that little if any of what he saw as the dynamic way of thinking took root in economics. Indeed, economics today is distinguished from almost all other sciences by a central reliance upon static analysis, rather than the dynamic and evolutionary approaches which characterize other disciplines. If Veblen were alive today, his seminal methodological work would not be titled "Why is Economics not an Evolutionary Science?", but "Why is Economics the Only Non-Evolutionary Science?"

Much of the answer lies in the strengths, failings and foibles of Roy F. Harrod, the economist who most openly and prominently championed economic dynamics, and in the reactions of other economists to his challenge. While this point is taken up in Besomi's epilogue, the main purpose of Besomi's treatise is to uncover the process by which Harrod developed his approach to dynamics.

Through his comprehensive survey of Harrod's published writings and correspondence with other economists, Besomi finds more method in Harrod's quest to develop a dynamic economics than is apparent to anyone who simply relies upon Harrod's major works on dynamics (I take these to be "An Essay in Dynamic Theory" [1939], "Towards a Dynamic Economics" [1948], "Second Essay in Dynamic Theory" [1960], "Themes in Dynamic Theory" [1963], and "Economic Dynamics" [1973]). These works in themselves appear a patchy melange of concepts to anyone well-versed in modern dynamic analysis, with inexplicable attempts to define the greater part of economics as the province of "Statics" rather than "Dynamics," a painfully slow development of analytic technique - if indeed there was any development at all -and gross inconsistency over time, as highlighted many years ago by David McCord Wright (1949).

However, there is methodological consistency, Besomi argues, but it can only be seen if one looks through Harrod's eyes and in Harrod's time.

Harrod believed that the Marshallian theory of value could be extended in stages to cover dynamics, with an essential first step in this being generalizing Marshallian analysis to cover imperfect competition. Thus those of us who normally associate the theory of imperfect competition with Robinson and Chamberlin learn that Harrod devised the concept of marginal revenue, and developed

the formula relating it to price and the inverse elasticity of demand.

The belief that there was continuity between static and dynamic analysis appears curious to a dynamicist today - as curious as the belief that, to ride a bike, one must first learn how to balance it while it is stationary. But it is explicable in the context of Harrod's time, especially when Harrod's extremely limited understanding of mathematics is taken into account. However, this perceived continuity, which he stressed so much in his writing, made it very difficult for Harrod to convince economists that he was saying something new - and made it more difficult still for economists to accept that, where Harrod was obviously saying something new, he was not also saying something quite wrong.

This problem was sharpest with the next essential element in Harrod's methodology, the belief that a dynamic economics required "abandoning the assumption of stability of equilibrium, and introducing some destabilizing factor in the model at the outset" (Besomi, p. 3). Here arose many misunderstandings of Harrod, largely because economists of his time (and many since) could not accept that an unstable equilibrium was a meaningful concept. Hicks's (1949) erroneous but widely believed statement that "A mathematically unstable system does not fluctuate; it just breaks down" was typical of the manner in which this crucial aspect of Harrod's dynamics was dismissed by economists.

Finally, Harrod was competing for attention at a time when many others - most notably Keynes - were vying for the attention of economists, with many calls of new ways to think economically. Besomi emphasises the extent to which Harrod was a participant in, and product of these debates, while at the same time establishing that Harrod was as guilty of misunderstanding his fellows - again, most notably Keynes - as they were of misunderstanding him.

Besomi also indicates the extent to which Keynes played an important role in helping Harrod formulate his ideas. Keynes proposed a "fundamental growth relation" - in terms of the multiplier and accelerator - to Harrod before he had devised his own (Besomi, pp. 138-150). Keynes's notion of dynamics was also much richer than Harrod's: "What Keynes held against Harrod in the course of their debate, and what Harrod did not understand, is that in dynamics time must be the ordinary time, full of hopes and disenchantment, enabling one to recognize the importance of the uncertainty regarding the future course of events as distinct from the certitudes of the past" (Besomi, p. 162).

Harrod's most direct competitors were the econometricians, following on from Frisch, who modeled the trade cycle as the product of a stable propagation mechanism reacting to exogenous shocks. Here he lost out completely - so much so that his own "model" of cycles, which he insisted could not be reduced to the mechanical interaction of lags, was recast in precisely that mould by Hicks, Samuelson et al in a fashion which divorced growth and trend

Harrod was not necessarily correct to oppose a lagged reformulation of his analysis. Elsewhere (Keen 2000) I argue that the Hicks-Samuelson second order difference equation was in fact the product of bad economics, and that Harrod's model can be stated in a lagged form in which growth and cycle are interdependent. However, he was correct to oppose basing cyclical behavior upon lags alone, and divorcing growth from cycle. But he had little chance of defending this position against the far more technically proficient Hicks, Samuelson, and econometricians generally. As Besomi observes, "We may thus agree with Goodwin, and Samuelson before him, that Harrod's intuition was far superior to his mastery of the analytical tools he devised for solving the important problems he was posing" (Besomi, p. 210).

Given all this, it is remarkable that anything of merit emerged from this sea of confusion. Yet something of merit did. Harrod's dynamics, with its well-known but misunderstood emphasis upon instability, and its poorly known but important emphasis upon nonlinearity, did contribute something new and important to economic theory - regardless of whether or not these ideas ever took root in mainstream economics.

Precisely because these valid ideas did not take root, Besomi's book is as much a study in the pathology of economics as is it a study of Harrod per se. Many of those who are critical of the twists and turns mainstream economics has taken since World War II point a finger at textbooks, and here Besomi is no exception. He observes that "The story of the textbook recognition of Harrod's contributions concludes even more sadly: since the 1980s, Harrod's name rarely appears in the author indexes of books on macroeconomics. Surveys of trade cycle theory hardly mention his contributions, while endogenous growth theorists seem to remember Harrod only in passing, for his 'production function with little substitutability among the inputs' used 'to argue that the capitalistic system is inherently unstable' " (Besomi, p. 209, citing Barro and Salai-Martin (1995), p. 10).

Besomi's Harrodian saga is a useful tonic to the textbook view of the history of economic thought. Far from showing a linear progression from old ideas to new, good ideas to better ideas, we see a haltering to and fro movement, with Harrod buffeted by the debates of his day, publishing what he thought would communicate well rather than what he necessarily thought, tailoring articles to the desires of editorsˆE All stuff with which practitioners in economics are familiar, but which has rarely been as well documented for a major figure like Harrod.

There is, therefore, much that economics could learn from Besomi's anatomy of Harrod's *Apollo 13*. However, I disagree with the conclusion Besomi himself draws, when he states that "it would seem that the success of a theory does not depend only upon its clarity, logical consistency, and heuristic value, let alone empirical accuracy, but also on its capacity to be integrated within the accepted currents of thought. To generate *interest*, a theory must contain some element of novelty; but to be understood and integrated, the break with tradition must not be too radical" (Besomi, p. 214).

I would instead argue that Harrod's false belief that dynamics did not constitute "too radical a break with tradition" is part of the reason why economic dynamics was stillborn in Harrod's hands.

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