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MAURO BOIANOVSKY ON SAMUELSONIAN DYNAMICS AND THE ECONOMIC ANALYSIS OF MARKET PROCESSES

BY
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In his article "Paul Samuelson's Ways to Macroeconomic Dynamics" Mauro Boianovsky has provided an account of the evolution of Samuelson's attempts to construct a dynamic theory that, because of Samuelson's centrality to modern economics, verges on being an account of the history of recent thought on the subject. This paper argues that Samuelson's views should be related not only to the formal dynamic modeling of Ragnar Frisch and Jan Tinbergen but also to the verbal accounts of dynamics and market processes found in work by Alfred Marshall, John Maurice Clark, Edward Chamberlin, and Joseph Schumpeter that he had studied very carefully. Broadening our perspective in this way helps make sense of some otherwise puzzling features of Samuelson's economics.

I. INTRODUCTION

Mauro Boianovsky's "Paul Samuelson's Ways to Macroeconomic Dynamics" (Boianovsky 2020) focused on a single economist, but in discussing other economists' reactions to Samuelson, it ranges far more widely. Because Samuelson was central to so many developments in economics, and because the article covers economists who challenged Samuelson, it comes close to being a history of economists' struggles to understand dynamic processes in general. Quite justifiably, Mauro focuses on explicit, mathematical accounts of dynamic processes that can be traced back to Ragnar Frisch and Jan Tinbergen. There are places where he hints about how his account fits into a wider account of economists' attempts to understand how markets work, but these remain brief hints. My aim here is to develop the wider story into which, I contend, Mauro's account of dynamics fits. First, I try to develop the connections between Mauro's history of dynamics and the history of disequilibrium macroeconomics that

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he and I wrote several years earlier. I argue that these are complementary ways of telling the story of how economists have sought to understand market processes, neither of which can be fully understood without taking the other into account. I then go back in time to relate Samuelson's understanding of dynamics, central to Mauro's history, not to the explicit models of economists such as Frisch and Tinbergen but to work on markets more generally, notably to Alfred Marshall and those responsible for the imperfect competition revolution. Not only does this strengthen the argument that dynamics are an integral part of the theory of market processes, but it also helps explain why Samuelson approached these problems in the way that he did.

II. DYNAMICS AND DISEQUILIBRIUM

In *Transforming Modern Macroeconomics* (Backhouse and Boianovsky 2013), Mauro and I told the story of attempts to model macroeconomic equilibrium in the second half of the twentieth century. We subtitled the book *Exploring Disequilibrium Microfoundations 1956–2003* because our intention was to focus on an episode that, in some histories of economics, had been relegated to the status of a detour that led nowhere. Our argument was that what came to be called "disequilibrium macroeconomics" was an important step in the history of macroeconomists' struggles to understand how markets worked. The motivation for this literature was that, starting with Oskar Lange and Franco Modigliani, economists had tried to understand Keynesian economics in the context of a competitive general equilibrium system. This raised a major problem for Keynesian economics as it was then understood. If markets are perfectly competitive, it is hard to explain how demand-deficient unemployment can arise. Through several twists and turns, attempts to solve this problem led to the conclusion that unemployment must arise because wages are rigid and do not fall so as to clear the labor market. We argued that a significant step was taken when Don Patinkin (1956, 1965), a student of Lange, realized that, if markets were not in equilibrium, demands and supplies would not be the same as if agents could always buy and sell whatever quantities they wished at the prevailing prices. If producers could not sell all the goods they would wish to sell, they would employ less labor than in a Walrasian equilibrium. Robert Clower (1965) used essentially the same argument to argue that such constraints were necessary to make sense of the Keynesian consumption function and the multiplier. The key issue in this literature was the way in which market processes were conceived.

Neither Patinkin nor Clower provided a satisfactory account of why prices did not adjust to ensure equality of supply and demand, even though Clower's early, unpublished work focused on models of price adjustment. Towards the end of the 1960s there appeared two attempts to provide explanations. One attempt, widely forgotten now though highly cited in the 1970s, was provided in Axel Leijonhufvud's *On Keynesian Economics and the Economics of Keynes* (1968).[1] He argued that markets were characterized by a Hayekian discovery process, in which adjustment to changes in market conditions took place in real time, not in the timeless world of a Walrasian

[1] The term "highly cited" is used in preference to a phrase such as "very influential" because it is debatable whether many of those citing the book understood the point of what he was saying.

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tâtonnement in which trade takes place only when demands and supplies have been reconciled. If price changes take place in real time, there will inevitably be times in which demands and supplies are not balanced, which implies that quantity constraints—rationing with consequent spillover effects—will occur. Keynesian economics was thus a theory about a world in which market processes took place in real time. The other explanation, now much more familiar, is associated primarily with Edmund Phelps, who postulated that information was imperfect, causing people to make mistakes. If workers misjudged the real wage, then unemployment could result, even though everyone was behaving rationally given the information available to them. This approach of assuming optimization was used to provide a range of further explanations of unemployment: asymmetric information, efficiency wages, and, above all, imperfect competition.

Embedded in these discussions is a tension between two concepts of equilibrium. In one, equilibrium means that people are taking optimal decisions given the circumstances and the information they face; in the other, equilibrium means a state of rest, which may or may not imply that people are being rational. Corresponding to these two concepts of equilibrium are, of course, two concepts of disequilibrium. In one, disequilibrium means simply that people are not making optimal choices. From this concept of disequilibrium, it is a short step to the models of Robert Lucas in which behavior is assumed to be rational, implying that disequilibrium is not observed. Indeed, it may not even make sense. The issue of how the system moves to a situation in which agents are optimizing simply does not arise, for there is equilibrium all the time; people are modeled as always being rational. Dynamics in such models relate to the way in which such an equilibrium changes when shocks occur. If these shocks are modeled as being stochastic, then movements are unpredictable and in the longer term one can speak only of stochastic equilibrium.

In contrast, if equilibrium is simply a state of rest, with no necessary connection to individual rationality, and processes take place in real time, there are two possibilities. One is that there is perpetual movement—there is no state of rest, just continual motion. The other is that the system settles down to an equilibrium state in which there may or may not be disequilibrium in the sense that markets do not clear. Although he was responsible for seeing that this was an implication of events happening in real time, thereby justifying John Maynard Keynes's claim to be providing a theory that was more general than the classical, Leijonhufvud never provided more than a verbal account of such a world. Construction of a formal algebraic model of such a process that could be presented geometrically was the work of others. Robert Barro and Herschel Grossman's "A General Disequilibrium Model of Income and Employment" (1971) provided an account of a static equilibrium in which markets might exhibit either excess demand or excess supply. They did this through bringing together the spillover effects identified by Patinkin and Clower. Soon after, Jean-Pascal Benassy (1973) developed the theory, not only relating it to general equilibrium theory but also (more relevant for the present argument) providing a formal account of the dynamics of such a system.[2]

[2] I am attributing this work to Barro, Grossman, and Benassy because it was their work that stimulated the explosion of literature on the subject in the 1970s. A very similar model had also been presented by Robert Solow and Joseph Stiglitz (Solow and Stiglitz 1968) several years earlier, but, being focused on the "two Cambridges debate," which was of little interest to most economists, their work was rarely cited. Leijonhufvud, though his name was routinely cited in this literature, rejected the approach on which it was based as missing the point he was trying to make.

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In providing formal models of disequilibrium processes, these economists clearly had to simplify, and, where dynamics were made explicit, it was assumed that prices responded to excess demand or supply with a lag. The length of these lags, equivalent to speeds of adjustment, was what Lucas (1980) later criticized as "free parameters" not grounded in optimizing behavior.

This is where Samuelson comes into the story because his *Foundations of Economic Analysis* (1947) provides the canonical statement of this approach to modeling market dynamics. Part Two of that book also contained an explanation of how predictions about the effects of parameter changes could be obtained from dynamic "Keynesian" models such as Alvin Hansen's multiplier-accelerator model, translated by Samuelson (1939a, 1939b) from a numerical example into a second-order difference equation. This class of model had more in common with the lagged-adjustment model of market processes than might be apparent at first glance. It relied on parameters that could not be directly derived from optimization. The marginal propensity to consume and the acceleration coefficient might be the outcome of economic agents' optimizing behavior, but any link was indirect and played no role in the model.

"Paul Samuelson's Ways to Macroeconomic Dynamics" (Boianovsky 2020) is of wider interest than its title suggests because, as was noted above, although its main subject is Samuelson, it traces the history of approaches that were much more widely used. After *Foundations*, Samuelsonian methods were pervasive, with the result that the article can also be seen as a very compressed account of the history of economic dynamics since the late 1940s. The model of lagged adjustment was of course familiar before *Foundations*, being a representation of Léon Walras's *tâtonnement* process and intuitively appealing as an application of simple supply and demand analysis. However, even where they acknowledged Walras, economists had no need to go back beyond *Foundations* as the source of the techniques that were used to analyze such processes. The model was tractable, leading to systems of differential and difference equations, meaning that solutions could be analyzed using the same methods as were used to analyze the types of macroeconomic model that became popular for analyzing the business cycle.

As Mauro's article explains, despite the usefulness and intuitive appeal of this method of handling market dynamics and thereby concepts of equilibrium and disequilibrium, it was criticized from the start. A notable dissenter was John Hicks, who saw the method outlined in *Value and Capital* (1939) not as Samuelson did—namely, preparing the way for Samuelson's approach—but as superior because it was less mechanical. His failure to specify the finite adjustment speeds that Samuelson argued were needed if one were to specify solvable differential or difference equations meant, so Hicks claimed, that he was able to explore the role of expectations. Taking proper account of expectations was difficult with Samuelson's approach (assuming it was possible at all). In the early 1950s, Patinkin pointed out that there were important cases where Samuelson's correspondence principle did not work, and others challenged the assumption that the world was dynamically stable (Boianovsky 2020, pp. 616–618). A common argument that was to become much more common in the 1970s was that Samuelson's adjustment mechanism did not make sense in a world of maximizing agents because, as Donald F. Gordon and Alan Hynes pointed out, a mechanical adjustment rule implies price changes are predictable, but profit-maximizing traders would exploit such profit opportunities and

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"destroy the stability of the hypothetical differential equation" (quoted in Boianovsky 2020, pp. 618).

This was the prelude to the widespread demise of the Samuelsonian approach to price adjustment in the 1970s, which Robert Lucas equated with the neoclassical synthesis, the term introduced by Samuelson, the meaning of which became stretched to embrace not only the Keynesian ([1936] 1973, ch. 24) but also theoretical syntheses such as Patinkin's and the Hicksian IS-LM model that dominated textbooks at the time. As Lucas (quoted in Boianovsky 2020, p. 620) noted in relation to Patinkin's model, "All the dynamics are the mechanical auctioneer dynamics that Samuelson introduced, where anything can happen." This led into what was arguably the most important transition in economics, the eclipse of postwar Keynesianism by a macroeconomics centered on optimization and rational expectations, and is justifiably presented in Boianovsky (2020) as a rejection of the Samuelsonian approach to dynamics.

This transition from the hegemony of Keynesian theory to new classical and new Keynesian theories is so widely known and so generally accepted that it is unnecessary to document it in detail.[3] However, to understand it, we need to see that it comprises two histories, which, though sometimes discussed separately, are tangled together. This can be seen by considering the key figures in the transition. Focusing on dynamics (as in Boianovsky 2020) leads to Lucas being the prominent figure; his focus on continuously optimizing agents who have rational expectations led to his formulation of stochastic models and radically changed the way macroeconomists approached dynamics. In contrast, focusing on the operation of markets results in a history in which Phelps emerges as the central figure.[4] Like Leijonhufvud, his main concern was to understand real-world markets, in which uncertainty and limited information are pervasive. However, where Leijonhufvud turned Hayekian arguments on their head, using Hayek's vision of markets to defend a Keynesian position, Phelps, from his early work on the Phillips curve to his work on structural slumps, provided models of a type that economists trained in the methods codified in *Foundations* could appreciate and develop.[5]

III. SAMUELSON AND HIS INHERITANCE

Samuelson's *Foundations* was divided into two parts. The first and most well-known part, comprising seven chapters, was about equilibrium, covering comparative statics, and maximization or optimization. Fundamental to these chapters was the observation that, because they involve maximization, there are parallels between the theory of the utility-maximizing consumer and the profit-maximizing firm, and the importance of second-order conditions for comparative statics results. Aside from a chapter on rationing and one on welfare economics, these were substantially the same as the doctoral dissertation he had submitted to Harvard in 1940.[6] In explaining his approach, the method of comparative statics, he made it clear that although the behavior of the

[3] See, for example, De Vroey (2016, 2004). I deliberately leave aside discussions of the Ramsey model and overlapping generations models that also feature prominently in Boianovsky (2020).
[4] The works of Lucas and Phelps are, of course, connected, Lucas being a contributor to Phelps (1970).
[5] See Backhouse (2024).
[6] See Backhouse (2017) on the writing of this dissertation.

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system was, as in physics, "defined in terms of a given set of functional equations and initial conditions," a point on which he cited Ragnar Frisch and Jan Tinbergen, he proposed to abstract from problems involving dynamics, reserving those for later treatment (1947, p. 8). However, although he shares responsibility for separating the study of dynamics from the study of maximization, they were connected, as we shall see.

The second part of the book is crucial for interpreting what he was doing. Economists might distinguish equilibrium as the solution to a maximizing problem from equilibrium as a point of rest in a dynamic system, but remarks in this chapter make no sense unless he believed these could, at least in certain contexts, be equivalent. However, equilibrium would not necessarily be reached. In arguing in this way, he was framing his approach as partial equilibrium analysis: *Ceteris-paribus* conditions were *always* involved. As for the general equilibrium system of Walras, this, too, was in a sense a partial equilibrium system in that the factors taken as data "happen to be matters which economists have traditionally chosen not to consider as within their province," not just tastes and technology but also "the governmental and institutional framework, and many others" (Samuelson 1947, p. 8). There was nothing sacrosanct about these boundaries of the discipline.

When Samuelson turned to dynamics, drawing not on his thesis but on articles written and published in the early 1940s, he began with a chapter on the dynamic processes that came to be associated with his name. Hicks (1939) had derived stability conditions, but the only way to evaluate them was to deduce them from an explicit dynamic model. Samuelson makes it clear that he is trying to formalize Hicks's system using the methods laid out by Frisch and Tinbergen—namely, formulating the problem as a set of differential or difference equations. He then went on to analyze a modified version of this system proposed by Oskar Lange (1942, 1944) in which prices in different markets responded to excess demands at different speeds. His last example of the stability of multi-market equilibrium involved postulating that, instead of depending on flows of goods and services, prices changed in response to the level of stocks relative to an equilibrium amount, with a footnote noting that prices might change in response to both stocks and flows. He makes it even clearer that he is not proposing a specific dynamic theory but merely proposing a method that can be used to evaluate claims in the following section on the Keynesian model, as formulated by James Meade, Hicks, and Lange, as well as a simpler model involving dynamics. Here, as is well known, there is no optimization and so the assumption of stability must be used to derive comparative statics results.

In the following two chapters (10 and 11) Samuelson discussed other types of dynamic processes, including non-linear and stochastic systems. One way to view this is to follow some contemporaries in viewing the economic content as subordinate to the mathematics (see Backhouse 2017, pp. 475–478). For example, although Samuelson had a discussion of business cycle theory, he focused on the mathematical techniques involved, not on theories of the business cycle. One result of this is that, although *Foundations* provided an account of different concepts of equilibrium, including those related to the stochastic systems that came to be an important feature of macroeconomic modeling in the 1970s, the economic implications of such approaches were not explored. Dynamic economics were therefore left as depending on what Lucas (1980) was later to describe as "free parameters" not grounded in optimizing behavior. The result was that Samuelsonian dynamics came to be seen as centered on the simplified market models discussed in chapter 9.

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A further point he made was that:

> when we leave single economic units, the determination of unknowns is found to be unrelated to an extremum position. In even the simplest business cycle theories there is lacking symmetry in the conditions of equilibrium so that there is no possibility of directly reducing the problem to that of a maximum or minimum. (Samuelson 1947, p. 5)

This does not say that macroeconomic relationships, including much dynamic analysis, are not the result of optimizing behavior (much hangs on the meaning of the word "directly"), but it comes very close. This was far from an incidental, throwaway remark, for it was the basis for introducing a concept to which he attached great importance—the "correspondence principle"—the notion that comparative statics results can be derived from the assumption that equilibria are stable. In problems involving optimization, this is redundant, for sufficient information is contained in the second-order conditions for an optimum.

However, though there is much that is correct in this perspective, there is another way to view Samuelson's final chapter, "Some Fundamentals of Dynamical Theory." He does not focus solely on the dynamics criticized by Lucas but distinguishes between no fewer than six types of system: 1. Static and stationary; 2. Static and historical; 3. Dynamic and causal (non-historical); 4. Dynamic and historical; 5. Stochastic and non-historical; and 6. Stochastic and historical (Samuelson 1947, pp. 315, 316). The conclusion I want to draw from this list is not that he endorses one or another of these different systems but that they are rooted in a discussion of economists who were not mathematical economists. In his discussion he cites Frank Knight, Alfred Marshall, Joseph Schumpeter, Jacob Viner, Gustav Cassel, John Bates Clark, John Maurice Clark, and Lionel Robbins. Perhaps understanding him requires that we pay as much attention to these economists as to Frisch and Tinbergen.

Clearly, Samuelson was not a Marshallian, but to portray him simply as a Walrasian critic of Marshall would be going too far. His position is consistent with the view that modern market economies are so complex that to model them in every detail is, in practice, impossible, making partial equilibrium analysis inevitable. Markets operate in many ways, sometimes according to formal rules but more often in ways that are continuously evolving as new problems emerge. Even the notion of a "market" is an abstraction from much institutional detail. When we consider a system in which millions of goods and services are being traded, the problems multiply. It becomes necessary to find ways to simplify the resulting highly complex system in such a way that its operations can be understood: to look for common patterns found in individual markets and emergent properties arising in systems with many markets. These are all issues of which Samuelson was aware. Indeed, it is only when certain factors are determined outside the model that comparative statics results, to which Samuelson attached great importance, can be obtained. Simplification and hence partial equilibrium analysis were therefore preconditions for applying the mathematical techniques he believed were important.[7]

[7] Note that his famous criticism of Marshall was for not recognizing that where concepts were essentially mathematical, it was wrong to disparage the use of mathematics. Samuelson did not deny that there might be problems that could not be formulated as simple mathematics. The qualification "where concepts are essentially mathematical" is sometimes overlooked.

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Despite their many disagreements, Samuelson was adopting a Marshallian approach when he argued in this way: The price mechanism was too complex to be modeled in all its detail and the simplification of *ceteris paribus* was necessary. The mathematics of Marshall's theory of markets, analyzing one industry at a time using a mathematical framework inherited from Augustin Cournot (1838), was provided in a Mathematical Appendix to his *Principles of Economics* (Marshall 1961), comprising a mere 21 pages in a volume of 858 pages. It could be brief because he used his mathematics to provide no more than a skeleton for the vastly more complex ideas that he sought to explain verbally.[8] One reason for the length of the book compared with the brevity of the Mathematical Appendix is that, although his algebra was about maximum conditions and market equilibrium, he was obsessed with the problem of time, which he believed to be the essence of many economic problems and which he handled through dividing it into a series of periods, each one evolving into the next. This leads to the distinction, found in modern economics, between the short run, in which the capital stock is assumed to be fixed because there is insufficient time to change it, and the long run, in which investment can increase the capital stock. This is partial equilibrium analysis, holding constant things that adjust slowly, in order to analyze processes that happen more quickly.

In addition, growing up in an environment dominated by the evolutionary ideas of Charles Darwin and Herbert Spencer, Marshall was convinced that economic processes were evolutionary and that economics should model itself on biology. By this, he did not mean the mathematical analysis of modern evolutionary biology so much as classification and looking for patterns in a complex world. Evolution was everywhere. This meant that, although he used the mathematics of utility maximization, he claimed that preferences evolved in response to consumers' economic activities in ways that defied mathematical analysis. Similarly, the conventional use of fixity of the capital stock to distinguish the short from the long run was a simplification of a much more complex evolutionary process. Being an evolutionary process, this involved continuous and often slow change. However, his Mathematical Appendix covers none of this; dynamic analysis is completely absent, the only appearance of time in his algebra being in equations for optimization over time and expressions for present value. Samuelson was critical of some of Marshall's claims about biology, but his views were being formed in reaction to them.

A source to which Samuelson surprisingly attached much importance was John Maurice Clark's *Studies in the Economics of Overhead Costs* (1923). In 1946 he wrote Clark that on rereading the book, he had been "astonished" to discover how many of his ideas could be traced back to his first reading of the book as an undergraduate at Chicago (Backhouse 2017, p. 530). He did not say what he drew from it, aside from saying that they related to current debates over unemployment, but given that we know he studied the book carefully, it is worth noting that Clark concluded his chapter on general laws of value and distribution by saying that the problem of overhead costs was a dynamic problem, and that the modern economy was "dynamic and organic," a thoroughly Marshallian perspective (Clark 1923, p. 479). "Dynamic economics," Clark wrote,

[8] I ignore the use of diagrams, which can be considered mathematics, in footnotes. The boundary between mathematics and verbal analysis, as with so many other boundaries in his system, was blurred.

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"must not merely take account of them [overhead costs], it must be built around them, for they are part of its essential framework" (Clark 1923, p. 479).

Another way forward from Marshall in the 1930s with which Samuelson was very familiar was provided in Joan Robinson's *Economics of Imperfect Competition* (1933). Its significance for the present argument lies not in her codification of the diagrammatic apparatus of marginal and average cost and revenue curves that form the basis of much elementary economics, including Samuelson's textbook, but in her method. Without using algebra, Robinson was engaging in constructing what economists would soon describe as "models," arguing with a degree of precision that could have been captured using algebra. To achieve this, she abstracted from the complexities of Marshall's evolutionary processes. Where Marshall had assumed firms to be heterogeneous, going through a life cycle in which their costs and other characteristics changed, Robinson assumed competitors were identical. It was as though Marshall's evolutionary dynamics had happened instantaneously. She still had the distinction between the long and short run, but this was no longer the multi-dimensional process perceived by Marshall, to which formal analysis could be no more than a simplification, or the equally informally expressed dynamics of Clark. In her book, a short-run market equilibrium in which firms were making more than the normal level of profit induced both investment in new capital and the entry of new firms into the industry until zero profits were achieved. This was a dynamic process but a simplified, almost mechanical one, in which, in accordance with Marshall's method of partial equilibrium analysis, the focus was on different types of equilibrium.

Though sometimes bracketed with Robinson, the approach of Edward Chamberlin (1933) was very different. Not only did Samuelson read *The Theory of Monopolistic Competition* (1933), but he took Chamberlin's course in the subject. Samuelson believed Chamberlin to be antisemitic and a one-trick pony, but he considered his work to be very important. Chamberlin differed from Robinson in expanding the range of market structures far beyond those she considered, but the significant point for the present argument is that he provided a fuller account of how markets adjust to disturbances in the modern world where businesses do not simply respond to market conditions but incur costs in order to change them, such as by increasing or reducing their spending on advertising and taking measures to differentiate their products from those of other firms. Like Marshall, this material was expressed verbally and barely formalized, but he made it much clearer than did Robinson that dynamic processes were important.[9]

Clearly, Samuelson was indebted to much of Robinson's geometric analysis of competitive and imperfect markets, but there is a deep methodological parallel, too. Robinson justified her method of discussing highly abstract models by saying, "This book is presented to the analytical economist as a box of tools … and can make only an indirect contribution of our knowledge of the actual world" (Robinson 1933, p. 1).[10] Such a description would apply equally well to Samuelson's *Foundations*, even though, where Robinson focused on the need for precise assumptions and logical rigor, Samuelson focused on the use of algebra. Indeed, Samuelson's claim that many problems could be reduced to problems of optimization and his provision of substantial appendices

[9] Another teacher who was very important to Samuelson was Schumpeter, who offered an informal but highly dynamic view of competitive processes.

[10] She attributes the metaphor of tools to Pigou, another economist whom Samuelson held in high regard.

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on the mathematics of optimization and on difference equations make the "box of tools" analogy even more appropriate. His debts to Chamberlin are perhaps clearest in his elementary textbook, where he identified different types of market, including monopolies sustained by research and advertising (problems important for Chamberlin) and publicly regulated monopolies (Samuelson 1948, pp. 513–514). His discussion of chronically overcrowded, sick industries, with its account of firms continually entering and leaving, is reminiscent of Marshall's treatment of markets. He may not have portrayed firms as going through a life cycle, but Samuelson made it clear that firms with different costs and levels of efficiency will coexist. It is not a simple static equilibrium as is found in Robinson's long run.

IV. SAMUELSON AND MARKET PROCESSES

In "Paul Samuelson's Ways to Macroeconomic Dynamics" (Boianovsky 2020), Mauro has provided a thorough account of the many ways Samuelson approached economic dynamics:

(1) the adjustment of prices in response to excess demands and supplies (the *tâtonnement*);
(2) the multiplier-accelerator model of the business cycle;
(3) the overlapping generations model; and
(4) the theory of efficient markets.

Mauro even covered the extension of the multiplier-accelerator model that Samuelson wrote towards the end of his life as a tribute to Hansen (Samuelson 1988). Arguably, the only aspect of Samuelson's dynamics that he did not discuss is his excursions into demography, presumably on the ground that this is mathematical biology, not economics.[11] Of these four approaches, the first two dominated economic theorizing during the 1950s and 1960s, whereas the last two came into widespread use only after the revolution in macroeconomics associated with Lucas. Mauro also recounts the reactions of many of Samuelson's critics to these ideas, notably criticisms of the theory of price dynamics and the stability of general equilibrium, and the correspondence principle, thereby providing an overview of economists' treatment of dynamics that goes far beyond Samuelson.

Samuelson never abandoned his belief that there was a fundamental distinction between static equilibrium analysis based on maximization and dynamic models in which there was no maximization. This is a puzzle given that one might have expected an economist who attached such importance to revealing the mathematical structures common to apparently different problems to have sought an integration. Indeed, this is precisely what Lucas and his followers did, applying maximization techniques learned from *Foundations* to the analysis of dynamics and the business cycle. Mauro offers three explanations:

[11] Samuelson's mathematical biology is discussed in detail in Backhouse (2026).

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(1) Microfoundations of macroeconomics were not needed, because economists had models that had been shown to work.
(2) Samuelson was aware of the aggregation problems that arise when agents are heterogeneous.
(3) Competition is imperfect.

These explanations are all convincing, but there is more to the story.

Foundations was the work of a young economist, still finding his way. Although not published until 1947, the bulk of the book derived from a PhD dissertation submitted to Harvard in 1940, when Samuelson was twenty-five, based on work he had done even earlier as a graduate student and Junior Fellow. His discussion of welfare economics was new, but his discussion of dynamics, though not in the dissertation, had been published in articles written before he was thirty, during wartime when when he was busy with other commitments. The book had the character of a manifesto for mathematical economics; in some ways it was not so much a treatise on economics as a manual on how to construct economic theories, replete with appendices on some of the required mathematical techniques—optimization and difference equations. As such, it was more open-ended than it has often been read as being, notably in the very brief penultimate chapter (11) on the fundamentals of dynamic theory in which he distinguished between the six types of dynamic system listed earlier, including the notion of a stochastic system and, at least implicitly, a stochastic equilibrium of the type later used by Lucas, in which he was taking note of both mathematics and non-mathematical accounts of dynamics. However, although there were hints about other approaches, the view that maximization and dynamics were separate dominated *Foundations* both in terms of the number of pages devoted to different approaches and through his emphasis, reiterated in his final chapter (12), on the correspondence principle.

However, youth and finding his way cannot account for why he continued to uphold this position. This is where we need to turn to the explanations Mauro offered. As he points out, when questioned on the subject, Samuelson focused on the first of these: his belief that microfoundations were not necessary. This was consistent with his operationalism and no doubt reflects his lifelong admiration for Hansen.[12]

Mauro notes Kenneth Arrow's (Arrow 1967) criticism of Samuelson for his lack of interest in microfoundations. This argument can be taken further because, as was shown in section II, economic dynamics are so closely connected to understanding market processes, and Samuelson had what Arrow (1967, pp. 733–734) described as a "guarded and agnostic" attitude towards neoclassical price theory. Samuelson considered the monopolistic competition revolution comparable in importance to the Keynesian revolution, a point he made clear in his tribute to Chamberlin, his former teacher; Chamberlin had created a vision of the economic world in which there was a need for "market description and classification" (Samuelson [1967] 1972, p. 28). Samuelson's commitment to such a vision can be seen in his elementary textbook, in which he had argued that "pure competition" represented "the unusual and rare case where the firm knows its demand curve," citing examples of the intermingling of monopoly and competition that included "chronically overcrowded sick industries" and "monopolies maintained by

[12] This clearly motivated his last dynamic model (Samuelson 1988). See Boianovsky (2020) and Assous et al. (2024).

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constant research and advertising" (Samuelson 1948, pp. 509, 511, 513). Such skepticism about the relevance of pure competition (and *a fortiori* perfect competition) does much to explain his preference for "mathematical versions of the *General Theory*" over "Keynes's subtle intuitions" about market processes and why he did not take more interest in attempts to reconcile Keynesian economics with theories of general competitive equilibrium (Samuelson and Barnett 2004, p. 524). He therefore rejected Leijonhufvud's ideas, though he held Phelps, with his mathematical models of limited information, in high regard.[13]

The implication of this is that "Paul Samuelson's Ways to Macroeconomic Dynamics" does more than provide an account of the topic indicated by its title. Not only does it provide a history of work on economic dynamics that goes beyond Samuelson, but it also points towards a history in which studies of economic dynamics are more closely linked to wider discussions of market mechanisms than is usually the case.

COMPETING INTERESTS

The author declares no competing interests exist.

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[13] Phelps describes his close relationship with Samuelson in Phelps (2023).

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