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THE FORMALIST REVOLUTION OF THE 1950s

BY

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I. INTRODUCTION

Something happened to economics in the decade of the 1950s that is little appreciated by most economists and even by professional historians of economic thought. The subject went through an intellectual revolution as profound in its impact as the so-called Keynesian Revolution of pre-war years. I call it the Formalist Revolution after Ward (1972, pp. 40–41), who was the first to recognize the profound intellectual transformation of economics in the years after World War II.

It is common to think of interwar economics in terms of a struggle between institutionalists and neoclassicists but as a number of historians have recently reminded us (Morgan and Rutherford 1998, pp. 21–5; Mehrling 1997; Yonay 1998; Mirowski 2002, pp. 157, 190), "pluralism" is a more accurate description of the state of play in economics between the two World Wars. The extraordinary global uniformity in the analytical style of the economics profession that we nowadays characterise as neoclassical economics only dates from the 1950s.

The metamorphosis of economics in the late 1940s and 1950s is aptly called a "formalist revolution" because it was marked, not just by a preference, but by an absolute preference for the form of an economic argument over its content. This frequently, but not necessarily, implied reliance on mathematical modeling because its ultimate objective was to emulate the notorious turn-of-the-century Hilbert program in mathematics by achieving the complete axiomatization of economic theories. Do I exaggerate? Well, consider some of the leading publications of the 1950s.

The Formalist Revolution of the 1950s

1. Arrow, *Social Choice and Individual Values*, 1951.
2. Arrow and Debreu, "Existence of Equilibrium for a Competitive Economy," 1954.
3. Patinkin, *Money, Interest and Prices*, 1956.

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4. Solow, "Technical Change and the Aggregate Production Function," 1957.
5. Koopmans, *Three Essays on the State of Economic Science*, 1957.
6. Dorfman, Samuelson and Solow, *Linear Programming and Economic Analysis*, 1958.
7. Debreu, *Theory of Value*, 1959.
8. Sraffa, *Production of Commodities by Means of Commodities*, 1960.

We lack time to consider each of these in turn (but see Blaug 2003). However, the centerpiece is (surely?) the Arrow-Debreu proof of the existence of general equilibrium. It neatly exhibits the worst features of formalism, which is not just the application of mathematical techniques to economics, but rather reveling in mathematical modeling as an end in itself and treating the equilibrium solution of the mathematically formulated economic model as the final answer to the question that prompted the investigation in the first place.

The Formalist Revolution made the existence and determinacy of equilibrium the be all and end all of economic analysis. But what is new in that? Surely, pinning down the equilibrium solution of a model has always been the aim of economic theory? Well, yes and no. Equilibrium is the end-state of a process that we economists think of as competition. Economic analysis can emphasize the nature of the end-state or the nature of the competitive process that may converge on an end-state, but it can rarely do both in equal measure. Samuelson's *Foundations* (1947) had emphasized comparative statics as the predictive tool of price theory and that implied what he called the "correspondence principle," according to which the stability properties of an equilibrium complement the analysis of its determinants. Fruitful theorems in comparative statics, he argued, required a definite modeling of disequilibrium adjustments. What is little understood about the Formalist Revolution of the 1950's is precisely that the process-conception of equilibrium was so effectively buried in that period that what is now called neoclassical orthodox, mainstream economics, consists entirely of static end-state equilibrium theorizing with little attention to the stability of equilibrium: dynamic adjustment processes appeared only in unorthodox Austrian economics or equally unorthodox evolutionary economics. Let me explain.

II. THE ARROW-DEBREU RESTATEMENT OF WALRAS

The famous 1954 paper by Arrow and Debreu is regarded to this day as a truly rigorous proof of the existence of general equilibrium in a market economy, the fulfillment of Walras's dream eighty years earlier, but from our point of view it is also the perfect example of how concentration on the nature of equilibrium can crowd out analysis of disequilibrium processes. As soon as it appeared, it was hailed for its bold use of new mathematical techniques, replacing differential calculus by convex analysis, characterizing equilibria by separation theorems instead of tangencies, and employing the then relatively new tools of game theory and Nash equilibria (Weintraub 1991, pp. 104–107). What was little noticed at

the time was that this was also one of the earliest dramatic uses in economics of the so-called "indirect, non-constructive proof method" of modern mathematics. Arrow and Debreu used Brouwer's "fixed-point theorem" to prove the existence of general equilibrium and the essence of the fixed-point logic is to demonstrate a conclusion by showing that its violation involves a logical inconsistency by contradicting one or more axioms of the model. Such a "non-constructive" proof jumps directly from the axioms of the model to its final outcome: instead of constructing an example of whatever it is that is being justified, in this case the existence of equilibrium, it argues instead that equilibrium is logically implied by one or more of the axioms.[1] Modern existence proofs *à la* Arrow and Debreu are invariably non-constructive in that they make no effort to show how equilibrium comes about but merely that the existence of equilibrium is logically implied by certain plausible institution-free assumptions about economic behavior. One might say that they are possibility-of-existence proofs, not actual existence proofs.

Furthermore, Arrow and Debreu are perfectly frank in disavowing any claims that general equilibrium theory provides a descriptively accurate picture of the economy. In order to prove the existence of multi-market equilibrium, they are compelled to assume the existence of forward markets for all goods and services traded, a complete set of contingent commodity markets, the absence of idle money balances held by individual agents, the absence of market-makers holding inventories, the absence of bank credit, etc., and even so, they find that they can throw no light on the uniqueness or stability of general equilibrium. As they concede (Arrow and Debreu 1954, p. 266): "The latter study [of stability] would require specification of the dynamics of a competitive market." No wonder then that they made use of Nash's relatively new concept of equilibrium to solve the game of "an abstract economy." Because the justification for a Nash equilibrium is a negative one: a Nash equilibrium in a non-co-operative game is such that each player's independent strategy is the best response to the strategies actually played by his or her rivals and this is true for each player in turn; in short, nothing other than a Nash equilibrium can be the equilibrium solution of such a game because in the end no player can improve on the outcome. Note that this says nothing about the process whereby the equilibrium is obtained; it is absolutely silent about the expectations of the players, the correctness of their conjectures about the behavior of other players, their epistemic learning capacities, and so forth. Equilibrium is simply imposed as a fixed point in which market adjustments have come to an end (Weintraub 1991, p. 108).

It is not difficult to see that the Arrow-Debreu article is formalism run riot in the sense that what was once an economic problem—is simultaneous multi-

[1] For a child's guide to fixed-point theorems, see Dorfman, Samuelson, and Solow (1958), pp. 371–75. Arrow's (1951) social choice theory, which preceded the Arrow-Debreu paper by three years, is an even better example of the indirect-proof method: a set of plausible conditions are posited that restrict the individual and social welfare functions and then a mathematical proof is offered to show that it is impossible to satisfy these conditions simultaneously except under a dictatorial regime (Mirowski 2002, pp. 303–305). This impossibility theorem seems to imply that social choice in a democracy should be limited to no more than two alternatives, but whether this follows depends on the political constitution of society, which is a question that Arrow does not discuss.

market equilibrium actually possible in a real economy?—has been transformed into a mathematical problem about a virtual economy, which is solved not by the standards of the economics profession, but by those of the mathematics profession (Walker 1997, chap. 4). This is Bourbakism pure and simple, named after a continually evolving group of French mathematicians who since 1939 have been producing an encyclopedic work on mathematical structures exemplifying the Hilbertian axiomatic method (Weintraub and Mirowski 1994; Mirowski 2002, pp. 392–94). Debreu was a self-declared Bourbakian and produced his own *Theory of Value* (1959), which proudly proclaimed the virtues of formal axiomatization: "Allegiance of rigor dictates the axiomatic form of the analysis where the theory, in its strict sense, is logically entirely disconnected from its interpretation (Debreu 1959, p. 3). Koopmans (1957) had provided a child's guide to this new Bourbakian economics of decentralized decision-making two years earlier.

III. THE RISE AND FALL OF GAME THEORY

One of the historical puzzles that lies directly across our central decade of the 1950s is the virtual disappearance of game theory in the 1950s and 1960s after bursting on the scene in 1944 with the publication of *The Theory of Games and Economic Behavior* by von Neumann and Morgenstern. There is little doubt about the widespread disillusion among economists with early game theory, probably because it only offered definite solutions for two-person cooperative zero-sum games, which are largely irrelevant for economics (Luce and Raiffa 1957, pp. 10–11; Dorfman, Samuelson, and Solow 1958, pp. 445). After virtually passing into oblivion in the 1970s, game theory made an astonishing comeback to become just about the only language in economics with which to analyze the interactive behavior of rational agents. When we consider that game theory is perhaps the only example of a mathematical theory explicitly invented for the social sciences, its steady decline for something like a generation is almost as mysterious as its enthusiastic revival in the last two decades.[2]

The key to an explanation of the fall and rise of game theory in economics is the disappearance of disequilibrium analysis in Walrasian-inspired microeconomics and the increasing concentration on the end-state of equilibrium that was a marked feature of orthodox economics in the 1950s. Both interwar microeconomics and business cycle theory focused analysis on what Giocoli (2000a, 2000b) calls the "how and why" of equilibrium. Equilibrium had long been represented in economics as a balance of forces but it was Hayek in a number of essays in the 1930s who broke with this standard mechanical conception of equilibrium by introducing the essentially dynamic concept of equilibrium as a situation in which all the plans of agents are reconciled and made mutually

[2] As Mirowski (2002, p. 479) observes: "When the history of economics in the last quarter of the twentieth century comes to be written, the most important and incongruous phenomenon that will cry out for explanation will be the fall and rise of game theory from disparaged minor adjunct of operations research and decision theory to glistering centerpiece of orthodox neoclassical economics."

consistent, such as to confirm their plans and expectations (Ingrao and Israel 1987, chap. 8; Weintraub 1991, chaps. 2, 5). In short, what emerged as the central question in pre-war economics was just how self-interested agents in a multi-period decision-making context learn to formulate and revise their plans and expectations. However, early game theory as summed up in Neumann and Morgenstern's opus did not derive from these concerns in pre-war orthodox economics but from the mathematical formalism descended from Hilbert. The average economist in our decade of the 1950s, despite Arrow and Debreu, found it difficult to grasp an equilibrium concept based on the formal logic of fixed-point proofs, lacking any positive interpretation in a process converging to equilibrium. It is this reluctance that accounts for the delayed acceptance of early game theory by the economics profession: this delayed acceptance included the now ubiquitous Nash equilibrium concept because, as published in 1951, Nash's papers defended the idea of Nash equilibrium by a negative, fixed-point justification. In his doctoral dissertation, Nash (1996, pp. 32–33) offered a positive justification for his equilibrium concept in what he called "mass action" or what we now call an "evolutionary" interpretation (Milath 1998)—in an iterative adjustment process: boundedly rational players gradually learn to adjust their own strategies to get a higher pay-off after observing other players, a process which eventually converges to a Nash equilibrium. However, at the prompting of his supervisor, David Gale, Nash cut out the pages proposing this from the published version of the thesis in the 1951 *Annals of Mathematics* (Leonard 1994; Jacobson 1996). Instead, he used the Neumann-Morgenstern argument that if each player had common knowledge of the game-structure and was hyper-rational in possessing instant and perfect computational powers, not to mention perfect empathy despite the absence of any communication between players, then equilibrium in a game would necessarily be a set of pay-offs whose violation would be inconsistent with these assumptions (Mirowski 2002, pp. 339–49). This is precisely what we earlier called a negative justification for equilibrium. All the old criticisms that had been constantly hurled at classical duopoly theory—why should duopolists continue myopically to assume constant reactions from their rivals irrespective of experience?—were swept away by Nash's invitation to leap directly to the final long run equilibrium without regard to any time-consuming process of adjustments converging on equilibrium.[3] Equilibrium could now be defined without getting into the messy question of who actually announces the price that is finally determined.

When Arrow and Debreu employed game theory and the Nash equilibrium to prove existence of general equilibrium in the 1950s, the Formalist Revolution was still in its early stages. It took another decade or more for formalism and Bourbakianism to break down all resistance to game theory and fixed-point proofs of non-cooperative equilibria. It was only in the 1970s that Nash equilibrium was accepted as the basic equilibrium concept of neoclassical

[3] As Ken Binmore (Nash 1996, p. xii) rightly observed: "Nash's 1951 paper allowed economists, not only to appreciate the immensely wide range of possible applications of the idea of a Nash equilibrium, it also freed them of the need they had previously perceived to spell out the dynamics of the relevant equilibriating process before being able to talk about the equilibrium to which it will converge in the long run."

economics when, of course, it was characterized as the very embodiment of the criterion of rationality that had always been an essential feature of economic theory.

IV. BACK TO WALRAS

We have described the Arrow and Debreu paper as the capstone of the Walrasian program but we must now try to appraise their achievement from the vantage point of a half-century later. The ascendancy of the end-state conception of equilibrium and the almost total disappearance of the process-conception of equilibrium, which is my language for what Arrow and Debreu managed to accomplish, has its roots in Walras himself, who in successive editions of his *Elements of Pure Economics*, allowed the existence-of-equilibrium question to drown the problems of uniqueness and stability of equilibrium.

The fate of Walras's *Elements* is not unlike that of Neumann and Morgenstern's *Theory of Games and Economic Behavior*: it suffered a gradual demise after Walras's death in 1910 and by 1930 it is doubtful that there were more than a half-dozen economists in the world who had ever read Walras, much less understood him. From this state of total neglect began the rise, which eventually brought general equilibrium theory to the front ranks of economic theory in the post-war years. Hicks, Hotelling, Lange, and Samuelson were responsible in the golden decade of the 1930s for bringing about this remarkable revival of general equilibrium theory (Blaug 1997, pp. 77–78; Samuelson 1985, p. 1384n). In the writings of these earlier defenders of Walras, general equilibrium theory was treated as a quasi-realistic description of a market economy, which was perfectly capable of confronting practical questions, such as the feasibility of "market socialism." But in the work of contemporary Viennese mathematicians, general equilibrium theory gradually began to undergo axiomatization, setting aside all concerns with verisimilitude, let alone empirical verification, leading directly to the Arrow-Debreu paper and Debreu's *Theory of Value* in which general equilibrium theory is boldly defended as a self-sufficient mathematical structure, having no necessary contact with reality, or at most, as in Arrow and Hahn's *General Competitive Analysis* (1971), providing a purely formal picture of the determination of economic equilibrium in an idealized decentralized competitive economy. Considering that this fundamental reinterpretation of Walras took less than a generation, this is really one of the most remarkable *gestalt*-switches in the interpretation of a major economic theory in the entire history of economic thought.

V. IS GENERAL EQUILIBRIUM THEORY MORIBUND?

Let us briefly consider how the neo-Walrasian Research Program has turned out some fifty years after Arrow and Debreu. The existence proof of Arrow and Debreu stands up today as it did in 1954, if only because the method of indirect proof that they employed is logically impeccable and is immune to revision on grounds of new evidence, being concerned with little else than the notional

consistency of the trading plans of purely virtual agents. What it signifies, however, is another question. It is difficult to see how or why such negative proofs should ever have been thought to be of economic interest inasmuch as the method of proof bears no resemblance to any recognizable economic mechanism. Even if we suppose that disequilibrium prices are ruled out by assumption, the interesting question of how trading plans based on predetermined equilibrium prices can actually be carried out is never even raised. Indeed, the very idea of demonstrating a link between the mathematical solution of the existence problem and the outcome of market interactions was simply abandoned by Arrow and Debreu. In short, what is missing in general equilibrium theory and hence in neo-Walrasian microeconomics is, quite simply, competitive rivalry between transactors in actual markets. We have forgotten, as Clower (1997, p. 806) aptly expressed it, that "the invisible hand also has 'fingers'" (see Costa 1998, chap. 4).

So much then for the existence problem. As for uniqueness, it has been shown that general equilibrium entails one and only one price vector if, and only if, all commodities are gross substitutes for one another, an assumption that is, to put it mildly, highly unlikely to be true. Finally, there is the crucial question of stability. The static properties of equilibrium have no practical meaning, unless they persist in the face of small disturbances and emerge fairly quickly after the appearance of disturbances. To believe in the empirical relevance of general equilibrium theory is to rely on the dynamic stability of equilibrium (Fisher 1983, pp. 2, 3, 17, 216). Now it is perfectly true that the hypothesis of relative stability possesses an inherent plausibility because, as Samuelson (1947, p. 5) once said, "how many times has the reader seen an egg standing on its end?" But that is probably due to the presence of non-price coordinating mechanisms, such as prevailing conventions, market rules and procedures, technological constraints and the like, all of which do little to establish the stabilizing properties of general equilibrium pricing models. Such standard stability assumptions, originating with Walras's analysis of *tâtonnement*, such that the price of each commodity changes proportionately to its excess demand, does not follow from any well-established market institution or plausible behavioral rule (Walker 1997, chap. 5). Despite a considerable literature on both local and global stability, the upshot of the discussion so far is a more or less total impasse: not only are we unable to prove that competitive markets are invariably stable but we have gained little insight as to the features of markets that render them more or less stable (Ingrao and Israel 1990, p. 361–62). Thus, a leading textbook in microeconomics, after claiming that the Arrow-Debreu existence proof is one of the great achievements of modern economics, admits that "economists are good (or so we hope) at recognizing a state of equilibrium but are poor at predicting precisely how an economy in disequilibrium will evolve" (Mas-Collel *et al.*, 1995, p. 620; also Luenberger 1995, p. 224).

We reach the curious conclusion that equilibrium in general equilibrium theory is known not to be either unique or stable and that its very existence can only be demonstrated indirectly by a negative proof. Nevertheless, general equilibrium theory continues to be regarded as the fundamental framework for theoretical discourse and the basis of computable macroeconomic models. It is even taken

to be the essential basis of project evaluations in welfare economics (Starr 1997, pp. 151, 194). Is this yet another example of an emperor who has no clothes? (Kirman 1989).

VI. RESPONSES TO THE FAILURE OF GENERAL EQUILIBRIUM THEORY

There have been a number of responses to the apparent failure of general equilibrium theory to live up to its own promise to deliver rigorous solutions to the problems of existence, uniqueness, and stability of equilibrium. One response is to claim that general equilibrium theory, despite its limitations, can somehow be employed negatively to refute certain widely held economic propositions. That was Frank Hahn's classic defense, and I have elsewhere argued against this *jujitsu* move (Blaug 1990, chap. 8). Another response is simply to hedge ones bets in the hope that any moment now general equilibrium theory will suddenly and mysteriously be transformed by a dose of realism (Ingrao and Israel 1990, p. 362). More interesting than any of these is Weintraub's defense by way of "constructivism." For Weintraub (1991, pp. 108–109), "equilibrium is a feature of our models, not the world" and stability of equilibrium is not something "out there" in the economy. His study of the stability literature is "constructivist." Knowledge in science, he argues, is socially constructed in the sense that it has meaning only within the discourse of the relevant community, in this case that of theoretical economists. So, questions about scientific validity or empirical support for general equilibrium theory have no meaning if only because the theorists that play the Wittgenstein language game called general equilibrium theory do not concern themselves with such questions. The book is studiously, almost painfully, constructivist in never endorsing or criticizing the epistemic claims of general equilibrium theory.

Constructivism has a multiplicity of meanings. In its most extreme form, it argues that scientific knowledge, including economic knowledge, is the product of intentional human activity and is therefore essentially a "fabrication"—it is made rather than discovered (Ziman 2000, pp. 333–39). It is not clear whether Weintraub wants to go quite so far. Are we really to believe claims that derive from comparative static propositions, such that a rise in unemployment compensations will increase unemployment, are just assertions about the logical properties of models and say nothing about the state of the world? Whatever happened to the "correspondence rules" that all of us attach to economic theories, explicitly or implicitly? When economists are told that a tax on butter will raise the equilibrium price of butter they have learned from the "correspondence rules" of the theory of market equilibrium that this conjecture is true only if price-elasticities are such and such. They will regard the proposition in question as having considerable policy-relevance because it involves definite assertion about the nature of reality and not just moves in a language game.

Weintraub's "constructivist" interpretation of equilibrium is the last stage in his long journey over several books and many years to construct an impregnable defense of general equilibrium theory. If general equilibrium is not an actual real

state of affairs that could conceivably happen, but just a heuristic device, a point of reference, a way of talking, then to ask whether there are missing markets for some goods or whether agents have perfect foresight has the same sort of meaning as to ask whether there really are an infinite number of primes or whether the square root of a negative number is only defined in terms of the imaginary number *i*. But this is bad history because whatever we now say, since Arrow and Debreu, the followers of Walras and Pareto, not to mention Walras and Pareto themselves, had no doubt that general equilibrium theory dealt with substantive real-world, policy-relevant issues.

VII. PERFECT COMPETITION AND ALL THAT

There is one element in the story that we have so far ignored but we must now bring in to round off the argument about the shortcomings of general equilibrium theory. It is the concept of perfect competition, which surprisingly enough, was invented *de novo* by Cournot in 1838 (Machovec 1995, chap. 2; Blaug 1997a, pp. 67–71; Kirzner 2000). The concept itself and the analytical habits of thought associated with it, particularly the concentration on an end-state conception of competitive equilibrium in which firms appear solely as passive price-takers, was alien not just to the great economists of the classical past but even to the early marginalists in the last quarter of the nineteenth century (with the sole exception of Edgeworth). The perfectly competitive model, which we now think of as standard neoclassical microeconomics, made its debut in the writings of Frank Knight in the 1920s and then hardened into dogma by the spread of imperfect and monopolistic competition theory in the 1930s (Machovec 1995, chap. 8; Blaug 1997b, p. 68; High 2001, pp. xiii–xlv).

It involved the suppression of the idea that markets might adjust, not in terms of price but in terms of quantity, or at least more quickly in terms of quantity than in terms of price. Marshall and Walras never saw eye-to-eye in respect of the stability conditions of a competitive market, but neither made it clear that the disagreement was a disagreement about the concrete process of competition (Blaug 1997a, pp. 72–76). In Marshall it is a production economy in which sellers adjust output in response to excess demand price that is the paradigmatic case of market adjustment, whereas in Walras it is an exchange economy in which buyers adjust price offers in response to excess demand that is taken to be the typical case. The revival of general equilibrium theory in the 1930s buried the very idea of quantity-adjustments even in labor markets, and once the Formalist Revolution got under way in the 1950s, the virtual ban on disequilibrium analysis completed the triumph of price adjustments as the only way that markets ever respond to shocks. In a brand of economics that was increasingly static, all the non-price forms of competition—favorable locations, product innovations, advertising wars, quicker deliveries, improved maintenance, and service guarantees, etc.—were assigned to such low prestige subjects as Marketing and Business Management. Even Industrial Organization, the one sub-field in economics where students of business behavior might expect to learn something about competitive rivalry, only survived as part of the standard curriculum

offering of a university economics department in the 1970s and '80s by adapting game theory as its principal analytical tool.

Perfect competition never was, nor ever could be, all the textbooks agree (Blaug 1997, pp. 70–71), and yet the real world is said to be approximately like, not far from, very close to, the idealized world of perfect competition. How do we know? Because historical comparisons tell us so. It is such informal, non-rigorous appraisals that convince us that competitive markets perform better than centrally planned economies. Market economies are informationally parsimonious, technically dynamic, and responsive to consumer demand, and that is why we rate capitalism over socialism despite periodic business depressions and unequal income distributions (Nelson 1981). In short, we appraise the private enterprise system in terms of the dynamic consequences of market processes and leave all the beautiful statical properties of end-state equilibria to classroom examination questions.

VIII. CONCLUSION

The central question of orthodox pre-war microtheory—how is market equilibrium actually attained?—has been shunted aside ever since the Formalist Revolution of the 1950s. In general equilibrium theory, the question of whether it exists at all dominated the issue of convergence to equilibrium so successfully as to swallow it up entirely. Even game theory begged the question because its definition of equilibrium as the solution of a game makes sense once we have arrived at the solution but in no way explains how we got there.[4] That everything depends on everything else is no reason to think that it depends on everything else simultaneously and instantly without the passage of real time, that neither prices or quantities are ever sticky, that since information is usually symmetric for both sides of the market there are no missing markets, that price-taking is just as universal out of equilibrium as in equilibrium, in short, that the metaphor to thinking about price determination in terms of the mathematics of solving simultaneous equations may be misleading. Indeed, I would contend that the simultaneity of price determination that is deeply embedded in general equilibrium theory has proved in the fullness of time to be a grossly misleading metaphor. The best way *not* to learn how markets function and how a competitive economy actually works is to study general equilibrium theory.

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[4] As Dosi and Winter (2002, p. 146) once said: "Evolutionary theories share the methodological imperative of '*dynamics first*'! That is, the explanation of why something exists, or why a variable takes the value it does, ought to rest on a process account of how it became what it is." The hallmark of all general equilibrium explanations of economic phenomena is the very opposite: something exists because it does and nevermind how it came to be there.

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