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Is the Theory of Competitive Equilibrium With It?

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Recent discussions in the literature have raised serious challenges to the theory of competitive equilibrium. Looking at the debate, I perceive two main issues. I put them in the form of two questions to which, by way of advance summary of my remarks, my own tentative answers are attached.

One. Does the model of competitive equilibrium (the "*CE* model") in its simplest form represent one useful pure and special case, one valuable foothold for a steep climb? My answer: Yes.

Two. Can we as yet evaluate the merit or promise of the various ramifications of the theory in recent literature? My answer: I find it hard to assess this fascinating blend of high achievements, challenging starts and possible dead-ends.

I shall mix the motivation of my answers in with comments on some recent criticisms. I am thinking in particular of John Kenneth Galbraith, Nicholas Kaldor, Janos Kornai and Martin Shubik. Since Professor Galbraith is with us, I shall not try to anticipate him.

Beginning with question one, I think the issue does not lie in the mathematical form of the theory. The contributions made by mathematical reasoning were necessary if the problems put were to be answered. The issue is the problems put. I do not hesitate about my "yes" to question one, because of the great value to economic theory of a fully worked out special case. I entirely agree with Kornai about the many aspects of reality ignored in the *CE* model which are brought out in his book: the importance of the control system in all existing economies, the role in that system of information about quantities as well as prices, the importance of increasing returns to scale, etc. I differ only on what to do from here on. Kornai's "revolutionary" proposal is now to start afresh with entirely new approaches embodying other aspects of reality, which he enumerates with care and perception. The "reformist" alternative, which Kornai rejects, is to amend and extend the given special case by grafting other important aspects on to it. I think *both* should be attempted.

Kaldor also calls for "a major act of demolition" of the basic conceptual framework of equilibrium theory, without which "it is impossible to make any real progress." The case for the reform approach can be illustrated by what seems to me Kaldor's gravest charge: that equilibrium theory ignores the pervasiveness and inexhaustibility of increasing returns to scale. I think that this important point can be met at least half-way, by introducing further assumptions that bear on the way time and space enter into the problem. As to time, the construction of the next generation of capital goods for larger scales of processing requires time. Kaldor recognizes that the allocation of the existing capital stock and other factors to current production can meanwhile be administered by short-run equilibrium prices. A recent paper by M. L. Weitzman for a two-sector model leads me to expect that an efficient stringing together of a sequence of such

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temporary equilibria is mathematically feasible in the presence of increasing returns, at least in terms of an "optimal growth" model. If so, this would support an allocation scheme on the "Hungarian plan," in which investment in large capital units is centralized, while capital utilization decisions are decentralized in a price-guided way. However, extension to a complete intertemporal market model might be subject to the same realistic limitation as that noted by Kaldor in regard to exhaustible resources: Firms might not exercise foresight long enough ahead for the time scale required by the problem. A relevant spatial factor is the cost of transporting the product from producer to user. This will slow down the increase in scale, and may even balance it out in an ultimate equilibrium for a number of goods.

Both Kornai and Shubik stress the need for a more detailed modelling of the circumstances and constraints under which suppliers and demanders obtain access to each other and conclude their trades. In particular, Shubik is concerned with constraints on credit that are expressed by a liquidity requirement constraining an agent at every moment of time. This is in contrast to a budget constraint applicable just to the sum of expenditures minus receipts over an accounting period. It will require modelling the economy as a process, a sequence over time of moves fully feasible to the individual acting by himself. In the presence of uncertainty this should include bankruptcy proceedings as a possible outcome for the agent. Shubik may well be right in his claim that this model cannot be accommodated within the framework of the *CE* model. However, logical links are undoubtedly present.

The asperity of some of the criticisms seems to me in part provoked by the spill-over into mathematical economics of attitudes and traditions imparted by training in mathematics. Mathematical economics, while long on rigor, has generally been short on interpretation and practically silent on motivation for its choice of problems. Thus, the sheer volume of work expended on the theory of equilibrium is taken by the general economist as evidence of a strong belief in the explanatory reach of the model. I think the explanation is different. While I was searching for words to express the phenomenon, the precise words were presented to me by David Freedman, mathematician, probabilist and statistician, in a recent seminar at Yale: "Mathematicians are incapable of leaving well enough alone." In other words, any particular problem or model should be plumbed to its full depth and given its most general formulation. As a result, the shifts of focus, the breaking out of molds needed from time to time in any empirical science, take longer to come about.

The counterweight to this element of mathematical style must and has come from economists with a more pragmatic outlook. There is a continuing need for general and critical discussion of the choice of problems to be examined in economic theory. By its nature, rigor has no hold on this question. While the criticism takes shape, substantial further progress has been and is being made within the *CE* model, due also to its great flexibility of interpretation. I shall expand on my answer to question two in a brief discussion of this work. The task is facilitated by the important recent book by Kenneth J. Arrow and F. H. Hahn. It breaks a tradition by being generous with interpretation and by providing in the preface a motivation that is specific, frank and subtle. Important to the general economist is its careful discussion of present knowledge about the uniqueness and stability of equilibrium. The validity of the widely used method of policy analysis by "comparative statics" is found to be as yet subject to qualifications.

Another innovative recent book, by Herbert Scarf (with the collaboration of T. Hansen), enables us to make such applications with greater precision and to cases involving more variables and relationships. The algorithm approximates an equilibrium as a fixed point of a continuous mapping. An application to a problem of taxation has been made by John B. Shoven and John Whalley.

Two earlier developments are in the nature of magnificent *tours de force*, enriching our insight, but with a somewhat strained relation to reality. One of these, originated by Arrow (1953) and extended by Gerard Debreu (ch. 7), introduces uncertainty by considering trade in commodities contingent on the "state of nature." The approach is a heroic attempt to stretch the *CE* model as far as it will go. Roy Radner (1967, 1968) examines the limitations of the attempt connected with the different information requirements placed on the agents.

The other *tour de force* concerns the connection between the set of competitive equilibria and the game-theoretical concept of the core of an economy when the number of agents increases without limit. Roughly, an allocation is in the core if no coalition of any number of agents can do better for its members by coordinated quantitative bargaining. The principal result, suggested or proved with increasing generality by Shubik, Scarf, Debreu and Scarf, and Arrow and Hahn (ch. 8), is that the larger the number of agents, the closer any outcome of core-type bargaining comes to some competitive (price-guided) equilibrium. A fanciful extension by R. J. Aumann (1964, 1966), Karl Vind, Debreu, W. Hildebrand, Arrow and Hahn (see ch. 8) and others utilizes the concept of an economy with an infinity of agents, whose characteristics are continuously distributed in a measure space.

The interpretative weakness of both *tours de force* lies in the information handling requirements implicitly placed on the agents without regard to cost or even feasibility. In the case of contingent trading, these requirements go up in proportion to the number of states of nature, hence much more steeply than the number of future periods beset with uncertainty. With regard to the concept of the core, they go up in proportion to the number of coalitions containing a given member, that is, much more steeply than the number of members. It seems to me that these two lines of work will either remain standing as essentially unused but brilliant interpretational or mathematical feats—or one or both may possibly be the beginning of a long development in which institutional detail is introduced piece by piece, to represent the limitations of markets that deal with the future, or the various barriers on coalition formation existing in society.

I conclude with a few remarks about the finite competitive economy that assumes objective certainty about the outcome of given actions by all agents. There has been an increasing concern in the recent literature with fitting adjustment processes into that model. I attribute this concern to a weakness in the model—not a logical but an interpretative weakness. Optimizing responses of economic agents are simultaneously feasible only if the proper prices are already known to them. But these prices must somehow themselves be the result of these same responses. Thus there is something circular in the description of events. The market participants must be endowed with extrasensory perception (if acting simultaneously) or with supernatural premonition (if acting successively). One is thus led to think in terms of a process in which information flows, the individually feasible actions they induce, and the new flows generated by the actions, etc., are spelled out sequentially. I believe (with Kornai and Shubik) that

such processes are now the more challenging objects of research, for the following reasons:

1. The notion of a process does not presuppose the approach to an equilibrium.
2. A process that does not approach an equilibrium (under constant technology and preferences) can be of great interest in itself.
3. An equilibrium that is not approached by any process that starts from a different initial state is of no interest in itself.
4. With the process notion, one can relieve the strain on our credulity and experiment with diminished degrees of individual rationality, perception, foresight, capacity for computation, and formalization of states of uncertainty. We could then at least ask how much of such relaxing of rationality still allows an equilibrium with some optimality properties to be approached.
5. In a world of continuing but only dimly foreseeable change in technology and in preferences, the notion of equilibrium disappears, but that of an adjustment process remains.

Possible connections between processes and equilibrium can be glimpsed from some recent work in "optimal growth" theory. One can look at the study of optimal growth as a scouting device that suggests useful conjectures for more complicated market processes which do approach an optimizing equilibrium. Some pointers of this kind are contained in work on many-sector models by W. R. S. Sutherland and by T. Hansen and Koopmans, and in analogies with known properties of one- and two-sector models. The "invariant capital stock," which generalizes to n dimensions the "golden rule stock (modified by discounting)," has a mathematical structure similar to that of a competitive equilibrium. It is a "fixed point" of a mapping and can be computed by fixed-point methods. This approach may therefore allow a reconnaissance of the problem of distinguishing interesting equilibria from uninteresting ones.

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