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On the nature and utility of the concept of equilibrium

The first part of this article presents a brief and conclusionary commentary on “equilibrium,” the subject of this minisymposium. Its conclusionary character is emphasized as a caution: I shall present conclusions substantially without specific historical example or complex analytical exegesis. Basically, I will make a number of points, each of which both stands on its own and should be considered in conjunction with the other points. This part was written, prior to my reading of Mark Setterfield’s article, to assemble, hopefully in a coherent (but general and conclusionary) form, ideas on equilibrium which I have developed over a period of about forty years. The second part comments on Setterfield’s article using the arguments made in the first part.

The concept of equilibrium

1. The concept of equilibrium is a metaphor, a story design, and a tool.

It is a metaphor for statics and stability as well as for harmony. Since those terms themselves can be considered metaphors, such demonstrates the fragility, ambiguity, and inconclusivity of human reason.

It is a story design insofar as it is used to define economic “reality” and to structure the stories told by economists. As such, it is one of numerous designs of this kind.

It is a tool in that it is an instrument of analysis. It does not directly describe or explain the actual economy but is a tool to be used in inquiry.

Like any tool, equilibrium has strengths and limitations. Moreover, it is only one tool in the research toolkit of economists.

To the extent that economics is a discipline of one tool, however, its toolkit is severely narrowed and its work impoverished.

Aspects of the foregoing will be considered below.

2. As a concept, equilibrium has been given numerous specifications.

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Accordingly, except insofar as any piece of economic analysis explicitly or implicitly specifies its definition of equilibrium, the concept is used as what philosophers call a primitive, undefined term. And when a definition is provided, the analysis applies, strictly speaking, only to that definition.

The content and nuances of equilibrium, however defined, vary as between different conceptual times periods (market period, short run, long run, etc.), different intertemporal positions (*ex ante*, *ex post*), and different conceptual structures (business cycle, secular movements).

3. Consider the proposition, “The Invisible Hand is the competitive market which generates, *inter alia*, equilibrium.” The term “Invisible Hand” is a metaphor, as is the term “market.” The terms “competitive” and “equilibrium” each have been defined in different ways, with the various definitions sometimes, perhaps often, in conflict with one another in terms of their interpretation of economic phenomena.

4. The equilibrium tool is widely useful for a particular purpose: the tracing out of the logical if not also substantive consequences of changing one (or more) of a number of variables in a model. In this use, it supplements the even more useful tool of abstraction, that is, of reducing the number of active variables to a manageable few and holding the rest stable or constant under the rule of *ceteris paribus*.

Another, related, function of the equilibrium tool is to serve as a check on the logicity of the user’s treatment of adjustment.

The equilibrium concept is part of a deductive system. Deductive systems produce results that are valid (properly derived from premises), and not necessarily results that are true (correct description or explanation).

The significance (and limits of significance) of equilibrium results differs between such uses as equilibrium within the confines of a particular model and equilibrium in the actual economy.

Equilibrium, or the specification of the formal conditions and/or substantive content of equilibrium, is derived from the content of the model used. It is erroneous to posit some equilibrium position—as if it were transcendental, self-subsistent, and commanding—and then consider certain phenomena as disturbances or deviations from it. Outside the confines of a particular model, no equilibrium position exists. Phenomena have meaning, so far as is relevant to this discussion, only with regard to the *working out* of equilibrium (actually equilibration) and to other variables within the adjustment or equilibration process, and only within the posited model, and not with regard to the equilibrium

result treated as some teleological or ontological given. It is all very instrumental, problematic, and conditional.

5. The equilibrium tool in economics has been combined with other conceptual tools in the construction of the now-conventional neoclassical research protocol. This protocol is constructed with the objective of generating *unique determinate optimal equilibrium* results for any analytical problem. In order to produce such results, both methodologically and substantively limiting assumptions must be made, so that the outcome is (a) determinate, (b) unique, (c) optimal, and (d) equilibrium. All variables that might interfere with the generation of such an outcome are excluded by assumption. (No wonder a common joke about economists portrays them as solving problems by assumption.)

6. Application of the conventional neoclassical research protocol, for all its utility, has several consequences in the nature of limitations. These consequences issue from the equilibrium tool in combination with the other elements of the protocol.

The focus is on the existence, conditions, and stability of the equilibrium result.

Generation of the equilibrium result is channeled and constrained by the requirements that the result also be determinate, unique, and optimal.

The practice strongly tends to ignore, or at least typically to treat only secondarily, three matters of enormous concern in actual economies: the substantive factors and forces that drive the economy, the resulting disequilibrium process, and the substance and process of adjustment to (conceptual) equilibrium.

7. One can define economic space and system such that some equilibrium is projected to exist at every moment. This is a particular specification of the tool and, with it, of the economy. But in actual economies it is either more accurate or more useful a tool to affirm that equilibrium never actually exists. Economic agents and economies are therefore never in equilibrium and tend toward equilibrium only in a very weak sense, the sense in which equilibrating adjustments are continuously and ubiquitously taking place, but no equilibrium situation ever actually comes to exist. Equilibrium is a modeling technique, a tool, not a definition of reality. Equilibration and disequilibration take place but no actual equilibrium exists.

8. With due regard to what Fritz Machlup called their kaleidoscopic definitions, equilibrium analysis is typically conducted in a static

manner, and even dynamic analysis is constrained when conducted in pursuit of unique determinate optimal equilibrium results. Among the conditions that equilibrium analysis obfuscates are those that encompass what Joseph Schumpeter called creative destruction. These are factors and forces that generate the actual disequilibrium situations and processes to which the equilibrium tool is brought to bear. These include changing demographics, technology, power structures and group psychology, and the factors and forces that generate them, including conditions leading to changing choices, such as the presence of multiple principles, nonsingular decision making (the heterogeneity of decision-making structures and organizations), the continuum of ends and means, complex and circular causation, the ambiguity and inconclusiveness of terms used in principles employed in decision making, and so on. All these generate both endogenous and exogenous change and disequilibrium.

9. However further specified or amplified, the concept of equilibrium can in principle be utilized as a tool in the analysis of any process in which change, or the balancing of continuity and change, or of any other dialectical variables, takes place.

It would seem, however, that just as pliers, saw, scissors, screwdriver, and wok have different uses and therefore different penumbras of meaning, so too does the equilibrium tool, especially when used in conjunction with the other conceptual tools ensconced in the neoclassical research protocol. The metaphysical penumbra of “equilibrium” is comprised of notions of stability and harmony. If all social theories can be classified as either conflict theories or harmony theories, then equilibrium theories strongly tend to fall in the latter category. Insofar as considerations of systemic and structural change involve conflict, equilibrium analysis is either irrelevant or generative of potentially quite misleading implications and nuances. That is not to say that the equilibrium tool cannot be used in such cases. It can be used, but only with care and caution if it is not to convey misleading results. For example, social movements can be analyzed in an equilibrium manner but one must be wary of nuances of uniqueness, determinism, and legitimacy. It especially cannot substitute for analyses of the operative factors and forces and of the mechanisms and processes of adjustment, say, between movements.

10. Models are selectively chosen sets of variables structured in a particular way that is itself selectively chosen. Among the methodologically and substantively limiting assumptions are those that restrict (and also channel the operation of) variables in order to generate the desired

compass of equilibrium (and also an equilibrium that is unique, determinate, and optimal). The problem always arises—implicitly, even when not explicitly—of the relationship between the included and the excluded variables, that is to say, of whether the range of operative variables is wide enough.

But what constitutes a “wide enough” range of variables depends on the objectives and other preconceptions or prepossessions of the formulator or user of the model.

The fact is that no model—whatever the range of variables—no concept, no theory can satisfy all potential users. No tool can serve every potential use. No model, concept, or theory can answer every question we might have.

11. Use of the equilibrium tool channels both the way in which the answers to economists’ questions are pursued and the substance of the answers. But the use of the equilibrium tool also channels and thereby limits the questions the economist is likely to ask or the questions that conventional protocol and procedure permit the economist to ask. The unasked questions tend to be those dealing with system and structure, with conflict, with operative factors and forces, and with the substance and operation of the adjustment process. The use of one useful tool for certain questions tends to eclipse, trivialize, and marginalize both other tools and other questions.

12. By various technical and/or not-so-technical criteria, economics is a “science.” Certainly economists want their discipline to be recognized as a science. But “science” is more than the deployment of only one tool, or only one family of tools, and/or only one set of problems.

13. There is disagreement over whether and in what ways the equilibrium tool provides a satisfactory set of answers to the questions that its use permits economists to ask. These and perhaps all other relevant disagreements are matters of subjective belief and expectation. Tools are not independently given, self-subsistent phenomena. Their meaning and utility derive not from themselves but from the users’ expectations, which is to say, from what the users have been led to expect and to accept as satisfactory. The equilibrium concept is a tool, and its meaning and utility arise from and within the bounds of the sociology and epistemology of disciplinary practice.

14. The foregoing is not an argument against analysis focusing on the conditions of existence and stability of equilibrium. It is, in part, an

argument that such should not be the exclusive core of economic analysis. It is an affirmation of analysis of the factors and forces that operate in the economy, or in sectors of the economy, and of their interaction and other relations in the continuing adjustment process.

One example, using the tools of econometrics, is a study by Cutler and McClellan (1996) of the determinants of technological change in heart attack treatment. The genius of this study is that it does not seek anything so presumptuous as the optimal equilibrium price or quantity of such medical care (correlatively, it does not seek conclusions about “distortions” or “unnecessary” costs) but to identify the operative factors and forces. The authors conclude that essentially all the growth of cost in expenditures in heart attack treatment is due to the diffusion of particular intensive technologies, with the prices paid for a given level of technology being constant or falling over time. The factors include organizational factors within hospitals, the insurance environment in which technology is reimbursed, public policy regulating new technology, malpractice concerns, competitive or cooperative interactions among providers, and demographic composition. Insurance variables, technology regulation, and provider interactions having the largest quantitative effect on technological diffusion. The question of the limits of econometric analysis in reaching meaningful conclusions in this or any other area is not at issue here. The neoclassical research protocol which combines optimization and equilibrium techniques to yield ostensibly unique determinate results, applied to these authors’ topic, would clearly have to encompass presumptuous assumptions that prematurely foreclose the operation of these and other factors and forces. One implication of the foregoing discussion is that the attractiveness of unique determinate solutions is driven in part by their use in grounding so-called optimal solutions.

Comments on Setterfield

I am, frankly, ambivalent about Mark Setterfield’s article.¹ On the positive side:

1. I sympathize with what Setterfield is trying to do—namely, rescue the notion of equilibrium from its sterile usage.

¹ The text in this section frequently uses paraphrased language from Setterfield’s article.

2. Setterfield has a deep, subtle, perceptive, and comprehensive grasp of the relevant literature.
3. What Setterfield calls the *ceteris paribus* approach is, I think, consistent with the approach outlined above.
4. Setterfield is correct both that the equilibrium concept must be understood as an approach to time and that path dependency is in fact and should be in practice a critical part of all relevant analysis. In particular, his emphasis on the expectational interpretation of equilibrium is to be applauded.

He is also to be applauded for his emphasis on cumulative causation, hysteresis, and lock-in. The equilibrium tool can be used “even” in a world of path dependency in which history (as well as institutions) matters, for example, are endogenized.

He is correct in both identifying the “innate *conditionality*” of all equilibrium constructs, properly understood, and emphasizing it in the context and conduct of the *ceteris paribus* approach.

Such conditionality is for me the logical equivalent of specifying (1) the tautological relationship between the results of an equilibrium analysis and the assumptions that lead to those results, and (2) the essentially *fictional* character of all equilibrium analysis, specifically here the *ceteris paribus* approach, and indeed of all models.

5. Setterfield is to be applauded for his arguing, and establishing, that “equilibrium” has been conceptualized in many different ways in economics. If one pays strict attention to the epistemological credentials of propositions, then the fact of these different formulations of the equilibrium conceptual tool is important.
6. Setterfield is correct in identifying existence and stability as twin features of conventional equilibrium methodology. In particular, concern with existence gives effect to the desire for determinacy if not also for uniqueness; and concern with stability, as he says, follows almost automatically from concern with the concept of equilibrium itself.
7. Setterfield is correct in saying that the development of a nonequilibrium economics is by no means an easy task. But I hasten to add that what, in my view, makes the task both more difficult and professionally disreputable to pursue is the mindset of equilibrium, combined with the mindset seeking (logically or substantively) predictable outcomes, especially but solely as embodied in the neoclassical research protocol of generating unique determi-

nate optimal equilibrium results. Still, one has to accept, for example, the identification of operative factors and forces, as in the Cutler and McClellan paper, as worthy of effort.

On the negative side:

1. I think that Setterfield has not fully escaped from the mindset of equilibrium theorizing and therefore fails in part to rescue the notion of equilibrium from its sterile usage.
2. Setterfield, at least in this article, gives almost no attention to either the factors and forces that drive the economy, the arguable plethora of adjustment mechanisms, or the working of the adjustment process. This, I surmise, is because he remains overly beholden to the equilibrium mindset. Setterfield is too preoccupied with predictable results and not enough with operative factors and forces, adjustment mechanisms, and adjustment process—just as is the conventional equilibrium theorist whose work he criticizes. He acknowledges that there is more to the idea that history matters than the notion that initial conditions matter, but he fails to pursue it in a manner independent of equilibrium. Thus, he correctly emphasizes endogenous *creation* of outcomes but seems too affected by determinism to pursue it.
3. Although Setterfield rightly emphasizes the expectational interpretation of equilibrium, he does not, in my judgment, carry that emphasis far enough. Properly comprehended, the role of volatile expectations renders highly problematic any use of the equilibrium tool in quest of unique determinate results. Using clever, if not ad-hoc, lines of reasoning to finesse both radical indeterminacy and its resultant expectations (e.g., the theory of rational expectations and the postulate of “correct” expectations) is but one example of how assumptions are made to generate unique and determinate results. Setterfield does say that the rational expectations hypothesis is designed to close models and remove the independent influence of expectations, not to help us think about the possible impact that expectations might have upon economic outcomes, but he himself seems overly preoccupied with determinacy (I acknowledge that the seeming preoccupation may be due to this reader, and not to the author, of his article). Continued concern with producing *some* kind of equilibrium *result* leads to continued finessing of historicity, path dependency, radical indeterminacy, and institutions. I feel, alas, that Setterfield does not

go far enough in dispensing with the traditional economic approach to economic theorizing; he is still too taken with it, perhaps too much its product, though I readily acknowledge that his critique is solid, well thought out, and goes pretty far.

One manifestation of his continued involvement is his acceptance of the dichotomy of definite-indeterminate and indefinite-indeterminate outcomes. The distinction is subtle, interesting, and useful to a point, but its use, I think, continues the myopic preoccupations of traditional equilibrium analysis.

I find it odd that Setterfield accepts without criticism the notion that *the economy* “updates” itself through time. Such reification (“the economy”) is very much a part of the traditional equilibrium approach. It fails even to recognize, much less to come to grips with, the process of working things out—and therefore with the operative factors and forces, adjustment mechanisms, and adjustment process; a mindset that so readily gives effect to concerns with existence and stability. “The economy” is a useful metaphor, but one must be wary of both reification and attributing ontological, independent agent, status to it. “The economy” is only a name given to the sum total of actors, interactions, and processes; it has no independent existence. “It” neither updates itself through time nor does anything else.

Setterfield also accepts without criticism the “problem” of nonuniqueness (or of multiple equilibria) and the conventional view of how this problem can be “overcome.” Such overcoming—that is, such production of “the one final equilibrium outcome”—requires assumptions that finesse the deep problems with conventional equilibrist methodology Setterfield identifies.

Setterfield’s equilibrist mindset is neatly illustrated by his concern that conditional closure of economic systems not be entirely arbitrary. His solution is instructive: It is to invoke the role of conventions and conventional behavior. First, this concern and this solution are akin to conventional finessing assumptions and lines of reasoning in which obstructions to closure (unique determinate equilibrium) are assumed away. Second, what makes the assumption of conventions and conventionalized behavior nonarbitrary? Indeed, the assumption reifies the status quo in order to reach closure and thereby forecloses the process by which all the relevant key variables continue to be worked out. (Thus does conventional economic reasoning tend to treat legal change not only as “intervention” but as introducing “distortions.”) Invoking such

an assumption has the effect of eliminating historicity, cumulative causation, hysteresis, path dependency, and so on. The teleology long ago criticized by Thorstein Veblen remains alive and hegemonic. Where is the vaunted *conditionality*—not least in regard to economists' wanton practice of disregarding the methodological and substantive limiting assumptions of their work? Such concerns also apply to Setterfield's affirmation of making economic theory "truly 'general'."

4. Setterfield believes that the *ceteris paribus* approach suggests that economics is best thought of as fundamentally descriptive rather than predictive in nature. Now that topic, like many others raised here, is more complex than can be considered here. I am sympathetic to the idea—if indeed this is his—that one can predict only within the confines of a model and not in the real world; so I am bearish on prediction (certainly as a test of "truth"). But equilibrium analysis is not descriptive. For reasons given above, it is fictional. Putting it that way is not intended to be negative, only accurate. Every equilibrium analysis tells a substantively and methodologically limited story, and by virtue of those limitations it is fiction, not a direct description. However well it portrays its domain, it is still a partial story.

I think Setterfield has done serious yeoman work but remains caught in the niceties of the conventional equilibrist mindset more than he may realize. I would urge him to reconsider the implications of recognizing the limitations of equilibrium as a tool. The equilibrium tool is useful, but along the lines presented in the first section here. Setterfield's *ceteris paribus* approach is consistent with what I said above, but, I fear, still understood too much along conventional equilibrist lines. I hope that I have not misinterpreted his article.

Nor is the pursuit of conventional equilibrium methodology reprehensible—so long as one both recognizes and emphasizes the limits of what one is doing and that the technique does not comport well, *especially as it has been conventionally used* (with the foci on unique determinate results and on existence and stability), with the study of other topics, such as the study of the operative factors and forces, adjustment mechanisms, and adjustment process. Even in those studies, however, the equilibrium tool is useful, along the lines discussed above. But these other studies must be given professional respectability before the sensible use of equilibrist methodology can be effectively deployed in their domains.

I can illustrate what I have in mind as follows: First, Vilfredo Pareto, following Leon Walras, analyzed the economy in general equilibrium terms. But he also analyzed society, including polity and economy, in general equilibrium terms. He was either not at all or very little interested in articulating the technical conditions of existence and stability of equilibrium. He was very much interested, and rather successful, in identifying the major elements of social equilibration and the processes of their interaction and mutual adjustment. These elements included material (narrow economic) interests but also psychic states (sentiments and residues), belief in the form of both logico-experimental knowledge and non-logico-experimental derivations (rationalizations), class structure, circulation of the elite, and so on. I have restated his overall model in modern terms (Samuels, 1974): Policy (equilibrium) as a function of sets of psychological, knowledge, and power variables. In this model, each set of variables interacts with the other sets (psychology as a function of knowledge, and vice versa, and so on) and a leading theme is how belief (knowledge) is manipulated by power players in order to mobilize political psychology in the interests of the power players.

Second, consider the conventional model of the equilibrium, and the equilibrating processes, of economic interests—of perceived benefits and costs—*both* within the individual (the equimarginal principle) and, through the market, within the economy. Now also consider an equilibrium, and attendant equilibrating processes, of the pleasure and pain, or what not, derived from social, cultural, and emotive values—such as, for example, contemplated by Adam Smith's principles of approbation and disapprobation of the actions of oneself and of others, mediated by some notion of the impartial spectator (conscience). Then consider an equilibrium, and the equilibrating processes, encompassing both the first—economic—domain and the second—emotive—sphere. And for good measure, consider, and incorporate, the domain of government action, with similar equilibrating forces, perhaps along Paretian lines (as above). I submit that the equilibrium metaphor is useful in such matters. But the equilibrist methodology, especially when made attractive by formalization, should not obscure the factors and forces operating in the economy, the adjustment mechanisms, or the adjustment processes. Considerations of closure, determinacy, nonuniqueness, prediction, and so on—that is, the matters on which I think Setterfield has bitten the

fruit of equilibrist methodology—are not irrelevant but should not become the tail that wags the dog.

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