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## The (confused) state of equilibrium analysis in modern economics: an explanation

**Abstract:** *Modern economics produces many interpretations of the category of equilibrium as well as competing views of its relevance or worth for economic theorizing. In particular, interpretations and valuations often differ systematically between mainstream and heterodox contributions. I argue that these differences are best explained through understanding the competing ontological presuppositions of the mainstream and heterodox traditions. If correct, this explanation reinforces the assessment advanced elsewhere (Lawson, 2003) that mainstream and heterodox traditions are best distinguished not according to substantive claims or policy stances but rather precisely in terms of their ontological commitments.*

**Key words:** *equilibrium, heterodox economics, mainstream economics, ontology.*

A survey of the economic literature on equilibrium theorizing reveals various *prima facie* problematic features, confusions, or at least curiosities, some of the most significant of which I wish to focus on here. These are all features that, in due course, I shall seek to explain.

The first is that there are various competing conceptions of equilibrium, with the range of notions apparently resistant to successful systematization, despite the best efforts of some. The result, inevitably, is a lack of clarity over what is being discussed. Machlup sums this up situation with the assessment that equilibrium is “[a] term which has so many meanings that we never know what its users are talking about” (1991, p. 43).

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A second notable feature of equilibrium theorizing in modern economics is that at any point in time many authors seem capable of providing (or perhaps, more accurately, seem incapable of avoiding) inconsistent accounts of the nature of their project. In particular, many oscillate between (1) supposing that an equilibrium exists and is something to be explained and (2) asserting that its existence is something to be established.

Thus, for example, Arrow and Hahn, in *General Competitive Analysis* (1971—one of the seminal contributions to general equilibrium theory), early on claim the heritage of Adam Smith (asserting that “Smith was a creator of general equilibrium theory,” *ibid.*, p. 2) and note that Smith’s project was to explain an *a posteriori* state of affairs that was no part of anyone’s design. Indeed, they hold the view that Smith’s

notion that a social system moved by independent actions in pursuit of different values is consistent with a final coherent state of balance, and one in which the outcomes may be quite different from those intended by the agents, is surely the most important intellectual contribution that economic thought has made to the general understanding of social processes. (*ibid.*, p. 1)

Yet no sooner do they assign to economics the task of explaining this state of affairs, one they interpret as an equilibrium, than Hahn, writing at the time Arrow and Hahn (*ibid.*) would have been in press, warns us to caution against supposing an equilibrium exists:

it cannot be denied that there is something scandalous in the spectacle of so many people refining the analyses of economic [equilibrium] states which they give no reason to suppose will ever, or have ever, come about. It probably is also dangerous. Equilibrium economics . . . is easily convertible into an apologia for existing economic arrangements and it is frequently so converted. (Hahn, 1970, pp. 88–89)

A third remarkable phenomenon is that, among economists who bother to concern themselves with notions of equilibrium, there is a polarization of responses. Most contributors are either (1) strongly in favor of retaining some equilibrium notion in economics or (2) strongly against doing so. This polarization is, *prima facie*, somewhat surprising in a situation that lacks a consensus about what the concept means and even supports a widespread awareness that interpretations are indeed multiple.

Yet examples abound. Thus, Machlup maintains that “it is impossible to exclude the terms ‘equilibrium’ and ‘disequilibrium’ from the economist’s discourse” (1991, p. 43); Hahn insists that “[w]herever economics is used or thought about, equilibrium is a central organising idea” (1984, p. 43); and Backhouse recently concludes that “[t]he strongest

defence of equilibrium analysis . . . is that it is indispensable” (2003, p. 8). In the opposite camp, Kaldor writes of the irrelevance of equilibrium economics (1972), Robinson states that the “metaphor of equilibrium is treacherous” (1956, p. 59), whereas Hayek eventually chooses to avoid it as “[a] somewhat unfortunate term” (1968, p. 184).

An interesting aspect of this situation also warranting explanatory comment is that those most insistent on maintaining the notion are contributors to the mainstream project of modern economics, whereas those rejecting the equilibrium notion are mostly associated with modern heterodox traditions.

That said, however, I should note that the figures with whom the modern heterodox traditions are most associated were often accommodating of the equilibrium idea initially, before becoming less enchanted over time. This, I think, is true of the likes of Robinson, Keynes, and Hayek, now ineradicably associated with modern heterodox reasoning. Thus, for example, Robinson came to contrast equilibrium theorizing negatively to a preferred historical approach, whereas Hayek, for reasons we will discuss in due course, came to prefer the “concept of order . . . to that of equilibrium” (*ibid.*, p. 15). This latter set of developments also calls for some kind of explanation or further insight.

My aim with this paper is precisely to outline one way of rendering the phenomena expressed in these observations intelligible. That is, I want to advance and defend an interpretation of what is going on that can account for the

1. various competing conceptions of equilibrium;
2. recurrent incoherencies that arise in equilibrium theorizing; and
3. polarization of attitudes toward equilibrium theorizing, including a tendency for heterodox figures to become increasingly skeptical over time.

The elaboration of an account that can explain these observed features constitutes the objective of the main body of this paper. In a final section, I draw out implications of the analysis sustained.

### **Explaining the phenomena noted**

My explanation of the phenomena under examination follows from a broad thesis about the nature of modern economics that I defend elsewhere. Here I mostly outline relevant components of this thesis. I shall not rehearse previous extended defenses of the overall thesis (for this, see Lawson, 2003, especially ch. 1), though I shall provide some motivation

for it. However, I do interpret its ability (demonstrated below) to render intelligible the phenomena before us as further evidence of its explanatory power and, thus, adequacy. The relevant components of this broader thesis are as follows:

1. The modern economics academy is dominated by a mainstream tradition, the essence of which is an insistence on mathematical-deductive modeling.
2. As an intellectual project, modern mainstream economics is not in a healthy state. (It achieves few explanatory or predictive successes, is plagued by theory practice inconsistencies, relies on constructs recognized as quite fictitious, and generally lacks direction.)
3. The explanation of the situation noted under item (2) is that mathematical-deductive methods are regularly applied in conditions for which they are not appropriate.
4. If the heterodox alternatives are defined by a reaction to the mainstream insistence on the ubiquitous employment of methods of mathematical modeling, the explanation of this opposition is a shared vision largely at odds with the (atomistic and closed-system) ontological presuppositions of methods of formalistic modeling.
5. The ontological nature of the heterodox opposition to the mainstream is undertheorized and often unrecognized within the heterodox traditions themselves (or at least this has been the case until very recently) being manifest mostly in the defense of alternative economic categories.

Let me briefly give some indication as to why I accept these particular assessments.

The first claim—that the modern economics academy is dominated by a mainstream tradition that insists that mathematical-deductive modeling be utilized everywhere—surely no longer needs justification. Consider the observations of Richard Lipsey, the author of a best-selling mainstream economic textbook:

to get an article published in most of today's top rank economic journals, you must provide a mathematical model, even if it adds nothing to your verbal analysis. I have been at seminars where the presenter was asked after a few minutes, "Where is your model?" When he answered "I have not got one as I do not need one, or cannot yet develop one, to consider my problem" the response was to turn off and figuratively, if not literally, to walk out. (2001, p. 184)

Simply put, an insistence on formalistic modeling methods, whatever the problem, is an edict accepted by, but only by, the mainstream and is the only recurring feature of the mainstream (see Lawson, 2003, ch. 1).

My second claim—that as an intellectual project, modern mainstream economics is not in a healthy state—is again one that needs little substantiation, being a matter that the more reflective of mainstream economists seem increasingly prepared to acknowledge themselves.

Thus, we find winners of the Nobel Memorial Prize in economic sciences noting that “[p]age after page of professional economic journals are filled with mathematical formulas leading the reader from sets of more or less plausible but entirely arbitrary assumptions to precisely stated but irrelevant theoretical conclusions” (Leontief, 1982, p. 104); that “economics has become increasingly an arcane branch of mathematics rather than dealing with real economic problems” (Friedman, 1999, p. 137); that “[e]xisting economics is a theoretical system which floats in the air and which bears little relation to what happens in the real world” (Coase, 1999, p. 2).

Further, mainstream “theorist” Ariel Rubinstein admits that “economic theory has not delivered the goods” adding that “the link between economic theory and practical problems . . . is tenuous at best” (1995, p. 12). Indeed, he concludes, “[e]conomic theory lacks a consensus as to its purpose and interpretation. Again and again, we find ourselves asking the question ‘where does it lead?’” (ibid., p. 12).

Nor is the problem just the project’s lack of direction and limited explanatory and predictive power. In addition, the project’s theory and practice are highly inconsistent. For example, econometricians put huge resources into elaborating the methods they take to be appropriate and justified, yet their practices diverge significantly from their own methodological strictures (see, e.g., Hendry et al., 1990, p. 179; Leamer, 1978, p. vi).

All in all, the discipline is replete with theory/practice inconsistencies, fares poorly by its own criteria, and lacks any clear idea as to where it is going. It is also full of anomalies that range over its various subprograms. Consider the observations of Lipsey once more:

anomalies, particularly those that cut across the sub-disciplines and that can be studied with various technical levels of sophistication, are tolerated on a scale that would be impossible in most natural sciences—and would be regarded as a scandal if they were. (2001, p. 173)

If a summary statement is required, it is perhaps provided by Mark Blaug, a methodologically oriented economist who has spent considerable

resources throughout his career attempting to shore up the mainstream tradition. His current assessment runs as follows:

Modern economics is sick. Economics has increasingly become an intellectual game played for its own sake and not for its practical consequences for understanding the economic world. Economists have converted the subject into a sort of social mathematics in which analytical rigor is everything and practical relevance is nothing. (Blaug, 1997, p. 3)

My third claim—that the disarray of modern economics follows because methods of mathematical-deductive modeling are regularly applied in conditions for which they are not appropriate—requires a little elaboration (for a defense, see Lawson, 2003, ch. 1).

All methods are appropriate in some conditions and not others. As Keynes long ago, in effect, recognized, the sorts of mathematical methods economists use presuppose a closed world of isolated atoms (Keynes focused on the econometrics of Tinbergen, of course). To describe a causal factor as atomistic in this fashion is not to make a claim about size, but to indicate a presupposition that it exercises its own separate independent and invariable effect, whatever the context, thus guaranteeing that under some repeated conditions,  $x$ , the same predictable outcome,  $y$ , will always follow.

The point is that social reality does not comprise merely closed atomistic systems. Indeed, it is easy enough to show that social reality is not only open (it consists in more than systems supporting event regularities) but also structured (irreducible to the course of events), intrinsically dynamic (its mode of being is as a process), and highly internally related (consisting of parts and wholes each constituted through their [ever-changing] relations to other parts and wholes—think of positions of teachers and students, or employers and employees), among much else. From this perspective, it is not at all surprising that attempts to analyze social life using only methods that presuppose a world that is closed and atomistic fare so poorly.

The fourth and fifth claims can be run together. Here I am suggesting that heterodox contributions tend to presuppose a shared vision largely at odds with the (atomistic and closed-system) ontological presuppositions of methods of formalistic modeling. Rather, the heterodox contributions tend to advance substantive, methodological, or policy claims with ontological presuppositions that are essentially those of openness, structure, process, internal-relationality, and so on. However (with a few important exceptions, most notably Paul Davidson's emphasis on

nonergodic systems),<sup>1</sup> the ontological nature of the heterodox opposition to the mainstream is undertheorized and often unrecognized within the heterodox traditions themselves.

Thus, in Post Keynesianism, we find an emphasis on uncertainty (presupposing openness) in place of risk; in feminism, the emphasis is on caring and identity relations (presupposing internal-relationality) instead of selfish individuals; and in old institutionalism, the emphasis is on the evolutionary method (process) rather than theorizing an equilibrating or teleological system. However, as I say, the ontological presuppositions, *per se*, are rarely emphasized. I believe it is because the ontological basis of heterodoxy goes unrecognized that its criticisms of the mainstream have usually been less effectual than they deserve.

### **A brief sketch of my explanation of the state of equilibrium theorizing**

I want to use this five-part thesis (which, I repeat, is defended at length elsewhere—see, e.g., Lawson, 1997; 2003) to explain the phenomena noted at the outset. The nature of my argument is perhaps unfamiliar. So it may be useful at this point if I provide a schematic overview of its basic thrust and direction.

The limited power of formalistic methods to illuminate social reality, the lack of fit of the former to the latter, necessarily results in mainstream economists inventing “a reality” of a form that their modeling methods can address (i.e., a world of isolated atomistic individuals possessed, for example, of perfect foresight, or rational expectations, omniscience, pure greed, and so forth). But this is not all. It also imparts meaning to macro or system-categories of a sort that is driven by the needs or constraints of formalistic modeling (rather than meeting with their more usual, historical, or intuitive understandings). And this happens in ways that are often unappreciated (if ultimately explicable). We shall see that *equilibrium* is one such system-category that suffers such a fate (a further one of interest, but not considered here, is the econometric idea of a data generation process [DGP], see Pratten, 2005; another is that of complexity, see Perona, 2004).

If I can use the term *theoretic* to denote the quality of being a feature of a model and the term *ontic* to denote the quality of being features of the world the economist presumes to illuminate, a more succinct way of

<sup>1</sup> See, for example, Davidson (1989; 1996).



describing the problem that arises through the prioritization of the modeling orientation is a conflation of the theoretic and ontic, with the latter reduced to the former.<sup>2</sup>

In mostly neglecting to engage in systematic ontological elaboration, the heterodox opposition has tended to take the mainstream constructs at face value, and thereby to counterpoise alternative conceptions at the same (substantive or system) level, mostly failing to appreciate that the two sides to the discussion are talking about entirely different worlds.

Only with a turn to systematic ontology, however, can we make sense of the total situation. For only then are we in a position both (1) to clearly distinguish the ontological presuppositions of the mainstream methods and those of the heterodox traditions, and (2) to see that not only are they differently derived but also (given the lack of fit of social reality and the formalistic methods used) necessarily very different in character. And we shall see that it is only through sustaining the theoretic/ontic distinction that we can ultimately comprehensively explain (1) the variety of equilibrium notions on offer, (2) the confusions and inconsistencies as they arise, and (3) the debates and polarizations (including trends to increase skepticism in the contributions of some) such as are observed. Let me now defend these claims in detail.

### The explanation in detail

In the context of equilibrium analysis, my central claim translates into the idea that some conceptions of equilibrium found in the literature are theoretic and others are ontic, but that this difference in the nature of the competing conceptions goes largely unnoticed. This, I believe, is the most fundamental distinction to draw in any attempt to systematize in a meaningful way the various conceptions of equilibrium.

The closest we come to finding this step in the economics literature is where specific theoretic and ontic conceptions are distinguished, but where the theoretic/ontic basis of the differentiation made is unnoticed and mistaken for something else.

To illustrate, we can consider the most frequently occurring examples of contrasted notions of equilibrium in the economics literature. These are those of *system determinateness*, on one hand, and *balance or order*, on the other. And an examination of actual texts quickly reveals that

<sup>2</sup> Elsewhere I have described this error as based on the *epistemic fallacy*. The fallacy in question is the supposition that questions about being can always be rephrased as questions about knowledge (of being).

those who have emphasized determinateness have mostly meant by this the *determinateness of particular representations or formalizations* of the economy. On the other hand, those who have emphasized balance or order have interpreted this as *an aspect of the economy they are attempting to represent*. Whereas the former is theoretic, a sought-after property of theories or more typically models, the latter is ontic, a property of society that the investigator is seeking to understand and explain.

However, it is a generalized failure to recognize that this is the nature of the distinction being drawn that has led to such confusion as abounds. Typically, the rhetoric of equilibrium analysis supports images of order or balance, while its real content has concerned the properties of formalistic models. The failure to distinguish the two has resulted in a literature that is often incoherent, with contributors tending to talk past each other. Ultimately, we shall see, this state of affairs also throws insight on the plethora of equilibrium concepts in contention as well as the polarizations in attitudes to equilibrium theorizing.

### The equilibrium dichotomy

It is useful at this point to consider the classic statement of equilibrium theory in the modern period provided by Arrow and Hahn (1971). This is useful because these authors start their book with a “historical introduction,” which emphasizes precisely the general dichotomy just noted. Indeed, their opening sentence runs as follows:

There are two basic, incompletely separable, aspects of the notion of general equilibrium as it has been used in economics: the simple notion of determinateness, that the relations describing the economic system must be sufficiently complete to determine the values of its variables, and the more specific notion that each relation represents a balance of forces. (ibid., p. 1)

If we examine this passage closely, we can indeed see the different nature of the two conceptions. The first criterion, *determinateness*, is precisely a property of relations used to describe the economic system, whereas a *balance of forces* is an aspect of the economy, one that each equation is said to represent. The former is a property of the theoretical conception; the latter is thought to be a property of what the theoretical conception is about. The former is theoretic, the latter ontic.

Arrow and Hahn, though, like most modern economists, are so oriented to the theorizing aspect that they misunderstand the nature of the difference in the two conceptions they describe. As the noted passage

also indicates, they suppose that the difference to which they are drawing attention is one of level generality. Specifically, they emphasize that the idea of representing a *balance of forces* is a “more specific notion” than that of “determinateness.” Indeed, they go on to maintain that the latter notion of determinateness is sufficiently general that “almost any attempt to give a theory of the whole economic system implies the acceptance of [this] equilibrium notion” (ibid., p. 1).

It is at this point that they claim the heritage of Adam Smith, asserting that “Smith was a creator of general equilibrium theory” (ibid., p. 2). In a general way, Arrow and Hahn draw attention to Smith’s mention of the “invisible hand.” Here Smith writes:

Every individual necessarily labours to render the annual revenue of the society as great as he can. He generally neither intends to promote the public interest, nor knows how much he is promoting it. . . . He intends only his own gain, and he is in this, as in many other cases, led by an invisible hand to promote an end which was no part of his intention. Nor is it always the worse for society that it was no part of his intention. By pursuing his own interest he frequently promotes that of the society more effectually than when he really intends to promote it. I have never known much good done by those who affected to trade for the public good. (1975, book 4, ch. 2)

Of course, Arrow and Hahn interpret equilibrium as a set of prices—those that equate supply and demand on each market under a given set of conditions. And it is true that Smith, at one place, talks of a system of natural prices being influenced by forces of supply and demand. But if Smith’s endeavor is treated as the rhetorical justification of the project of Arrow and Hahn; the latter, in nature, is quite different.

As already noted, Smith’s objective is to explain such order or outcomes as he believes occur. Thus, in the context of his consideration of natural prices, Smith writes:

There is in every society or neighbourhood an ordinary or average rate both of wages and profit in every different employment of labour and stock. This rate is naturally regulated, as I shall show hereafter, partly by the general circumstances of the society, their riches or poverty, their advancing, stationary, or declining condition; and partly by the particular nature of each employment.

There is likewise in every society or neighbourhood an ordinary or average rate of rent, which is regulated too, as I shall show hereafter, partly by the general circumstances of the society or neighbourhood in which the land is situated, and partly by the natural or improved fertility of the land.

These ordinary or average rates may be called the natural rates of wages, profit, and rent, at the time and place in which they commonly prevail.

When the price of any commodity is neither more nor less than what is sufficient to pay the rent of the land, the wages of the labour, and the profits of the stock employed in raising, preparing, and bringing it to market, according to their natural rates, the commodity is then sold for what may be called its natural price (*ibid.*, book 1, ch. 7)

Smith's aim in the relevant four chapters of his book is to explain the causes of variations in these phenomena, including the natural price of which the others are component parts:

The natural price itself varies with the natural rate of each of its component parts, of wages, profit, and rent; and in every society this rate varies according to their circumstances, according to their riches or poverty, their advancing, stationary, or declining condition. I shall, in the four following chapters, endeavour to explain, as fully and distinctly as I can, the causes of those different variations. (*ibid.*, book 1, ch. 7)

It is not clear whether Arrow and Hahn suppose Smith's contribution to general equilibrium theory is explaining the state of affairs described by Smith in his passage referring to the invisible hand or the variations in natural prices. But, either way, there is a very significant difference between Smith's project and that of Arrow and Hahn. Whereas Smith is concerned with explaining a particular state of affairs, Arrow and Hahn are instead concerned with showing that one exists. That is, Smith is concerned with the ontic, with a real-world state of affairs, whereas Arrow and Hahn are concerned only with whether the model system has a formal property.<sup>3</sup> However, the failure to explicitly distinguish the theoretic and the ontic results is sloppy use of language. Thus, as we saw in the earlier passage from Hahn, he engages only in formal modeling but draws conclusions expressed as follows.

it cannot be denied that there is something scandalous in the spectacle of so many people refining the analyses of economic [equilibrium] states

<sup>3</sup> For example, they turn next to Walras, to whom, they suggest, the "full recognition of the general equilibrium concept can be attributed unmistakably" (Arrow and Hahn, 1971, p. 3). Here we are in the realm of models. Things are confused, because variables in models are referred to as prices, demand and supply, and so forth. However, it is clear from the discussion that when equilibrium is now conceived as a set of prices, being those that equate supply and demand on each market under a given set of conditions, the category is a property of models, not of states of affairs they are purported to represent: "That there was an equilibrium set of prices was argued from the equality of the number of prices to be determined with the number of equations expressing the equality of supply and demand on all markets" (*ibid.*, p. 5).

which they give no reason to suppose will ever, or have ever, come about. (1970, pp. 88–89)

Of course, when Hahn refers to an equilibrium that may never come about, it does seem like he is using an ontic notion. However, this is not so. He is really saying that in an imagined world consistent with his model there is nothing to ensure that an equilibrium position would result. Or more accurately, he is saying that if, for a set of equations used to construct a description of the economy, there is a manner—a specification—whereby the various equations are found to be mutually consistent, then the solution to the consistency question, stylized as an equilibrium, is not a part of the model description, and so is not a necessary outcome even in such a counterfactual (closed and atomistic) world as described by the model specification.

In short, the equilibrium is merely a solution to a system of equations. It is a vector that renders the equations consistent. Hahn's point is that there is nothing in the apparatus of the model to ensure that even if, *per impossible*, the model accurately represented the world, the equilibrium situation, expressed by the model's consistency condition, would emerge.

I do not want to suggest that Hahn intentionally misleads or always fails to acknowledge the limits of his endeavor. Certainly, Hahn seems to have become increasingly clear with the passage of time on what his constructions entitle him to conclude. Indeed, in an "Intellectual Retrospect," he is very clear about what is taking place in his theory contributions:

The great virtue of mathematical reasoning in economics is that by its precise account of assumptions it becomes crystal clear that applications to the "real" world could at best be provisional. When a mathematical economist assumes that there is a three good economy lasting two periods, or that agents are infinitely lived (perhaps because they value the utility of their descendants which they know!), everyone can see that we are not dealing with any actual economy. The assumptions are there to enable certain results to emerge and not because they are to be taken descriptively. (Hahn, 1994, p. 246)

It seems reasonable to suppose that if Hahn had been clearer on this score from the outset, however, some of the earlier (nonconnecting) discussion might have been avoided. Robinson (e.g., 1978, p. 127), in particular, might have been spared the effort of responding to Hahn in terms of outlining, and defending as more realistic, a particular (ontic) conception of an equilibrium.

To repeat, then, my explanatory thesis (conditioned on the description of modern economics described above) is that, in modern economics,

there is an erroneous (if explicable) tendency to conflate theoretic and ontic features of an analysis. And this thesis can be seen to account for much of the incoherency of equilibrium analysis as abounds.

### **The remaining problematic features**

How does this thesis account for the two remaining sets of observations noted at the outset, namely,

1. of a plethora of competing equilibrium conceptions, especially of those conceptions that can be viewed as versions of system determinateness; and
2. of a polarization of orientations, divided among mainstream/heterodox lines?

#### *The plethora of competing conceptions*

The plethora of conceptions is easily explained. For where equilibrium is merely a solution concept for a model, a property of a system of equations, there can clearly be as many definitions of equilibrium as there are possibilities for system-model construction. And scope for the latter seems limitless.

This situation is grasped by some, but seemingly not by most. Thus, a heroic attempt to bring clarity by Machlup ends up doing no more than rendering both the equilibrium as *balance* and equilibrium as *determinateness* notions as theoretic:

Equilibrium, in economic analysis [is] a constellation of selected interrelated variables so adjusted to one another that no inherent tendency to change prevails in the model which they constitute. . . . As an alternative definition of equilibrium we may propose mutual compatibility of a selected set of interrelated variables of particular magnitudes. (1991, pp. 54–55)

But Dixon, among others, hits the nail on the head precisely:

At its most general, we can say that “equilibrium” is a method of solving economic models. At a superficial level, an equilibrium is simply a solution to a set of equations. (1990, p. 356)

#### *The polarization of orientations*

It is equally possible to explain our remaining puzzle, the polarization of attitudes over the relevance of an equilibrium notion. I have already noted that attitudes have tended to divide along mainstream/heterodox

lines, with the mainstream, unlike heterodoxy, insisting the equilibrium notion is essential, and with the heterodox opposition becoming increasingly marked over time. We now have before us the resources to understand why.

Consider first the mainstream insistence that the notion be retained. The reason for this must now be clear. This mainstream project is defined by its insistence that mathematical methods be everywhere and always employed, despite the dearth of explanatory successes to date. But in a situation where model equations are found almost always to be inappropriate to the analysis of the economic system, what other goal can be accepted for modelers than the questioning of their equations' mutual consistency? Where the emphasis is on a formalistic system, attention is always going to turn to the question of whether the system has some sort of mathematical solution. And the obvious, certainly traditional, way to try and present this as an economic activity is to present the mathematical exercise as the search for an economic equilibrium. Associating the process with Smith is merely an attempt to grant the exercise a historical legitimacy, an endeavor that significantly misleads.

How about the heterodox rejection of the use of the term? If the mainstream was always going to require a notion to express the model-property of consistency or determinateness, was it equally predictable that heterodoxy was always going to abandon the term?

The answer, I think, is yes, if not necessarily immediately. I earlier suggested that a feature of the heterodox traditions is that, although they emphasize categories with ontological presuppositions different from those of the mainstream mathematical methods, they rarely acknowledge that this is so. Specifically, the mainstream methods presuppose a closed atomistic reality, whereas heterodox conceptions can be shown to be based on a vision of social reality as open, structured, processual, highly internally related, among much else (see Lawson, 2003). As I say, though, the ontological basis of the opposition has rarely been explicitly identified.

Even so, heterodox economists have been oriented to ontic elaboration, focusing mostly on equilibrium as a balance or form of order. In consequence, the tension between the conceptions of social order they have been seeking to explain and the more dominant definitions of equilibrium have usually been apparent, even if the ontological basis of the distinction remained untheorized. This has resulted in equilibrium notions being employed, if at all, often in a hesitant and cautious manner. Robinson provides an obvious example:



The word equilibrium, in ordinary speech, describes a relation between bodies in space. The scales of a balance are in equilibrium when the balance is at rest. . . . If we are continually throwing coppers at random into either scale, the balance is continually wobbling and never reaches equilibrium; but, at any moment, there is a definite equilibrium position which it would quickly reach if, from that moment, we left it alone. (1956, p. 57)

She concludes:

Nor can we apply the metaphor of a balance which is seeking or tending towards a position of equilibrium though prevented from actually reaching it through constant disturbances. In economic affairs the fact that disturbances are known to be liable to occur makes expectations about the future uncertain and has an important effect on any conduct (which, in fact, is all economic conduct) directed towards future results. . . . A belief that a particular share is going to rise in price causes people to offer to buy it and so raises its price. . . . This element of “thinking makes it so” creates a situation where a cunning guesser who can guess what the other guessers are going to guess is able to make a fortune. There are then no solid weights to give us analogy with a pair of scales in balance. The metaphor of equilibrium is treacherous. (ibid., p. 59)

The more that the ontic orientation has been manifest in a sustained concern with the nature of the actually existing social order, the more heterodox economists have grasped the irrelevancy of the equilibrium framework. Thus, with time, of course, Robinson turned from equilibrium thinking to history.

In the history of heterodox economics, though, I think one of the more interesting stories, in this regard, is the progressive development of the thinking of Hayek. Hayek is interesting in that he recognizes the theoretic or *a priori* nature of the dominant framework, interpreting it as a logic of choice, while being driven himself always to provide an ontic account. This is especially true of his work of the late 1930s. Had Hayek, at that time, undertaken the sort of ontological theorizing that he later pursued, he would likely have avoided the earlier exercise. But he had not, and his “Economics and Knowledge” paper (1937) is a particularly ingenious attempt to reconcile two ultimately incompatible endeavors: an *a priori* logical framework (presupposing a closed system) and a desire for a realistic (open-system) vision of the actual social world. By examining this particular paper by Hayek, we can see clearly the sorts of incompatibilities that arise. And by looking very briefly at Hayek’s subsequent work, we can understand why heterodox economists, initially receptive to equilibrium theorizing, often took a very different tack over time.



### Hayek's "economics and knowledge" position

The substantive part of Hayek's argument in "Economics and Knowledge" need not overly concern us here; it is, in any case, well known. In brief, Hayek takes the view that equilibrium economics can be rendered coherent if it concerns itself with the manner in which knowledge is acquired and communicated. Specifically, he first argues that the equilibrium concept makes most sense if it is couched in terms of the consistency of an individual's actions with his or her plans over time. He then argues that societal equilibrium, if it means anything, is the consistency of these separate plans across all individuals.

However, it is Hayek's broader methodological stance that is most interesting. It is this I want mostly to focus on here.

The first point to note on this score is that Hayek is indeed concerned with equilibrium as an ontic notion; he is concerned with prioritizing the understanding of real-world situations or causation:

the question to what extent formal economic analysis conveys any knowledge about what happens in the real world. Indeed, my main contention will be that the tautologies, of which formal equilibrium analysis in economics essentially consists, can be turned into propositions which tell us anything about causation in the real world only in so far as we are able to fill those formal propositions with definite statements about how knowledge is acquired and communicated. (ibid., p. 33)

At the same time, Hayek is aware that modern equilibrium analysis has taken on the character of a system of formal logic:

I am certain that there are many who regard with impatience and distrust the whole tendency, which is inherent in all modern equilibrium analysis, to turn economics into a branch of pure logic, a set of self-evident propositions which, like mathematics or geometry, are subject to no other test but internal consistency. (ibid., p. 35)

However, Hayek is not yet convinced that the two are incompatible. In seeking a resolution, he actually argues that there is a sense in which the noted tendency to turn economics into a branch of logic has not gone far enough. His assessment is that the logical framework ought to comprise statements concerning only highly general or *a priori* factors, when, in fact, it has increasingly included statements about all sorts of concerns that are not of this nature. The point is to empty the logical framework of everything but *a priori* factors:

But it seems that if only this process is carried far enough it carries its own remedy with it. In distilling from our reasoning about the facts of

economic life those parts which are truly *a priori*, we not only isolate one element of our reasoning as a sort of Pure Logic of Choice in all its purity, but we also isolate, and emphasize the importance of, another element which has been too much neglected. My criticism of the recent tendencies to make economic theory more and more formal is not that they have gone too far, but that they have not yet been carried far enough to complete the isolation of this branch of logic and to restore to its rightful place the investigation of causal processes, using formal economic theory as a tool in the same way as mathematics. (ibid., p. 35)

In so arguing, Hayek is clearly accepting that a closed-systems framework might be of use at least at a broad level of generality. Of course, Hayek does not use the language of closed systems. And with the hindsight of modern ontological analysis, we know social reality to be open and not easily rendered closed, so that any conception of equilibrium that emerges from Hayek's is likely to be fictitious. But, though not employing this or an equivalent terminology, Hayek recognizes this result all the same. And it marks an important step in his argument. For Hayek seems to accept that the formal logical system of *a priori* considerations must give us an equilibrium notion (one in which different individual plans for action in time are mutually compatible) that is fictitious as a claim about the existing state of affairs. However, he appears to believe this is not fatal for equilibrium theorizing. And the reason is a presumption that *a posteriori* considerations can render the approach realistic nonetheless by investigation of the possibility of, or conditions for, a real-world tendency in the equilibrium direction:

We shall not get much further here unless we ask for the reasons for our concern with the admittedly fictitious state of equilibrium. Whatever may occasionally have been said by over-pure economists, there seems to be no possible doubt that the only justification for this is the supposed existence of a tendency toward equilibrium. It is only by this assertion that economics ceases to be an exercise in pure logic and becomes an empirical science; and it is to economics as an empirical science that we must now turn. (ibid., pp. 43–44)

So Hayek's strategy for rescuing the equilibrium program in a manner that maintains its theoretic and ontic aspects is to insist that *a priori* reasoning determines the fictitious state of equilibrium, whereas empirical reasoning elaborates the tendencies toward it. Hayek provides no justification of this role for *a priori* reasoning. Nor, I think, is there any. In truth, Hayek, in effect, acknowledges this in admitting that there is no obvious basis on which to argue for such a tendency. Specifically, in the context of his own knowledge-based conception, Hayek accepts that a

tendency to equilibrium requires that individuals' expectations of each other become more and more accurate. However, he admits that he does not know why or how such an eventuality should come about:

In the light of our analysis of the meaning of a state of equilibrium it should be easy to say what is the real content of the assertion that a tendency toward equilibrium exists. It can hardly mean anything but that under certain conditions the knowledge and intentions of the different members of society are supposed to come more and more into agreement, or, to put the same thing in less general and less exact but more concrete terms, that the expectations of the people and particularly of the entrepreneurs will become more and more correct. In this form the assertion of the existence of a tendency toward equilibrium is clearly an empirical proposition, that is, an assertion about what happens in the real world which ought, at least in principle, to be capable of verification. And it gives our somewhat abstract statement a rather plausible common-sense meaning. The only trouble is that we are still pretty much in the dark about (a) the *conditions* under which this tendency is supposed to exist and (b) the nature of the *process* by which individual knowledge is changed. (ibid., p. 44)

Indeed, after further discussion of concrete hypotheses concerning the conditions under which people are supposed to acquire relevant knowledge, Hayek concludes:

But I am afraid that I am now getting to a stage where it becomes exceedingly difficult to say what exactly are the assumptions on the basis of which we assert that there will be a tendency toward equilibrium, and to claim that our analysis has an application to the real world. I cannot pretend that I have as yet got much further on this point. (ibid., p. 47)

Mindful of his audience, though, Hayek attempts to be upbeat:

I do not want to appear unduly despondent about what we have already achieved. . . . I still believe that by what is implicit in its reasoning, economics has come nearer than any other social science to an answer to that central question of all social sciences: how the combination of fragments of knowledge existing in different minds can bring about results which, if they were to be brought about deliberately, would require a knowledge on the part of the directing mind which no single person can possess. To show that in this sense the spontaneous actions of individuals will under conditions which we can define bring about a distribution of resources which can be understood as if it were made according to a single plan, although nobody has planned it, seems to me indeed an answer to the problem which has sometimes been metaphorically described as that of the "social mind." But we must not be surprised that such claims on our

part have usually been rejected by sociologists, since we have not based them on the right grounds. (ibid., p. 52)

But this is mostly (mere) rhetoric. Following these comments, Hayek considers an issue not previously raised, one that further reveals the fragility (or meaninglessness) of any supposed tendency to equilibrium. Following this final assessment, Hayek remarks: "With these rather desultory remarks on topics which would deserve much more careful examination I must conclude my survey of these problems" (ibid., p. 53).

I have focused upon Hayek's "Economics and Knowledge" contribution, because I think it reveals the incompatibility of the theoretic and ontic aspects of equilibrium analysis in an interesting, and perhaps most compelling, fashion, in the writings of a serious contributor clearly motivated to reconcile the two. I think Hayek also provides an intelligible attempt at reconciliation. But, ultimately, he fails. And, despite some of his rhetoric, I think it is clear that Hayek is aware of this.

Not surprisingly perhaps, this failure spurred Hayek into a form of ontological reasoning. After initially trying to maintain an equilibrium idea, Hayek's ontic orientation led him increasingly to appreciate its limitations. Some time after the "Economics and Knowledge" paper, in fact, Hayek was emphasizing the idea of social order rather than equilibrium:

The concept of "order," which . . . I prefer to that of equilibrium, has the advantage that we can speak about order being approached to varying degrees, and that order can be preserved throughout the process of change. (1968, p. 184)

Eventually, of course, Hayek elaborates a social ontology of rules and other aspects of social structure and develops his conception of spontaneous order:

What reconciles the individuals and knits them into a common and enduring pattern of society is that . . . they respond in accordance with the same abstract rules. . . . What . . . enables . . . men to live and work together in peace is that the pursuit of their individual ends and the particular monetary impulses which impel their efforts . . . are guided and restrained by the same abstract rules. If emulsion or impulse tells them what they want, the conventional rules tell them how they will be able and allowed to achieve it. (1976, p. 12)

A catallaxy is thus a special kind of spontaneous order produced by the market through people acting within the rules of the law of property, tort and contract. (1982, p. 109)

This is no longer a conception of a state of order in which expectations are always met; rather, it is one in which disappointments are unavoidable.

In a spontaneous order, undeserved disappointments cannot be avoided. . . . It is only because countless others constantly submit to disappointments of their reasonable expectations that everyone has as high an income as he has. (*ibid.*, p. 128)

With this being so, Hayek's conception is now far more in line with the world we daily experience. It is quite different from Hayek's original notion, but it reveals the sort of direction that is ultimately to be expected where there is a consistent emphasis on the ontic.

### **Implications and conclusion**

Modern economics is not in a healthy state. And the reason for it is that it, or rather the dominant mainstream tradition, defines itself in terms of its method, that of formalistic-deductive modeling, and does so in a context in which this method has little application. I have indicated before how this emphasis has resulted in limited explanatory successes, theory practice inconsistencies, and other pathologies. Here I have focused on a further problematic feature created by the mainstream prioritizing of modeling over illumination: the confusing of claims about models and their properties with properties of the reality that the models putatively aim to represent.

In truth, modern economics supports two broad sets of traditions: the mainstream project and the heterodox alternatives. The mainstream prioritizes modeling, whereas the heterodoxy prioritizes social illumination. And because the implicit (though rarely examined) ontological commitments of the heterodoxy (of openness, structure, internal-relationality, and process) are quite different from those (of atomism and closure) presupposed by the mainstream modeling emphasis, the two projects rarely find common ground.

However, the true ontological nature of the differences is rarely explored. One of the many debilitating results of this is that when common categories are employed, the real nature of the differences in arguments mostly goes unrecognized, resulting in participants in debates talking past each other. I have illustrated this theme in the context of equilibrium analysis.

If all parties agree that Adam Smith set (and contributed to answering) one of the fundamental question of economics—namely, how social order emerges in the absence of central or any intentional design, and, indeed, with individuals pursuing largely independent goals—it is clear that the inheritors of Smith's project are not economic equilibrium theorists concerned with formalistic modeling. Rather, it is those working in

the traditions of Marx, Keynes, Hayek, and others who make the explaining of the actually existing social order the priority.

The project of formalistic modeling can be misinterpreted as one concerned with explaining the actual social order only if the atomistic pre-suppositions of the former go unrecognized, or their irrelevance remains unappreciated. Once we turn to social ontology, to theorizing the nature of social reality, the impotence of the equilibrium notion becomes apparent. The real question, Smith's question in modern terms, is how social reproduction of complex, internally related, dynamic, social structures occurs in an open world of individuals seeking their own ends. As I say, this eventually was the concern of Hayek and Keynes as well as Marx. How successful they were in the details of their analyses, of course, is a different question.

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