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Reorienting history (of economics)

Abstract: *In his response to my original paper "The (Confused) State of Equilibrium Analysis in Modern Economics: An Explanation," Weintraub sets forth a competing account of the nature of equilibrium theorizing in economics. Weintraub supposes (1) his account is better because (2) his approach to understanding economics is more historical than my own. I suggest that neither of these claims is correct.*

Key words: *equilibrium, history, mathematics, ontology.*

In my original paper (this issue, pp. 423–444), I point to confusion surrounding economic equilibrium analysis and advance the explanation that the term *equilibrium* is used to cover two very different conceptions. The first of these, which I refer to as the *ontic* notion of equilibrium, expresses an aspect of reality that the theorist is attempting to understand, describe, explain, or represent, and so on. The second conception, which I refer to as a *theoretic* notion, expresses a property of a mathematical-deductive model (or system of equations) formulated seemingly with the intention of explaining, representing, or somehow increasing our understanding of social reality. In the former case, the term *equilibrium* expresses a feature of (model independent) reality, in the latter, a (possible) property of a model.

I argue further that not only are these two conceptions distinct, with confusion arising where the distinction goes unrecognized, but also, in practice, they have little bearing on each other. The reason for this is that the implicit ontology of mathematical deductivist modeling (of atomism and closure) is so at variance with the open, structured, intrinsically dynamic, and highly interconnected nature of social reality, that the chances of equilibrium modeling providing insight on any aspect of social reality are slim.

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Roy Weintraub's objective in his comment is to give an alternative account of equilibrium theorizing to my own. First, he appears not to recognize any confusion of conceptions of equilibrium in economic theorizing. Second, although in places he appears to accept that the categories of theoretic and ontic are appropriate for characterizing the distinction I draw between equilibrium notions, at other points he seems to suggest instead that the distinction in question is one that holds only between two types of theoretic notions, the mechanical (a legacy of Marshall) and the mathematical.[1] Either way, Weintraub supposes the distinction in question does not cause confusion, just for the reason that it identifies concepts in usage at different points in history.

In supporting this latter contention, Weintraub reminds us that, over time, mathematics, and the way it is understood by its practitioners, has changed significantly. Such changes in understandings or "images" of mathematics, of its nature, role, and meaning, and so on, are those formed or held by not only mathematicians but also economists. According to Weintraub, I fail to appreciate this. In consequence, I also fail to recognize that the conceptual distinctions I draw reflect nothing more than "contingencies of how equilibrium was manifest in mathematical discourse in different periods of time" (Weintraub, this issue, p. 450).

In setting out his alternative interpretation of the state of economic equilibrium analysis, Weintraub informs us that his driving concern is to ensure that history is not abused. And his explanation of our differences is clearly that he is doing history, while I am doing logic. Hence, I see only *logical inconsistency* at a given point in time, whereas, if only I did history, I might instead perceive quite coherent changes taking place over time.

I now want to put forward my own explanation of our differences. In doing so, it is fairly easy to demonstrate that Weintraub's account, and his explanation of our differences, are rather too quick and easy.

[1] Whereas I interpret conceptions like determinateness as theoretic notions and others like a "balance of forces," consistency of an individual's actions and plans over time, or the consistency of separate plans across individuals, as ontic ones, Weintraub, at times, views the latter sort, or at least the specific idea of a balance of forces, as theoretic. Specifically, Weintraub views any notion of a balance of forces as a property of mechanical models dating back to Marshall:

> The example Lawson presents in his section called "the equilibrium dichotomy" thus can be glossed historically as "determinateness is a property of mathematical models, and a balance of forces is a property of mechanical models, and thinking in terms of mechanical models is a historical legacy of the early days of Marshallian economics." (this issue, p. 451)

Explaining Weintraub

In giving an analysis of the equilibrium concept in my original paper, I draw heavily on my recent book *Reorienting Economics* (Lawson, 2003). Weintraub shows no indication of having read it. If he had, he would realize that I too address the historical question of how economics became mathematized (see especially chapter 10, the longest chapter; see also Lawson, 2001). What is more, in this historical research, I, like Weintraub, uncover a story of contingency. Mine, indeed, is a nonteleological account in which the changing interpretation of mathematics is a central feature. For the question that concerns me is how a mathematical way of doing economics came eventually to dominate the discipline without, or so I argue (and seemingly in contradistinction to Weintraub), adding to its explanatory power. And changes in the way mathematics has been interpreted is a significant component of my explanation.

So Weintraub and I, if starting from different motivating preconceptions, both give stories of change and contingency. However, there is a way in which our *historical* analyses (and not just motivations) also part company. For, as well as outlining a story of change and contingency, I also uncover a story of continuity.

My claim here is that throughout the history of mathematics in economics—and the latter is our ultimate concern, not the history of mathematics, per se—economists have concerned themselves with a form of deductivist reasoning. By this, I mean a form of reasoning that rests on the use of regularities of the form "whenever event or state of affairs *x*, then event or state of affairs *y*," essentially methods involving functional analysis. Systems in which event regularities of this sort occur can be referred to as closed. These regularities, in turn, presuppose an ontology of isolated atoms. By an atom, I refer not to something that is small but, rather, to something that, if triggered, has its own separate, independent, and invariable effect, whatever the context.

So my claim is that throughout the various changes in the forms and interpretations of mathematics, something has remained stable: an implicit ontology of closure, of atomism and isolationism, has been repeatedly reproduced.

Of course, a further assessment, discussed in my original paper (this issue, pp. 423–444), is that social reality is open and not much like the atomistic world presupposed by all the various forms of mathematical economics, however the mathematics has been interpreted. In consequence, the widespread perception, which I too share, that projects of modern economics have appeared not to fare very well in explanatory terms, is really none too surprising.

The point, though, is that I argue for *both* commonality and change: commonality in ontological presuppositions, change in mathematical form, interpretation, meaning, relatedness to disciplines such as physics, and so forth.

And it is the continuing ontological commitments of the mathematizing project that lie behind its explanatory weaknesses. In particular, it is this aspect, I argue, that renders the theoretic notion of equilibrium unhelpful as a tool of social analysis, being quite unlike any conception obtained by addressing directly how social reality actually is.

How does, or might, Weintraub deal with any claim, such as my own, of continuity of ontological presupposition (and any implications drawn concerning the explanatory fruitfulness of modern economics)? The answer seems to be as follows:

> As a historian, I have no particular quarrel with [Lawson's] critique itself, as not only am I uninterested in ontological reflections, but I have no particular interest in either attacking or defending mainstream economics. I do, however, have an interest in making sure that the history of economics (or historical argumentation itself) is not abused. (Weintraub, this issue, p. 425)

So the difference between Weintraub and myself is not that Weintraub does history while I do logic (or ontology) *instead.* Rather, the real difference is that the only sort of history that Weintraub will contemplate doing himself is one that neglects ontology.

Complementary projects?

One could be tempted to conclude from all this that, broadly speaking, Weintraub and I are just engaged in different historical projects, and that Weintraub has simply failed to recognize this.

After all, in his more cautious moments at least, Weintraub acknowledges that "there are many ways to tell the story of the mathematization of economic theory, and connected to each of those ways is a reason, perhaps more than one reason" (2002, p. 4). In addition, Weintraub indicates that his account ought not to be viewed as a totalizing theory (ibid., p. 7). And he further assures us that his history is not in the service of any wider claim such as "this is the real story of economics in the twentieth century" (ibid., p. 8). So, as I say, perhaps we are just providing separate equally partial accounts?

Despite his rhetoric, I suspect Weintraub really takes himself to be achieving something rather more than this. But before indicating why I suggest this, it is worth emphasizing that if we take Weintraub's qualifications at face value, his take on history should be recognized as a particular one indeed (see Pratten, 2004). In his book, Weintraub informs us early on that "each chapter will explore, more or less directly, how economics has been shaped by economist's idea's about the nature and purpose and function and meaning of mathematics" (2002, p. 2). Part of the "nature" of the sorts of mathematical methods that economists wield are its ontological presuppositions. What is more, it is a view on these presuppositions (whether or not the word ontology is used) that has underpinned most of the concern of those who, over the years, have resisted the extensive imposition of mathematics into economics and thereby significantly influenced the manner in which the discipline has become mathematized.

In particular, as I show at length in *Reorienting Economics* (Lawson, 2003), criticism of the mainstream's insistence on using mathematical-deductivist methods is a fundamental aspect of all the heterodox contributions, including Austrian, feminist, old institutionalist, Post Keynesian, social economists, and others. And it is easy enough to show that this criticism arises precisely because the various heterodox traditions implicitly accept a different ontology to the closed-system (atomism and isolationism) presuppositions of the mathematical mainstream (see ibid., especially chs. 1, 7, 8, and 9). Increasingly, moreover, heterodox contributors are acknowledging that this is indeed the case.

Furthermore, this sort of criticism is hardly novel. Indeed, the heterodox traditions have developed out of the contributions of the likes of Keynes, Veblen, Hayek, and others, all of whom, I also show in *Reorienting Economics* (and elsewhere, especially, Lawson, 1997), held ontological preconceptions at variance with those of mathematical deductivist methods, and most of whom explicitly recognized this.[2]

In fact, whole episodes of the history of the mathematizing project are necessarily ignored in Weintraub's account. For example, in France, attempts to mathematize the discipline have been underway at least since the Enlightenment, including the efforts of the Physiocrats, especially Quesnay, but also Turgot, Dupont de Nemours, Condorcet, Achylle-Nicolas Isnard, Canard, Dupuit, and Cournot. But their efforts were resisted, not least by Jean-Baptiste Say (and his followers such as Wolowski,

[2] Nor is criticism of modern economics advanced only by heterodox economists. In my original paper (this issue, pp. 423–444) and *Reorienting Economics* (2003), I note leading (recent and not so recent) mainstream proponents, including Nobel Memorial Prize winners, who explicitly acknowledge that the economics discipline is in none too healthy a state and who put the blame on the inappropriate excessive reliance on mathematics.

Reybaud, and Baudrillart) but also by the mathematician Laplace, and essentially on ontological grounds (see Lawson, 2003). Yet all these individuals, bar one,[3] are missing from Weintraub's history.

Walras figures, of course, though even he is dealt with extremely briefly, at least in Weintraub's most recent book (one mention on one page; 2002). And there is no mention of Levasseur or even Poincaré or any others who questioned the relevance of the ontological presuppositions of Walras's analysis.

I am not, I repeat, suggesting that Weintraub's history could be complete, only that we recognize it for the extremely partial story that it is. The point is that if we accept that Weintraub is really looking only at issues that have no bearing on the worth of the mathematizing project, and most fundamentally neglecting all aspects of the project's history where ontological reflection has been consequential, there is a good deal of the story that is necessarily absent.

However, despite his qualifications, in the end it is not clear that Weintraub is really content to be seen as adopting but one approach, and giving one account, among many. For, on the one side, and despite his official stance,[4] Weintraub does give the impression that he believes his contribution to be superior to alternatives. After all, even in Weintraub's current piece, his own historical account is summoned up on the grounds that he wishes to ensure that history is not abused. And it is clear that Weintraub believes that any contribution that does not adopt his approach is somehow deficient (see also, for example, Weintraub, 2004, where an alternative approach to his own is described as "making up history"). In the current paper, most specifically, Weintraub wishes to suggest that his history provides a proper interpretation of how the equilibrium notion is, and should be, interpreted.

On the other side, Weintraub's professed disinterest in ontology does not prevent his straying into some areas where ontological reflection has underpinned criticism of the excessive reliance on mathematics in economics and promoting an alternative explanation of this criticism. This is most obviously the case where Weintraub makes references to contributions emanating from Cambridge (UK), not least in his discussion of Marshall. In fact, a consideration of Marshall's rejection of the heavy reliance on mathematics forms the topic of the first (and possibly most

[3] The exception is the inclusion of a short quote from Cournot defending his use of mathematics.

[4] Most emphatically expressed in the final chapter of his 2002 book.

important) chapter of Weintraub's book (2002). In consequence, it is insightful, perhaps, to consider Weintraub's comments here in some detail.

As is well known, in February 1906, Marshall wrote a letter to Arthur Bowley, which famously stated: "But I know I had a growing feeling in the later years of my work at the subject that a good mathematical theorem dealing with economic hypotheses was very unlikely to be good economics" (Groenewegen, 1995, p. 413).

How did Marshall reach this conclusion? If by self-imposed constraint, Weintraub cannot contemplate the obvious, and I believe correct, explanation that it was as a result of informed ontological reflection on Marshall's part, what options are open to him, given that he has made a decision to discuss Marshall at all? What other grounds could there be for opposing developments in mathematics?

The obvious answer is to suggest that Marshall (and anyone else who is acknowledged as resisting the trend to mathematizing the discipline) was ignorant of the math, or at least unsure of the latest developments in it, or insecure about it because not properly trained in it. This is indeed Weintraub's strategy, his answer (in his recent book, Weintraub, 2002) being little more than a speculation that Marshall might have felt discomfort with developments that occurred only after Marshall's sitting the Cambridge Mathematical Tripos (university exams) as a student:

> By the time of Marshall's writing to Bowley, we have an emergent mathematical economics with the works of Pareto, Panteloni, and others. . . . These books reflected a mathematical sophistication and use of mathematics in essentially new ways. For a product of the old Tripos like Marshall, . . . this new way of using mathematics might have been discomforting. The point is that for Marshall, his image of what mathematics was, and how it was to be done, and especially how it was to be applied to problems, was forged by the Mathematical Tripos of his Cambridge student years, and his preprofessorial days there. (ibid., p. 23)

Notice the use of the term *might* before *discomforting*. This term is used because Weintraub does not provide any evidence for his view. It is merely a suggestion, plausible at best, only in the absence of any alternative explanation being seriously considered.[5] But, as I have already

[5] We might also notice, parenthetically, that if this view is stated with caution in his 2002 book, in the current paper it is remembered as an argument:

> As I have argued elsewhere, Marshall did not understand this [that mathematics was being interpreted in new ways], and neither did his students and those wedded to Marshallian methods. This, too, is not a matter of logic but, rather, of historical

noted, there are more plausible explanations of Marshall's resistance to the extended reliance by economists on mathematics. The explanation that is most empirically grounded, to repeat, is the most obvious one, that Marshall's resistance is (as with most opponents of the excessive application of mathematics in economics) a result of ontological reflection. Let me elaborate briefly.

There can be no denying that Marshall engaged in ontological reflection, if not always consistently. At one point, he grants some plausibility to J.S. Mill's view "that the forces with which economics deals" combined more in the manner of those forces studied in mechanics rather than in chemistry (Marshall, 1920, p. 637). However, he quickly adds that Mill exaggerated the case; and he also notes that:

> the forces of which economics has to take account are more numerous, less definite, less well known, and more diverse in character than those of mechanics; while the material on which they act is more uncertain and less homogeneous. Again the cases in which economic forces combine with more of the apparent arbitrariness of chemistry than of the simple regularity of pure mechanics, are neither rare nor unimportant. (ibid., p. 637)

Actually, the material of economics is eventually regarded as being more complicated still than that of chemistry:

> The matter with which the chemist deals is the same always: but economics like biology, deals with a matter, of which the inner nature and constitution, as well as the outer form, are constantly changing. (ibid., p. 637)

We can already see that if Weintraub is indeed suggesting that Marshall was interested not in ontic notions but only in theoretic properties of mechanical models, these passages are sufficient to refute such a view. Marshall's focus is the nature of the matter with which the various disciplines deal. And rather than merely assuming that the material of economics everywhere resembles that dealt with in mechanics, Marshall weighs the similarities and differences between the material of economics and that studied by not only mechanics but also chemistry and biology (and also, indeed, history).

And significantly, after numerous pages of such considerations, Marshall finally broaches the role of mathematical methods.

> contingency, because mathematical sophistication among British economists worsened dramatically (Robinson, Hicks, Harrod, etc.) once the Economics Tripos replaced the Mathematics Tripos as the entrance mechanism for economic students. (this issue, p. 451)

> It is obvious that there is no room in economics for long trains of deductive reasoning; no economist, not even Ricardo, attempted them. It may indeed appear at first sight that the contrary is suggested by the frequent use of mathematical formulae in economic studies. But on investigation it will be found that this suggestion is illusory, except perhaps when a pure mathematician uses economic hypotheses for the purpose of mathematical diversions; for then his concern is only to show the potentialities of mathematical methods on the supposition that material appropriate to their use had been supplied by economic study. He takes no technical responsibility for the material, and is often unaware how inadequate the material is to bear the strains of his powerful machinery. (ibid., p. 644)

Clearly, we do not have to rely on Weintraub's speculation when Marshall himself seems to provide some explanation of his resistance to too much formalism. Marshall is resisting an overreliance on mathematics not because he is stuck with a specific mechanics model or knows only of Euclidean geometry, rather he is assessing the nature of the economic material and suggesting that mathematical-deductivist methods (irrespective of their form or the interpretation of the mathematics) are not appropriate for its analysis.

Parenthetically, I might note here that, at one stage, Weintraub suggests that I see a continuous Cambridge tradition from Marshall, through Keynes and others to myself, adopting a consistent usage of equilibrium. I do not know why he suggests this. All I would claim is that a (prominent) strand of Cambridge economists have opposed the overreliance on mathematical methods in economics on the basis of (not a fear or ignorance of ongoing developments, but) a recognition that the ontological presuppositions of the methods in question have limited relevance to social analysis. It is in this light, for example, that we must view Keynes's seminal contribution to our understanding of the scope of econometrics. This is easily seen if we reproduce a central passage from Keynes's review of Tinbergen's advocacy of (recent developments in) econometrics. Here Keynes writes:

> There is first of all the central question of methodology, the logic of applying the method of multiple correlation to unanalysed economic material, which we know to be non-homogeneous through time. *If we are dealing with the action of numerically measurable, independent forces, adequately analyzed so that we were dealing with independent atomic factors and between them completely comprehensive,* acting with fluctuating relative strength on material constant and homogeneous through time, we might be able to use the method of multiple correlation with some confidence for disentangling the laws of their action. . . .

> In fact we know that every one of these conditions is far from being satisfied by the economic material under investigation. . . .
>
> To proceed to some more detailed comments. The coefficients arrived at are apparently assumed to be constant for 10 years or for a larger period. Yet, surely we know that they are not constant. There is no reason at all why they should not be different every year. (1973, pp. 285–286, emphasis added)

Keynes, like Marshall, does not use the word ontology, but uncovering ontological presuppositions is what he is doing here. As the italicized passage indicates, Keynes is identifying the presuppositions of atomism and isolation (a comprehensive list is equivalent to an isolated set of factors) that I have, above and elsewhere, suggested are presupposed by the use of mathematical-deductivist methods more widely. Keynes's is an ontological contribution that remains as relevant today as it was more than 60 years ago. It is notable that Keynes's contribution, and most others like it, fails to get even a mention in Weintraub's story.

Disinterested history

To this point, I have taken at face value both Weintraub's proclaimed disinterest in ontological reflection (even if it has sometimes meant his not so much avoiding areas where such reflection has been consequential as implying it never happened) as well as his declaration that he has no particular interest in either attacking or defending the mainstream. It seems to me, though, that, despite disclaimers, Weintraub does basically side with the mainstream, and this is one obvious explanation for his wishing to sideline ontological reflection (a normally critical activity), while providing alternative explanations for the outcomes of the latter activity.

For example, when Weintraub mentions science, he seems to be positively disposed toward it. This actually is something I share with him. But in my recent book (2003), I argue that the endeavour to mathematize economics mostly serves to undermine the discipline's ambitions of being scientific on any reasonable conception of the latter. Weintraub, on the other hand, cedes to the mathematization project the status of science, even with the title of his book (2002); he is, in effect, siding with the mainstream even before we turn to the first page. It does not matter here who is correct. The point is that we are both taking sides in a debate; both are interested parties.

Notice also that when in the paper above (this issue, pp. 423–444) I criticize the current mainstream, Weintraub seems compelled to provide a reply. I have suggested that historical figures, such as Marshall, Keynes, and Hayek, critical of a heavy reliance on mathematics, are either omitted from Weintraub's history or have their opposition interpreted as based not on ontological reflection but on a discomfort with ongoing developments. Such maneuvers, of course, serve to protect the relevant developments in the project of mathematizing economics from criticism. Now I myself am explicitly emphasizing the ontological nature of the orientation that underpins my criticism. If Weintraub cannot really deny this ontological orientation, given that he is reluctant to engage in ontological argumentation, he could have chosen to avoid discussing my criticisms of the mainstream altogether. Yet, as I say, he seems compelled to do otherwise. As it turns out, just because Weintraub refuses to engage in ontological reflection, and perhaps because he still hopes to give some impression of neutrality, his defense of the current mainstream borders on the trivial. But it is there all the same.

Thus, on noticing criticisms of the current mainstream, Weintraub offers the suggestion that the sole reason for it is that "nobody likes being marginalized" (this issue, p. 452). This thought is followed by the observation that mainstream economists, on finding their approach criticized, respond querulously (scratching their heads). Weintraub then seeks to provide further "support" for the mainstream with a list of the topics of students doctoral research; with a denial that mainstream modeling mostly reduces to axiomatic nonempirical models (whoever suggested otherwise?); with an (untenable, and certainly unsupported) assertion that all argument in science is model based; with a list of questions that he would like economics to answer; and finally with an assertion that those mainstream contributors, including Nobel Memorial Prize winners, who despair of the explanatory failures (and over-mathematization) of the discipline are not in a position to know what they are talking about.

This is clearly not a compelling case for the defense. However, my point, to repeat, is not that Weintraub's attempt to defend the mainstream (or criticize opposition to it) is not especially solid, but that he feels the need to take a stand at all. This is not disinterested or neutral history but, at best, self-deception. Weintraub's underlying support for the mainstream generates tension in his writing, a tension manifest in various ways, but not least in a declaration of disinterest in ontology, when seeking to write a history of a process in which ontological reflection has been (and continues to be) a central element.

Still, in the end, these are details. The main point here is that the issue that really divides us is not that one of us is concerned with history while the other is concerned only with (onto)logic. Rather, the real divide is between two approaches to history, one that is open to anything of relevance to the question pursued, and the other that, in advance of analysis, rules out any concern with ontological reflection as uninteresting. One approach recognizes its situated, partial, and interested nature; the other sets itself up both as protector of standards and as disinterested, but, in seeking to ensure that history is not abused, dogmatically rules out fundamental features of the story in advance and without reason.

Equilibrium analysis

Of course, the foregoing serves mostly as a backdrop to the main (a very specific) point of disagreement here. The above discussion does, I think, identify the real differences between Weintraub's *approach* and my own. And it is included to indicate what I believe lies behind Weintraub's responses to my original paper (this issue, pp. 423–444). But it must now be admitted that, all things considered, in principle, it could yet be the case, however dogmatic or constraining is Weintraub's orientation, he has, nevertheless, produced the more explanatorily powerful account of the state of modern equilibrium analysis. Let me now consider this issue head on, the topic of my original paper.

I start by emphasizing that I, in fact, accept much of Weintraub's periodization of developments in mathematics. In particular, I agree that the theoretic notion of equilibrium came to dominate over the ontic one only with the passing of time. Indeed, I could not deny this, for an important part of my project has been to question the fruitfulness of precisely the more *recent* emphasis on mathematical modeling, an emphasis that Weintraub correctly highlights.

Of course, I am here accepting that the earlier equilibrium notion is indeed an ontic one. This is a point that, in the context of Marshall at least, Weintraub does, in places, deny, claiming that, for Marshall, it was already theoretic, albeit a feature of a mechanical rather than a mathematical model. I have already shown that Marshall's general considerations were ontological. Let me now briefly, if somewhat parenthetically, note what Marshall does say about equilibrium specifically. The first time it seems to crop up in his *Principles,* Marshall writes: "A business firm grows and attains great strength, and afterwards perhaps stagnates and decays; and at the turning point there is a balancing or equilibrium of the forces of life and decay" (1920, p. 269).

The margin notes for the above passage read: "Biological and mechanical notions of the balancing of opposed forces" (ibid., p. 269).

Marshall quickly announces that his analysis will become complicated as he advances his study, adding: "But to prepare for this advanced study we want first to look at a simpler balancing of forces which corresponds rather to mechanical equilibrium of a stone hanging by an elastic string, or a number of balls resting against one another in a basin" (ibid., p. 269).

At the next mention of equilibrium, Marshall writes: "Again, markets vary with regard to the period of time which is allowed to the forces of demand and supply to bring themselves into equilibrium with one another, as well as with regard to the area over which they extend" (ibid., p. 274).

It seems, then, clear enough that whether Marshall draws his inspiration from mechanics or (and certainly it is often from) biology, his notion of equilibrium is continually ontic rather than theoretic.

Now, if I agree roughly with Weintraub's periodization (albeit maintaining that the earlier notion is ontic not theoretic), I might also add that in suggesting in my first paper that there is a confusion in the literature on equilibrium theorizing, I was not in any case referring only to mainstream contributions. To the contrary, because heterodox economists are continually concerned with, and can be distinguished from, the mainstream just through their commitment to addressing social reality (see Lawson, 2003, chs. 7–9), there is more reason to suppose that heterodox economists concerned with ideas of equilibrium will maintain ontic notions alongside any theoretic ones.

But I claim that confusion remains in mainstream contributions as well. And this is the claim that Weintraub resists. To see this confusion, we need only return to the contribution of Arrow and Hahn (1971), reviewed in my paper above (this issue, pp. 423–444). The review I provided is subsequently rejected by Weintraub, though I think we can easily see without due cause.

Notice, first, that Arrow and Hahn (1971) is indeed a relevant study to consider. It is not a marginal contribution; indeed, it is usually regarded as one of the "modern classics of general equilibrium theory" (Mas-Colell et al., 1995, p. 513). And it was published after the 1950s, by which time, according to Weintraub, more or less everyone (or at least everyone outside Cambridge, UK) had abandoned the notion of equilibrium as a balance (although Hahn eventually wound up in Cambridge, he was at this point at the London School of Economics and seemingly uncontaminated by Cambridge's apparent mathematical tardiness).

As I note in my original paper, Arrow and Hahn actually open their account with the following statement:

> 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. (Arrow and Hahn, 1971, p. 1)

Notice that these authors do not claim that the latter notion (or aspects of a notion) concerning a "balance of forces" has now been replaced by the (more) modern notion (or aspect) of determinateness. Rather, they merely see the former as being an "incompletely separable," more specific, notion. Contra Arrow and Hahn (ibid.), I suggest that the categories in question are, after all, completely separable notions of equilibrium, and specifically that (what I am calling) the ontic notion is not simply (or at all) a more specific notion, but something quite different from the theoretic one. It is clear, though, that Arrow and Hahn often run together the two concepts (as two inseparable aspects of a one notion) just because the theoretic/ontic distinction is untheorized. However, in referencing examples of the supposedly more specific notion of a balance, they unwittingly provide an ontic formulation. Consider more of Arrow and Hahn:

> In a sense, almost any attempt to give a theory of the whole economic system implies the acceptance of the first part of the equilibrium notion; and Adam Smith's "invisible hand" is a poetic expression of the most fundamental of economic relations, the equalization of rates of return, as enforced by the tendency of factors to move from low to high returns.
>
> The notion of equilibrium ("equal weight," referring to the condition for balancing a lever pivoted at its centre) was familiar to mechanics long before the publication of *The Wealth of Nations* in 1776, and with it the notion that the effects of a force may annihilate it (e.g., water finding its own level), but there is no obvious evidence that Smith drew his ideas from any analogy with mechanics. Whatever the source of the concept, the notion that a social system moved by independent actions in pursuit of different values is consistent with a final coherent balance, and one in which the outcomes may be 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)

This passage (apart from the first clause) deals solely with the way the economy works. The concern is with the balance of a social system. Its focus has nothing to do with properties of models, and everything to do with the forces of society. Yet Arrow and Hahn move from this discussion to immediately suggest thereby that "Smith was a creator of general equilibrium theory," a program involving a purely theoretic notion, thus indeed confusing the discussion of equilibrium theorizing (1971, p. 2).

How does Weintraub deal with this? He seems to misunderstand me. He points to my reference to a second paper by Hahn (1970), and argues, as if against me, that the equilibrium notion of Hahn is theoretic. I agree; this is precisely what I argue. At this point, Hahn is discussing whether models have formal solutions, and I write that in this specific context "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" (this issue, p. 434). However, in the quote from Arrow and Hahn (1971), both theoretic and ontic notions appear. I repeat that I agree that Arrow and Hahn do not recognize fully that the idea of "a balance of forces" is ontic. I also write that they are really only interested in the theoretic. My point is that theirs is precisely a sloppy use of language and categories, generating confusion, and resulting from a failure consistently to sustain, or even properly recognize, the theoretic/ontic distinction.

Conclusion

The differences in the approaches of Weintraub and myself are clear, although they do not lie where Weintraub locates them. Weintraub excludes ontological considerations from his history of economics, whereas I do not. And these contrasting orientations to prior constraints ultimately underpin different histories.

Weintraub finishes his piece suggesting that the ontic/theoretic distinction I draw might be thought of as an "archaeological–geological strata in which the 'ontic' is an impermeable layer onto which the 'theoretic' has dumped its volcanic ash"; and he proceeds to "suggest we simply get out our shovels" (this issue, pp. 453–454). Although Weintraub fails to see how both formulations could, and often do, lie side-by-side at the same level, an emphasis on mining the precious, if often hidden, ontic notions is surely a good idea.

In fact, the task of unearthing ontological presuppositions, wherever they are to be found, is one that still mostly lies in the waiting. Indeed, I believe it constitutes a task to which we need to turn rather urgently. This is especially so if, in the near future, we wish (as I and many others believe we should) not only to reorient economics so as to enable the discipline to move forward (again) but also to reorient the history of economics so that it can explain better how economics has come to possess such a pressing need for remedial action.

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