Walker's EQUILIBRIUM: A Review Essay

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Abstract

Donald Walker has provided us with a three-volume collection of work drawn from the literature of the past half-century intended to illustrate the variations over that period in the form and substance of the economist's concept of "equilibrium" and, in so doing, to complement the well-known Debreu collection on *General Equilibrium Theory*. Like all such projects, this collection, through the act of inclusion (and, hence, exclusion), serves to delineate for the non-specialist the range of the relevant questions, to identify points of controversy, and, by the ordering of the selected items, to reveal lines of intellectual development that otherwise might be obscured. Although this collection takes for its theme the question of equilibrium in all its dimensions, Walker's earlier broadside against contemporary general equilibrium literature in the Arrow-Debreu tradition looms over these volumes from start to finish. That is unfortunate since Walker's standard of a "functioning model," exhibiting "the highest possible degree of [institutional] realism" serves to obscure rather than to illuminate the long-standing methodological division between those, on the one hand, for whom the concept of equilibrium is no more than a "useful fiction" whose meaning is conditioned upon the model under consideration and those others for whom the concept expresses an observable feature of the world in which we live. In this review essay, we develop an alternative organizational framework which permits us to more clearly reveal the methodological fault lines running through the literature sampled by Walker's selections and thereby to identify the intellectual filiations between the objections raised by Blaug, McCloskey, and other recent critics and those advanced long ago by Keynes, Kaldor, Robinson, and Hicks.

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Donald A. Walker, ed., Equilibrium. 3 vols. Vol. 1. *Introduction to Equilibrium in Economics*, pp. xxiii, 583; Vol. 2. *Equilibrium in Traditional Models*, pp. x, 598; Vol. 3. *Some Recent Types of Equilibrium Models*, pp. x, 621. Cheltenham, UK and Northampton, MA, USA: Edward Elgar, 2000. ISBN 1-85898-928-0. $640.00.

I. THE COLLECTION

The three volumes before us comprise the second title in the "Elgar Reference Collection" of *Critical Ideas in Economics*, a new series which, we learn from the book cover, aims to provide "an essential reference source for students, researchers and lecturers in economics." Each volume in the series will bring together a collection of previously-published articles and book-chapters which "focuses on [a] concept widely used in economics," and will thereby "improve access to important areas of literature which will not be available in the archives of many of the newer libraries." No one can deny that Professor Walker's topic is ideally suited to this stated intent; is there a concept more "widely used in economics" than that of equilibrium? A collection of previously-published items cannot, of course, be appraised in terms of the originality of its content. Such a work offers a different sort of contribution. In addition to the publisher's stated aim of an improved access to those key articles which, either because of their age or the location of their publication, are not widely available, a work such as this can perform a function not unlike that which Weintraub (1991, pp. 129-30) ascribes to the survey article. The act of selection (and, hence, of exclusion) serves to delineate the field for the non-specialist, and the ordering of the items in the collection can reveal instructive lines of intellectual development—a "filiation of scientific ideas" to adopt Schumpeter's (1954, p. 6) felicitous phrase—that otherwise might be obscured.

The value of the work before us is enhanced by its standing as a complement to the earlier

Elgar collection on *General Equilibrium Theory* (Debreu, 1996). In his introduction to that

collection, Debreu identified several topics that had to be excluded from his volumes. At least a

half-dozen of those excluded topics—temporary equilibrium, fix-price models, rational

expectations, overlapping generations, sunspot equilibria, and game theory—find representation in

Walker's collection. Further, Walker has quite sensibly avoided reprinting items already included

in that earlier collection and has provided, at the end of his second and third volumes, lists of

supplementary readings relevant to the topics treated in those volumes, noting in those lists the

particular items available in the earlier, Debreu collection. As to the ordering of the items and the

matter of improved access, Walker's strategy differs from that adopted by Debreu, permitting some

improvement in the one case but perhaps not in the other. The improvement comes in the broad

topical categories governing the arrangement of the Walker selections. No such topical ordering is

found in the Debreu collection, which simply lists its entries alphabetically by first author's

surname. To be sure, Walker's arrangement is not beyond objection. As we shall see, a few items

are misclassified, and at least two interesting intellectual lines of descent are obscured by the

chosen scheme. Nevertheless, even if imperfect, the topical ordering provides the reader with what

is at least a provisional frame of reference and is consequently a welcome improvement over the

simple, alphabetical arrangement of the earlier, *General Equilibrium* collection. It is that earlier

collection, however, which apparently reflects a greater attention given to the availability of the

potential entries. At any rate, Debreu (1996, I, p. xii) tells us that he excluded from his volumes

any items "published in the 1990s" on the principle that "papers written in the more distant past . . .

are less readily available and critical judgement about them is less unreliable." Walker obviously

did not apply the same rule. Of the eighty-five items comprising the collection before us, forty-four

appeared in the 1990s, sixteen of which appeared after 1995. Still, the Walker collection manages to achieve a rather greater breadth of sources. In both cases, the most frequently-cited source is *Econometrica*, but that journal provides only nine of Walker's selections while, in keeping with its role in the history of general equilibrium theory (Ingrao and Israel, 1990, chap. 9; Weintraub, 1983; II, 1) [1], it provides thirty-five of the ninety items selected by Debreu.

II. GENERAL EQUILIBRIUM AND "REALISM"

Although the title of this collection suggests a concern with the concept of equilibrium in all its dimensions, Walker's (1997) earlier broadside against contemporary general equilibrium literature in the Arrow-Debreu tradition looms over these volumes from start to finish. Indeed, chapters four and five of that critique are among the selections chosen for this collection. Stripped of the rather tiresome repetition, Walker's criticism comes down to two fundamental points, both of which are embodied in his demand that an analytical model satisfy his standard of a "functioning system." This last is a concept that is central to the argument conveyed in the two chapters of that critique reproduced here (in Vol. II, items 5 and 17), but the reader will likely have some difficulty with those selections since the notion of a "functioning system" is defined and explained only in chapter one of that earlier work, which is not included in this collection. This problem is, of course, inevitable when an anthology includes selections drawn from a larger, integrated work. Fortunately, that occurs only infrequently here, the chief instances being the Walker chapters and a selection from Fisher (1983, chap. 2; II, 20). To be sure, the difficulty

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[1] When citing an item appearing in our collection, its location will be indicated by the relevant volume number (as Roman numeral) and chapter (Arabic notation). Further, since the collection provides the included items in "facsimile reproduction, inclusive of footnotes and pagination to facilitate ease of reference," I shall follow the publisher's lead and indicate cited passages by the original pagination.

can be ameliorated by the addition of an essay introducing the selection or of an explanatory

footnote at the relevant point; but, apart from the list of supplemental readings already mentioned

and a brief, general essay introducing the collection as a whole at the opening of Volume I, these

volumes, like those of the earlier, Debreu collection, offer no such scholarly appurtenances.

Readers familiar with Walker's complaints against the general equilibrium literature will

recall his appeal to the notion of a "functioning system" as a standard of appraisal. To meet that

standard, the model specification must contain sufficient institutional detail to motivate and

direct the behavior under investigation. It must, in other words, identify the "physical structures,

institutions, technology, laws, procedures, etc. . . . [that] condition, permit, or enforce ways of

behaving on the part of the participants" (Walker, 1997, p. 9). A model that fails to measure up

is "incomplete" and therefore can tell us nothing useful about the economic reality that surrounds

us. Lest the reader fail to grasp the exacting nature of this standard, Walker (p. 35) lists

explicitly the elements that must be included if a general equilibrium structure is to be deemed

"complete" and, hence, a "functioning system":

> The main purpose [of the chapter] has been to show that the general
> equilibrium models constructed in this century are not functioning systems
> because they are incomplete. . . .
>   A model should contain firms, consumers, and resource suppliers, all
> with specified characteristics. It should contain general economic
> institutions, by which is meant institutions other than those which are
> within markets; and it should contain technology, productive process, and
> systems of communication that link economic agents with markets. The
> model should have markets with well-defined physical features that enable
> the participants to make purchases and sales, and with marketplace
> institutions, technology, rules, and pricing conventions and procedures.
> The model should specify concretely how the traders are enabled to come
> together, how prices are quoted and publicized in each particular market,
> how and when information about prices and sales in each particular market
> is transmitted to traders in other markets, by whom and how prices are
> changed, and how offers to buy and sell are made public.

To be sure, general equilibrium theorists have long acknowledged a need to invest their models with at least the minimum institutional framework necessary to give structure to the posited behavior. Here is the source of the infamous "auctioneer," introduced to get round the awkward realization that a strict understanding of a competitive market as a case where "no one can affect the terms on which he may transact" seems to exclude all possible means by which such agents can find a set of prices at which the quantities offered for sale are just matched by the quantities demanded. If the existing prices do not produce such a match and if no agent has the power to alter his offer price, what mechanism changes market prices in the direction of the supposed equilibrium? The answer, of course, is the hypothetical market-maker, or "super-auctioneer," who is introduced strictly to perform this function (Arrow and Hahn, 1971, pp. 264-6; see also Walker, 1997, pp. 13-25). This august personage is able to collect information on the individual supply and demand offers at each price, calculate the price change necessary to move the market in the direction of equilibrium, then call out a new set of prices for a new round, thereby moving the market through a succession of converging steps to equilibrium, where all trades are conducted. But even here the market-maker cannot rest: he must also transmit to each of the participants information regarding the location and magnitude of all other individual supply and demand offers, thus enabling them to identify trading counterparts. Even aficionados of this literature find the implied information burden borne by the auctioneer to be beyond the credible. Arrow and Hahn (1971, pp. 266, 324) themselves characterize their market-maker as no more than an "extremely artificial," "as if" simplification. Those who find the "as if" expedient unsatisfactory will, no doubt, agree with Walker's (1997, p. 22) conclusion that "the process is obviously absurd." Hence, it is not surprising that Walker should insist, as a second condition defining his notion of a "functioning system," that the specified institutional structure

have a clearly recognizable congruence with the reality to be understood: "The objective of models that are intended to explain the real economy should be to achieve the highest possible degree of realism regarding the phenomena and relations that are deemed important" (Walker, 1997, pp. 38-9). All this, then, is the necessary background to the bipartite complaint we encounter in the first of the earlier Walker chapters reproduced in this collection. There we learn that the general equilibrium literature exhibits two distinct flaws: "One problem," with these studies "is that . . . their postulates have no justifications provided by foundations in a functioning model," and the "second problem is that the postulates and the process of manipulating the equations have no contact with real economic conditions" (Walker, 1997, pp. 101-2; II, 5).

We see these earlier complaints reflected in the two, overlapping classification schemes advanced in Walker's introductory essay to these volumes. The first of these draws upon an inference regarding the investigator's methodological stance. Under this head we have two classes of models. The "first type is constructed on the presumption that the real economy is an equilibrating system because it does not exhibit much instability." In the "second type of model," however, "equilibrium is not deliberately created." Instead, "the procedure is to incorporate assumptions into a model which are believed to be accurate representations of properties of the real economy, and the object is to see whether or not the model is an equilibrating system" in the hope that one can then draw a judgment "about whether or not a position of equilibrium exists in the real economy" (I, pp. xiv-xv). Overlying this distinction as to the methodological status of the equilibrium concept we find a second classification scheme reflecting Walker's repeated complaint regarding the insufficient institutional "realism" of these models. This class too is divided in two parts. The first encompasses the "perfectly competitive

virtual models"—that is, those which are "virtual in the sense that the only behavior that occurs in disequilibrium . . . is the quotation and changing of prices and the determination of the associated desired supply and demand quantities of the participants." Here are the constructions of the Arrow-Debreu-McKenzie type and those of their intellectual descendants. Standing in contrast to these are the "non-virtual models," those which permit some form of "disequilibrium behavior other than just the changing of prices and the reporting of the associated supply and demand quantities . . . ." These possess the merit that they "are closer to reality than the virtual variety," and their extension "beyond perfect competition to a number of other types of situations that are found in the real economy" is a "further step in the right direction" (I, pp. xviii-xx). Apparently, however, this ranking according to the degree of institutional "realism" offers little help in drawing instructive distinctions. At any rate, Walker tells us that a "superabundance" of models "have little or no relation to real economic situations." They are no more than "toys, games of logic with their own internal problems and rules that derive not from contacts with the real economy but from the previous literature on the subject." Hence, it is of no consequence whether "[t]hey are endowed with equilibrating properties or are examined to determine the presence or absence of those features," since any results obtained from these toy models "are not interesting from an economic point of view" (I, p. xv).

Nevertheless, the question of institutional realism provides the framework for the papers collected in Volume II, about half of which is devoted to the treatment of "perfectly competitive virtual models." Within that class, the papers are arranged according to the three classical problems of general equilibrium theory: the "existence" of equilibrium (can the model at hand be shown to possess such an equilibrium), its "uniqueness" (can the model be shown to exhibit only one such equilibrium), and its (local and global) "stability" (see Ingrao and Israel, 1990, p. 360).

The remainder of the volume is divided between "non-virtual purely competitive models" and the extension of the "non-virtual" class to "various forms of market imperfection." This classification by institutional realism is abandoned in Volume III, which, we are told, is simply intended to illustrate some of the "many special concepts of equilibrium [that] have been developed recently" (I, p. xx), identifying, among others, "stationary," "temporary," "rational expectations," Bayesian" equilibria, game theoretic equilibria (the longest section, containing fourteen items), and, finally, a catch-all, "other" category. Finally, Volume I provides the introduction to this theoretical work with a series of papers ostensibly devoted to "historical perspectives," to "criticisms of the equilibrium concept," and to investigations of the "meaning an concepts of modern economic equilibrium theory."[2]

III. THE MANY FACES OF EQUILIBRIUM

A. "Positivists" vs. "Pragmatists"

The reader comes away from this jumble yearning for a sense of coherence. The nature and content of the notion conveyed by the term "equilibrium" vary widely throughout these volumes. Walker's preoccupation with the acknowledged lack of institutional detail in these models fails to satisfy our desire for a framework that can help us to form these differing conceptions into an

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[2] The attentive reader will notice the curious omission from this litany of any mention of applications of computational general equilibrium models to various historical and contemporary policy questions, an omission that is remarked as well by the reviewer of Walker's *Advances in General Equilibrium Theory* (Hoass, 2000). Walker could reply that such applications have frequently provoked the same criticism he raises here—namely the objection that they fail to capture the relevant empirical relationships. For an illustration of a recent application in a historical context and the associated criticism, see Harley and Crafts (2000) and Temin (2000). Nevertheless, such models have found extensive use in the evaluation of trade policy (Srinivasan and Whalley, 1986), and one such paper does, indeed, find its way into Walker' collection, though it is "mislaid" in the section devoted to "market imperfection" (Mercenier, 1995; II, 25). As we will see below, this is not the only case of misclassification. The contribution of Mercenier's paper is not its incorporation of imperfect information and scale economies, which have been abiding themes in trade applications; it is rather his warning that trade economists err in their long-standing assumption that "nonuniqueness is largely a theoretical curiosum" (p. 162). Mercenier obtains, with a calibration of his model to EEC data, two plausible but wildly divergent equilibrium outcomes arising from the policy experiment under investigation.

orderly structure and thereby to reveal the lines of thematic progression running through this literature. We do, however, find a hint as to where we might look for such a framework in Walker's passing observation that each of the "specific concepts of equilibrium" treated in his third volume is model-specific, quoting for illustration Milgate's earlier lament that equilibrium has "become a category with no meaning independent of the exact specification of the initial conditions for *any* model" (Milgate, 1987, p. 182; emphasis in original; quoted in I, p. xx). This calls to mind Weintraub's recent efforts to remind us of the growing realization among philosophers and sociologists of science that "scientific work is knowledge creation in a context and that such knowledge is shared knowledge within a particular community" (Weintraub, 1991, p. 4). Viewed from this perspective, the concept of equilibrium derives its meaning strictly from the theoretical context in which it is employed by the scientific community, that meaning changing with that context. As Weintraub puts the point, this approach "maintains that there is no meaning of 'equilibrium' except as that word is used by the community of economists who read and write texts in which the word 'equilibrium' appears; the meaning of 'equilibrium' is derived from the use to which the word is put by the community of readers of texts on equilibrium analysis." Or, more bluntly, "equilibrium is a feature of our models, not the world." This view, which Weintraub designates as that of "the pragmatist," stands in opposition to that of the "positivist," who "argues that the idea of equilibrium is associated with some aspect of the real world and that the task in the scientific analysis of competitive equilibrium is to create better, or more realistic, models of equilibrium; the test of the theory of equilibrium is thus . . . correspondence with the real world in which equilibrium is to be found" (Weintraub, 1991, pp. 107-109).

Viewed through the prism of this distinction between the "positivist" and the "pragmatist,"
the epistemological fault lines running through this collection become strikingly apparent. The
positivists are, of course, well represented by Walker's repeated call for "the highest possible
degree of realism" in economic modeling, but his is not a lone voice. We have also Kaldor's
powerful lament that general equilibrium theory of the Arrow-Debreu type is not only "barren
and irrelevant as an apparatus of thought to deal with the manner of operation of economic
forces," but, worse, because of "the powerful attraction of the habits of thought engendered by
'equilibrium economics,' [it] has become a major obstacle to the development of economics as a
science—meaning by the term 'science' a body of theorems based on assumptions that are
empirically derived (from observations) and which embody hypotheses that are capable of
verification both in regard to the assumptions and the predictions." Consequently, by Kaldor's
standard, which seeks the conversion of general equilibrium theory "into a set of theorems
directly related to observable phenomena, the development of theoretical economics was one of
continual *degress*, not *progress*."   But Kaldor's complaint is not limited to the self-consciously
axiomatic nature of general equilibrium theory. On the contrary, he calls into question the very
notion of equilibrium itself. Here we encounter, in a particularly strong form, a theme which
recurs frequently in the critical and evaluative papers reproduced in Volume I—namely, the
realization that the dynamic process of adjustment necessarily involves continuous changes in
wealth positions and in the structure of markets, firms, and other institutions as well, all of which
alter the nature of the model's equilibrium, thereby making it all the more uncertain that any
such "point of rest" will ever be achieved. The particular form of this path-dependence problem
that troubles Kaldor is that which arises when we acknowledge a widespread existence of scale
economies: "When every change in the use of resources—every reorganization of productive

activities—creates the opportunity for a further change *which would not have existed otherwise,*
the notion of an 'optimum' allocation of resources . . . becomes a meaningless and contradictory
notion." Fortunately for our classroom exercises, Kaldor is willing to grant an exception for a
narrow range of comparative statics applications: the concept of an "optimal" allocation of
resources "falls apart—except perhaps for the consideration of short-run problems, where the
framework of social organization and the distribution of the major part of available 'resources,'
such as durable equipment and trained or educated labour, can be treated as given as a heritage of
the past, and the effects of current decisions on future development are ignored" (Kaldor, 1972,
pp. 1237-39; 1245-46; I, 7; emphasis in original). On this point, Kaldor seems to carry the
objection further even than Walker, who, while acknowledging in his introduction that "the
structure of the economy varies with parametric changes [which] will alter its equilibrium values
and its equilibrating properties," nevertheless insists that "this does not obviate the value of
equilibrium analysis" so long as we can "expect the revision of a model in order to reflect the
structural and behavioral changes that are seen to result from a change of conditions" (I, p. xvii).
No one, however, carries the criticism of equilibrium further than John Henry, who, in an essay
that would have been better positioned in the section devoted to "criticisms of the equilibrium
concept" than in its current grouping with papers exploring the "meaning and concepts" of
equilibrium theory, nevertheless delivers the ultimate positivist critique: "unless equilibrium
itself can be *proved* to be an actual state of society, the argument is hollow" (Henry, 1983-84,
220; I, 22; emphasis in original).

Weintraub's "pragmatists" would have little patience with such a demand, and theirs is
unmistakably the majority view among the papers collected here. Though written nearly a half-
century ago, the best exemplar remains Machlup's cogent expression of the equilibrium concept as

"a mental tool" to aid "in establishing to our satisfaction a causal nexus between different events or changes" contemplated within the "mental experiments" of our models. Long before Weintraub christened the position with the "pragmatist" label, Machlup ([1958] 1991, p. 53, n.7; I, 6) insisted that the equilibrium concept can be understood only within the context of the model under investigation, warning his reader that "equilibrium and disequilibrium refer to whatever model you may have in mind."[3] Indeed, "to characterize a concrete situation 'observed' in reality as one of 'equilibrium,'" as Henry and, apparently, Walker demand as a necessary condition to the application of the concept, "is to commit the fallacy of misplaced concreteness"—a fallacy that arises when we find ourselves "forgetting the relativity of equilibrium with respect to variables and relations selected," and consequently "jumping the distance between a useful fiction and particular data of observation" (pp. 57-8). To make that leap raises the further danger of the "fallacy of implicit evaluation or disguised politics" (p. 60). If we succumb to "the popular association of equilibrium with a Good Thing and of disequilibrium with a Bad Thing," the concept becomes a tool of persuasion, employed to gain support for some policy stance. It that event, we are left with "an equilibrium concept so drastically restricted by built-in political criteria [that it] becomes less useful, if not useless, in the analysis of most problems" (pp. 70-71).

Machlup's definition of equilibrium as a "mental tool" to aid in analysis is, of course, widely accepted, and it recurs frequently throughout these volumes. Indeed, Dore reminds us that we heard its echo in Keynes's stated goal to develop "an organized and orderly method of thinking out particular problems" (Keynes, [1936] 1964, p. 297; quoted in Dore, 1984-85, p. 194; I, 24). We hear it too, more recently, in Caravale's (1994, p. 28; I, 28) "logical conception" of equilibrating

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[3] Here again we have a case of apparent misclassification. Machlup's careful exposition "of the equilibrium concept . . . as a methodological device in abstract theory" (p. 44) is by no means a "criticism" of that concept, though the paper appears as the first entry in the section so titled. It would be better placed in the "meaning and concepts" section.

adjustment as "a process of potential convergence in the direction of the 'center of gravity' which can be identified on the basis of some fundamental data of the model."

B. Alternative Structures: Sraffian Surplus

There is also ample illustration of that "relativity of equilibrium" which Machlup described so long ago and which Walker employs as the theme for the third of our volumes. Instructive cases of this model-specific variation in the equilibrium concept occur in the earlier volumes as well, but the point is obscured by the classification scheme adopted. We have, for example, at least two papers devoted to an examination of the concept of equilibrium employed in the Sraffian exposition of Classical theory—that is, the line of development that Blaug (1999, pp. 214-15) has characterized as a "rational reconstruction" of Ricardo, though one "capable of affording a springboard for a wholly new style of long-run equilibrium theorizing . . . as an alternative heritage to the mainstream lineage of neoclassical economics." The two papers illustrating this "alternative heritage" appear in Volume I, though one (Harris, 1991; I, 4) is curiously located in the section promising "historical perspective," and the second (Bharadwaj, 1991; I, 13) appears nearly two hundred pages later near the end of the section devoted to "criticisms of the equilibrium concept."[4] Bharadwaj's is indeed a criticism of the neoclassical concept of equilibrium—one which takes as its point of departure Joan Robinson's (1974; I, 8) influential objection to the "timelessness" of general equilibrium modeling. The solution

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[4] The selection criteria employed to identify the papers comprising the "historical" section remains a mystery. The section opens reasonably enough with the Arrow and Hahn (1971, chap. 1) "Historical Introduction," and that is followed by an old survey of "Smith's concept of Equilibrium" as it is expressed in *The Theory of Moral Sentiments* and *The Wealth of Nations* (Myers, 1976). From there, however, we leap to a rigorous investigation of the "cross-dual" adjustment process found in those general equilibrium studies where price adjustment is specified as a function of excess demands and quantity adjustments are modeled as a response to profit differentials (Flaschel and Semmler, 1987). This is followed by the Harris paper on Sraffian equilibrium, after which the section closes with a return to a more obviously "historical" work, namely Magnan de Bornier's (1992) careful reconstruction from the original texts of the substance of the "Cournot-Bertrand Debate" over the nature of duopoly behavior.

proposed by Bharadwaj is an appeal to the Sraffian approach, which, he insists, is to be preferred

for its "ability fruitfully to combine historical elements abstracted from concrete observation and

to bring them together in short chains of logical reasoning." The source of this superiority lies in

the "central concept of classical theory," which Bharadwaj identifies, in the Sraffian tradition, as

the notion of a social surplus—roughly analogous to the nonlabor share of the net national

product. Because it must determine the extent of this surplus, "Classical political economy

(CPE) . . . encompasses theories of the determination of wages, of methods of production, and of

social demand," but it is significant that "these magnitudes are not simultaneously and co-

terminously determined as a subproblem of relative price formation." For this reason, says

Bharadwaj, "CPE has a much more dynamic story to tell about the interrelationships between

levels and changes in output *and* wages, or between the changes in output *and* techniques, and

vice versa." Indeed, "one of the important consequences" of this "surplus" structure is that

"'factor prices' are not determined from within the same process and by the same mechanism as

'commodity prices,'" (Bharadwaj, 1991, pp. 81; 85-86; I, 13, emphasis in original). This, of

course, is a characteristic feature of the classical model, but Bharadwaj's claims regarding a

supposed greater "dynamic" scope in the Sraffian, "surplus" approach to that model would carry

more weight had he confronted the fundamental circularity that bedevils that approach. Since

the surplus is defined as the residual left after deducting from total output the wage bill of the

labor employed in the production of that output, the magnitude of that total product must be

known prior to the determination of the surplus. Further, the rate of profit in this framework is

defined as the ratio of that surplus to that wage bill. Now, if the total product comprises more

than one commodity, it must be expressed in value terms; but if the value coefficient is itself

dependent upon the rate of profit, then to claim that the magnitude of the total product is

determined prior to the rate of profit is to involve one in circular reasoning (see Garegnani, 1987). The problem can be resolved by taking one of the variables (usually the wage rate) as given exogenously, but Bharadwaj tells us nothing of the implications raised by these analytical difficulties for his claim of a greater opportunity within the surplus tradition for a more fruitful dynamic analysis.

From Harris, on the other hand, we learn that those writing in the Walrasian tradition are not alone in their concerns regarding the stability characteristics of their equilibrium models. Sraffa's intellectual heirs have raised similar concerns. In this tradition, "the appropriate and relevant equilibrium concept to consider is that of 'long period equilibrium,' . . . characterised by the existence of 'prices of production' at a uniform rate of profit on the supply price of capital, those prices being said to constitute a center of gravitation for 'market prices.'" However, if we presume that it is profit-rate differentials that drive firm output decisions, we find that such an adjustment mechanism "cannot be guaranteed to provide the correct signals to profit seeking firms in their investment and output decisions that would cause the set of all firms to act so as to bring into existence [the equilibrium] prices of production and corresponding profit rate." Hence, just as in the Walrasian tradition so also in the Sraffian approach, "what is at issue . . . is whether the idea of a convergence of market prices to production prices is sustainable under *any* economically meaningful description of capitalist behaviour as regards decisions on prices, output, and investment." Further, the Sraffian literature is no more scrupulous in its attention to the kind of institutional content whose omission from the Walrasian models Walker finds so distressing. Certainly we hear echo of Walker's complaint in Harris's objection that the "analytic solutions" obtained from the surplus models "are, in many cases, not susceptible to any economically meaningful interpretation," because those models embody a "specification of economic behaviour

and institutional structure [that] is seriously lacking in the very elements that are relevant to evaluating the dynamic behaviour of real-world economies." The particular missing elements identified by Harris reproduce almost exactly Walker's list: labor and financial market institutions, technological and organizational structures, and firm pricing behavior. (Harris, 1991, pp. 90-93; I, 4)

Finally, we learn from Harris that the Sraffian approach is no less vulnerable to the more fundamental charge of path dependence so frequently leveled at the Walrasian models. In both traditions, the realization that disequilibrium trades alter the model's parameter values makes "the question of convergence to a predetermined equilibrium position necessarily . . . problematical unless resort is had to 'very rigid assumptions.'" Conversely, any attempt to produce such an equilibrium by the introduction of a presumed adjustment process "related to the equilibrium position must necessarily rule out features of actual economic behaviour in so far as such behaviour entails path dependence." All this leads Harris to view the Sraffian equilibrium framework with a nihilism not unlike that which characterizes Kaldor's judgment of the Walrasian equilibrium as "a meaningless and contradictory notion." For Harris, the problem of path dependence "provides general grounds for objecting to the conception of a determinate equilibrium of production prices in Classical theory quite apart from any finding of stability or instability in the gravitation process," a conclusion that makes Bharadwaj's promise of "a much more dynamic story" in the Classical tradition appear all the more unreasonably optimistic (Harris, 1991, pp. 93-94; I, 4).

C. "toys, games of logic"

Though tiresome in its repetition, no one can deny the validity of the complaint that the development of general equilibrium structures in the Walrasian tradition has long since slipped its

moorings to anything remotely approaching the reality we all observe outside the protective walls of our seminar rooms. There can be no surprise, then, that a collection of such work will contain abundant examples of Walker's "toys, games of logic." The first three entries in Volume 3 will suffice for our purposes. In the first of these, we find a make-believe economy in which all utility functions, endowments, and production processes "fluctuate according to a stationary probability law" and, further, every agent "observes the underlying stochastic process which governs all exogenous fluctuations in the economy" and presumes the continued operation of that process. Combining these principles with a permanent-income characterization of consumption behavior, we are led to the less than remarkable conclusion that markets for contingent claims are unnecessary in this world. Consumers provide themselves with all the insurance they require through their holdings of money balances, which absorb the stochastically predictable shocks to income and endowments; and "forward markets are not needed to coordinate intertemporal supply and demand, for agents have rational expectations and full information." These results are offered as "a limited answer to the question of why we do not in reality observe complete markets for contingent claims," a problem that troubled Arrow (1974) in his presidential address to the American Economic Association. That answer, says our author, "is that self-insurance and rational expectations can take care of every day fluctuations" (Bewley, 1981, pp. 266, 267; III, 1). The reader can be forgiven the view that this is a very "limited answer" indeed, there being, in this paradise, no uncertainty that cannot be reduced to a known probabilistic calculus. Most will, no doubt, find greater promise in a passing comment encountered a few entries later in Grandmont's contribution to the collection: "one should expect markets for contingent contracts to be *incomplete* in actual economies because it would be too difficult and costly to describe exhaustively in advance all possible contingencies of a complex, uncertain environment"

(Grandmont, 1991, p. 4; III, 5). Indeed, Arrow himself acknowledged that the answer to his question is likely to be found in a due regard to the costs involved in the construction and enforcement of forward and contingent contracts (Arrow, 1974, pp. 8-10).

We need have no fear of the unknown in moving from Bewley's world to that of the next entry in the volume. On the contrary, we find there a world much like that which we left behind. Here too all "shocks" follow a known probabilistic pattern ("an exogenous, time-homogeneous Markov process") in which "the current state is a sufficient statistic for the future evolution of the system" (Duffie et al., 1994; III, 2). As we move on to the third entry in the volume, our powerful foresight dims somewhat, but it is not lost entirely. Now we enter a world of overlapping generations, in which agents age across two periods (Gottardi, 1996; III, 3). Their tastes and endowments change across those two periods according to an unspecified stochastic process, but they are unaffected by the agent's date of birth and hence are independent of events occurring prior to that date. Although the agents have no information as to the probability associated with any particular state of the future, they are aware of all the possibilities. Consequently, they incorporate all possible future states in their consumption plans, a problem that is held within manageable limits by the assumption that this economy contains only a single consumption good, which, further, cannot be transferred across periods. Indeed, the only commodity that can be held across periods is non-interest-bearing, fiat money. Although "young" agents may acquire assets as well as the single consumption good, those assets mature when the agents enter their "old" period, paying out in money an amount determined by the state of the world that occurs in that period. The point of this peculiar apparatus is to provide yet another means of incorporating positive money holdings into the general equilibrium framework, though, as with Bewley, the result here is hardly surprising. In both cases, the models are

carefully constructed to accord to the monetary asset, and to it alone, a critical function. In Bewley's case, that function is to absorb the shocks of known stochastic fluctuations in income, while for Gottardi's agents money provides the only available means to transfer wealth across periods.

IV. History or "Timeless" Equilibrium?

To say that these, and indeed all work in the Walrasian tradition, describe hypothetical economies that bear little resemblance to the world we all encounter outside our classrooms is to state the obvious twice over. It is a commonplace that any effort at analysis must abstract from institutional detail to some extent. What distinguishes the Walrasian tradition is only its conscious and unwavering resolve to press that abstraction to the highest possible level. This was, indeed, the very characteristic that was advanced by its proponents as the theory's greatest strength. Recall Debreu's well-known appeal to an "effort toward rigor [that] substitutes correct reasonings and results for incorrect ones" and which "dictates the axiomatic form of the analysis where the theory, in the strict sense, is logically entirely disconnected from its interpretations" (Debreu, 1959, p. x). What we require is a standard of assessment: at what point does the gain of analytical generality obscure rather than illuminate the world we inhabit? Walker's repeated lamentations over the lack of institutional detail offer little help on this point. Indeed, his lists of the elements defining a "functioning system" suggest an impossible standard of nearly complete institutional content. Yet, difficult though it may be to meet his standard, Walker nowhere questions the fruitfulness of the equilibrium concept itself. Although the "real economy is always in disequilibrium" and "does not adjust with sufficient speed to reach equilibrium before it changes," he nevertheless insists that "those features do not mean that comparative equilibrium predictions are not interesting. We are interested in equilibrium in order to discover the

tendencies of the variables in disequilibrium occasioned by a parametric change." Hence, we can fruitfully appeal to the notion of equilibrium in our policy discourse as a point of reference from which to discern the impact that a proposed change will "have upon the directions in which variables tend to move, . . . even if equilibrium is never reached nor closely approached" (Walker, 1997, p. 128; II, 17)

A. "Time's Arrow"

However, though obscured by the organizational scheme imposed upon the collection, there is lurking throughout these volumes a second line of attack that, though related to Walker's criticism, nevertheless constitutes a more fundamental threat to neo-classical theory since, in this alternative view, it is the legitimacy of the equilibrium concept itself that is under assault. We have already encountered the Kaldorian form of this argument in his objection that the existence of scale economies reduces the equilibrium concept to a "meaningless and contradictory notion" (Kaldor, 1972; I, 7; the argument is repeated in Kaldor 1975; I, 9; and 1979; I, 10; the reader might well wonder at the propriety of the decision to include all of these in the collection). However, a more general statement of the critique is contained in two essays by Joan Robinson (1974; I, 8; and 1977), one of which found its way into our collection, and in the ruminations on the matter by Sir John Hicks (1976), which did not.

The problem can be described as the economist's confrontation with what physicists have long known as "time's arrow" (Eddington, 1929, pp. 68-9). The irreversibility of the arrow's flight presents difficulties for our theoretical speculations at both its leading and trailing ends. First, as we contemplate the influence of anticipated future outcomes on current market behavior, it forces us to follow Hicks in admitting the great divide in the nature and precision of our knowledge: "The knowledge that we have, or can have, of the past is different in kind from what

we can know of the future; for the latter, at best, is no more than a knowledge of probabilities" (Hicks, 1976, p. 135). This presents a troubling inconsistency with our concept of equilibrium, where, it is said, no agent faces any inducement to change current production or consumption behavior. If agents are able to make such a determination, then, as Robinson (1974, p. 203) points out, "every one knows exactly and in full detail what consequences would follow any action that he may take." Now, as we have seen in the work of Bewley, Duffie, *et. al.*, and other recent laborers in the Walrasian vineyard, it is possible to construct such equilibria so long as we infuse our agents with a degree of foresight no less than that described by Hicks's "best" case—"a knowledge of probabilities" of future occurrences. Further, we find in Bewley's contribution to our collection that even this tamed and house-broken breed of uncertainty is sufficient to demonstrate Hicks's earlier observation that "the holding of liquid reserves . . . is a matter of provision against an uncertain future—. . . providing oneself with the ability to take action to meet emergencies which may arise in the future and which are such that their particular shape cannot be accurately foreseen" (Hicks, 1976, p. 139). This focus on money's role as a means of insurance made necessary by "our distrust of our own calculations and conventions concerning the future" is, of course, the answer Keynes himself gave long ago to the question of money-holding. The question arises because "it is a recognized characteristic of money as a store of wealth that it is barren; whereas practically every other form of storing wealth yields some interest or profit." Why, then, asks Keynes, "should anyone outside a lunatic asylum wish to use money as a store of wealth?" (Keynes, 1937, pp. 215-216). We see now that Gottardi has offered an alternative answer: in a world in which there exists no "other form of storing wealth" across periods, even those outside the asylum will hold "barren" money balances to serve that purpose.

Nevertheless, though scholars have been remarkably ingenious in redefining our familiar notion of equilibrium to incorporate at least this well-behaved, "stochastically known" form of "uncertainty," this work only seems to have imparted all the more weight to Keynes's prescient warning on the matter: we deceive ourselves when we presume the "calculus of probability . . . to be capable of reducing uncertainty to the same calculable status as that of certainty itself." Our world presents us with countless, incalculable uncertainties: the prospect of war or terrorist violence, "or the price of copper and the rate of interest twenty years hence, or the obsolescence of a new invention." Keynes's admonition is no less apt for our world than for his own: "About these matters there is no scientific basis on which to form any calculable probability whatever. We simply do not know" (Keynes, 1937, pp. 213-14). Here is the particular "reality" whose omission from the general equilibrium literature troubles Robinson: "The assumption of 'perfect foresight' carries the argument out of this world into a system of mathematical abstraction, which, although the symbols may be given economic names, has no point of contact with empirical reality" (Robinson, 1977, p. 1322).

At the trailing end of time's arrow we are faced with the irreversible influence of the past on present behavior. Again as Robinson reminds us (1974, p. 206; I, 8), the vector of solution values to a timeless "system of mathematical abstraction" is of no help in determining how a real economy will respond to a change in any of the parameters held within the *ceteris paribus* impound. A simple change in taste alters the pattern of production; but that, in turn, "must involve investment and dis-investment, at least in work in progress, and windfall losses and gains on stocks that have become inappropriate." Hence, we cannot judge the character (or, indeed, the existence) of the new equilibrium unless we "fill in a whole story about the behaviour of the economy when it is out of equilibrium, including the effect of disappointed expectations on the decisions being taken by its

inhabitants." Of course, when we do so, we must acknowledge that the nature of the new

equilibrium depends critically on the nature of the disequilibrium behavior that brought it into

existence; each equilibrium depends upon the disequilibrium path that leads to it.

This path-dependency consequence of disequilibrium trading is central to Walker's critique

as well. As we have seen, the treatment of disequilibrium transactions defines his distinction

between "virtual" and "non-virtual" models, itself an element in his repeated demand that general

equilibrium models meet his standard of a "functioning system." It is the failure of that literature

to meet that standard that explains its preoccupation with the three classical questions of

existence, uniqueness and stability, a preoccupation that is the subject of the two chapters of his

earlier critique included here. The argument of these chapters is a familiar lament. Because the

literature originates in a strictly mathematical perspective, these triune dimensions of the

equilibrium solution are likewise understood strictly as properties of the equation systems under

consideration: "The question of existence is thus a matter of mathematical logic, of the properties

and interrelations of equations, a question of their solution, separate from the question of

stability." But this separability arises solely from the model's failure to specify the process by

which the move between equilibria is accomplished. In such an "incomplete," "virtual" model,

> during the equilibrating process there can be no endogenously caused changes in
> the parameters of the model such as preferences, technology, the amount of land
> and the number of workers. To assume that changes in prices and notional supplies
> and demands do not affect those conditions is itself a supposition about what
> transpires in disequilibrium in the model. The stocks of capital goods and of
> consumer durables and inventories of nondurables possessed by consumers and
> businesses do not vary because in disequilibrium there are no transactions, no
> production, no consumption . . . . There are no changes in real wealth during the
> equilibrating process and therefore no wealth-induced changes in supply or demand
> functions.

Like Robinson, Walker calls for the construction of more complete models that "fill in" the

disequilibrium story. In such a system, "certain of the parameters of the equations intended to

describe the model would in fact be endogenous variables, changing as the equilibrating process unfolds. There would be outputs and incomes, transactions at non-general-equilibrium prices and changes in such matters as the distribution and value of assets." Obviously, such a model would exhibit the very path dependency that troubled Robinson. In Walker, that characteristic prompts a criticism of the traditional treatment of existence and stability as separable questions, for in a properly specified "functioning system," "the question of ascertaining the existence of equilibrium is inextricably linked to the dynamic behaviour of the model" (Walker, 1997, pp. 103-4; 107-8; II, 5; see also II, 17).

There is, of course, nothing here that is new or controversial. Notwithstanding Debreu's famous refusal to consider the problems of stability or uniqueness (see for example Hildenbrand's recollection on this point in his "Introduction" to Debreu, 1983, p. 26), researchers have long understood that the proliferation of existence proofs serves only, adopting McCloskey's (1994, p. 133) blunt expression, to "show that certain equations describing a certain blackboard economy have a solution, but they do not give the actual solution to the blackboard problem, much less to an extant economy." Indeed, thirty years earlier, Chipman (1965, p. 36; I, 17) in a very useful survey that is included in our collection, observed that the "real content of the equilibrium concept is to be found not so much in the state itself as in the laws of change which it implies . . . . Fruitful analysis of equilibrium therefore requires analysis of stability conditions." Nevertheless, there is an important difference to be drawn in the way that scholars respond to this common insight. As we have seen, Walker's call for more fully specified models is not an attack on the equilibrium concept itself. For others, however, Kaldor and Robinson in our collection, for example, the admitted problem of path dependence is an insuperable bar to the fruitful application of the equilibrium framework. It is not just the pattern of production and wealth holdings that is

influenced by the presumed disequilibrium transactions, the very nature of society's technical knowledge is the product of that adjustment path. The channel of influence can operate either through Kaldor's scale economies or through relative price effects on the direction of technical change, as Robinson suggests following a long tradition among economic historians.[5] We can, of course, work out the solution values for a Walkerian, "virtual" model and determine how those values change in response to, say, a change in demand from potatoes to wheat. But we cannot deny the force of Robinson's objection that "even this is a somewhat idle exercise, for the path an economy follows necessarily influences its technology. An economy that has developed the technology for growing potatoes does not have the same spectrum of technical knowledge as one which only grows wheat." Hence, by Walker's lights, a model that presumes to trace out the change in equilibria must identify not only the disequilibrium changes in production patterns, factor supplies, and wealth holdings but must also be able to predict the change in technological knowledge. All this seems an impossible standard, leading Robinson to conclude, "As soon as the uncertainty of the expectations that guide economic behaviour is admitted, equilibrium drops out of the argument and history takes its place" (Robinson, 1974, pp. 202-204; I, 8)

We can discern in this collection the whisper of time's arrow even in our "virtual" models, in spite of our best efforts there to avoid its demands. In support of the decision to limit his agents

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[5] The objection raised by Robinson has long been common fare among economic historians—one of those areas of our discipline which takes seriously the injunction to take care with the institutional and technical details of the economy under consideration. It was Rosenberg's study of the history of technological advance that prompted his observation that the economist's common distinction between factor substitution (represented as a simple movement *along* a known isoquant in response to changing factor prices) and technological change (portrayed as a shift in the entire production function) is, from the perspective of the agents involved, a distinction without a difference. Since technological knowledge, like all knowledge, can be acquired only through the expenditure of scarce resources, we should expect that "the *known* portion of an isoquant typically [will] be . . . a relatively small segment." Viewed in this light, the problem presents us with a discomfiting question, which Rosenberg will not allow us to avoid: "If, in response to a change in factor prices, a firm has to commit resources to establishing new optimal input mixes, should not the activity leading to the new knowledge be described as technological change and not factor substitution? . . . . Once a substantial research expenditure is required for what is called 'factor substitution,' what is left of the economic basis for the distinction between technological change and factor substitution?" (Rosenberg, 1975, p. 459, emphasis added.)

to a single consumption commodity per period, Gottardi (1996, p. 77; III, 3) observes,

"Uncertainty in fact destroys the symmetry between past and future and hence the equivalence

between transfers across generations within the same period and across the different periods of an

individual's life." Indeed; time's arrow permits movement in only one direction: "past and future

are different" (Hicks, 1976, p. 135). That irreversibility of time poses a problem for Bewley as

well, and here too the issue is one that Hicks foresaw. If Bewley's consumer is to maximize

utility subject to a long-run budget constraint, he must know the value of his marginal utility of

money (λ_i in Bewley's notation). But this requires that the consumer determine his utility across

all goods and equilibrium prices in the current state. "However," says Bewley, this need not

impose too heavy a burden if we "can think of the consumer as having found the appropriate level

of λ_i by trial and error" (Bewley, 1981, p. 276; III, 1). In other words, as Hicks observed earlier, at

least when making large and infrequent purchases, the consumer likely does *not* (as our theory

supposes) "re-think his whole budget, identifying the collection of goods which would have to be

given up if [the contemplated purchase were made]." The decision rests instead on a rough

estimate of "what one can afford," and that is indicated by the marginal utility of money. That

familiar Marshallian concept "is much more than the mere Lagrange multiplier . . . . It is the

means by which the consumer is enabled to make his separate decisions, and to make them fairly

rationally, without being obliged to take the trouble to weigh all conceivable alternatives." But

we now have another point where the decisions of the past intrude upon our derivation of present

equilibrium. If the consumer obtains the estimate of his marginal utility of money by "trial and

error" over "past experience," then different pasts will produce different present demand functions

and thus different present equilibrium vectors (Hicks, 1976, pp. 137-38).

B. Obscured Filiations

Several years ago, Weintraub pointed out that Hicks's *Value and Capital* (1939) advanced "two related sets of ideas . . . concerning equilibrium and stability." In the first half of the book we find the now familiar, Walrasian question regarding the tendency of a particular equilibrium state to be reestablished after a temporary perturbation. In the second half of the book, where Hicks takes up the "foundations of dynamic economics," the argument turns to the possibility of a coherent process of economic evolution across time. Because of the considerable influence of *Value and Capital*, "we see that from Hicks there was a bifurcation of the dynamics literature into two separate lines" concerning, on the one hand, "the stability of a competitive equilibrium" and, on the other, "growth dynamics and capital theory" (Weintraub, 1991, p. 36). Now, it is evident that both of these distinct lines of thought are encompassed in Robinson's objection that economic analysis would be better served by a notion of historical evolution rather than by continued appeal to a discredited concept of equilibrium. If we acknowledge that transactions occur at "disequilibrium" prices, then what reason have we for our claim that a particular "equilibrium" state will be reestablished after a disturbance? Likewise, if we acknowledge that the present state of the world in all its dimensions (the pattern of wealth holdings, the nature and extent of available inputs; the nature of the technology, and so on) is the product of actions and decisions taken in the past under the influence of (possibly unrealized) expectations held at the time, what reason have we to claim that there exists an identifiable, "equilibrium" growth path to which the economy will tend in the future, much less that we can actually identify that path? It is evident from the collection before us that these complaints have not fallen on deaf ears. Though unwilling to abandon entirely the equilibrium concept, theorists have sought to modify the analytical content of that concept in response to these objections. Further, though the organizational structure of the

collection does not bring out this point, we can nevertheless discern a pattern of intellectual evolution that exhibits the same "bifurcation" in the conceptualization of the equilibrium problem that Weintraub found in the earlier developments prompted by Hicks's *Value and Capital*.

We find the second of these themes—that pursuing the economy's evolution across time—in the third of our volumes, where we encounter a recent survey of the "temporary equilibrium" research program by Grandmont (1991; III, 5), his earlier (1977), seminal paper laying out that agenda having already been included in the Debreu collection. Dispelling any remaining doubt regarding Hicks's influence on this literature, Grandmont describes in detail his debt to the Hicksian analytical "Monday"—that one point in the "week" when the markets are open, permitting agents to contract for their planned production and consumption over the remainder of the week. Although "Monday's" prices are presumed to be determined by the equality of the *current* quantities supplied and demanded, since no further recontracting is permitted—thereby maintaining prices unchanged over the remainder of the "week"—the plans and expectations formed for the future may well, and indeed will likely, be unsatisfied (Hicks, 1939, chap. ix; for a more recent summary and evaluation, see Hicks, 1976, pp. 141-2). Hence, the equilibrium established on "Monday" is strictly temporary. The analytical problem then is to trace out "the evolution in time of the sequence of temporary equilibria" (Grandmont, 1991, p. 4; III, 5). Obviously, that sequence can be specified so as to allow the agents to adjust their plans on the basis of new information revealed in each succeeding period, permitting an adaptive learning process. As Punzo (1991, pp. 31-2; III, 6) puts the point in a comment on the Grandmont paper, "Given the sequential character of temporary equilibrium analysis, whereby the equilibrium at each point of time depends on agents' beliefs about the values which the relevant variables might take in the future, a great deal of work has been devoted to the scrutiny of those assumptions

which reflect the way in which information is processed (learning) and expectations are formulated." Our collection offers two examples of that work, although the relationship of those papers to the temporary equilibrium theme will not be readily apparent since they are not classed under that heading (which is limited to the Grandmont and Punzo entries). Both papers illustrate one of the three sets of questions identified by Grandmont as comprising the temporary equilibrium research program—namely the question of "the stability of long-run steady-states when traders employ given learning procedures." The lone entry under the heading of "rational expectations" is the Evans and Honkapohja (1994, III, 9) demonstration that the introduction of adaptive learning processes in models with multiple such equilibria will not necessarily produce a convergence to a unique, stable equilibrium. The paper is certainly an illustration of a common class of rational expectations equilibria, but its chief contribution is its investigation of the effect of learning processes on the character of those equilibria, locating it securely within that segment of the temporary equilibrium literature that seeks to determine whether the "sequences of temporary equilibria do or do not converge to steady states" when the agents are "described as learning progressively the dynamics of their environment" (Grandmont, 1991, p. 19; III, 5). Likewise, Bullard's (1994; III, 27) paper, buried near the end of Volume III in the catch-all, "other" category, extends the Evans-Honkapohja analysis to the class of overlapping generations models and finds that the introduction of a similar learning process can produce a sequence of equilibria in which forecasting errors never vanish, raising the possibility of endogenous cycles.

Ironically, that "other" category at the end of Volume III also contains a key contribution to the second leg of that "bifurcated" analytical response to the "timeless" character of Walrasian models. We find there Hahn's (1978; III, 26) early attempt to incorporate disequilibrium trades in a context where agents face quantity constraints in some markets. This paper would be better

placed as the first item in Volume II, part 3, "models with various forms of market imperfection."

As it stands, that section opens with a more recent paper by Dehez and Drèze ([1984] 1991; II,

22), which is itself simply a variation on the theme considered in the earlier Hahn paper (and

begun by a yet earlier paper by Drèze, 1975, which is reproduced in the Debreu collection.)

Indeed, the whole of parts 2 and 3 of Volume II offer a coherent, nearly unbroken sequence of

work illustrating the evolution of theorists' attempts to expand the familiar Walrasian framework

to incorporate disequilibrium trades. Part 2 (containing just four items) opens with the earliest

published statement of the so-called Hahn trading process (Hahn and Negishi, 1962; II, 18),

includes Fisher's (1976; II, 19) extension to introduce disequilibrium production and consumption

and ends with two very useful surveys by Fisher (1983, chap. 2; II, 20) and Busetto (1995; II, 21).

Part 3 (with the addition of the Hahn, 1978, paper "mislaid" to Volume III) carries the sequence

forward to include illustrations of those efforts to incorporate elements of imperfect competition

and consequent quantity restraints on the one hand and price rigidities on the other. The only

apparent "break" in the thread occurs when we encounter the Mercenier (1995; II, 25) paper,

which, as we have seen above in note 2, is the collection's sole illustration of computational

general equilibrium models. It is true that his model includes a set of industries exhibiting a

degree of monopoly power arising from the introduction of scale economies, but the paper differs

from all others in the sequence (and, indeed, in the entire collection) in that its object is not the

investigation of the theoretical implications of such a structure but rather to offer a warning to

practitioners of the dilemma raised for policy evaluation when the parameterization of such

models produces multiple plausible solutions.[6]

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[6] One further curious classification is worth noting. Volume III contains a section devoted to "Bayesian"
  equilibrium comprising a single chapter on the subject by Laffont (1991; III, 10). However, in this context,
  "Bayesian" refers to an equilibrium concept employed in dynamic games in which each player is endowed with a
  subjective probability distribution over the information privately available to the other players. Hence, this item

C. Equilibrium as "Process"

We have here a sample of over a quarter-century of work devoted to the introduction of

disequilibrium behavior into the general equilibrium framework. To those who expect their

economic models to bear some remote connection to the behavior actually observed outside the

seminar room, these more recent developments will appear, when viewed in contrast to those

early Arrow-Debreu-McKenzie models, as "a step in the right direction." But they are still a very

long way from Walker's standard of a "functioning system," and Walker is not alone in his

complaint on this point. Within our collection, Busetto is particularly forthright in his assessment

of this work: these disequilibrium models remain "firmly linked to the GE [general equilibrium]

traditional assumptions, safeguarding the central role of the equilibrium notion and of the

properties underlying it. But this involves the impossibility of explaining the observable

behaviour of the individual agents in disequilibrium and of determining the actual time-path of the

economies investigated and the particular GE position eventually attained by them." The best

these models can do is to identify the properties that produce convergence to *some* (variously

defined) equilibrium position (Busetto, 1995, p. 96; II, 21).

To some observers, the past quarter-century of labor in these barren fields has only returned

us full circle to the very complaints raised by Kaldor, Robinson, and Hicks: we must, they

conclude, abandon the static notion of equilibrium at the core of the neo-Walrasian research

program and substitute in its stead a dynamic vision that captures the processes by which agents

adjust their behavior to changing market forces. The most provocative statement of this type in

our collection comes from Blaug and rests on a distinction between "two very different notions of

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would be more logically placed in the long section devoted to game theory, perhaps at the end of that section, after
the Mailath (1998) paper on evolutionary games, since the Bayesian concept can be viewed as an extension of an
evolutionary game in which the players modify their expectations of their competitors' future actions on the basis
of observed current actions.

what is meant by competition": on the one hand, "competition as an *end-state* of rest in the rivalry between buyers and sellers" and on the other, "competition as a *process* of rivalry that may or may not terminate in an end-state." It is the latter of these, Blaug insists, that has the longer pedigree; and certainly no on who has read their work would question his observation that the Classicals typically employed the "competition" term "with a definite or indefinite article attached to it," writing of "*a* competition between capitals" and thereby describing "an active process of jockeying for advantage, tending towards but never actually culminating in an equilibrium end-state." It is only because of "the invidious legacy of Walras's influence" that economics has, since the 1930s exhibited a regrettable "failure to address questions of active competition and instead to fall back uncritically on a model of perfect [end-state] competition as an 'ideal type.'" The work of the general equilibrium theorists over the past quarter-century, in which they have sought to expand their models to account for disequilibrium behavior, carries no weight for Blaug. On the contrary, he, like Busetto, dismisses that work for its failure to abandon that notion of an equilibrium "end-state" that is the chief fault of the Walrasian program: " this is a curious kind of disequilibrium analysis in which there is no process of rivalry between unrepresentative individual households and firms, no real contest between economic agents, but rather various end-state equilibrium points *called* disequilibria, the entire exercise differing from standard analysis only in that non-clearance of at least one of the subsectors of the economy is possible" (Blaug, 1997, pp. 241-51; I, 15).

What then are we to do? Are we to discard our comfortable legends of perfectly competitive end-states of equilibrium? Yes indeed. Blaug would take a very broad broom to the discipline and sweep away much of our curriculum, leaving only industrial organization (and presumably economic history and perhaps history of doctrines):

> But what are we then left with? We are left with the content of every chapter in every textbook on imperfect or monopolistic competition, on oligopoly, duopoly and monopoly, in short, on industrial organization as a subdiscipline of economics. In those chapters, firms jostle for advantage by price and non-price competition, undercutting and outbidding rivals in the market-place by advertising outlays and promotional expenses, launching new differentiated products, new technical processes, new methods of marketing and new organizational forms, and even new reward structures for their employees, all for the sake of head-start profits that they know will soon be eroded. In these chapters, there is never any doubt that competition is an active process, of discovery, of knowledge formation, of 'creative destruction' [Blaug, 1997, pp. 255-6; I, 15].

In what may be yet another illustration of the principle that new insights in a science appear not uniquely, to a single scholar, but rather in "multiples," independently advanced by several investigators, we find, about two hundred pages later in our collection, two remarkable parallels to Blaug's critique, written a decade earlier. Although his remedy is not so drastic as that proposed by Blaug, Jacques Henry (1987, pp. 464-6; I, 26) likewise asks us to relinquish our familiar concept of "equilibrium as a state" and to substitute a "notion of process," defined as "an ordered sequence of procedures, decisions or actions . . . motivated by a perceived opportunity or advantage . . . [and] that will result in a change in the very conditions that motivated the action in the first place." Unfortunately, beyond the proposal of a new category of "inequilibrium"— defined as a "state of affairs in which elements of endogenous change, novelty and permanency coexist"—Henry is unable to give operational expression to this "process" approach. Dore (1984-85; I, 24), however, independently prompted by like concerns, reminds us that the similar concept of "neutral" equilibrium has been employed in certain of the "non-Walrasian" corners of the discipline since the early efforts to investigate the conditions giving rise to regular cycles, the work on cobweb cycles being an obvious example (see Goodwin, 1947). The concept conveys more than "simply" an expression of an "unstable equilibrium," as that phrase is understood in a Walrasian context; it refers to models that seek to specify the conditions producing the move to

subsequent equilibria and to identify the nature of that dynamic path, as we see, for example, in the work on endogenous business cycles. By now, of course, we have strayed far from our Walrasian heritage, but these comments from Henry and Dore together with the similar but more vigorous critique from Blaug serve to bring home the realization that the very flaws passed down to us by that heritage that so troubled Kaldor and Robinson a quarter-century ago still haunt us today.

V. Where Do We Stand?

Although there is room to quibble at the organizing framework employed or at occasional omissions from or misclassifications within that framework, Walker has nevertheless performed his editorial duties with effect: he has compiled a collection that offers a commanding view of the broad sweep of work in general equilibrium theory. No one but the most committed enthusiasts can come away from this collection without at least a nagging suspicion that the critics are right. What is worse, one senses a growing suspicion that a long line of work stretching back more than a century has failed to advance our understanding of market outcomes to any significant degree, a conclusion that suggests a distressing waste of intellectual resources, just as Kaldor foresaw thirty years ago. Once again Blaug is uncompromising in his judgment: "after a century or more of endless refinements of the central core of GE theory, an exercise which has absorbed some of the best brains in twentieth-century economics, the theory is unable to shed any light on how market equilibrium is actually attained, not just in a real-world decentralized market economy but even in the toy economies beloved of GE theorists." To put the point bluntly, "We may conclude that GE theory as such is a cul-de-sac: it has no empirical content and never will have empirical content" (Blaug, 1997, p. 252; I, 15; see also McCloskey, 1994, p. 135 for a similar claim of resource misallocation). Notice the point of Blaug's attack: the theory's Achilles heel is its inability to

identify that *disequilibrium* process by which "market equilibrium is actually attained." This

failure comes not for want of trying. Steven Smale's work of the 1970s reintroduced his

"mathematics of time"—differential calculus and global analysis of differential equations—quite

deliberately to give mathematical expression to the process of disequilibrium adjustment across

time.[7] However, although Small (1976a) is able to identify functional forms that ensure a globally

stable process of convergence to equilibrium, no one has been able to invest his differential

equations with any meaningful economic interpretation. Hahn's (1982, p. 767) assessment is

typical: "While these results are of interest as algorithms, they have the drawback that it does not

seem possible to give them an economic motivation." It is that "drawback," in its various guises,

that has led even some of those who have labored long and tirelessly in the Walrasian fields to

express agreement with Blaug's assessment that this line of inquiry has brought us to a "cul-de-

sac." In their marvelously rich history of that research program, Ingrao and Israel close their

survey of the stability literature with the observation that Smale's new approach to that problem,

together with the work of those who followed him in that path, has "not only not modified but

actually confirmed the impression of the existence of a complete *impasse* in the theory of global

stability." These results, they conclude, "provide conclusive evidence that any attempt to obtain a

globally defined and *globally stable* process of price adjustment is doomed to failure.

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[7] Our collection contains Smale's (1976b; I, 21) paper in which he offers his justification for returning to the calculus techniques earlier displaced by Debreu's axioms of topology. The reader will likely agree with Ingrao and Israel (1990, p. 353) in their observation that "it is really amusing to see the overturning of values proposed by Smale: what was first viewed as a progressive development becomes an obstacle to be swept away." It is not quite correct to say, as Walker (1997, pp. 96-7; II, 5) seems to suggest, that Smale's reintroduction of the calculus "demonstrated" that the topological framework employed in Arrow's and Debreu's work of the 1950s was a "methodologically inferior" approach adopted because it was "convenient from a mathematical point of view." While it is true that Smale's reformulation "recognizes time rates of change," it is also true that it rests on techniques of global analysis that were not widely developed among mathematicians until roughly a decade after Arrow and Debreu developed their existence proofs. See Ingrao and Israel (1990, pp. 305-8)

Consequently, research in this direction must be considered as having come to a dead end"

(Ingrao and Israel, 1990, pp. 358-9; emphasis in original).

The gravity of this problem for the continued vitality of the general equilibrium research

program cannot be overstated. As we are reminded in the opening entry to our collection, Arrow

and Hahn set as a goal of that program the rigorous formulation of Smith's invisible hand

metaphor, which they understand as the "poetic expression" of "the most important intellectual

contribution that economic thought has made to the general understanding of social processes"—

namely, "the notion that a social system moved by independent actions in pursuit of different

values is consistent with a final coherent state of balance" (Arrow and Hahn, 1971, p. 1; I, 1). But

if this is truly to be our contribution, then it is not enough simply to prove the existence of such a

state. Our theory must also explain how the economy actually achieves that position. The

memorable statement given the problem by Ingrao and Israel (1990, p. 331) is worth repeating:

> An ideological standpoint that regards the market as possessing the virtue or
> intrinsic property of combining subjective behavior harmoniously cannot content
> itself with simply knowing that a final state of equilibrium exists. It has to show
> that the economy is capable of attaining this state spontaneously . . . . Otherwise,
> one would be forced to acknowledge that market forces are not capable of leading
> the market itself to equilibrium and that Smith's "invisible hand" wavers Sisyphus-
> like around the actually existing equilibrium position without having the strength to
> push the economic system into it.

This inability of the theory to trace out the economy's movement across time was earlier

glimpsed in Sonnenschein's (1972) famous demonstration that the classic, very general conditions

imposed upon individual utility functions are insufficient to permit the aggregation of those

functions into uniquely specified market excess demand relationships. Hence, without additional

restraints on the structure of the model, one can have no hope of obtaining a unique equilibrium or

even a set of discrete equilibria. This result struck at the foundation of the Walrasian program—

namely, "the belief that significant results could be obtained by starting from very general

hypotheses about the behavior of economic agents. The endeavor to keep the theory at the highest level of generality thus proved to be one of its weakest points" (Ingrao and Israel, 1990, p. 316). It is ironic, in view of his repeated call for highly specified "functioning systems,' that Walker fails to emphasize the significance of this result and of those elaborations that followed in its wake. On the contrary, Walker (1997, pp. 137-8; II, 17) holds that "the sorrow and pessimism over the implications of [Sonnenschein's] conclusions regarding the impossibility of making assumptions about individual demand functions that would restrict the form of aggregate excess demand functions are unnecessary." It is certainly true that Sonnenschein "analysed only the relationship between individual and aggregate excess demand functions" and consequently "did not specify or even imply the many structural and behavioural features necessary to create a complete model," but that is precisely the point. It is Sonnenschein's result and the later work that it prompted that has led Hildenbrand, and others who, like him, have contributed so much to the Walrasian tradition, to conclude, however reluctantly, that "an exchange economy can no longer serve as an appropriate prototype example for an economy if one wants to go beyond the existence and optimality problem." This is, as Hildenbrand properly insists, "an extremely important insight that must have an impact on future research projects" ("Introduction" to Debreu, 1983, p. 26)[8].

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[8] Hildenbrand's (1994, p. ix) memoir of his own reaction to the Sonnenschein result strikes a poignant chord:

> I was deeply consternated. Up to that time I had the naive illusion that the microeconomic foundation of the general equilibrium model, which I admired so much, does not only allow us to prove that the model and the concept of equilibrium are logically consistent (existence of equilibria), but also allows us to show that the equilibrium is well determined. This illusion, or should I say rather, this hope, was destroyed, once and for all, at least for the traditional model of exchange economies.
>
> I was tempted to repress this insight and to continue to find satisfaction in proving existence of equilibria for more general models under still weaker assumptions. However, I did not succeed in repressing the newly gained insight because I believe that a theory of economic equilibrium is incomplete if the equilibrium is not well determined.

But what is that impact to be? Certainly it involves a movement in the very direction that

Walker has urged. Hildenbrand (1994) himself has taken a step in that direction with his recent

effort to draw upon the Family Expenditure Surveys to structure his theory of market demand

functions. But this amounts to a renunciation of Debreu's principle that the theory must be

pressed to ever higher levels of abstraction, thereby remaining "entirely disconnected from its

interpretations"—a principle that has guided work in this tradition for the past half-century. The

irony in the present state of affairs is nicely stated by Ingrao and Israel in their concluding

assessment of that tradition: we are enabled to see so clearly the theory's flaws precisely because

of the axiomatic form so strenuously urged by Debreu. That "axiomatic approach has 'x-rayed'

the state of the theory in a complete and even pitiless way," revealing an unchanging

"programmatic core" characterized by the persistent focus on the problems of existence,

uniqueness, and stability. But, after a half-century of refinements, we are left with "a

contradiction between the theory's aims and the consequences derived from the system of

hypotheses constituting its structure." Most readers will likely find in Walker's collection ample

cause to agree with Ingrao and Israel that "the only way out of this situation is to jettison

explicitly the programmatic central core that has been so carefully preserved." What exactly is to

replace that barren core remains an open question, but it certainly "cannot avoid the highly

difficult and crucial question of the relations between theory and empirical reality" (Ingrao and

Israel, 1990, pp. 361-2), a "question" which Hildenbrand has already begun to explore. In their

fascinating history of the rise of the "Bourbakist" tradition in mathematics and its apparent

influence through Debreu on the course of general equilibrium theory, Weintraub and Mirowski

recall the comment of one of the tradition's founders, who characterized its product as "a very

well arranged cemetery with a beautiful array of tombstones, . . . useless for teaching" (Wientraub and Mirowski, 1994, p. 251). Is there a more apt epitaph for its economic progeny?

What then does all this mean for our old friend "equilibrium"? Must we follow the counsel of Kaldor, John Henry, Robinson, Blaug, and others who would have us "jettison" that concept along with the extreme abstraction and wholly static axiomatization that has characterized the Walrasian tradition? No, on this our collection suggests a broad consensus: the concept is central to our reasoning process and hence cannot be abandoned. Without it we can have no hope of getting beyond description to a level of analysis that can at least aspire to prediction. As we saw at the outset of this essay, that view, though widely represented in our collection, was most cogently expressed nearly a half-century ago in Machlup's reminder that equilibrium is properly understood as a "useful fiction" that permits our models to illuminate those causal relationships that are the object of our study. To presume equilibrium at the initial position is a means of ensuring that the postulated shock is "the *sole* disturbing change, the *sole* cause of anything that follows in the model." Further, to trace out "the sequence of adjusting changes until we reach a situation in which, barring another disturbance from the outside, everything could go on as it is," is simply to complete the argument and thereby assure ourselves "that 'no further adjustments' are required by the situation" (Machlup [1958] 1991, p. 48; I, 6; emphasis in original). Of course, our theories must contain enough institutional detail that we can discern in them at least a dim reflection of the world in which we live; and Walker's precept of a complete, "functioning system" is, no doubt, a laudable goal. But the unqualified demands with which he invests that principle prompt the same response as that which Hicks gave to a like charge from a critic of his time: "His ideal economics is not so far away from my own ideal economics; but I regard it as a

target set up in heaven. We cannot hope to reach it; we must just get as near to it as we can"

(Hicks, 1976, pp. 145-6).

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