The Notion Of Equilibrium In Microeconomic Theory

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1. INTRODUCTION.

This paper is essentially a brief – actually, a somewhat truncated – critical survey. It addresses a problem which can be put in the form of a query: is the notion of equilibrium in neoclassical microeconomic theory, as it was developed in Frank Hahn's Cambridge Inaugural Lecture (Hahn, 1973) and elsewhere, salvageable in the face of attack? Our answer is "no." The argument that yields this answer proceeds on the level of the individual agent (firm), and, where appropriate, on the level of the market as well. General equilibrium theory will be left virtually unexplored, except for very brief encounters in our discussions of Franklin Fisher and Joan Robinson. Matters having to do with consumer behaviour will not be examined.

The contexts for working out the negative reply to our query are provided, principally, by (1), the model of trading in a country-town corn market that Marshall presented in Chapter II, Book V of the Principles, (2), the context that Kenneth Arrow made use of in his "Toward a Theory of Price Adjustment" (Arrow, 1959), and, finally, (3), by team production in contemporary manufacturing industry (assuming that team production is near-universal).

What it was that led to the negative response to our query includes not only what we discovered in the above contexts, but, as well, our reading of Hahn; and, in addition, a variety of "critical" (and potentially critical) contributions in the literature, ranging from Georgescu-Roegen's Ely Lecture to the American Economic Association (Georgescu-Roegen, 1970), to contemporary "Austrian" theory, to Franklin Fisher's Disequilibrium Foundations of Equilibrium Economics (Fisher, 1983), and to Joan Robinson's essay, "History vs Equilibrium" (Robinson, 1974 and 1980). Obviously, these "critiques" vary considerably in intent, in explicitness and in potential for damage.

Section 2 deals with some of the principal highlights of Hahn's lecture. This section, and indeed a good deal of the entire paper, will be sited primarily on the "micro-micro" level; that is, the level occupied by firms or business decision makers.

In Section 3 there is an examination, of those instances, inside the "temple" (i.e., the mainstream), where there is at least a potential for discord concerning the notion of equilibrium. Thus, subsection 3.1 looks at the trading that takes place in Marshall's country-town corn market. In 3.2, and still within the mainstream, we discover additional discordant notes (or at least potentially discordant) in Kenneth Arrow's "Toward a Theory of Price Adjustment" (Arrow, 1959). In 3.3, we report on the scope for potential dissonance that can be found in Franklin Fisher's Disequilibrium Foundations of Equilibrium Economics (3.3). The latter also provides an exception to the decision we took not to deal with general equilibrium theory. (This exception, although a matter of some consequence, will be treated quite cursorily.)

Section 4, which could be regarded as being located either inside or outside the mainstream (or both, which, incidentally, can also be said of Marshall's corn market – at least on our view) deals with the difficulties that are posed for the equilibrium notion by various team-production phenomena.

Section 5 examines the threats (as we see them) to the notion of equilibrium that come from the Austrian presence. Whether or not this presence belongs inside or outside the mainstream is not a matter of concern.

Section 6, describes Joan Robinson's place in the story. Since some of Mrs. Robinson's sharpest barbs were directed against Walrasian and post-Walrasian general equilibrium theory, here, too, we shall ease up on our undertaking not to consider GE theory.

Section 7 looks very briefly at the von Neumann-Morgenstern universe of discourse.

Section 8 offers some concluding comments.

2. THE INDIVIDUAL AGENT AND MARKET EQUILIBRIUM; FRANK HAHN AND "MUTUAL CONSISTENCIES."

The principal notion of the set of notions that add up to Hahn's view of market equilibrium must be that of an open but bounded set of mutual consistencies across individual agents. That is, the messages or signals agents receive do not cause them to change their theories (about the state of the market) and hence change their courses of action. However, note that the openness of this set (of mutual consistencies) will preclude what Melvin Reder in his essay on "Chicago Economics" described as "tight prior equilibrium" (Reder, 1982). There is scarce any room in that world for inexactness, or imprecision -- common enough phenomena in a reality in which knowledge is far from "complete."

The mutual-consistencies view of equilibrium is central to Hahn's characterization of what he considers to be the "traditional" notion of equilibrium – a state of the market "in which the intended actions of rational economic agents are mutually consistent and can therefore be implemented" (Hahn, 1973, p.2). However, Hahn's description of this equilibrium is scarcely a "traditional" one, because he soon ensconces it in a set of information-theoretic problems. Agents hold "theories" about the state of the market. Presumably, a market will be in equilibrium not only when the agents' theories are mutually compatible but also, as pointed out above, when the signals that agents receive do not cause them to adopt different theories and different courses of action.

But what will cause an agent to change his theory? This is a complex problem. To model a solution to it could involve, Hahn points out, recourse to statistical decision theory. Thus , Hahn asks, does an agent abandon his theory when it is "sufficiently and systematically falsified" (Hahn, 1973, p. 26)? But, what does "sufficiently and systematically" mean in this context?

At this juncture, we should think of the planning horizon of the agent and of the "price" which becomes, to him, a parameter for the duration of his planning horizon. Conceivably, the quantities actually sold at this price ex post – and hence in real time – may be such as to falsify the agent's theory and cause the agent to change his theory. To envisage a set of agents abandoning their theories, devising new theories and embarking upon new courses of action is to provide a picture of a market in "disequilibrium"; but does it also to provide a picture of reacting in an "equilibrating" manner?

Here is where Hahn enters the fray with his modification of "mutual consistencies"; that is, with his "weaker" condition for equilibrium which has the consequence of bringing in its wake still more ambiguousness. The amended version of the mutual-consistencies notion would have it that the theories and actions of agents must not be "systematically and persistently inconsistent" (ibid., p. 28, emphasis added). Will statistical decision theory provide a resolution of the problem of defining "systematically and persistently" here? For example, should the requirement be imposed that the frequency distribution of actual prices in the relevant market (which distribution is conditional on phenomena that are exogenous to whatever makes up the decision set of the agent) correspond "closely enough with the prior conditional distribution" actually held by agents (Hahn, 1973, p. 27)? But how close is "closely enough"?

Now, the activity and processes that go on amid all this are surely not necessarily "equilibrating", as I believe Hahn himself concedes when he envisages decision makers in capitalist economies attempting to adapt to "fluctuating prices and noise" (Hahn, 1973, p. 28). It would seem that Hahn would reject the gridlock of tight prior equilibrium. What he offers in its place is not entirely clear - unless one can shed light upon requirements such as "closely enough," etc.

Return now to the heart of Hahn's mutual-consistencies characterization of the "traditional" concept of equilibrium; namely, a state in which the intentions of rational agents are consistent and hence implementable. What is pertinent here, however, is the addendum that Hahn provides having to do with information assimilation. Remember that, according to Hahn, equilibrium requires that agents receive signals that do not cause them to change their theories (about the state of the market) and hence their behaviour. But, again, what can cause an agent to change his theory? An agent will only change his theory when it is "sufficiently and systematically" falsified was Hahn's not entirely unambiguous answer. This is part of the "strong" condition for traditional equilibrium which, recall, was replaced by Hahn by the "weaker" condition that agents' actions and theories must not be "systematically and persistently inconsistent" (emphasis added). Neither version of the equilibrium condition is a paragon of preciseness.

The adverbs "sufficiently" and "persistently" fail to take us out of the realm of impreciseness. And, our sojourn in that realm seems to be assured when Hahn calls up, without success – self-confessedly, it would appear – statistical decision theory for assistance. But, again, as regards this "assistance," what is one to make of the requirement that the frequency distribution of "actual" prices must correspond "closely enough" to the prior distribution held by agents.

Notice that the difficulties that have been posed here stem from the fact that the "equilibrium" set of phenomena include the thoughts and behaviour of human beings who do not have perfect knowledge. No wonder, then, that Hahn wrote that the traditional theory of equilibrium "does best when the individual is of no importance . . . [i.e., when] he is of measure zero" and that his (Hahn's) theory also does best when "all of the . . . theoretical problems arising from the individual's mattering do not have to be taken into account. The social institutions of property and markets [should] have the dominant role" here (Hahn, 1973, p. 33). Hahn's reference to the "social institutions of property and . . .," phenomena of reality, after all, would seem to take us away from the "fact" of equilibrium.

3. MORE DISSONANCE INSIDE THE TEMPLE.

Some dissonance has already been heard inside the temple – from Frank Hahn, no less. We reported, in section 2, what we took to be a "concession" by Hahn that the trading activity and processes that take place in the environment of capitalist economies in which decision makers attempt to adapt to fluctuating prices and noise will not necessarily be equilibrating. The difficulties besetting such attempts seemed to stem from the fact, as Hahn noted, that the "equilibrium" set of phenomena include the thoughts and behaviour of human beings who do not have perfect knowledge. We noted, too, that we should not be surprised to see Hahn writing that the "traditional" theory of equilibrium "does best when the individual is of no importance. . . ."

Then, recall, came Hahn's "blockbuster": "The social institutions of property and markets . . . have the dominant role" here.

But perhaps most destructive of all: without straining text for interpretation, it does not appear that Frank Hahn is inclined to burden the notion of equilibrium with a great deal of weight. Consider this: ". . . one of the reasons why an equilibrium notion is useful is that it seems to make precise the limits of economic analysis" (Hahn, 1973, p. 21).

3.1 Marshall.

For an historical add-on – but still presumably within the temple -- consider the trading (non-tâtonnement?) that takes place in Marshall's celebrated country-town corn market. In a non-tâtonnement process, trading activity is necessarily equilibrating (although examples can be found of "non-convergent" non-tâtonnement in speculative trading in financial markets). In what we have chosen to call "quasi" non-tâtonnement (hereafter QNT) and which we believe we can discern in Marshall's corn-market, trading is not necessarily equilibrating. An excellent context for the consideration of QNT is, we believe, provided by the country-town corn market of chapter 2 of Book V of Marshall's Principles. The market is a competitive one: not, of course, the pure and perfect competition of the textbooks; for, as Marshall reminds us, "it is not necessary for our argument that traders have a thorough knowledge of the market" (Marshall, 1920, p. 334). Trading takes place in real time; that is, throughout a market period which is presumably a trading day. Moreover, there appears to be a notion of market equilibrium; specifically, the familiar "anchor" notion, or an *ex ante* market-clearing price to which individual decision making may be said to be "anchored." This is admittedly surmise, for there is virtually no analysis or discussion of what equilibrium is as a phenomenon. Instead, there is merely an appearance of a "market-equilibrium" cross in a hypothetical market demand/supply schedule, introduced for illustrative purposes.

In short, treatment in depth of equilibrium as a phenomenon remains a gap in Marshall's account. Moreover, the question of convergence, as we shall presently see, makes only a rather shadowy appearance. Without attempting to pronounce any further upon what it is that passes for an equilibrium concept in Marshall's market period, and simply accepting the market-clearing cross as a point of departure, we might nevertheless attempt to see what, if anything, can be extracted by way of a convergence story. Marshall writes that the market will close at a price which will "probably" be at the equilibrium level (Marshall, 1920, p. 334), or, if not, then at a price which "would be very unlikely to diverge widely" from the equilibrium level (*ibid*., p. 335, ftnte 1). Marshall's use of the adverb "probably" should not be taken to mean that he has here introduced probability considerations. Far from it. What Marshall is doing here is conducting an analysis in a context that is really best described as one of fuzziness or imprecision. Item: "very unlikely to diverge widely from . . . " And, there is the clear intimation that different agents might well perceive different market-clearing prices for planning and trading (after all, it is "not necessary for our argument" for traders to have a "thorough" knowledge of the market).

A price of 36s. a quarter is presented in Marshall's market demand/supply schedule as the "market-clearing" price. Though, one is dogged by the question, will every agent so perceive it?

Many traders who could have been counted on to sell their corn for 36s/quarter were in fact only willing to sell at that price because they were "in urgent need of a certain amount of ready money" (ibid., p. 335, ftnte 1). If they had been able to overcome this "need" and had actually succeeded in selling some of their inventory at a somewhat higher price, there might have been a diminution in the marginal utility of money to them. And, if that had happened, they might have refused to sell at 36s. "all the corn they would have sold if the price had been 36s. throughout" (ibid.) Thus, such sellers, as a consequence of "getting an advantage of bargaining at the beginning of the market" (ibid.) might hold, to the end, to a price higher than the "equilibrium" price. In this situation Marshall suggests that one might well envisage the market closing at "an" equilibrium price rather than "the" equilibrium price – although, in a somewhat hesitant afterthought, Marshall wrings from himself the concession that the price, "an" equilibrium price, would, as already indicated, be very unlikely to diverge widely from "the" equilibrium price (ibid., emphasis in the original).

So where are we? The notion of the market equilibrium which is determinate in the Kaldor (Kaldor, 1933-4)-Negishi (Negishi, 1961) sense, – for such a notion of determinateness, the equilibrium "conditions" must be invariant to the equilibrating process (Kaldor, 1933-4, p. 123)or, we may say, the equilibrium conditions must be path-independent – and unique, has taken a severe buffeting. Finally, the "equilibrium condition" in question is simply that set of prices which could bring about the "equilibrium" position (Kaldor, 1933-4, p. 124).

Yielding to, and resisting, the need for "ready cash" in the here and now might be accompanied by small-scale market-testing or information-enhancing experiments on the part of traders, and, if so, will have changed Kaldor's "condition" for market equilibrium; again, the price vector which could actually secure an "equilibrium position." Now, the information concerning market conditions that is derived from these little "experiments" might, as already suggested, be qualitatively different across agents, since, obviously some will be more skilful than others in both acquiring and evaluating information. To where will all this lead? That market equilibrium condition in Kaldor's sense might well be perceived differently across agents. A fortiori, this will apply to that component of Kaldor's price vector which is the ex ante "market-clearing" price that might appear in the tactical planning of agents.

Exacerbating matters will be the following considerations. The strength of the need for ready cash (or the marginal utility of money to the trader, one of the influences upon trading behaviour in Marshall's scenario) will doubtless vary during the course of the trading day. Specifically, one should think of an inter-temporal distribution (across the trading day) of the intensity of this need. Obviously, the intensity of the need for ready cash should be regarded as a constraint on market-testing experiments by agents.

3.2 Arrow.

Going into fast forward now, would it be succumbing to excessively selective perception to claim that one can detect a dissonant note in Arrow's well-known "Toward a Theory of Price Adjustment" (Arrow, 1959)?

> ". . . there exists a logical gap in the usual formulation of the perfectly competitive economy. . . . It is not explained whose decision it is to change prices. . . . Each individual participant in the economy is supposed to take prices as given and determine his choices as to purchases and sales accordingly; there is no one left over whose job is to make a decision on price." (Arrow, 1959, pp. 41 and 43)

Whatever there was of the "anchor" notion of equilibrium in Marshall, we found to be rather non-robust, a victim, essentially, of the context; that is, of that (optimally) loose view of competitive conditions that left ample room for information-theoretic problems. Arrow's model of price dispersion (Arrow, 1959) in "competitive conditions," but equipped with an assumption of product homogeneity takes us further into these matters.

A purely competitive market or industry is characterized by a multiplicity of firms (and customers) and by product homogeneity. A perfectly competitive market is made so by perfect knowledge and perfect mobility of factors of production, including that collection of factors known as a business firm. The "extreme" form of competitive industry of elementary microeconomic theory is endowed with both sets of characteristics which, when brought together, spell a perfectly elastic demand curve facing the product of the individual firm. Now, Marshall's loose "competitive conditions" can encompass that imperfection which is of the Robinsonian-Chamberlinian variety or the competitive firm as a "monopolist with a special environment," as Arrow put it (Arrow, 1959, p. 45). Arrow makes no mention of Marshall's "particular demand curve" of the firm; in fact, he assumes product homogeneity, as well as a multiplicity of firms. We retain both assumptions.

In Arrow's model, the perfectly elastic demand curve facing the purely and perfectly competitive firm can only obtain when $D = S$. But, at the same time, we are faced with the seeming anomaly of having an equilibrium concept without an equilibrating process in a deterministic sense. When $D \neq S$, we get a picture of price-setting activity among individual firms when Arrow provides an information-theoretic answer to the riddle, whose decision is it, in the purely and perfectly competitive model, to change price in accordance with the equation, $\frac{dp}{dt} = h(S-D)$; that is, in accordance with the "Law of Supply and Demand?" Equilibrium in the anchor sense therefore appears to be retained, minus, of course, the key assumption of perfect knowledge. And, it is this "minus" which means that the price-setting activity of individual firms who face a (to them) "fuzzy" decision-making environment will not necessarily be equilibrating in nature. In a fuzzy environment, a single, uniform perception across agents of the anchor, or the market-clearing price that falls out of the "equilibrium" cross to serve as the anchor for the duration of the agents' planning horizon is likely, as in Marshall's corn market, to be "smothered" in what is in reality a set of different perceptions, opinions or whatever, across agents, of the market "equilibrium."

An equilibrating process may not survive such smothering; which, as we see it, brings Arrow into an interface with quasi non-tâtonnement. $S = D$, or the market "equilibrium" – or, for that matter, $S \neq D$ – are, then, at bottom matters of uniformity, across agents, of perceptions and of responses to events in an imperfectly known (and understood) environment. However, in his canvass of (some of) the relevant-information-theoretic issues here, Arrow does not arrive at quite such a dispensation. On the other hand, his canvass does yield the less-than-perfectly-elastic demand function facing the individual firm in a competitive milieu where there is product homogeneity. Now, it may appear that to reach this result requires the above-mentioned "uniformity" of perceptions condition or assumption. But, a fuzzy decision-making environment, plus differences in the quality of the information sets across agents, permit no such assumption. So, where knowledge is imperfect, whether $S = D$, or $S \neq D$, is, on this view, a matter of perceptions and information-processing skills.

When responses to inventory signals or the modes of inventory management across agents indicate that there is a widespread belief that $S \neq D$, the stage is set to fill in the missing pieces of the jigsaw puzzle - Arrow's "logical gap" - concerning the who's, how's and why's of actually bringing about a change in price (now presumably an average?) that is encapsulated in the time derivative which is the "Law of Supply and Demand."

Two cases are considered by Arrow: (1) First, according to a great mass of perceptions of the state of the market, and inventory management signals and rules, S > D. Obviously, in this situation, that centrepiece assumption of the purely competitive market must go; that is, the assumption that firms can sell all they wish to, right up to their physical capacity to produce and sell, at the "given" market price. In such circumstances our purely competitive firms will be facing less-than-perfectly-elastic demand functions for their wares; they become in effect monopolists of sorts. What happens to the notion of the market price in such circumstances? Inventory signals suggest to individual firms that a testing of the state of the market by lowering price may be warranted, with the result that there can no longer be the market price. Different firms will be charging different prices for identical goods. And, what is so implausible about that in the absence of an assumption of perfect knowledge among buyers (See Reder, 1948, p. 148)? So much for Jevons' "Law of Indifference," according to which there can only be one price for a good in a competitive market. To refer parenthetically to a more recent development: Stiglitz, in taking up the question of consumer search technology and search costs, has developed a model of a "price distribution" in "equilibrium" in "competitive" markets in which "essentially identical" products are sold (Stiglitz, 1987).

(2) Secondly, and of greater importance for present purposes, imagine a situation which is the exact opposite to that just described. That is, in the world of perceptions, inventory signals, etc., it now appears that D > S. Compared to case no. 1, in this second situation an even greater plethora of information-theoretic problems may be thrown up. In any event, once again we must imagine less-than-perfectly-elastic demand curves facing individual firms; so that the competitive firm has become a partial monopolist once more, a "monopolist" who will embark upon still more rounds of

exploration of the demand curve he faces by adjusting price, except that this time, since D > S, the exploratory forays - specifically, upward adjustments of price - may be somewhat bolder because, of course, of the belief in positive excess demand and also because of the belief that no firm can increase the amount it supplies in a very short period, which will allay the "oligopoly"-type fear of the consequences of a price increase. A fuzzy decision-making environment plus a propensity to explore in the manner of monopolists, will once again have the result of more than one price. However, in this particular circumstance, problems of decision making in a fuzzy environment may be compounded; for not only will the exploration exercises differ somewhat across decision makers, but, assuming that they are all embarking upon unidirectional - i.e., upward - adjustments of price, one could envisage the individual firm's entire demand function being displaced in an upward direction simultaneously with the individual explorations (see Arrow, 1959, p. 46). That is, D > S becomes in effect a shift parameter for the individual firm's entire demand function. Once again we find ourselves in a regime of price dispersion. After all, firms may not all raise their prices to the same extent.

What is of crucial importance here is that all this trading, all this exploratory activity, will not necessarily be "equilibrating." Arrow concedes that "the adjustment process is apt to be very irregular" (ibid.). For our characterization of "quasi" non-tâtonnement, we would go even further: there is merely trading and changing prices. A unique market equilibrium is surely lost sight of amid all the fuzziness in the decision-making environment, amid all the inter-individual differences in perceptions and in responses to "signals." Moreover, that trading will be non-equilibrating and the regime turn out to be one of price dispersion, will be virtually assured by the fact that the "quality" of the information used by traders in decision making will differ across traders, and, furthermore, if information sets "improve," they will improve at different rates (across traders).

In sum, and going beyond Arrow, we may say that competition in this "competitive" firm's world is non-pure and imperfect, not with respect to product differentiation, but, rather, with respect to something that Arrow does not explicitly acknowledge; namely, interfirm differences in the quality of information sets (twinned, of course, with interfirm differences in perceptions) and interfirm differences in rates of improvement in this quality.

These realities render somewhat implausible an equilibrating process - this, Arrow tacitly concedes - but it also obliterates what Arrow seems to want to retain; namely, the static, anchor notion of a unique market equilibrium D/S cross, perceived as such by all agents, and determinate in the Kaldor-Negishi sense. But a "deterministic" model must yield to the demands of an imperfectly known world. To have said this is to have said a good deal of what is important about quasi non-tâtonnement. And, certainly, it is to have said that in place of the unique cross - a single anchor for planning that is to the same for all agents - we will have a set of such crosses; for different agents will doubtless have different perceptions.

3.3 Franklin Fisher.

We conclude our search for instances of dissonance within the temple with a brief examination of Franklin Fisher's Disequilibrium Foundations of Equilibrium Economics (Fisher, 1983). There, the discordant notes are, one must say, relatively non-numerous and decidedly non-shrill. Still, with Fisher as our guide, one does find some cracks in the structure - and this (to mix a metaphor), without making mountains out of molehills.

Furthermore, to let Schumpeter in, at this point, as Fisher does, is surely to introduce dissonance:

> "Following Schumpeter, we cannot suppose it to be true that an economy in which new opportunities constantly arise will converge to equilibrium. Such new opportunities are to be broadly interpreted; apart from the traditional forms of Schumpeterian innovation - technology, new raw materials, new forms of organization and so on - they include the weakening of previously binding constraints. . . ." (Fisher, 1983, p. 90).

Then, there is the matter of the "mainstream" assumption that Fisher describes as "No Favourable Surprise." As regards price adjustment, such an assumption, Fisher writes, "is not appealing in the absence of arbitrage trading experience, ..." (ibid., p.217). Further, "the lesson of the tâtonnement literature should make economists wary of assuming that rapid price adjustment alone will produce price stability." (ibid.) And, issues like these cannot be dealt with "by assuming them away." (ibid.)

The lack of an "explicit transactions technology is unattractive," in part "because the way in which transactions take place is the basic stuff of disequilibrium" (ibid., p. 218). And, to identify yet another gap in the mainstream that drew Fisher's attention: nothing is said about how "perceptions [concerning demand and supply curves, and the like] are formed . . . This is far from a minor matter" (ibid., p. 215, emphasis added).

Indeed, it does appear that under the tutelage of Franklin Fisher one can discover some not insignificant fractures in the walls of the temple!

4. TEAM PRODUCTION.

What we decided to call teamness (to be dealt with in somewhat greater detail presently) can manifest itself obscurely. And, the impreciseness in which it is often enveloped renders difficult exact description of the effects on overall firm performance of managerial and/or employee "contributions" (in terms of "marginal product" or whatever) to the productive process.

What, though, are the phenomena that may be brought under the head, "teamness", and what are the issues that will loom large as a result of the appearance of these phenomena? The phenomena in question really reduce to only one of concern; namely, networking, which is seen here as the necessity of having to interact with others, at various levels of the firm's hierarchy, in order to perform. The problems that will normally be associated with the need to interact will obviously include imperfect information on how a managerial performance will affect the quality of employee performance (and perhaps vice versa).

Alchian and Demsetz presented a simple two-employee or two team-member model that rested on a who is doing what to whom cross partial derivative which in the presence of teamness phenomena surely tells us that it should have a non-zero value, but just as surely poses the problem of how to sign it (i.e., is worker i having a favourable or deleterious effect upon the performance of worker j)?

In a burst of enthusiasm, Eugene Fama referred to the problem of how to sign the cross partial as a "striking insight" (Fama, 1980, p. 289). Fama's investigation into these matters was informed, as was the Alchian-Demsetz paper, by the "firm as a nexus of contracts" point of view. The main burden of Fama's brief was the monitoring of the performance of the managerial members of the team, in order to establish appropriate levels of remuneration. Not surprisingly, the principal stumbling block lay in measuring the marginal product of a manager. Fama's way around this stumbling block was expressed in the following equation:

$$z_t = \bar{z}_t + \epsilon_t,$$

where z_t is a manager's "measured" marginal product, and $\bar{z}_t$ the expected value of this marginal product. It is assumed that the latter follows a random walk that is independent of the white noise, ϵ_t. Simply put, a manager's "measured" marginal product is here conceived to be a "random walk plus white noise" (Fama, 1980, p. 299, emphasis added).

Measurement problems aside, though, why does the application of managerial effort not yield results that are certain? Is it primarily because this individual's talents and tastes do not evolve in a way which is predictable, as Fama would have it (Fama, p. 299)? But, surely, the presence of uncertainty here is not primarily a matter of the manager's "evolution" – a process which might take so long as to be quite irrelevant to the question of behaviour caught up in the flow of events in the here and now. Rather, the fact that the expected marginal product is essentially a stochastic phenomenon is primarily due to ignorance – uncertainty – about how his (the manager's) performance will mesh in with that of others (and vice versa). This kind of uncertainty is surely the problem of modelling team production. It is this which makes one reluctant to confer a zero value upon the Alchian-Demsetz cross partial, and, more importantly, hesitant when it comes to deciding upon the appropriate sign for the latter.

Although Fama broached the problem of how to characterize and measure "productivity" in so far as one category of inputs - managerial services - is concerned, it must be said that his extremely mechanistic "solution" to this problem leaves much to be desired. The simple fact is that Fama did not really explore the uncertainty that seems to inhere in team relationships, choosing instead to address the uncertainty problem within the context of monitoring performance, enforcing contractual terms and revising managerial rates of remuneration in the case of an (analytically) relatively "isolated" manager - isolated from all except boards of directors, managerial labour markets and capital markets. Without presenting all the details (as they are discussed in Fama) of the problem of characterizing and measuring managerial productivity, let it merely be said that Fama's "solution" is essentially John Muth's (Muth, 1960). That is, commencing with the assumption that expected marginal product, $\bar{z}_t$, follows, as we have already pointed out, a random walk, the steps of which are independent of ϵ_t (white noise), Fama turns to Muth's demonstration of the "evolution" of expected managerial marginal product (and hence managerial rates of remuneration) to a "full ex post settling up" (Fama, 1980, p. 300). Muth's "general case" permits $\bar{z}_t$ to follow <u>any</u> nonstationary process "which has the property that the differences of the marginal product are stationary" (Fama, 1980, p. 302, and Muth, 1960.) Clearly, white noise does not "dominate" there!

True, imperfect information, as well as uncertainty, make it difficult to describe with precision (and hence to model) the kind of networking which may develop between employers and employees, and especially among employees themselves.

What was described above as <u>the</u> teamness problem should have made an appearance in the Fama scenario. It did not. At any rate, the following is something that I devised in order to provide an example of the teamness problem on the "ordinary-worker" level. Consider the inputs of a peer group of either blue- or white-collar workers who have to undertake, while on the job, a certain amount of task-related reading or other modes of information acquisition (e.g., an oral presentation by an equipment manufacturer's representative). The blue-collar workers could be, say, installers of home swimming pools; the white collar workers a group of clerks in the credit department of a large department store. In these instances, the input could be described as worker comprehension of information received. The comprehension – and, also, of course, the linkage between comprehension and productive effectiveness – would surely be in part a function of "teamness." That is, although comprehension and hence effectiveness of effort would depend upon attributes of the individual worker providing this input, it would no doubt depend in part, too, upon the worker's interaction with other workers in efforts to understand what is read or acquired in other ways. Here, not only the capriciousness of a particular kind of individual ability, but the vagaries of teamness would make the consequences for productive effectiveness and overall firm performance of this input – assimilating and comprehending information – a decidedly "fuzzy" matter, as fuzzy indeed as the input itself. Notice that in this instance the inputs are rather more complicated or sophisticated than what is usually assumed to be supplied in the way of mental-cum-physical effort in conventional models in the theory of the firm or production theory. To move outside the theory of the firm for a moment, consider what, if anything, distinguishes workers from horses in the von Neumann equilibrium growth model. But, really, to make this point, there is no need to move outside the theory of the firm and the theory of production. For not only is there a neglect, in the latter, of the less obtrusive aspects of team production (i.e., teamness et al.), but the question might well be asked, how much difference is there, really, between those uncomplicated, undifferentiated homogeneous physical lumps which are inputs of labour services in the von Neumann equilibrium growth model, and the inputs of labour services in the standard models that are within the neoclassical theory of the firm and/or theory of production.

5. A CONTEMPORARY "AUSTRIAN" PRESENCE.

Is an "Austrian" presence to be considered "non-mainstream?" To those who would return a negative response to this query, it has to be pointed out that an equilibrium mindset is really quite alien to the Austrian presence. One would have thought that most economists would need no such reminder. Yet, R. N. Langlois, in his review in the Journal of Economic Literature of Jack High's Maximizing, Action, and Market Adjustment: An Inquiry into the Theory of Economic Disequilibrium (Langlois, 1993) seems to think that such a reminder is still in order. Economists may have been troubled by the problem of disequilibrium "ever since Walras brought forth the auctioneer" (Langlois, 1993, p. 1973), but evidently not sufficiently troubled to address issues such as, "what is the mechanism by which prices and quantities adjust," what actually happens in disequilibrium? The truth is that most economists "have bundled these issues away" (Langlois, ibid).

Here is how High, a contemporary American economist of the Austrian persuasion, strikes the note which sets the tone for his little book: "The Austrians look on the market as always being in a state of disequilibrium, so that disequilibrium theory is the main object, the core of economics" (High, 1990, p. 36, emphasis in original).

The following are some of the principal propositions that reveal High's position. Defining "action" as purposeful or goal-directed behaviour, High contends that "action rather than maximizing should serve as the basis of economic theory" (High, 1990, pp. 50 and 58). If we make this substitution - that is, base our theory on action - High argues, we will be able to give greater weight to ignorance, alertness, uncertainty, judgment and learning.

Because "maximizing is an equilibrium construction" (High, p. 77), High believes that this will equip him for an attack on neoclassical search theory. That is, models of the latter, because they are, among other things, equilibrium models, show "how little distance search has taken us from the world of maximizing determinate functions" (High, p. 88). Continuing with the same line of attack, High writes that in "spirit, search models are models of ignorance and learning; in fact, search models are models of near-perfect knowledge, predictable learning, and riskless circumstance" (High, ibid., p.89). High's penultimate blow (against neoclassical search theory) is this. "Because the searcher knows precisely..., what he is ignorant of, he also knows precisely what he is looking for. There is no spontaneous learning in the search model, no discovery of the new and unexpected." (High, ibid.) Finally, the coup de grace: neoclassical search theory "ignores [and here High brings in a well-armed Kenneth Boulding] those 'things that we ought to know, and which we do not know we that we ought to know, that remain largely unknown and unsought for'" (Kenneth Boulding, as quoted by Kirzner in his "Hayek, Knowledge and Market Processes" [Kirzner, 1979]).

6. JOAN ROBINSON'S PLACE IN THE STORY.

The Joan Robinson to be presented in the following paragraph is clearly targeting general equilibrium theory. Her observations should be cited in spite of our undertaking to neglect GE theory. Besides, there is some quintessentially Joan Robinson stuff here!

First, Mrs. Robinson once described an equilibrium position as a "logical place" (Robinson, 1962): it has nothing to do with reality. But, at the same time, it rests upon a set of data which have very much to do with reality: for example, the form of the relevant utility function (admittedly, though, not much "reality" there), the form of the relevant production functions - tastes and technology, in short - and whatever may be subsumed under the head, "social institutions", plus the set of expectations of agents. Now, these data pertain to a point sited in real time, or "history". In other words, and continuing with Mrs. Robinson, one could imagine this "logical place," which is a conceptual equilibrium state, being "dumped down" at a particular point in historic time (Robinson, ibid., p. 25). But those expectations which are part of the data and which guide behaviour can of course change and when that happens, "equilibrium drops out of sight and history takes its place" (Robinson, 1980, p. 48) - history replete with incompatibilities that can always be relied upon to knock an economy out of a "state" (Robinson, 1962, p. 26) Or, "[t]o make the argument applicable to actual situations, we have to leave equilibrium analysis behind and approach the problem in terms of an historical process, the system continually lurching from one out-of-equilibrium position to another" (Robinson, 1962a, pp. 6-7).

Furthermore, Mrs. Robinson in effect contends that the notion of equilibrium should never be permitted to bear too much weight. Certainly, the weight that she assigned to the notion does not appear to be very great: if equilibrium positions are kept in their "logical places", "there is much to be learned from a priori comparisons" across such positions (Robinson, 1962, p. 25).

7. VON NEUMANN-MORGENSTERN.

Like that set of problems which is team production, the von Neumann Morgenstern universe of discourse (Theory of Games and Economic Behaviour) is both "inside" and "outside" the mainstream. But whether it is inside or outside, or both, one must agree that it directs some very heavy artillery against some of the mathematical techniques that have been deployed for equilibrium theorising (and that, in a negative way, motivated the present paper).

Here is a sampling from von Neumann on this question of choice of mathematical technique:

> "The importance of the social phenomena, the wealth and multiplicity of their manifestations, and the complexity of their structure, are at least equal to those in physics. It is therefore to be expected - or feared - that mathematical discoveries of a stature comparable to that of calculus will be needed to produce decisive success in this field...A fortiori it is unlikely that a mere repetition of the tricks which served us so well in physics will do for social phenomena too...."
>
> These observations should be remembered in connection with the current overemphasis on the use of calculus, differential equations, etc., as the main tools of mathematical economics (von Neumann and Morgenstern, 1964, p. 6).

And these "main tools" are to be associated with what we may describe as an "implicit determinism," of which more, presently.

In his finely wrought essay, in the Journal of Economic Literature, Robert Leonard of the Université de Québec à Montréal summed up the matter in this way:

> "It...becomes evident that the theory of games was intended to constitute a radical rupture with the Hicks-Samuelson variant of neoclassical economics. Von Neumann completely rejected the latter's underlying physical metaphor of classical mechanics and the associated mathematics based on the differential calculus: (Leonard, 1995, p. 755)."

Then, in their Theory of Games and Economic Behaviour von Neumann and Morgenstern strike the note that von Neumann had struck earlier on in his Mathematical Foundations of Quantum Mechanics (von Neumann, 1932) to the effect that, as Leonard put it, in characterizing von Neumann's position, "social phenomena clearly require theoretical categories and mathematical methods of a different kind" (Leonard, 1995, p. 756, emphasis in original).

A final bit of trenchant comment from Leonard:

> "...the theory of games becomes more than a response to Morgenstern's earlier calls for a theory of interdependent economic decision. It becomes part of a general shift in science which involved, broadly speaking, the abandonment of determinism, continuity, calculus, and the metaphor of the 'machine,' to allow for indeterminism, probability, and discontinuous changes of state. The mechanical language of the calculus capitulated to what [Hermann] Weyl called 'Ars Cominatoria' (in Weyl's Philosophy of Mathematics and Natural Science, 1949), the use of set theory, [and] the simple exploration of structure" (Leonard, 1995, p. 756, emphasis in original).

8. CONCLUDING COMMENTS.

Recall that we were of the opinion that it did seem that Frank Hahn, like Joan Robinson, was not inclined to burden the notion of equilibrium with a great deal of weight. To Hahn, the notion of equilibrium would merely seem to bring with it an appreciation of what the limits of economic analysis are. So, the lion roared and brought forth a mouse? Or, is this merely a case of damning with faint praise? And, remember, too, our reference to Hahn on decision-makers in capitalist economies attempting to adapt to "fluctuating prices and noise" and, as well, our reference to Hahn's insistence on the dominant role of the social institution of property, etc.

The reference to "history" in Section 6 (pace Mrs. Robinson) suggests a grand theme, but what is at issue here is really something quite prosaic. For "history," simply read, trading activity of agents who, in the pursuit of rents, are continually attempting to adjust behaviour to changes in that "datum" which is a set of expectations. This is reality. On the level of concept, trading activity, together with inter-agent differences in perceptions and hence in information sets, practically ensures non-fulfilment of Hahn's mutual-consistency conditions for "equilibrium" and to all intents and purposes eliminates Kaldor's price-vector "condition" for a determinate equilibrium (that is, the set of prices that can "support" an "equilibrium" position.)

In reality, there can be no such thing as conventional non-tâtonnement trading activity with its requirement of convergence to (market-clearing) equilibrium. What is more, even on the level of concept, there seems to be little room for conventional non-tâtonnement, unless, at the same time, one can produce a model of trading behaviour that will guarantee convergence. But, to model a "guarantee of convergence" in an other than excessively artificial way is to produce a casualty: the very notion of market equilibrium.

Which brings us to what we referred to as "quasi" non-tâtonnement (QNT.) The defining characteristic there is that trading is merely trading. There are no Hicksian "false" prices (Hicks, 1946, pp. 127-9). A "false" price is a price other than the so-called equilibrium price; "it could be revised when the equilibrium price is reached" (Hicks, 1946, p. 128). Agents are transactors in a fuzzy environment, repeatedly "adjusting." But the adjusting will not necessarily result in an unwavering market D/S "equilibrium" cross.

What, though, to do about Frank Hahn's reference to the "equilibrium <u>action</u> of agents" (Hahn, 1973, p. 37, emphasis added)? Such action or behaviour appears to be that behaviour which is caught up in a set of mutual consistencies across agents (see Hahn, 1973, pp. 28-9, 39). But at the same time these "mutual consistencies," together with the decision-making modes and actions which are their referents, seem to disappear into Hahn's "noise" (cf. Hahn, 1973, p. 28).

The apparent (to us) non-salvageability of the notion of equilibrium on the micro level is of course part of a larger story. It is a story, it should be added, which resonates with the so-called "New Institutionalism" that is still extant in the profession. Franklin Fisher invoked the name of

Schumpeter in his Disequilibrium Foundations of Equilibrium Economics. Furthermore, and also related to these observations and reminders, is the fact that yet another part of the "larger story" is a refurbished behavioural theory of the firm.

Finally, let it be said that none of this - the ghost of Schumpeter, the New Institutionalism and all the pertinent accoutrements of the latter - can possibly make one very sanguine concerning a prospect of either robustness or longevity for the notion of equilibrium in microeconomic theory.

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