EQUILIBRIUM AND STABILITY IN CLASSICAL THEORY

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1. Introduction

Kaldor begins his paper of 1972 with a resounding blast

against what he calls "equilibrium economics". The opening

paragraph is worth quoting in full for its candid irreverence:

> "The purpose of my lecture today is to explain why, in my
> view, the prevailing theory of value - what I called, in a
> shorthand way, 'equilibrium economics' - is barren and
> irrelevant as an apparatus of thought to deal with the
> manner of operation of economic forces, or as an instrument
> for non-trivial predictions concerning the effects of
> economic changes, whether induced by political action or by
> other causes. I should go further and say that the powerful
> attraction of the habits of thought engendered by
> 'equilibrium economics' 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." (Kaldor, 1972, p.
> 1237).

Kaldor recognizes, of course, that there are different concepts

and uses of the idea of equilibrium in economic analysis.

Accordingly, he goes on immediately to clarify what he means by

"equilibrium economics":

> "... the notion of equilibrium to which I refer is that of
> the general economic equilibrium originally formulated by
> Walras, and developed, with ever-increasing elegance,
> exactness, and logical precision by the mathematical
> economists of our own generation..." (p. 1237).

This clarification would appear to restrict sharply the

application of his criticism to what is now commonly referred

to as neoclassical general equilibrium theory in its specific

version of Arrow and Debreu. However, as the argument proceeds,

it becomes clear that he means to bring into the same domain of

"equilibrium economics" a much wider range of economic theory.

For, only a few pages later, in seeking "to pinpoint the

critical area where economic theory went astray" he asserts:

"To locate the source of error with more precision, I would
put it in the middle of the fourth chapter of Vol. I of the
Wealth of Nations ... [where] Smith suddenly gets fascinated
by the distinction between money price, real price and
exchange value, and from then on, hey presto, his interest
gets bogged down in the question of how values and prices
for products and factors are determined. One can trace a
more or less continuous development of price theory from the
subsequent chapters of Smith through Ricardo, Walras,
Marshall, right up to Debreu..." (pp. 1240-41).

It is evident from this assertion, and from the subsequent

discussion, that he means to subject to the same line of

criticism both the neoclassical theory and the theory of the

Classical Economists (chiefly Smith and Ricardo) for what he

sees as common elements in the basic structure of both sets of

theory at the level of their respective theory of value.

Kaldor is returning in this paper to a theme that he had

broached many years before, as far back as in 1934 (see Kaldor

1934a, 1934b). Indeed, the 1972 paper could be considered an

extension and updating of the 1934 papers, in the light of

certain principles emergent from Keynesian economics, to deal

more explicitly with the implications of the problem of increasing returns for "equilibrium economics". The issues are further considered in his Okun Memorial Lectures (Kaldor, 1985).

These papers, taken together, address fundamental questions regarding the logic and explanatory significance of equilibrium analysis as Kaldor sees it. In this connection, the earliest paper (1934a) distinguishes between two sets of issues, though the distinction between them is rather blurred and remains so even in present-day practices of economic theorists. One is the issue of <u>stability</u> of equilibrium. The other is what Kaldor calls "determinateness of equilibrium". In some respects, this latter may be interpreted to conform to what is nowadays commonly discussed as the <u>existence</u> and <u>uniqueness</u> of equilibrium. On both of these issues, Kaldor's arguments reveal a great deal of skepticism concerning the explanatory significance of an economic analysis constructed in terms of the notion of equilibrium for understanding empirical and historical phenomena. 1/

Of course, so far as Kaldor's work as a whole is concerned, it is worth noting that he himself was not always consistent as regards the practice of "equilibrium economics". For instance, he sought to construct a theory of growth of capitalist economies which presumed the existence, uniqueness, and stability of steady-state growth with full employment. After many efforts in this direction, he was not able to show how this growth process could operate under plausible conditions of

capitalist markets and investment behaviour. Given his
expressed view of equilibrium analysis, one may find in this
work a striking paradox.

Nevertheless, the arguments presented in these papers are
still very fresh today. They are certainly worth considering in
the context of more recent developments on which they have a
direct bearing. This is what I propose to do here. In
particular, I wish to consider, in the light of the specific
criticisms of equilibrium analysis presented by Kaldor, what
advances have been made in developing the structure of classical
theory. For this purpose, I focus on the analytical structure
of classical theory that has emerged from modern efforts
(beginning with Sraffa, 1960) to revive and develop that theory,
as it relates to the formation of competitive value. In this
context, the appropriate and relevant equilibrium concept to
consider is that of "long period equilibrium". It is
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".

The arguments of Garegnani (1976) serve forcefully to remind
us that this particular notion of equilibrium constitutes a
method of analysis that is common to both the neoclassical and
classical traditions of theory, whatever may be the differences
between them as regards their respective theory of value. Of
course, if this is granted, there are important questions begged

as to how one is to separate the theory from the method and what is the real distinction that would then remain between classical and neoclassical theory. Kaldor, on his part, evidently takes for granted that there is "a more or less continuous development" between the two sets of theories (1972, p. 1241). Others may find this position objectionable. I do not directly address these questions in this paper.

2. The Problem of Stability

For a long time, detailed analytic treatment of the problem of stability of equilibrium took place mostly within the framework of the static Walrasian theory of general equilibrium, focussing on the idea of price adjustment through _tatonnement_. This idea is now generally recognized to be an extremely artificial construction of the process of price formation in real-world markets (Hahn, 1982). More recent efforts to advance alternative conceptions of the adjustment process in a variety of settings within the framework of neoclassical theory have shown that, under general conditions, the adjustment process may give rise to complex forms of motion that are unstable and equilibrium itself may be indeterminate (cf. Benhabib and Nishimura, 1985; Boldrin and Montrucchio, 1986; Cass and Shell, 1983; Grandmont, 1985). These results leave in doubt, even in the judgement of some of the practitioners, the explanatory significance of the specific neoclassical constructions of an equilibrium system.

On the other hand, analytic treatment of the stability problem in the context of the classical theory has lagged far behind this work. Recently, however, some strides have been made in the direction of a more structured treatment of this problem. It has turned out that a strong instability result can also be found within a range of parameter values for adjustment processes that are thought to be representative of the classical conception. This is so for even the simplest analytic construction of a Ricardian process of convergence to the stationary state in a highly aggregative setting (see Bhaduri and Harris, 1987). It is so, moreover, in more complex and disaggregated settings (see, for instance, Semmler, 1986).

What, then, is one to conclude from these instability results about the explanatory significance of the specific classical concept of gravitation to a stationary state or long period equilibrium with prices of production?

Certainly, as a matter of the logic of the concept of long-period equilibrium as a stationary position, the demonstration of stability is a necessary step in the argument if this concept is to be at all logically sustainable. Therefore, these results, on instability of the classical competitive process, have to be squarely faced. At the same time, it must be recognized how limited and primitive these constructions are. Generally, what this analysis has achieved so far, in my judgement, is to induce initial efforts to put forward an explicit conception of the behavioural and structural properties

that are thought to characterize the workings of real economies
in motion. In this respect, these efforts constitute, at best,
only tentative steps towards constructing a dynamic conception
of the economic process.

Insofar as they are based on simulation results, the
generality of the results is questionable. Where analytic
solutions have been derived, the parameter values demarcating
stable and unstable regions are, in many cases, not susceptible
to any economically meaningful interpretation. Most
importantly, the specification of economic behaviour and
institutional structure is seriously lacking in the very
elements that are relevant to evaluating the dynamic behaviour
of real-world economies. These missing links relate, for
example, to labor market interactions, production changes
associated with both technology and organisation, active (as
distinct from passive) price-quantity interventions by firms,
the role of financial variables and, last but not least, the
formation of expectations. This list represents a tall order of
items which, to be all included, would no doubt make the
analysis unwieldy and unmanageable. Therefore, perhaps a step-
by-step procedure is warranted. But without a specific
treatment of these complicating factors, it would seem premature
to make a final judgement of stability or instability as a
general rule.

One complication, which has attracted some attention, is
worth mentioning here because of the deeper issues underlying

it.   The problem (first pointed out by Steedman, 1984) is this.
When market prices differ from production prices, the direction
of the price deviation need not be the same as the direction of
the profit deviation. They could very well differ. This is for
a reason essentially related to the interdependent structure of
the economy: if there is a deviation in one industry this sends
ripples across the whole structure of prices of other industries
which necessarily feed back into the costs and profits of that
same industry, so that profits may move in a direction opposite
to the price deviation. If there are deviations in other
industries as well, the problem is compounded, making it
impossible to say a priori what is the relation of profit
deviations to price deviations.

It is well to see this lack of correspondence between price
and profit deviations as a logical possibility. But the
important question is: why is this necessarily troubling for the
theory? Here, a deeper inquiry is needed into the rules which
are supposed to guide economic behaviour in the theory. This
result is troubling for the theory if it is assumed that firms
are guided in their investment and output decisions by a
specific norm that serves as a benchmark from which to judge the
existing market situation. This is the norm of prices of
production and the profit rate associated with those prices,
assuming it is unique. This norm runs into difficulties because
it 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 those very same prices of production and corresponding profit rate.

It may be noted here that this norm is a very special one. It follows from the more general principle that firms are guided by the goal of increasing profits. But it is not the only norm that would follow from this principle - there are conceivably many others. This particular norm has the special significance that it is chosen so as to be fully consistent with the presumption that there does exist a unique set of prices of production and corresponding profit rate. The problem then is not simply whether there exists some other norm that is admissible on some arbitrary criterion (cf. Steedman, 1984, p.135, n. 20).

The problem is, first, that this particular norm, even though it so closely fits the criterion of consistency with prices of production, will clearly not do as a generally acceptable one. The fact is that this norm, when combined with a similar norm for output decisions on the quantity side, can be shown logically not to lead generally to the establishment of prices of production. Under certain conditions, such price-quantity interactions can definitely lead to instability. This is the damaging result that has come out of work done to date on stability analysis of the gravitation process, in which these norms are taken as the guiding principle in the adjustment process (see Semmler, 1986, and other works cited there).

Secondly, the problem is whether there exists <u>any</u>

<u>economically meaningful behavioural rule at all</u>, no matter how

simple or complex, consistent with the goal of increasing

profits, that would cause market prices to converge to

production prices, when this particular norm that is so

congenial to production prices will not do as a rule that is

generally capable of achieving that result. What is at issue,

therefore, is whether the idea of a convergence of market prices

to production prices is sustainable under <u>any</u> economically

meaningful description of capitalist behaviour as regards

decisions on prices, output, and investment. It remains to be

shown that it is so and what that description is. At this

point, that must be considered to be an open question.

All of the above-mentioned results are interesting and

useful. They also address matters that are central to the

internal logic of the classical theory. However, there is a

deeper issue which goes beyond anything that is touched by the

results discussed so far but which, when it is fully grasped,

also allows us to put those results into proper perspective. So

far as I can tell, this issue was first clearly and sharply

posed by Kaldor in the following terms that are worth quoting in

full:

"It is not possible, therefore, to determine the position of
equilibrium from a given system of data, since every
successive step taken in order to reach equilibrium will
alter the conditions of equilibrium (the set of prices
capable of bringing it about) and thus change the final

position - unless the conditions are such that either (1) an
equilibrium system of prices will be established
immediately, or (2) the set of prices actually established
leaves the conditions of equilibrium unaffected (in which
case the final position will be independent of the route
followed)" (Kaldor, 1960, p. 16).

What Kaldor proposes here, and elsewhere (for instance in 1960,

p. 45, and in 1972) is that there is a general problem of path

dependence affecting dynamic economic processes, attributable to

the effects of learning from experience (among other causal

factors). This problem has not been widely recognized until

recently (see, for example, Arthur, 1988; Arthur, et al., 1987).

Once it is recognized, however, then the question of convergence

to a predetermined equilibrium position necessarily becomes

problematical, unless resort is had to "very rigid assumptions"

satisfying the conditions indicated in the quoted passage (such

as in the Edgeworth-Walras theory of competition where the

assumption of "recontracting" is crucial, as Kaldor shows).

The issue then is this. The norm of behaviour that is

specifically oriented to knowing in advance the vector of

equilibrium prices is one which satisfies Kaldor's first

condition, insofar as it conforms to the idea of "full

experience", and it also satisfies his second condition. Yet,

despite this happy coincidence, that norm is clearly not

sufficient to guide the economy to the equilibrium position

associated with it. This indicates that Kaldor's conditions

while necessary are not sufficient. The problem still remains,

then, whether there is any behavioural rule that will do, in the

sense of being both necessary and sufficient. But, in addition,
what we can now see, and this is the deeper point, is the
following. Any such rule that is based on a norm related to the
equilibrium position must necessarily rule out features of
actual economic behaviour insofar as such behaviour entails path
dependence.

This point 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. In other words, this is
a case where, to quote Kaldor again,

> "... the postulate of the existence of such `laws` [here, the
> presumption of `economic equilibrium`] is refuted if they
> can be logically shown to be valid only under assumptions
> that are contrary to observed phenomena." (1972, p. 1245).

3. Relative Speeds of Adjustment

There are many other considerations, essentially of an
empirical nature, that may be introduced to question the
efficacy of the mechanism of adjustment to the classical long-
period equilibrium of production prices. These relate, for
instance, to various forms of "barriers to entry" and, hence, to
the question of the degree of mobility of both capital and
labor. Such "barriers" are known to occur in practice and, more
significantly, can be shown to derive from intrinsic features of
production, the innovation process, financial markets, labor
markets, and the formation of demand (the literature on this is

discussed in Scherer, 1980). Some would infer from these considerations that the case for presuming convergence to a uniform profit rate rests on very weak empirical foundations.

Against this line of criticism, it could be argued that existence of such barriers is only a cause of slowing down or inhibiting the speed of the adjustment mechanism and not a cause of permanently obstructing or negating its effectivity. The process of convergence to a uniform profit rate may still be presumed to occur, if only as a tendency which is never actually realized, as long as there are reasonable grounds for assuming that some of the crucial requirements for its operation are present.

However, there can be no cause for comfort in this latter position. This is because of another and potentially damaging complication. This is another point at which Kaldor's conditions for equilibrium turn out to have much relevance and meaning for the classical theory. I refer here to his condition concerning the "velocities of adjustment" (1960, pp. 31-33). It is a matter of the relative speeds of adjustment. Specifically, in the context of the classical theory, it is a matter of whether the speed of adjustment to the supposed centre of gravity is fast enough relative to that of the changes brought about by forces which determine that centre of gravity so that, for instance, any set of production conditions can be said to be "dominant" in a meaningful sense (for instance, in terms of the proportion of total output it produces). In particular, what if

the speed of adjustment to long period equilibrium is so slow relative to ongoing changes of a permanent nature in the structure and technology of production that those changes continually regenerate divergences in profit rates before any gravitational effect can occur? Under these circumstances, the very idea of a long period equilibrium position would become quite meaningless or irrelevant. To sustain the relevance of this position, therefore, it must be assumed that the pace of technological change is itself very slow or, viewed the other way round, that the rate of diffusion of new techniques is very fast relative to the rate of introduction of new techniques.

For this purpose, it could be assumed that technological change comes in discrete spurts that are widely separated in time so that, in the interim, the economy is sufficiently able to adjust to and absorb the existing "dominant technique". This is often supposed to be the form of technological change in the case of so-called "major" innovations. Indeed, this is the case on which part of the Schumpeterian analysis of capitalist dynamics rested (Schumpeter, 1954). But available research on the actual historical character of technological innovations does not support this view (see, for instance, Jewkes, Sawers, and Stillerman, 1968; Kamien and Schwartz, 1982; Sahal, 1981; Schmookler, 1966; Stoneman, 1983).

Available research suggests that the rate of diffusion of new technologies is actually quite slow on average. Furthermore, innovations appear "major" only ex post, that is, from the

standpoint of looking back at their cumulative effects (this

does not deny the existence of technological discontinuities or

so-called "radical innovations"). In actuality, they emerge as

continual "minor" improvements that may eventually displace

previously existing practices (regardless of whether they are

viewed as product or process innovations, a distinction which is

in any case difficult to sustain in practice). They achieve

apparent stability and "dominance" only after a long-drawn out

process of such incremental improvements, in the course of which

improvements in processes and products on the input side

originating in other industries are continually disturbing the

apparent tendency to a fixed shape and form of the product or

process in a given industry.

Going deeper into the process of technical change, it is

evident from what we now know about the actual character of this

process, that the classical construction of a long period

position is seriously at risk (see, for instance, Arthur, 1988;

Dosi, 1984; Dosi, et al., 1987; Nelson and Winter, 1982; Sahal,

1981). What is significant for present purposes is, first of

all, that there are costly "search" procedures involved in the

process, such procedures being themselves an integral part of

the investment decision so that they cannot meaningfully be

separated off as an independent and autonomous factor.

Secondly, the process is known to entail powerful learning

effects from experience in both "doing" and "using", so that the

economic advantages accruing therefrom are dependent on

previously accumulated experience. Thirdly, there is a "lock-
in" effect along any trajectory of development of technology
such as not only to entail significant advantages for those
producers that are ahead (whether they be firms, sectors,
regions, or national economies) but also, and at the same time,
make it costly to change over to other trajectories.

All of this adds up to the recognition that technological
change is a path-dependent process in which current economic
performance is at every moment crucially dependent on past
performance. To this extent, the outcomes of the process, in
terms of the set of techniques observed, cannot be meaningfully
conceived to be independent of the path pursued. This result
calls into question the basic idea involved in the classical
analysis of convergence to a predetermined equilibrium position
defined by a known technique.

The idea of "increasing returns" is a common and familiar
reference point for discussion of these effects (as, for
instance, in Kaldor, 1972), but the point to be emphasized here
is that there is a pervasive and general process underlying this
idea.

4. Whither, Then, Goes Classical Theory?

So far as study of the problem of stability is concerned, one
of the more constructive and potentially fruitful lines of
development, in my judgement, is that introduced by Goodwin and
his followers (see Goodwin, 1982, and accompanying papers;

Goodwin and Punzo, 1987). It has the distinct advantage that it identifies from the outset a general principle, namely, that a possible source of instability lies in the existence of non-linearities associated with cumulative feedback effects in the economic process. One such non-linearity analyzed by Goodwin is the interplay between the "reserve army" of unemployed labor and the investment behaviour of capitalists, an idea which in his own words led him back to Marx's formulation of capitalist dynamics (Goodwin, 1982). There may be other such non-linearities, and these are worth exploring. Surely, the buildup of such analysis can provide a more solid analytical basis for understanding the workings of real economies and for organising study of concrete historical processes.

The high level of aggregation of the analysis that has been done so far along this line is in some respects a serious limitation, but there is a possibly wider significance that might be conjectured, coming from that very aggregative feature. In particular, if there are real macroeconomic balancing conditions and boundaries that the economy must satisfy (isn't the structure of class relations one of these?), then aggregative models can still tell us something about the sources of instability no matter what the level of disaggregation to which the analysis is subsequently taken. Furthermore, even if the classical convergence to prices of production held at the microeconomic level, problems of instability might still arise at the macroeconomic level. To this extent, aggregative models

may be said to be intrinsically of interest in the study of stability of the competitive process.

Another potentially fruitful line of approach is that of evolutionary models (Iwai, 1984a, 1984b; Nelson and Winter, 1982). They bring into full play a biological analogy (as does the Goodwin model) in contrast with the typical mechanical analogy that has all along been at the heart of economic method. Their emphasis on selection mechanisms and the reproduction of diversity has the potential of generating useful insights into the economic processes by which differentiation of performance, and hence of profit rates, is reproduced. Within this line of analysis, one might conjecture another possible advantage that is the opposite of that stated above for aggregative models, arising in this case from the intrinsically microeconomic level of analysis of evolutionary models. In particular, might it not be the case that some elements of system stability are generated from processes of selection, adaptation, and reproduction of diversity at the microeconomic level?

By combining, in some way not yet clear, the insights obtained from these two lines of analysis it may yet be possible to provide a deeper and more far-reaching answer to the question of stability/instability of the competitive process in capitalist economies.

A larger, more difficult, analytically intractable, and yet more interesting question, concerns the conditions under which one might be able to conceive of a process which builds up

through cumulative movements in one direction, as distinct from maintained oscillations, to a point of structural break or discontinuity. This problem, which might be called a "regime change", takes us far beyond what has been done so far or what can be handled analytically with methods now being used. However, the formal demonstration of the general possibility of chaotic dynamic paths may well be pointing to the need for grappling with this problem. And is this not, after all, the "grand theme" that really concerned classical thought, especially that of Marx, regarding the process of internal change within capitalism?

A potential danger comes from seeking to collapse history into the classical model of prices of production. Robinson insistently warned of this danger in her recurrent complaint that the long-period equilibrium method is a way of turning history on its head (hence the title of her essay "history versus equilibrium", 1978, ch.12; see also 1980, passim). Her point, as I interpret it, is simply this. If all past history had been one of equilibrium, then one may infer that any perturbation which occurs here and now would set into operation forces that cause the perturbation to cancel itself out and bring about a return to equilibrium. The economic system would then be self-correcting, at least for small perturbations. It is quite another thing, however, if history has never been anywhere near equilibrium. It would be illegitimate then to claim that, starting from today, there will come into play a

process of getting to equilibrium. The system could, and would

likely, wander off into the unknown without ever achieving

equilibrium.

A mathematician would correctly reply that, from the

standpoint of an abstract analysis of stability, these two cases

are not qualitatively different. But, for the social theorist

and historian, there is a world of difference between them.

Specifically, the difference is that, in the one case, the

properties of equilibrium have already been learned in history

and can confidently be expected to persist. In the other case,

there can be no necessary presumption that a real process of

learning, which is in general a path dependent process, will

lead to an equilibrium, if any exists and whether it is unique

or not. The difference may be represented formally in terms of

the following two dynamic processes:

Case 1: $\qquad \dot{x} = F_{x_e}(x), \qquad x_e = $ equilibrium point

Case 2: $\qquad \dot{x} = F_{x_0}(x), \qquad x_0 = $ initial condition

Case 1 is an "equilibrium process" in which the function

governing the adjustment process is uniquely defined in terms of

the equilibrium solution x_e that is known "in advance" and

invariant to the starting point. Case 2 is a "path dependent

process" in which the function governing movement along any path is uniquely dependent on the initial condition or state variable x_0 and (for full generality) may be considered to shift as experience builds up along a given path. As long as one recognizes the intrinsic and general characteristic of the social process as one of learning on the basis of accumulated experience, as in Case 2, then the presumption of an adjustment to a predetermined equilibrium, whether unique or not, cannot generally be sustained.

FOOTNOTES

* The argument of this paper is drawn from a larger project of the author, reported in Harris (1988).

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1. At the opposite pole to Kaldor stands Samuelson who unabashedly claims to be an "equilibrium theorist" following in what he sees as the tradition of the classical economists and Harriet Martineau: "Remember that the classical economists were fatalists (a synonym for 'believers in equilibrium'!). Harriet Martineau, who made fairy tales out of economics (unlike modern economists who make economics out of fairy tales), believed that if the state redivided income each morning, by night the rich would again be sleeping in their comfortable beds and the poor under the bridges. (I think she thought this a cogent argument against egalitarian taxes.) Now, Paul Samuelson, aged 20 a hundred years later, was not Harriet Martineau or even David Ricardo; but as an equilibrium theorist he naturally tended to think of models in which things settle down to a unique position independently of initial conditions" (1968, p. 12).

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