Economic Policy and Endogenous Cycles

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Source: *Journal of Post Keynesian Economics*, Summer, 1983, Vol. 5, No. 4 (Summer, 1983), pp. 635-647

Published by: Taylor & Francis, Ltd.

Stable URL: https://www.jstor.org/stable/4537777

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I

Economists use long-run equilibrium models as the basis for understanding the real world and for providing policy suggestions to political leaders. The aspect which interests us in this paper is the use by economists of the concept of the long-run steady state equilibrium, which is a state which can maintain itself unchanged through many time periods.[1]

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[1] In the literature of economics, the word "equilibrium" is used in at least two, very different, senses: (a) A short-run, or market, equilibrium, meaning a balance between supply and demand in a particular market, or in an interconnected set of markets, at a given time; and (b) A long-run equilibrium, meaning a state of the economy which can maintain itself through time indefinitely, or at least through a long time interval. In this paper, we are concerned *only* with this second type (b). Thus, investigations of stability of type (a) equilibrium, for example, in the book by Arrow and Hahn (1971), have nothing to do with the present discussion. We shall assume throughout that short-run (market) equilibrium is attained at any given time, i.e., that the system is stable in the sense of Arrow and Hahn. Such a system may still be unstable in the second, long-run, sense.

Following upon a comment by a referee, we wish to emphasize that the distinction between short-run and long-run equilibrium explains one of the many differences between our work and the idea of Lucas (1972) regarding "equilibrium prices and quantities [which] will be characterized mathematically as *functions* defined on the space of possible states of the economy." The word "equilibrium" is used in the short-run sense by Lucas, hence this is one reason that his approach has no relationship to what we are concerned with here. Lucas gives no rationale of any sort for the extremely far-fetched and implausible economic assumptions on which he bases his mathematical exercise in the abstract theory of optimal control. We are most eager to avoid any confusion between what Lucas does and what we do.

In order to use this concept for policy discussions, one must accept that this long-run equilibrium state is, in some sense, a reasonable indicator of what happens in the actual, nonequilibrium conditions; that is, the long-run equilibrium state is taken to be the state which the system approaches "naturally" if it is left undisturbed. Fluctuations away from this state are believed to have a tendency to disappear as time elapses. If this is true, then the equilibrium state is indeed a fair indicator of what is being approached in actuality.

Of course, econometric models must, and do, say something about the behavior of the economy being modeled when the economy is not precisely in long-run equilibrium. Thus, in principle at least, economic policy discussions could be carried out entirely in terms of such models, without relying on equilibrium concepts such as the multiplier. This is not practical, however, for several reasons: (1) The econometric models have a *very* poor track record when it comes to predicting the future course of economic events, and are therefore highly suspect as a basis for economic policy making. (2) Most econometric models are constructed on the basis of some theoretical conceptions about equilibrium, with deviations from that equilibrium assumed to be tolerably small; thus the models are themselves tied to long-run equilibrium ideas. (3) All realistic econometric models are so big that it is difficult, if not impossible, to get an intuitive understanding of what lies behind the numbers churned out by the computer. Yet, it is such an intuitive understanding, not mere numbers, which is absolutely necessary for discussions of economic policy. This intuitive understanding comes from simple theoretical concepts like the multiplier analysis, not from models with hundreds, or thousands, of estimated structural equations. Thus, theories are necessary, and these theories are usually, if not invariably, expressed in terms of the state of long-run economic equilibrium.

All this is well-known. But the following assumptions, which are implicit in these statements, are all too often taken for granted:

1. A long-run equilibrium state of the economy exists.
2. This state is approached by the economic system if the system remains unperturbed, starting from any reasonable initial state; that is, the long-run equilibrium state represents a *stable* equilibrium.

3. Deviations from this long-run equilibrium state exist, but are small enough so that the study of the equilibrium state itself (for example, the theory of the multiplier effect) gives meaningful information of great use for economic policy-making.

At first sight, these assumptions seem quite innocuous. However, let us now consider the well-known accelerator-multiplier model of Samuelson (1939*a*, 1939*b*). In its simplest form (see, for example, Rau, 1974) this model can be represented by the equations:

(1a)	$Y(t) = C(t) + I(t)$	Income equation
(1b)	$C(t) = a + mY(t-1)$	Consumption demand
(1c)	$I(t) = I_0 + v[Y(T-1) - Y(t-2)]$	Investment demand.

Samuelson's mathematical analysis (1939*a*, 1939*b*) shows that a state of long-run equilibrium exists, with equilibrium values:

(2a)	$\bar{Y} = (a + I_0)/(1 - m)$	Long-run equilibrium income
(2b)	$\bar{C} = (a + mI_0)/(1 - m)$	Long-run equilibrium consumption
(2c)	$\bar{I} = I_0$	Long-run equilibrium investment.

A more complete analysis of the time-dependent system (1) shows that long-run stability is tied to the value of the accelerator constant v. If and only if $0 < v < 1$, we have long-run stability, i.e., the cycle is dampened and the long-run equilibrium values (2) are actually approached by the system as time goes on. Conversely, if v exceeds unity, the equilibrium is *unstable* (explosive).

Samuelson's (1939*a*, 1939*b*) essential result is that a trade cycle develops "naturally" from any small initial disturbance. To get this, he *had* to assume long-run instability, i.e., $v > 1$. But if one lets Samuelson's equations proceed, the oscillations grow beyond all bounds, eventually leading to completely absurd results such as negative values of $Y(t)$. There are two distinct ways out of this dilemma:

1. Retain $v > 1$, so oscillations *start* "naturally," but allow for "ceilings" and "floors" to *limit* these oscillations (Hicks, 1950).

2. Insist on long-run stability (i.e., take $v < 1$), thereby abandoning the Samuelson explanation. The observed trade cycle is then attributed to "random shocks" (Frisch, 1933).

These two types of model are not only distinct, but directly contradictory in the sense that they cannot both be applicable. Either the long-run equilibrium state is locally stable, or it is locally unstable.[2] The Hicks model denies local stability, the Frisch model depends upon local stability.

Conventional wisdom has settled upon the Frisch alternative: The system is taken to be stable in the sense that the long-run equilibrium state is approached in the absence of shocks, and trade cycle oscillations are attributed to these shocks.

This conventional wisdom is, however, empirically inaccurate. Blatt (1980) demonstrated that no model based upon the ideas of Frisch can possibly fit well-known observations on trade cycles. Actual cycles take many years for the upswing, whereas the downswings are very rapid. *No* model of the Frisch type can possibly yield this result, hence all such models are excluded by the data.

Willy-nilly, we are therefore forced back to models of the Hicks type,[3] though of course not necessarily the particular model developed by Hicks (1950). *The trade cycle in the real world must be thought of as being endogenous to the system, not merely the result of accidental shocks.* An endogenous trade cycle is, of course, exactly what the most astute observers have always taken for granted (for example, see the descriptions of the cycle by Mitchell, 1913, and by Lavington, 1922).

Accepting the existence of endogenous trade cycles leads to interesting consequences for economic policy making. These are trivial from a mathematical point of view, but very far from trivial in an economic context.[4]

In sum, economic systems can be divided into two broad classes, according to whether:

1. They possess a long-run equilibrium state which is *stable* (damped). Such systems do not possess an endogenous trade cycle, and for such systems conclusions based on the long-run equilibrium state give fair approximations to their mathematical behavior but are inconsistent with empirical evidence.

2. They possess a long-run equilibrium state which is *unstable* (explosive). Such systems can*not* be described validly by linear

[2] *Both* types of model are stable "globally" in the sense that all economic quantities always stay within finite bounds. "Locally" stable refers to stability of the equilibrium state, only.

[3] In Blatt (1978) we have applied conventional econometric analysis to artificial data generated by a (locally unstable) Hicks model; this econometric analysis "proved" that the equilibrium is stable, a patent absurdity. Thus, the conventional method of analysis has been shown to be logically invalid.

[4] It is *not* the purpose of this paper to assert the triviality: "Static conceptions give faulty conclusions for dynamic questions." True enough, but that is not our point.

models, since linear models are incapable of keeping the oscillations within bounds. These systems perform endogenous trade cycle oscillations, but do not have any tendency to approach the long-run equilibrium state; rather, they keep oscillating around that state. We assert that for such systems, reasoning based on the long-run equilibrium state gives conclusions which are not only wrong in detail, but are *qualitatively wrong*. We further assert (but refer to earlier papers, Blatt, 1978, 1980, for the proof) that the actual system under which we live is of this second type.

If we live in a world of endogenous cycles, then long-run equilibrium values need not be of any use whatever for economic policy making. The long-run equilibrium state is *not* an actual state of the economy, and is *not* the state to which the economy would tend if left undisturbed. There is therfore no a priori reason why economic quantities appropriate to that state (i.e., the long-run equilibrium values) should have any relevance to what actually happens.

The second line of defense of those favoring long-run equilibrium analysis is: Even if it is true that the long-run equilibrium is unstable, and the system actually performs an endogenous cycle, nevertheless the average values of observed economic quantities over that cycle are equal to the long-run equilibrium values. (If not precisely equal, they are at least rather similar, similar enough for purposes of economic policy discussion.)

To anyone familiar with the mathematical theory of endogenous cycles in a nonlinear system (they are called "limit cycles" by the mathematicians and are well-known in various engineering fields) this seems a forlorn hope. It is the purpose of this paper to demonstrate that the long-run equilibrium values are *not* close to averages of actual values taken over the limit cycle (trade cycle). Moreover, the equilibrium values are particularly bad for discussions of economic policy, in that the sensitivities to changes in policy parameters are misrepresented by equilibrium values even worse than the actual values.[5]

[5] In order to avoid confusion, let us dispose of a minor point here: There exist systems with *stable* long-run equilibrium (no endogenous cycle) for which it is nonetheless true that equilibrium values differ slightly from long-term time average values. This can happen when the basic system equations are nonlinear and "shocks" are sufficiently large to disturb the system appreciably from its long-run equilibrium state, often enough to show up in the time average. Such systems are *not* our concern here, since they are of Frisch type and thus excluded by the data.

II

The existence of an endogenous cycle which makes equilibrium analysis inapplicable can be demonstrated in terms of an extremely simple mathematical model of an imaginary economy. (Arguments based on real data can be found in Blatt, 1980.) The simple model used in this paper bears a very close family resemblance to models which are often used in discussions of economic policy for trade cycles. Discrepancies between long-run equilibrium values and average values over the cycle for this type of model are not far-fetched, out-of-the-way possibilities of only purely logical interest for policy making. Similar discrepancies may be expected to occur for more realistic models.[6] Consequently, long-term time averages cannot be approximated validly by equilibrium values in an economy undergoing an endogenous trade cycle.

Our model is taken from Rau (1974), and is his simplified didactic version of the theory of Hicks (1950). The model equations differ from the Samuelson equations (1) through: (a) A ceiling on income called Y_c, and (b) a floor on the demand for investment called I_f (a slightly negative value, presumably, indicating the most rapid rate of net disinvestment due to physical depreciation when there is zero gross investment). The equations are:

$$Y(t) = \min\left[Y_c, C(t) + I(t)\right] \tag{3a}$$

$$C(t) = a + mY(t-1) \tag{3b}$$

$$I(t) = \max\left[I_f,\ I_0 + v\left\{Y(t-1) - Y(t-2)\right\}\right]. \tag{3c}$$

In these equations, $C(t)$ and $I(t)$ are the *demands* for consumption and investment, respectively; $Y(t)$ is *realized* national income. If and when the ceiling on income, Y_c, comes into play, then one or both of realized consumption and investment must fall below their demanded values. The simplified model does not state how the shortfall is distributed between consumption and investment, and we need not answer this question for the purposes of this paper.

The constants which appear in this model are limited by the following conditions:

[6] Note, however, that many models used conventionally have a *stable* long-run equilibrium and do *not* give rise to an endogenous cycle. In such models, equilibrium values are excellent approximations to time averages; but all such models are in conflict with basic observations on actual trade cycles (see Blatt, 1980). Such models are therefore *not* more realistic, but less so.

(4) $a > 0 \quad 0 < m < 1 \quad I_f < I_0 \quad v > 1 \quad Y_c > (a + I_0)/(1 - m)$,

i.e., consumption demand is always positive, the marginal propensity to consume lies between zero and one, the floor on investment demand is less than the autonomous investment level, while $v > 1$ is necessary to produce an endogenous cycle. (For $v < 1$, the system has a stable long-run equilibrium, no cycle.) The final condition ensures that the ceiling level of income lies above the long-run equilibrium level (2a). The constants we shall choose satisfy all these conditions.[7]

Let us now consider the nonequilibrium time development of this model economy for a particular choice of parameters and a particular choice of the initial state.[8] Our chosen parameters are:

(5) $a = 0.2 \quad m = 0.86 \quad I_f = 0 \quad I_0 = 0.3 \quad v = 2.0 \quad Y_c = 4.0$,

and our chosen initial values are:

(6) $$Y(1) = 4.0 \qquad Y(2) = 4.0.$$

That is, our economy starts right at the ceiling income, in two successive periods.

The subsequent time development is shown in Table 1.

After $t = 17$, the behavior repeats exactly. The simple model thus yields a perfect endogenous trade cycle of length fifteen time units.[9]

Since the model gives a recurrent cycle, we obtain the long-term time average of any economic quantity by averaging this quantity over one complete cycle.[10]

[7] The long-run equilibrium values are obtained by assuming $Y(t) = \overline{Y}$ (a constant value) for all times t, and similarly for $C(t) = \overline{C}$ and $I(t) = \overline{I}$. The results are exactly the same as for the Samuelson (1939) model and are given by equations (2). We note, for later reference, that the ceiling Y_c, the floor I_f, and the acceleration constant v have no influence of any sort on these long-run equilibrium values, provided that the conditions (4) are satisfied.

[8] The details of the time development depend on our choice of parameters, naturally. But the essential point (the existence of an endogenous trade cycle) depends on only one parameter, the acceleration constant v. The model yields an endogenous trade cycle whenever v exceeds unity.

[9] Note that the sum $C(t) + I(t)$ of demands exceeds realized income $Y(t)$ in periods 16 and 17, when the ceiling is effective. Thus some of these demands cannot be met at this point of the cycle.

[10] For example, the long-term time average income is obtained by adding the incomes in Table 1 for one cycle (say, for $t = 3, 4, 5, \ldots, 16, 17$) and dividing the sum by 15, the length of the cycle. This yields $45.085/15 = 3.0057$, which we have rounded to 3.01 in equation (7).

Table 1

Time t	Income (realized) $Y(t)$	Consumption (demand) $D(t)$	Investment (demand) $I(t)$
1	4.000		
2	4.000		
3	3.940	3.640	0.300
4	3.768	3.588	0.180
5	3.441	3.441	0.000
6	3.159	3.159	0.000
7	2.917	2.917	0.000
8	2.708	2.708	0.000
9	2.529	2.529	0.000
10	2.375	2.375	0.000
11	2.243	2.243	0.000
12	2.164	2.129	0.035
13	2.203	2.061	0.142
14	2.472	2.094	0.378
15	3.166	2.326	0.839
16	4.000	2.922	1.687
17	4.000	3.640	1.969

The long-term time average values of Y, C, and I are:

(7) $(Y)_{ave} = 3.01 \quad (C)_{ave} = 2.78 \quad (I)_{ave} = 0.37.$

These long-term time averages must be compared with the long-run equilibrium values obtained from formulas (2) with the constants (5). These are:

(8) $\bar{Y} = 3.57 \quad \bar{C} = 3.27 \quad \bar{I} = 0.30.$

These equilibrium values *differ* from the average values (7): $\bar{Y}$ is 19 percent too high, $\bar{C}$ is 18 percent too high, and $\bar{I}$ is 19 percent too low. These large discrepancies establish our first main point, that long-run equilibrium values are *not* representative of true long-term time averages.[11]

[11] Econometricians may query the significance of these discrepancies, and ask for statistical tests of significance. However, if they do so, they are wrong. The basic model is deterministic, not stochastic. There are no random shock terms in equations (3). There is therefore no basis whatever for applying mathematical statistics to this model. This includes statistical tests of signifi-

III

Although the numerical values (7) of the long-term time averages differ from the equilibrium values (8), some people are likely to contend that economists are not all that much interested in precise numerical values from a model. Rather, they want to: (1) predict future trends in the absence of government interference, and (2) estimate the likely effects of intentional changes in policy instruments. It is conceivable that equilibrium values, though not perfectly accurate numerically, do indicate the right trends, and can therefore be used validly to predict the long-term response of the economy to various policy measures.

We shall think of the six parameters of our model (that is: a, m, I_0, I_f, ν, and Y_c) as our possible "policy instruments." Let us therefore investigate how both the time-average values and the equilibrium values depend upon these parameters, in the neighborhood of our chosen values (5). In Table 2 we list partial derivatives of the time-average values, and of the equilibrium values, with respect to these parameters.

The top left-hand entry in this table, the number 7.14, is the value of the partial derivative of the equilibrium income $\bar{Y}$, with respect to the model parameter a which appears in the consumption equation (3b).[12] The table entry just below that, the number 5.32, represents the partial derivative of the long-term time average income, $(Y)_{ave}$, with respect to this same parameter a. This value was derived from a simple computer calculation in which a was varied slightly. We see that income and consumption are, in their time average values, somewhat less sensitive to the constant term in the consumption equation than one would have thought by looking at the equilibrium values. But the really striking difference appears in investment demand. The equilibrium analysis predicts that investment is completely insensitive to a (look at equation (2c)); whereas the time average investment demand goes down strongly as this parameter increases (i.e., the derivative is negative, -0.64).

cance. From a more commonsense, elementary point of view, discrepancies as large as those between (7) and (8) clearly cannot be ignored, and that is all we need to assert here.

[12] This entry can be checked directly: The formula for $\bar{Y}$ is equation (2a), and its derivative with respect to the parameter a is just the Keynesian multiplier $1/(1 - m) = 1/(1 - 0.86) = 7.14$.

Table 2

Sensitivities

Derivative with respect to		QUANTITY INCOME (REALIZED)	CONSUMPTION (DEMAND)	INVESTMENT (DEMAND)
a	EQU	7.14	7.14	0.00
	AVE	5.32	5.58	−0.64
m	EQU	25.53	25.53	0.00
	AVE	13.15	14.31	−1.99
I_0	EQU	7.14	6.14	1.00
	AVE	3.08	2.65	0.46
I_f	EQU	0.00	0.00	0.00
	AVE	2.25	1.94	−0.10
v	EQU	0.00	0.00	0.00
	AVE	−0.22	−0.19	0.11
Y_c	EQU	0.00	0.00	0.00
	AVE	0.26	0.23	0.09

The next series of derivatives, with respect to the marginal propensity to consume, m, shows that equilibrium analysis badly overestimates the effectiveness of this policy instrument. First of all, the derivatives of time-average income and of consumption demand are only about half as big as estimated from equilibrium; and second, the equilibrium analysis fails to show that there is a strongly *negative* influence on time-average investment demand, and thereby (in a more realistic world than the one of our model) on the true growth rate. "Encouraging consumption" may *not* have purely positive effects! This was well-known to Adam Smith: "Every prodigal appears to be a public enemy, and every frugal man a public benefactor" (1776, p. 324).

The derivatives with respect to I_0, the next series, are in the right direction, but equilibrium analysis overestimates them very badly. A policy of encouraging increases in the *equilibrium* value I_0 of investment does not perform nearly as well as equilibrium theory would suggest. Nor is this at all surprising when one looks at Table 1 again. The values of investment demand in this economy range from a low of $I_f = 0$ (achieved in seven separate time periods) to a high of 1.969 (just once each cycle, at the peak of the boom). The long-term time average of this enormous variation, $(I)_{ave} = 0.37$, bears very little resemblance to actual values of $I(t)$

in any one time period. No wonder equilibrium analysis fails particularly badly for such a variable.

Furthermore, equilibrium analysis completely ignores actually existing influences of I_f, ν, and Y_c, since none of these parameters enters the equilibrium calculation at all. Returning to Table 2, we see that the acceleration factor ν and the ceiling income Y_c have small effects, but the floor level of investment I_f appears to be an *excellent* policy instrument: Time average income and consumption go up significantly with I_f. There is a negative effect of I_f on time average investment demand, but this derivative is so small (-0.10) that we may ignore it altogether. It appears that a government policy of putting a lower floor on the level of investment (for example, through investment by the government itself, or strong investment incentives *during the trough* of the cycle) should be *very* effective in the economic world of this model.

Not only does equilibrium analysis fail to tell us that the investment floor is an excellent policy instrument; but also, equilibrium analysis concentrates our attention on equilibrium (or conceivably time-average) values, to the well-nigh complete exclusion of some very important other considerations. The severity of the cycle (its amplitude)[13] is obviously a most important factor, and any policy instrument which mitigates that severity is well worth using. But conventional theory denies the very existence of an endogenous cycle when it assumes a damped cycle. The observed trade cycle

[13] We shall measure the *amplitude* A of the cycle by the difference between the highest and the lowest national income within one complete cycle. This amplitude depends, obviously and directly, on the values of I_f and Y_c, and may be expected to depend on the value of ν also. In the cycle of Table 1, the highest income is 4.000 and the lowest is 2.164 (at time $t = 12$), hence the amplitude is $A = 4.000 - 2.164 = 1.836$, roughly half of the equilibrium income $\bar{Y} = 3.571$. The cycle is *not* a minor effect in this model (as it is not in reality, either).

Although equilibrium analysis of the model tells us nothing about the amplitude A of the cycle, the complete model equations do of course determine A, and these equations can be employed to derive a useful approximation to the cycle amplitude. This derivation is available from the author. Its result for the cycle amplitude is:

$$A \equiv Y_c - Y_f \cong Y_c - (a + I_f)/(1 - m) - m^2(I_0 - I_f)/\left[\nu(1 - m)\right].$$

For our parameter values (5), this formula gives $A = 1.78$, reasonably close to the computer value 1.84 (they differ by less than one part in thirty). Thus, it is by no means impossible to arrive at a useful expression for this important quantity, provided one starts from an endogenous cycle, rather than a cycle caused by some unexplained "shocks."

fluctuations are "explained" as merely the effects of "random shocks," and these shocks themselves are not explained at all. As a result, the standard approach leaves policy makers up in the air about what (if anything) can be done to improve economic behavior over the trade cycle! The "cause" of the trade cycle is said to be random shocks, and the orthodox "theory" can make no statement whatever about what can be done to decrease the size and/or frequency of these famous random shocks.

Our discussion has been based on a simple model economy, so that our detailed conclusions have been demonstrated logically only for that model, not for any actual economy. The Hicks model is *not* perfect by any means; it predicts a fast rise and a slow drop during the trade cycle, the exact opposite of what is observed; this is probably due to an analysis in "real" terms which pays insufficient attention to confidence and expectations, affecting investment. However, in spite of these shortcomings, there are enough similarities to actual economies, and (even more so) to the models used by economists to describe actual economies, to make our conclusions much more than just playing with mathematics.

Our main conclusion (inability of equilibrium analysis to provide adequate policy indications for an economy with an endogenous trade cycle) does not depend at all on whether our simple model is representative of the actual economy: *If* equilibrium analysis is good enough for policy advice in general, *then* it must be good enough in this model, also. It not only is *not* good enough, but is actually dreadfully bad for time averages and completely useless for the cycle amplitude! There is every reason to believe that exactly the same is true when equilibrium analysis is applied to actual economies with an endogenous trade cycle.[14]

From either this formula, or the computer results, one may confirm that raising the floor level I_f of investment is an excellent policy instrument also for decreasing the amplitude of the cycle, as of course one should expect on intuitive grounds. Also good is raising the floor level of consumption, a. Raising the propensity to consume, m, is good for the cycle amplitude and for the time-average income, but (as pointed out before) is bad for time-average investment demand.

[14] We emphasize that our point has no relation at all to disputes between different schools of thought in conventional economics, for example, between monetarists and Keynesians: *both* use equilibrium analysis in their work.

Summary

Equilibrium analysis is quite unreliable for economies going through endogenous cycles. This analysis is particularly misleading for policy purposes, since sensitivities of long-term averages to policy parameters are misrepresented much worse than the values themselves. Furthermore, equilibrium analysis is entirely powerless to understand, or to ensure effective policies for mitigating the severity of, the cycle of such a system.

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