

**METHODOLOGICAL (AND SUBSTANTIVE) PROBLEMS
IN BUILDING A CYCLICAL MODEL ***

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Even if one does not subscribe to Schumpeter's vision one cannot negate that bad and good times (depression and prosperity) alternate in economic history since at least the first part of the XIX century. It is not necessary at this point to give a precise definition of economic cycles. There are several preliminary points that need to be discussed to clarify and sharpen my viewpoint and in doing so I will throw some light also on the definition problem.

1. The first question is how to measure cycles or -which is the same thing- which indicators should be used to describe economic fluctuations. It is now common to rely on real indicators such as real total output or unemployment rate. In the old days when such indicators were not available (see Maddison 1982, app.A and 1987) nominal magnitudes were used instead, such as the general level of prices.

Up to fifteen years ago however the two kind of indicators had been broadly in agreement with each other, rising (falling) prices being associated with rising (falling) output. It may be tempting to presume that even in the old days real output had moved in accordance with the general level of prices. This presumption is indeed corroborated by other indicators either of qualitative or of quantitative nature (the latter being limited to local or industry level). Should the two diverge considerably real indicators are preferred. It is the whole economy as measured by total real output (and/or by the rate of unemployment) and its oscillations that it is the object of the analysis. However the last period with its new, opposite relationship between the general level of prices and real output can suggest theoretical developments. (For an example in a long cycle context see Di Matteo 1986).

2. Another preliminary question concerns the possibility of explaining the ups and downs we observe in the time series. Are they purely random phenomena or the expression of economic mechanisms at work ? To this -I am afraid- there is no clear and unconditional answer. The

starting point in modern cycle theory is Slutckij's brilliant paper (1937) published ten years earlier in Russian. In it the idea that fairly regular oscillations can be the result of averaging random numbers is pursued rigorously for the first time.

If this (sufficient) argument were to be accepted in its integrity that would be the end of the story and the search for any theory would become unwarranted. The lesson which can be derived from Slutckij's provocative essay is a negative one, namely that manipulations of time series (detrending and the like) are liable to 'create' cycles or to alter them quite substantially and so they have to be performed with great care. (For such an analysis see e.g. Kendall-Stuart-Ord 1983).

To be fair a random 'explanation' is no explanation at all and I cannot resist the idea that the behaviour of the economic agents or classes really matter (though sometimes in a unforeseen manner) for understanding cycles. This way of looking at time series is in contrast with the one accepted by those economists who think that prediction is the only criterion for building a model.⁽¹⁾

In shaping my attitude the study of individual cycles with the guide of economic historians proved essential. Moreover in this way one can also meet the objection that each cycle is a unique event with its own peculiar features that makes the search for a unifying theory a hopeless task. Whereas one cannot deny that cycles may be very different from one another, it is nevertheless true that, at least for 'limited' historical periods, cycles present themselves with a sufficient degree of uniformity to deserve a common theoretical explanation.

Accordingly the assistance we get from history is twofold. First it supplements our theoretical framework by suggesting specific elements that are (necessarily) considered exogenous in the theoretical model or represented as constant parameters. In a theoretical or econometric model both exogenous variable and constant parameters are (to a large extent) inevitable, though we know perfectly well that many

(1) I am not criticizing Friedman's approach but the one followed by those who put their faith in Arima models. For a balanced view see the considerations by Hendry (1980) and on a philosophical ground by Toulmin (1963) who draws a distinction between explanation and prediction. Darwin's theory being explanatory but not predictive is a case in point.

determinants, sometimes of a qualitative nature, do affect each dependent variable. We often use aggregate model since it is a well established regularity that all sectors move together within the business cycle. However, as has been authoritatively recognized long ago, there are industries that lead the cycle and industries that are led by the cycle. An historical analysis can considerably enrich our understanding of a cycle by performing a disaggregated analysis. (2) Secondly I will argue that as result of longer term (economic and extra economic) changes we need different theoretical schemata for selected group of cycles. As far as trade cycles are concerned for example historical analysis is needed because the existence of such a longer term movement shaping the economic environment can be conveniently studied also through historical analysis. It seems to me more important to develop this side of the explanation rather than the opposite one, namely the precise analysis of short term sequences of events.

3. Having said that I have not disposed of the topic since economists who reject the sufficient argument put forward by Slutskij still differ greatly as to the role of random elements in an economic model which aims at explaining persistent fluctuations. For some of them like Frisch (1933) and Kalecki (1954) random elements are the *condicio sine qua non* of a theory whereas for others like Goodwin (1951) and Hicks (1950) the persistence of oscillations has to be the result of the working of the model itself.

Within this dichotomy I side with the Goodwin-Hicks approach. The main reason is that again to resort to random elements is to give up an explanation of a very important feature of the cyclical process, namely its persistence. Indeed in the model by Kalecki (1954) we witness that two deterministic cyclical models with quite a different degree of stability, when completed with random shocks (normally distributed), become more or less indistinguishable from each other. It is now clear what I mean by a random 'explanation' of cycles persistence and why I find it unsatisfactory.

(2) This may seem obvious but it is not shared not only by those economists mentioned in the previous footnote but also by those who are not ready to take into account that economics is dealing with a system moving in historical time and where social and cultural factors do contribute in shaping some economic relations and parameters.

It is expedient at this point to precise the concept of random shock. I will define random shocks those events that are known not to be systematically related to the phenomenon in question: in this case the hypothesis of random shocks with a normal distribution seems appropriate. A case in point is the influence of the variety of tastes on aggregate consumption, as remarked long ago by Haavelmo (1944) in his pioneering work. If one accepts this definition, to depend on them for 'explanation' means to beg the question. It is worth quoting what Haberler (1937,p.167) wrote long ago:

"The mysterious thing about them is that they cannot be accounted for by such 'external' causes as bad harvests due to weather conditions diseases general strikes lock-outs earthquakes the sudden obstruction of international trade channels and the like."

Sometimes the concept of random shock is different i.e. it simply captures all the elements that we know have some influence on the independent variable though we are not sure about the precise nature of the relation in question. In other words random shocks are taken as a measure of our ignorance (see e.g. Chow 1975) and in this case a normal distribution of the random shock and its being serially uncorrelated are rather dubious assumptions.

It is therefore my opinion that one should opt for models that do not rely on stochastic elements for the explanation of persistent cycles. In general therefore we need non-linear models.(3) Many of them will have unstable equilibria which in turn requires either a floor and a ceiling (e.g.Goodwin 1951) or an endogenous change in the parameters (e.g.Kaldor 1940). There are of course exceptions: linear models can show persistent fluctuations (e.g.Frisch 1952) and non linear models do not possess unstable equilibria only (e.g.Goodwin 1967). (4)

In reducing the role played by stochastic elements one should not overlook two things. First owing to the existence of uncertainty random shocks should always be taken into account and in addition to other mechanisms (see e.g.Goodwin 1946) they can make the regular cycle

(3) It is only in this case that one can have limit cycles, the most important example of self sustained oscillation.

(4) In linear models self perpetuating oscillations can occur only for special values of the parameters that produce purely imaginary roots of the auxiliary equation.

produced by the model a bit irregular and therefore akin to the actual behaviour of the economic system.(5)

Secondly the presence of random shocks will have wildly different effects according to the nature of the model that has been hit by them. As we have already remarked, in a model with damped oscillations (and under suitable assumptions), they can keep the cycle alive. When the model is dynamically unstable they clearly set the model in motion. When the model is structurally unstable a random shock that affects a parameter can change in a dramatic way the quality of the dynamics showed by the model itself.

4. I am now led to touch on the concept of structural stability. Let me first dwell a little bit on the case of dynamic (asymptotic) stability. In this case the problem is whether the system, once displaced from the equilibrium point, tends to reach it again as time goes to infinity. In other words something has interfered with the equilibrium and as a result the system is off the equilibrium path. The situation is as if a new set of initial conditions were established. The economic system however is unchanged in its structural relations.

On the contrary in the case of structural stability we ask whether the system, once something has interfered with it, still maintains its dynamic features qualitatively intact. In other words we suppose that the system has changed, i.e. one (or more than one) of its parameters has changed as a result of the disturbance. The latter can also be of a nature different from that of a random shock, i.e. of an 'internal'

(5) This view seems to be superseded by the recent discovery of certain dynamical deterministic non linear equations that produce apparently stochastic movements (so called chaotic trajectories). This result however is restricted to a subset of parameters and to a particular value of the initial conditions. In the case of continuous time moreover we need at least a third order differential equation. (See e.g. Guckenheimer-Holmes 1983). The important aspect in my opinion of these developments lies essentially in the attack they launch against the essentiality of random elements in business fluctuations. The irregularity therefore can well be explained by random shocks but we can no longer exclude the possibility that it is the expression of chaotic movements. This is a difficulty in interpreting economic data and has been found to be a problem in other sciences as well. Indeed tests are now being proposed to try to detect whether empirical data are consistent with one or the other hypothesis. In economics see the pioneering but extremely difficult work by Brock (1986).

nature due to the dynamic working of the system itself. For example the consequences of an unstable growth path can lead economic agents to revise their behaviour and to induce a change in the propensity to save (see Harrod 1936).

If there is no equivalence (according to some definition) between the qualitative behaviour of the system before and after the perturbation (however small), then the said system is structurally unstable. The concept is very intricate and it is now realized that there is no accepted, unique definition of it. What can be said is that a model possesses a degree of structural instability according to the definition of equivalence and to the set of admissible perturbations. (For a full discussion of the definitions of structural stability and its implications see Vercelli 1982,1986 and for an example of a taxonomy of degrees of structural instability applicable to economics see Vercelli 1985).

A good case of structurally unstable system (and of the relativity of the definition) is Goodwin's celebrated growth cycle model. The latter is indeed structurally stable for some perturbations and unstable for others (see Cugno-Jade-Montrucchio 1979, Di Matteo 1982,1984).

5. In passing I have remarked that dynamically unstable systems can be useful (and are indeed used) for cycle analysis.(6) Can the same be true for structurally unstable systems? The answer by most scientists and economists is no, for a host of reasons. The first is that structurally unstable systems are not generic which is considered to

(6) This runs against the widespread belief that dynamically unstable systems should be discarded. Since reality is not unstable they would be a biased description of reality. There are many historical situations where the assumption of instability seems to be more at home than the opposite one (witness the German hyperinflation in the early twenties). Secondly stability has many definitions from the mathematical point of view. Following the pioneering work by Samuelson there is a tendency to identify stability with asymptotic stability which is the most stringent definition. There are occasions where a more relaxed definition would be appropriate. When we use equilibrium models we suppose that the system is always not too far from equilibrium and therefore we are entitled to use equilibrium concepts as a fairly good approximation. In this case the weakest definition of stability that of Poincare'-Poisson seems appropriate. In loose terms the latter states that although the system may not exactly repeat itself in general it will return to the vicinity of its initial state and nearly repeat its motion during an interval of time. (Cfr.Birkhoff-Lewis 1935)

be an important and useful property of the model (for a definition and extensive use of generic properties see Goodwin-Punzo 1987).

Structurally unstable systems can be used neither for prediction nor for observational purposes. They cannot be used for the latter purpose since a structurally unstable system expresses a form that is deemed to change. At the same time the prediction of a certain variable cannot be given any weight since it can be radically altered when we are moved in a small neighborhood of the value of the parameter.

Possible answers to this arguments may run as follows. First it is not possible to show that structural stability is a generic property for systems in dimensions higher than two.

Secondly I quote the following clear statement by Vercelli (1986,p.21) which I agree with:

"We believe that s(tructural)-stability is neither necessary nor sufficient condition for either observability or predictability. It is not a sufficient condition of observability as clarified by the classical discussion of observability of the behaviour of dynamical systems offered by System Theory (see e.g. Marro 1979). It is not a necessary condition of observability because 'it is an everyday experience that many common phenomena are [structurally] unstable' (Thom 1975,p.126) and because many models successfully employed in science are (...) s(tructurally) unstable. It is not a sufficient condition for predictability because the topological notion of s(tructural) stability cannot rule out the possibility that 'the perturbed system may have a complete different structure from the original system after sufficient time has passed' (Thom 1975,p.26). (...) It is not a necessary condition for predictability because e.g. for the sake of forecasting the results of a stabilization process we have to analyze first the characteristics of the s(tructural) unstable system which undergoes the stabilization process."

A further objection which - I think- has not been answered by Vercelli is the following. Due to measurement errors we can never be pretty sure that a coefficient (or a variable) has precisely that value. In a structurally unstable system this may have qualitative effects. This is connected with the first objection. Since in two dimensions structural stability is a property common to almost all systems (i.e. a generic property) this means that if we have a structurally unstable system this is surrounded so to speak by structurally stable systems as soon as the value of some parameters alters via measurement.

This is a serious objection in the case for example of the Goodwin model already referred to. Nevertheless if one is pretty sure that, e.g. in Goodwin's model, the rate of growth of each variable does not depend on the absolute value of the variable itself, then we can exclude a class of admissible perturbations.

There is a further point to be discussed. Vercelli (1986,p.17) states that the obvious condition for rejecting the methodological prescription of employing only structural stable systems lies in the assumed stability of the real economies. He argues that is far from being the case (see also Vercelli 1985).

However the concept of structural instability implies an abrupt change in the qualitative performance of the system following a certain class of admissible perturbations. This concept requires more than what is considered to be a structural change that affects the economy.

I have always thought that a structural change occurs in an economy when the structure of it is no longer a constant, i.e. when the behavioural parameters or the technical relations of the system change. This is clearly a much weaker notion of structural change than the one implied by the concept of structural instability. It is clear that not every change in the above relations entails a qualitative change in the dynamics of the system.

Therefore the argument by Vercelli has to be qualified unless we are prepared to identify structural change with structural instability. This seems arbitrary since the concept of structural change has been developed independently of the mathematical concept.

For example I think that the Goodwin's model can be defended as a useful, rough picture of the working of the major forces in a capitalistic economy over a very long period. It expresses very nicely that the capitalistic system is perpetually between Scylla and Charybdis though in various ways it manages- on average- to escape both.(See again Vercelli 1982,appendix). Goodwin's model can be a useful picture, though certainly a rough one, of the background against which single cycles happen to exist.

Therefore I do not think one should reject a model only because it is structurally unstable. Incidentally the approach of structural stability seems to be of a different nature from bifurcation analysis (though they could be treated in a unified formal framework). In the latter

every admissible value of the parameter (irrespective of the nature of the cause that changed it) is considered admissible in order to see how and when the structure of the system changes. It is an analysis that does not correlate the model with the empirical world but it remains at the level of the description of the properties of the model. This is not the case with the concept of structural stability which has been precisely proposed for a correct empirical use of the models. (See Andronov-Vitt-Khaikin 1966).

We can summarize by saying that random shocks affect the initial positions when we study dynamical stability and the value of the parameter(s) when studying structural stability. Whereas this is perfectly satisfactory from the theoretical point of view it is less so from the practical point of view. How can one judge whether that random shock is affecting the initial position of the system or one of its parameters? This is crucial for example in the much quoted Goodwin's model since a change in initial position has the effect of simply shifting the system to another closed orbit, the qualitative picture remaining unaltered.

At this point one has to reconsider briefly what are the possible settings within which we perform dynamical stability analysis. A change in initial conditions is a sufficient condition for the study of the dynamical stability of a system but is by no means necessary. Other sufficient conditions are variations in the exogenous variables (that according to my definition should not be confused with random shocks) and changes in the parameters of the system.

The latter changes are therefore a source of changes that are amenable to be studied both in the dynamical stability and in the structural stability settings. To see this one can compare a random shock to a change in investments in a simple Keynesian framework. We know that the latter changes both aggregate demand and the stock of capital goods, but we can separate the two effects by a suitable choice of models, the short run and the long run.

6. There is another problem which is of interest and it concerns whether it is appropriate to study dynamic stability without explicitly building disequilibrium models. On one hand we have economists who reject any model that does not allow for disequilibrium, on the other

many model builders totally overlook the problem. In principle the former position seems correct. However the procedure of explicitly considering disequilibrium leads to an increase in the order of the systems with growing mathematical difficulties in a non linear framework. Therefore at least for local analysis one can admit that equilibrium relations are used in place of the more correct procedure. Moreover there is at this point a further thing to note. Whether one should formulate a model in continuous or in discrete time is a considerably difficult question and no definite answer has been found to the dilemma (for an analysis of the pros and cons see Gandolfo 1981). In discrete time models we can easily choose the length of the lag in such a way that the above objection can (at least partly) overcome.

To see this think of Harrod growth model. One can define the lag as that period in time during which the multiplier process is completed so that short run equilibrium is always achieved. This offers a flexibility which is not open to models in continuous time though may be not a satisfactory way out in every situation.

7. Until now in my examples I made no reference to the works of the new classical school. I do not think that this is a major gap since the methodological problems treated here present themselves also in the works of this school. A qualification ought to be made at the outset, namely what I mean by new classical school. This is relevant because there are significant differences among neoclassical economists at least as far as trade cycle is concerned. I will include mainly the works of the representatives of the real business cycles (Long and Plosser, Kydland and Prescott) and will say something about Lucas and Sargent.

It is not correct to refer to the whole lot of these authors with the term rational expectations school since there are many models where the latter hypothesis is inserted and that produce wildly different results from those arrived at by the above quoted economists. (See *infra*). The discussion we have had up to now can throw some light on how can we interpret the results obtained by the real business model approach.

Sargent (1979 chapt.xi) himself argues that (linear) models need random shocks to produce fluctuations. He does not mention non linear models as a possible solution to the problem of persistent cycles and does not give any justification for his choice of linear models. He only states that in a non stochastic model perfect prediction would be possible and that this implication is not accepted by many economists as a reasonable one.

While this objection might be acceptable in a linear framework but not in a non linear one (see footnote 5) it does not apply to the large part of non-linear models plus random shocks. Furthermore even without random shocks perfect prediction requires no changes in the parameters and/or in exogenous variables. Finally as Blatt (1978) has shown econometric practice may not be able to distinguish between non linear and linear models. He generates observations from a non linear model (Hicks's model) and then estimates by normal econometric procedures the coefficients of the linear model which are found to be perfectly acceptable!

Be that as it may, the actual choice by Sargent and others is to build a large linear model since in this way it is possible to produce simulated patterns that resemble that of time series. Suppose we have a first order linear system of difference equations (see e.g. the model by Long-Plosser 1983) where there is a lag between demand and production. The production side is represented by a multisectoral model with constant returns to scale.

Such a deterministic model can be decomposed (according to the analysis by Hirsch-Smale 1974) into two subsystems, the first one having all the equations that produce real eigenvalues, the other by all couples of equations that have complex eigenvalues. In this way if there are many complex eigenvalues which give rise to different periodicities we can have an overall pattern for the system that looks like that of empirical time series.

In any case we need random shocks to keep the movement alive as in any simple linear model, though we could dispense with random shocks for taking into account the irregular pattern. As far as the celebrated Lucas's model (1975) is concerned the reader is referred to the appendix where it is shown that if we make all the assumptions made by Lucas his (deterministic) model is asymptotically unstable. Implicit

there is a peculiar concept of dynamics at least in comparison to Samuelson's classic definition. What Lucas and Sargent have really in mind is comparative dynamics (see Sargent 1979, introduction), i.e. alternative paths of endogenous variables associated to alternative paths in exogenous variables.

It is interesting to look at the way the new classical school sees random shocks. In the model by Long and Plosser (1983) the random shock has clearly the effect of changing one of the parameters of the production function. In their model next period output is no longer determined solely by the usual coefficients of labour and capital but also by a random element which is a constituent element of the production function (Long-Plosser reinterpret it as a sort of technological progress and indeed it appears a multiplicative factor in the production function).

In this way output has not varied for a change in initial conditions but because of a change in a parameter that also means a change in the constant(s) of integration and therefore it is as if the initial condition changed. But the latter aspect is not stressed by the new classical school which concentrates really on the exercise of comparative dynamics taking for granted that the equilibrium is stable.

8. There is finally a group of theorists who took 'seriously' the rational expectations revolution but simply to show by means of an ingenious enlargement of the domain of the hypothesis that the insertion in a model of rational expectations is not sufficient by itself to rule out the desirability and effectiveness of economic policy. I am referring to the so-called sunspot theories of fluctuations as represented, e.g. by Grandmont, Cass and Shell.

The starting point is a feature of most rational expectations model. It can be shown under quite general conditions (see Woodford 1987) that rational expectations equilibrium is undeterminate. Contrary to the new classical approach, let us suppose that there are random shocks that affect neither tastes nor technology and endowments, but that are believed by everybody to affect the economy all the same. Each agent in the economy believes that this variable (that can take on different values with different probabilities) is perfectly correlated with equilibrium prices and quantities according to some specified relation.

Now if this belief is self-fulfilling then we have an equilibrium (a so-called sunspot equilibrium) with many states one for each actual, characterization of the sunspot variable.⁷

What happens to day may well be undetermined -the argument runs- since it depends on what happens to-morrow and this in turn depends on what will happen the day after to-morrow in an infinite regress (unless the horizon of the economy is arbitrarily fixed at some point in the future). This result has been proven mainly in the context of match box size general equilibrium models, such as overlapping generations models. (See Woodford 1987 for a discussion of how general is the demonstration).

As has been remarked this class of models shows that in theory there is no objectively selected elements on which agents should base their expectations. Indeed the equilibrium is conditional on the (class of) beliefs of the agents. The problem of how this belief in the strict correlation between the sunspots and the equilibrium is formed has been eluded in the literature (for a thorough and deep review of the general problem of expectations formation in many of its difficult aspects see Bacharach 1986).

This formal argument has been given an economic interpretation purporting to show its usefulness for cycle theory. It has been indeed proposed as an analytically rigorous reformulation of some old fashioned theories of the trade cycle such as those by Pigou, Lavington and Keynes himself. If every agent having observed these random events expects that the economy will fluctuate this will be the case.

The novelty of the approach lies precisely in the orthodox neoclassical framework in which the argument is recast. In this way charges that cycles explained by psychological factors were based on irrational behaviour appear to be now unwarranted. And rational expectations are

(7) This state of affairs has been sometimes labelled extrinsic uncertainty as opposed to intrinsic uncertainty, the latter being represented by (true) sunspots a la Jevons that, by affecting output in agriculture, creates a stochastic and (hence) uncertain level of product. This terminology can be misleading since in neither case are we talking about uncertainty as defined by Knight long ago and still widely accepted. In both sunspots cases we have random variables with a known probability distribution. (One can think in the Jevons case that information about the weather is available upon which probabilities are drawn).

neither sufficient (as just shown) nor necessary (as it was the case with old macroeconomic models) to rule out fluctuations.

By far more controversial is the relevance of this class of models to account for actual trade cycles. In a paper by Grandmont (1986) cycles of period two are proved. This means that the cycle has a period of roughly fifty years if we assume that the economic lifetime of the representative agent is fifty years split into two periods.

Finally let me state some observations. First of all the whole result depends crucially on everybody sharing the same belief. How does all that come about? This has not been answered satisfactorily by the proponents of the sunspot approach at least for the time being. By the way an analogous problem is in Lucas's theory unless we are prepared to admit that the previous periods have been rather similar to one another. But even in this case we do not know a lot on how to model learning processes and whether they converge to rational expectations. It is clear in which sense some of the proponents describe this approach as one providing a stochastic endogenous theory of fluctuations. Random shocks are exogenous and are such as to modify expectations which being rational are endogenous. However without the continuance of random shocks no fluctuation could ever be brought about.

Another major difference with the new classical school is the following. In the latter approach there is no room for an economic policy aimed at reducing fluctuations since the latter are the way in which the minute by minute optimizing agents absorb random shocks. In the sunspots approach on the contrary fluctuations are merely the result of self fulfilling expectations that attach a random character to the final outcome (without any fluctuations in the fundamental features of the economy) and that therefore could be reduced by an appropriate economic policy. (See e.g. Grandmont 1986).

All in all the main result of this school of thought is a theoretical justification of the use of a welfare enhancing countercyclical economic policy. The general final remark is that it is a partial theory stressing the role that expectations about to-morrow do play in what happens to-day without necessarily excluding other factors in the explanation of cycles such as changes in the fundamentals.

9. Finally a word by way of conclusion. My position is that persistent cycles should be modelled in such way that random shocks are nothing but accessory elements that give to the deterministic behaviour that irregular character observable in empirical time series. There is a good example of what I have in mind in those few papers that add random shocks to a non linear model of the cycle, that of Kaldor. (See Kosobud-O'Neill 1972 where there is a detailed analysis which shows *inter alia* that the variance of the stochastic model is finite). This approach seems to me in line with the approach in other branches of economic theory and with common sense as I tried to emphasize in the previous pages.

What I said may well reflect a Schumpeterian vision where cycles are the essence of the capitalistic process and their persistence a sign of the latter's life. On the other hand it is not inconsistent with a post-Keynesian approach which stresses internal reasons for capitalistic instability. It might well be inconsistent with the (now) old fashioned monetarist approach that sees the government policy as the main reason for fluctuations.

Paradoxically it is not completely in contrast with the so-called real business approach that stresses again intrinsic, real causes of the fluctuations, though in the (mistaken) form of random shock. This may depend on the setting within which the analysis is developed. Equilibrium is found with respect to data which are tastes and technology. The simplest way to alter them is probably via random shocks that have the property (when appropriately restricted) to leave more or less unaltered the general structure of the model without introducing so-called ad hoc hypotheses.

Again in this approach basic themes of the old fashioned trade cycle analysis come to the fore such as the possibility of integrating growth theory and cycle theory (as in the work of Prescott 1986)!

Needless to say I feel myself much more in agreement with the first two approaches. Nevertheless a critical attention to the new classical school seems worth giving especially in order to clarify which differences are differences in the questions and in the methodology and which are differences in the answers.

APPENDIX

The purpose of the appendix is simply to prove that if we adhere to the hypotheses expressed by Lucas (1975) his (deterministic) model is dynamically unstable. The latter can be represented by two linear difference equations:

$$(1+bz)K(t+1) - dK(t) - cP(t+1) + cP(t) = a + bv$$

$fzK(t+1) + qK(t) - gP(t+1) + (1+g)P(t) = -e + fv + M_0(1+\tau)^t + \tau$, where K is the log of capital, P the log of prices, M_0 is the log of the quantity of money at time 0 and τ is the exogenous rate of growth of the money supply. For the meaning of the parameters one is referred to the first part of Lucas (1975). We can easily compute the auxiliary equation associated to the homogenous system (which is first order but not in normal form) to check whether its roots are less than one in absolute value. The auxiliary equation is the following:

$$[-g - zu]\mu^2 + [dg + cq + bz + g + zu + 1]\mu - d - dg - cq = 0$$

where $u \equiv bg - cf$ is greater than zero (See Lucas 1975, p.1117).

After some manipulations one of the (necessary and sufficient) stability conditions simplifies to $d > (1 + bz)$. According to Lucas (1975,p.1117) $d < 1$. Therefore the model is dynamically unstable.

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