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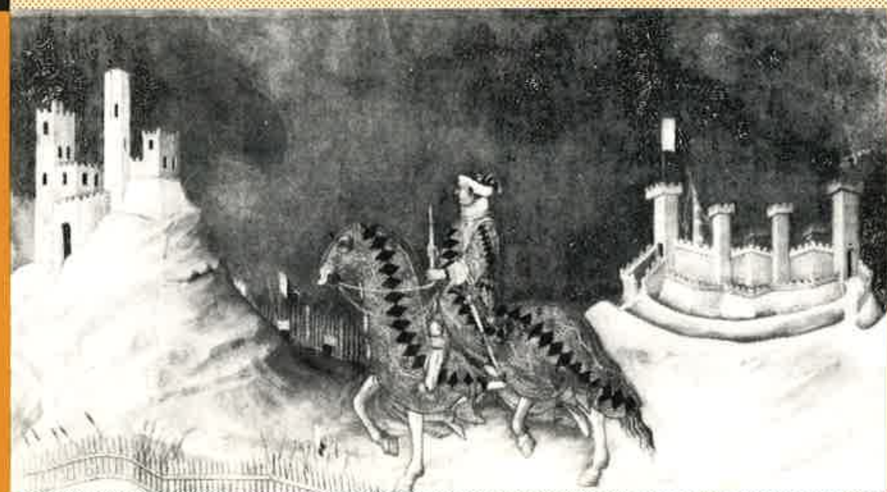
UNIVERSITA' DEGLI STUDI DI SIENA

**QUADERNI DEL DIPARTIMENTO DI ECONOMIA POLITICA**

Lionello F. Punzo

# J.-M. GRANDMONT ON "TEMPORARY EQUILIBRIUM: MONEY, EXPECTATIONS AND DYNAMICS." SOME COMMENTS.



# *QUADERNI DEL DIPARTIMENTO DI ECONOMIA POLITICA*

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Lionello F. Punzo

# J.-M. GRANDMONT ON "TEMPORARY EQUILIBRIUM: MONEY, EXPECTATIONS AND DYNAMICS." SOME COMMENTS

Siena, ottobre 1990

Forthcoming in the volume: Value and Capital, fifty years later, Proceedings of the International Economic Association Conference held in Bologna, September 1988; edited by L.W. McKenzie and S. Zamagni, (MacMillan, London). Reasearch project financed by the Ministry of Education with a grant from "Fondi 60%".

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J.-M. GRANDMONT ON "TEMPORARY EQUILIBRIUM: MONEY, EXPECTATIONS AND DYNAMICS". SOME COMMENTS.

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The theme of Grandmont's paper has a clear bearing on the general objective of this conference, which I take to be an attempt at appraisal and overall evaluation of the work originally inspired by <u>Value and Capital</u> (V&C). It is very accurate on technical aspects but I would rather not engage in a discussion about details. Actually, it prompted me to re-read some of the classic Hicksian papers (as well as V&C). I was struck by the comparison with the more recent literature reviewed in the paper, because at first sight it shows a fairly radical change in perspective. I have come to formulate a few questions, which I will present at the end together with some general remarks.

As recalled by Grandmont,on one hand V&C led historically, via Patinkin, to the elaboration of the so-called Neoclassical Synthesis; on the other, it inspired an extensive literature on the Microfoundations of aggregative Macroeconomics. On both these subsequent developments, J. Hicks has often expressed a certain uneasiness, however by now his work enjoys a life of its own, independent of his desires or "real intentions".

1. Grandmont's paper essentially offers a review of research work carried out on the three sets of issues which appear on the research agenda of the Temporary Equilibrium Approach (TEA) already outlined in his 1977 paper. Some of this work is still actively pursued, so that the report is quite lively and at times

tentative. This increases its interest.

The first item on the agenda prescribes the analysis of (the sufficient conditions for) the existence of a monetary temporary equilibrium given the fundamental characteristics of the agents and the institutional setup, and the study of the properties of such equilibria under fairly general assumptions. Given the sequential character of TE analysis, whereby the equilibrium at each point of time depends on agents' beliefs about the values the relevant variables might take in the future, a great deal of work has been devoted to the scrutiny of those assumptions that reflect the way in which information is processed (learning) and expectations are formulated. This is a conceptual issue upon which the attention of macroeconomists has concentrated in recent years. The paper can also usefully be read as a criticism of the RE modelling strategy from the point of view of an alternative, and somewhat competing, research strategy that began earlier and then developed in parallel.

Grandmont reminds us how the discussion on the V&C framework, extended further by Patinkin, did lead in the early seventies to the understanding of the "true" conditions for the existence of a monetary competitive equilibrium. Here again economics has shown its remarkable way of producing rabbits out of a hat, (to paraphrase J. Hicks). The conditions turned out to be logically more restrictive and empirically more difficult to justify than had generally been thought, and the very explanatory capability of the Hicks-Patinkin model was open to question. All this is fairly well known by now, it is a piece of history brilliantly discussed by the author elsewhere, (Grandmont



(1983)). It is however interesting to notice that TEA developed out of its ancestors by the same process as general equilibrium analysis of Walrasian descent: for both, the early phase saw a concern with the logical foundations of the model itself, before anything useful could be done with it. (Or at least so it was felt.)

2. Similarly in TEA we have recently witnessed a move towards more typically dynamical questions. This is largely ongoing research on the next two items listed on the TE research agenda, the first being the analysis of the existence and uniqueness (and of the properties) of certain long run "steady states" of an economy in which agents' expectations are realized. Here, the indirect reference to the Rational Expectations (RE) literature is apparent. TEA becomes a tool for the criticism of this approach (especially the business cycle theory of Robert E. Lucas Jr.). This line of research within TEA has produced remarkably interesting pieces of analysis, and more are expected to come. One such result (and appparently the strongest) tells us that under seemingly general assumptions, a plausible model can be constructed whereby, in an insulated (shock-free) and perfectly known economic environment in which fundamentals do not change, fluctuations of economic activity may be the outcome of the agents' optimizing behavior. Thus, contrary to REH beliefs, persistent fluctuations can be entirely endogenous and deterministic, and may be compatible with the most favourable hypotheses about agent rationality and system informational transparency. Furthermore, the coexistence of oscillations of almost any periodicity (yielding a chaotic overall behavior of the system) cannot

be excluded. Of related interest is the literature on the indeterminacy of equilibria and system instability due to revisions of self-fulfilling expectations vis-a-vis shocks (sunspot events). The mere possibility of a private economy exhibiting perverse dynamical behavior if left to itself, gives new scope to policy action.

This literature, to which Grandmont has made important contributions, firstly has one great merit. It has demonstrated that sophisticated tools of nonlinear mathematics (and the implied analytical viewpoint) should be used to analyse the dynamics that may appear from even a highly stylized representation of the world. Interaction and interdependence between a number of decision-makers can by itself be a structural source of instability from the dynamic, as from the static, point of view: it is not an inbuilt control mechanism, as in the laissez faire optimistic outlook. The same pessimism has been expressed by others in different contexts of criticism of the RE literature.

There is more than one reason to regard this as a revival of the "classical" approach to BC theory. In the first place, the results just recalled remind us that the complexity of economic systems is manifested by their dynamic behavior, as pointed out by Ragnar Frisch and the earlier Keynesian literature on economic oscillations. After so many years of linear models with exogenous causes of instability, there is a forceful appeal to go back to the dynamic approach of Hicks, Kaldor, and Goodwin, which was essentially nonlinear and permeated by the view of the inherent instability of an unguided capitalistic system. In fact,



in both the TE and these earlier (but still quite lively) theories, the manifestation of oscillations and their persistence are ascribed to the properties of the internal structure of the economic system. (It is important to notice, however, that they entertain different notions of "structure".) Certainly these developments of TEA have shown to those who have a bias towards dynamics, that the theoretical world of microfounded macrodynamics can be more exciting than they might have thought.

Most, if not all, of the results just recalled were obtained by means of low dimensional, though nonlinear, dynamical systems (in difference equation form). It is well known that even a linear dynamical system, provided it is of sufficiently high dimension, is capable of exhibiting a variety of behaviors which may combine in the most exotic ways. By analogy, one is inclined to conjecture that some of these results are likely to hold in a more general setting, but this is no proof of anything. At a higher level, composition effects can introduce compensations, and what might look like chaos at a microlevel could disappear into order and regularity once combined with similar but opposite forces. All macroeconomics rests upon the belief that at a macroscopic level, we find the phenomena of self-organization that in real systems often prevent self-destructive behaviors from arising. Hence that perverse behaviors might emerge does not imply that they do emerge, or even if they do, we may not be aware of it. How many of the results obtained so far will hold in higher dimensional models <u>and</u> still be understandable, is still unclear to me (and perhaps to others). Are we going to loose clearcut results and a relatively firm intuition of the economic relevance of the results for the sake of descriptive generality in modelling?

Most of the appeal of the REH literature is due to a purported easily understandable rationalization of "straight" empirical facts. Therefore, the battleground on which to engage the RE approach is not that of logical plausibility. On such terrain, the TEA criticism only shows that BC theory of New Classical Economics does not stand up at the level of formal rigor required of a general equilibrium model. The challenge is rather: do the dynamic models produced by TEA suggest nonobvious analogies with actual behaviors of present-day economies? In other words, do they embody a (perhaps still incomplete) theory? More generally, are they meant to be theories, and if so, in what sense?

Brilliant analysis of "model" economies should not make us forget the real conceptual issues, and microfounded macrodynamics could end up being reproached for its sterility as was Walrasian economics before V&C. I therefore cannot help wondering how many of these mental experiments, which no doubt are very interesting indeed, can be considered "empirically relevant" in some "scientific" sense. For the moment, one must probably be content with what is fairly well established. First of all we now know that certain propositions taught us by the RE literature (beginning with those on policy effectiveness) are not so firmly and generally established. We also know that a deeper understanding of the sources of fluctuations in a decentralized capitalistic economy is still badly wanted. The TEA examples of ill or perverse dynamic behavior of a relatively simple system supply further



support of the view that we lack a theory of fluctuations.

3. Given the possibility of multiple cyclic or stationary deterministic/stochastic equilibria , we need to face the crucial issue of their stability, which has been entirely neglected by the RE literature. In the TEA strategy, alternative learning processes must enter the set of exogenously specified data of each individual model. We get a whole variety of models.

For the last item on the research agenda, the relevant (classes of) learning processes must be identified, but the discussion of this problem is unfortunately only sketchy. Because in real life this selection is made by the agents themselves, one should specify the decisional process behind it. Recalling the crucial role of elasticities of expectations in static equilibrium analysis, one could ask how people change their previously adopted "model of learning" as they are likely to do when perceiving a change in economic regime, or a dramatic alteration in the environment, i.e. a structural change. The work done on these issues ( again partly stimulated by a critical attitude towards REH) is surely very important and interesting. It being obviously a difficult subject, progress is fairly slow. I believe that the difficulty lies in the fact that any model is intrinsically open in this respect. The formal mathematical exercise of modelling must give way to history, sociology and the like.

4. I can now turn to some methodological remarks. We are all inclined to conceive progress as a continuous (additional) process of building upon previous foundations. However, from time to time a discontinuity must take place for genuine progress to occur, and V&C can be seen as an instance of this. Grandmont's

paper begins with an important, though not really new, assertion: the novelty of _Value and Capital_ is in its proposing a unified conceptual framework, or a _scientific research programme_ "à la Lakatos". Accepting this thesis, one may ask: do all the post-war developments of the Neoclassical Synthesis, GE analysis and TEA belong to the same research programme as V&C?

The focus of the analysis in V&C is on dynamics and stability. This choice of priorities is the immediate justification for the fact that the existence of a ("static") equilibrium plays a peripheral role, compared to its role in the ensuing literature. Later, the model of V&C was subjected to the scrutiny of logically rigorous formal analysis in the Arrow, Debreu and McKenzie style. Accordingly, "existence" became a central conceptual issue. I think it is only fair to say, as does Grandmont, that the original achievement of TEA was the integration into the neo-Walrasian Research Programme of the V&C model to supply a monetary theory and a method of dynamical analysis. TEA has developed as _one_ of the approaches of GE analysis.

In assessing the more recent developments, we face a number of delicate (and still unsettled) methodological questions, which all descend from the following fact. The neo-Walrasian Programme suggests a specific understanding of a model as a plausible description of an actual economy, which is based on the extension to economics of the purely logical criterion of consistency of formal, axiomatic systems. As equilibrium and consistency are coextensive concepts, the necessity of providing explicit existence proofs derives from a specific use of "models"; but in



"applied sciences" or in a "discipline", the content of such a proof is either negative (it tells us which models are useless) or inconclusive. When the latter is the case, as it often is, we do not know what to do, at least on the ground of pure logical analysis. This, I believe, is the case with the model in V&C.

In this respect, V&C and the GE literature do not seem to do the same sorts of things. In GE, models are essentially conceived and exploited as logical constructions; to Hicks, models provide laboratories for testing concepts. The underlying reason for the absence of discussion of the existence issue in V&C is therefore Hicks' own conception of economic theory and of the role (and heuristic limits) of formal modelling. V&C belongs to the positive heuristics of his programme, if he had one, i.e. to the set of statements intented to interpret reality; therefore its validation rests upon criteria which are not logical in the sense required by formal logic.

We are now witnessing the spilling over of the GE habits of thought from statics into dynamics. This could account for the dissatisfaction of some with the results of TE dynamics. Investigating the minimal set of assumptions required to obtain certain results is surely an important activity but it belongs to the negative heuristics of an applied science, i.e.to the stage of finding counterexamples undermining the received body of theories or else examples to understand the new rules of the game of theorizing. I surmise that we lack a dynamic theory for the same reasons for which we do not well understand the scientific status of modern GE analysis, e.g. whether it is a theory or metatheory (see for instance the discussion in Weintraub (1979), (1985)).

The new endogenous "theories" of fluctuations do not seem to have (yet) reached the stage in which they can be offered as theories in the sense of V&C, (nor for that matter in the sense of earlier Keynesian macrodynamics): they do not allow us either to predict or to explain. We are still wanting a theory of capitalistic dynamics, although we have been assured that there is at least one. But we are actually suffering from the presence of an excessive number of theories between which we are unable to discriminate.



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