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The Notion of Long-Period Positions: A Useful Abstraction or a "Platonic Idea"?*

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I. INTRODUCTION

In this note I wish to discuss some methodological aspects of the notion of "long-period positions" (hereinafter LPP). Contrary to a widely-held opinion, the task of the methodologists is *not* to establish how a discipline (or even a single issue) should be approached, but to ask (somewhat provocatively) to its pratictioners *why* they do just what they actually do. Accordingly,[1] I will limit myself to raising some questions which seem to me particularly worthy of attention as far as the issue of the convergence to LPP is concerned.

Economics is today more and more partitioned into different areas of research, each requiring a methodological assessment of its own, so that, before investigating the methodological foundations of some piece of economic inquiry, we must first determine the research area it belongs to, in order to choose among different criteria of appraisal.

All this applies to the study of LPP, of course, though here we have the additional difficulty that the "method" of LPP has been discussed with reference to different issues, each requiring separate appraisal. Unnecessary misunderstandings, misdirected criticisms and pointless controversies usually arise just because all this is too often forgotten.

Accordingly, one of the aims of these notes is to show the inappropriatenes of looking for a unique methodological appraisal of the "method" of LPP.

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[1] A strong emphasis on the necessity of paying much attention to the effective practice of economics is emerging from the most recent work in economic methodology. Although approaching economic methodology in this way raises some problems not yet completely solved (cf. A. SALANTI, "Recent Work in Economic Methodology: Much Ado About What?", *Ricerche Economiche*, vol. 43, 1-2, 1989, pp. 21-39), it makes possible a better understanding of the problems economists face in their everyday research activity.

II. THE LPP LITERATURE: A PRELIMINARY OVERVIEW

Even a cursory glance at the above referred literature suffices to reveal the existence of different ways of emphasizing the importance of the notion of LLP. For the sake of brevity, let me take as an example the entries in the *New Palgrave*[2] which are more or less directly concerned with the notion of "gravitation" (further references to the literature may easily be found in the bibliographies appended to them).

There is a first group of such entries which, following Garegnani's well-known 1976 article,[3] provide arguments on behalf of the notion of LPP by repeatedly claiming that:[4]

1. The classical notion of competition rests on the existence of a mechanism (capital mobility between different "industries") which drives the system towards the general establishment of a uniform rate of profits;
2. Natural prices (that is those prices corresponding, among other things, to a situation of uniform rate of profits) are "centres of gravitation" for market prices which may happen to be lower (higher) than the former whenever the quantity supplied to the market is greater (smaller) than the "effectual demand" as defined by Adam Smith;
3. The rationale of focusing attention on natural prices is provided by the assumption that the forces which determine them are the more systematic and persistent and therefore, in the long-run, dominate the transitory and unsystematic ones (that is, those responsible for the fluctuations of market prices around their "centres of gravitation");
4. The method of LLP (that is the notion of a long-run equilibrium as a sort of benchmark for the actual state of the economy) was not peculiar to classical political economy, but was also employed by neoclassical economists such as Walras, Marshall, Wicksell and so on until the 1940's;
5. Subsequently, following Hicks's seminal work on general equilibrium,[5] the neo-Walrasian approach abandoned the method of LPP and progressively focused on temporary equilibria: in doing so, however, this approach would have lost any relevance precisely because it must rely upon "data", including "the state(s) of nature" and "expected prices", which cannot be assumed

[2] J. Eatwell, M. Milgate and P. Newman (Eds.), *The New Palgrave. A Dictionary of Economics*, London, Macmillan, 4 vols., 1987 (hereinafter referred as *NPD*).

[3] P. Garegnani, "On a Change in the Notion of Equilibrium in Recent Work on Value and Distribution: A Comment on Samuelson", in *Essays in Modern Capital Theory*, ed. by M. Brown, K. Sato and P. Zarembka, Amsterdam, North Holland, 1976, pp. 25-45.

[4] I am mainly referring to: J. Eatwell, "Competition: Classical Conceptions", *NPD*, vol. 1, pp. 537-540; J. Eatwell, "Natural and Normal Conditions", *NPD*, vol. 3, pp. 598-599; M. Milgate, "Equilibrium: Development of the Concept", *NPD*, vol. 2, pp. 179-182. C. Panico and F. Petri, "Long-Run and Short-Run", *NPD*, vol. 3, pp. 238-240; G. Vaggi, "Natural Price", *NPD*, vol. 3, pp. 605-608.

[5] J. R. Hicks, *Value and Capital*, Oxford, Clarendon Press, 1939.

to remain constant long enough to allow the economy to reach a meaningful position of equilibrium.[6]

While the first three of the above mentioned points undoubtedly provide quite useful insights for a sound "rational reconstruction" of the "pure theory" embedded in classical political economy, the other two are likely to raise more controversial issues. Behind them, indeed, it is not so difficult to see the suggestion of a return to the "method" of LPP in contemporary theorizing.[7] A proposal of this importance, however, requires first (and obviously enough, given the more demanding standard of rigour presently required to any theoretical framework), that formal proofs of stability of models which try to capture the basic elements of the notion of classical competition have to be provided (as, once again in the *New Palgrave*, both Boggio and Semmler aptly point out).[8] Secondly, such a proposal requires a convincing vindication of the implied definition of the object of economic theory, more restrictive than that currently prevailing.

All this immediately raises a number of important questions whose methodological implications deserve, as I will try to sketch in the next section, rather more attention than has so far usually been paid to them.

III. OPEN QUESTIONS IN THE METHODOLOGY OF LPP

Once it is agreed, as I am prepared to do, on the correctness of the notion of LPP as a consistent reconstruction of the theoretical core of classical political economy, it ought also to be beyond dispute that originally:

> "This particular 'tendency towards equilibrium' was held to be operative in the *actual* economic system at any given time".[9]

Hence one would expect that scholars who advocate a reappraisal of the "method" of LPP within contemporary economics were mainly engaged in finding empirical outcomes supporting their own views, but, as far as I know, this has not been the case. On the contrary, still today[10] their

[6] About this last point, see also F. Petri, "The Difference Between Long-Period and Short-Period General Equilibrium and the Capital Theory Controversy", *Australian Economic Papers*, vol. 17, 2, 1978, pp. 246-260.

[7] Let me incidentally note that these two different aspects (*i. e.* a reconstruction of the inner logic of classical economics and an explicit exhortation to follow it in the current research) are clearly recognizable in the whole attempt to reconstruct a "surplus approach" to the theory of value and distribution. See, for example, P. Garegnani, "Surplus Approach to Value and Distribution", *NPD*, vol. 4, pp. 560-574.

[8] Cf. L. Boggio, "Centre of Gravitation", *NPD*, vol. 1, pp. 392-394 and W. Semmler, "Competition: Marxian Conceptions", *NPD*, vol. 1, pp. 540-542.

[9] M. Milgate, *op. cit.*, p. 179.

[10] Indeed, this state of affairs is by no means new: as noted in G. J. Stigler, "Competition", *NPD*, vol. 1, p. 532, the Irish economist Cliffe Lesle repeatedly pointed out the empirical weakness in the assumption of the uniformity of the rates of profit as early as in the last two decades of the nineteenth century.

rhetoric (in McCloskey's sense) rests on arguments from which emerges a now quite unusual epistemological view, typically represented by the following statements:

> "Theory proceeds by the extraction from reality of those forces which are believed to be dominant and persistent, and the formation of those elements into a formal system, the solution of which is to determine the magnitude or the state of the variables under consideration. [...] the practice of analysis embodies the assumption that the forces comprising the theory *are* dominant, and that the determined magnitudes will, on average, tend to be established"; [11]
>
> "Equilibrium is, as Adam Smith might have put it (though he did not use the term), the centre of gravitation of the economic system — it is that configuration of values towards which all economic magnitudes are continually tending to conform. [...] 'Natural conditions' so defined and conceived are the formal expression of the idea that certain systematic or persistent forces, regular in their operation, are at work in the economic system"; [12]
>
> "In defining the object of the analysis and identifying the forces which determine it, the assumption is made, implicitly, that the forces of which the theory is constituted are the more dominant, systematic and persistent. [...] since the theory is constructed on the basis of dominant and persistent forces, the magnitude determined by the analysis is the *centre of gravity* of the actual magnitude of the object". [13]

To a methodologist's eyes the above statements clearly display a sort of methodological monism (implied by the repeated use of a mechanical analogy — or, if you prefer, metaphor — as a picture of the functioning of the economic system) and holistic view of the object of economic analysis. Both these premises sustain, in their turn, a realistic perspective which is quite explicitly revealed by the reiterated references to forces "at work in the economic system". Following a pluralist approach to economic methodology,[14] I am not going to oppose such a view by an alternative stance under the form of a differently articulated epistemology. I am here more interested in *criticizing in order to understand*, that is in pointing out what seems to be still lacking instead of looking for what might be possibly wrong.

In this respect a first point which would surely be worthy of attention is that the classical notion of competition needs an explanation involving intentional (and one could equally claim, maximizing) behaviour from capitalists. Apart from the well-known difficulties of appraising teleological explanations, the main problems here arise when we try to justify our commitment to models in which capitalists maximize (or, if you prefer, act in such a way that the resulting tendency is the equalisation of profit rates between different sectors), but in which nothing is said — even

[11] J. Eatwell, "Competition: Classical Conceptions", *op. cit.*, p. 539.

[12] M. Milgate, *op. cit.*, p. 179.

[13] J. Eatwell, "Natural and Normal Conditions", *op. cit.*, pp. 598-599.

[14] Cp. B. Caldwell, "The Case for Pluralism", in *The Popperian Legacy in Economics*, ed. by N. de Marchi, Cambridge, Cambridge University Press, 1988, pp. 231-244.

implicitly — about consumers' behaviour. Furthermore, and more importantly, the existence of discrepancies between market prices (which are the only ones that entrepreneurs can know with certainty) and prices of production (which are those toward which the system is said to converge) requires that more attention be given to assumptions about expectations than is usual in LPP literature.

A second, and in my opinion even more worrying, reason for dissatisfaction is due to the absence of any attempt to corroborate empirically what is said about the notion of LPP on a purely aprioristic basis: an absence, to be sure, even more difficult to justify given the underlying methodological beliefs. Paradoxically, those who are mainly interested in the mathematical intricacies of (the "stability" of) LPP seem to be more aware of this question. Consider, for example, the following passages (also from the *New Palgrave*):

> "[...] If it cannot be established theoretically how profit rate differentials are dynamically equalized through the forces of competition then the concept of prices of production would become empirically irrelevant";[15]
>
> "If the equilibrium is stable, the effects of changes in the data of the system can be approximately studied by means of the displacements of equilibrium positions [...] A condition for the correct use of this method [...] is that changes in the data should be slower than the movements of the state variables of the (dynamical) system."[16]

While, as so often happens when dynamical models are considered, the former raises the problem of the ontological necessity of stability properties,[17] the latter puts a finger on what is, in my opinion, the truly relevant question about the explanatory power of the "method" of LPP. On one hand we must not forget, indeed, that "data" change whenever a wage settlement occurs, a new technique is introduced in the system, a tax is modified and so on. On the other, we have no precise idea about the velocity of the adjustment process or the time required to overcome any kind of market friction.

In this respect it should be pointed out that seeing the notion of LPP as a sound interpretation of classical economics and at the same time having doubts about its usefulness for present economic theorizing are not necessarily mutually exclusive positions. In the same way, our judgement on the historical adequacy of classical analysis may well be positive without implying a similar attitude toward the contemporary relevance of the analysis

[15] W. SEMMLER, *op. cit.*, p. 542.

[16] L. BOGGIO, *op. cit.*, p. 393.

[17] On this point, see G. GANDOLFO, "Stability", *NPD*, vol. 3, pp. 461-463. It is interesting to note that emphasis is on stability rather than on existence of equilibrium. This is another sign of the epistemological realism previously detected. Indeed, when attention is focused on existence a conventionalist approach is implictly followed (first envisaged by von Neumann — as convincingly argued in L. PUNZO, "La teoria della produzione congiunta. Note ad un dibattito", in *Le teorie economiche della produzione*, ed. by S. ZAMAGNI, Bologna, Il Mulino, 1989), according to which it is the very existence of solutions that "legitimates" the axiomatic approach to the construction of mathematical models.

of LPP. One could maintain, indeed, that it is very likely that in the economic systems actually observed by classical economists "data" changed much less quickly than now.

IV. THE CRITIQUE OF THE NOTION OF TEMPORARY EQUILIBRIUM

As is well known, the previous characterization of the "method" of LPP, through a comparison with the "method" of temporary equilibria, has often been regarded as a critique of the latter. Panico and Petri, for instance, restate this critique as follows:[18]

> "[...] the long period method of analysis [...] attempts to represent states of the economy which have the role of centres of gravitation of observed day-to-day magnitudes: chance movements away from such a state set off forces tending to bring the economy back to it. Changes in the economy can then be studied by comparing the long-period positions corresponding to the situation before and after the change. Post-Walrasian equilibria cannot have such a role, because they rely on data some of which (the endowments of capital goods and, where futures markets are not complete, expectations) would be altered by any chance deviation from the equilibrium: thus the forces set off by this deviation would *not* tend to bring the economy back to the same equilibrium".

Frankly speaking, arguments like these seem to me the weakest ones of the whole LPP approach, and this for at least two reasons. First, as the empirical content of LPP has still to be appraised, we cannot refer to LPP as a sort of touchstone for any other notion of equilibrium which may have been set forth in economic literature. As I argued elsewhere,[19] the propositions that any equilibrium must be a "centre of gravitation" and that price expectations are too subjective to be taken as independent variables in any equilibrium model can only be accepted on the basis of an explicit methodological decision, *not* on the basis of their factual incontrovertibility.

Second, the negative conclusions about the role of expectations in the convergence to equilibrium may give rise to the false impression that the notion of LPP is devoid of any assumptions about expectations. On the contrary, because of the arbitrage argument about the rates of profits on which the classical notion of competition must rely, LPP need some (more or less explicit) assumptions about expectations in order to be fully intelligible. To avoid unnecessary discussion I refrain from guessing here which assumptions are implicit in the usual formulations of the "method" of LPP. Let me note, however, that Garegnani himself touched on the problem when he stated that:

[18] C. PANICO and F. PETRI, *op. cit.*, p. 240. For similar statements see also: P. GAREGNANI, "On a Change in the Notion of Equilibrium", *op. cit.*; J. EATWELL, "Competition: Classical Conceptions", *op. cit.*; M. MILGATE, *op. cit.*; F. PETRI, *op. cit.*.

[19] A. SALANTI, "'Internal' Criticisms in Economic Theory: Are They Really Conclusive?", *Economic Notes*, no. 1, 1989, p. 11.

"[...] the 'natural' or 'normal' price [...] is the only one that we may expect to experience under the non-accidental conditions that are likely to emerge through a repetition of the situation".[20]

When data on which agents must rely for taking decisions are continuously changing the meaning of the expression "a repetition of the situation" becomes obscure. Changes is such data imply that expectations matter and the "method" of temporary equilibrium constitutes an attempt to deal with this problem.[21] We may well criticize such a solution, but we cannot deny the existence of the problem.

V. FINAL REMARKS

To sum up, the methodological adequacy of the notion of LPP can be discussed with reference to (at least) three different issues: *i)* LPP as a basic concept for the interpretation of classical political economy; *ii)* the claim that contemporary theorizing ought to resume this theoretical framework and *iii)* the irrelevance, by contrast, of the post-Walrasian approach in that it relies on the notion of temporary equilibrium.

In the previous sections I have argued that there can be no contradictions in maintaining that (*i*) provides a sound interpretation of classical economics, (*ii*) raises serious methodological problems, and (*iii*) is the weakest part of the whole argument. Indeed, what I regard as the most unsatisfactory aspect of the LPP approach to economic theory is the impossibility of providing an adequate account of expectations within such a theoretical framework. For this reason it seems to me that J. Robinson's question:

"[...] the conception of the long period, in particular of the normal rate of profit on capital, is not easy to grasp. Does [it] mean what the rate of profit on capital will be in the future or what it has been in the past or does it float above historical time as a Platonic Idea?",[22]

has still to be (convincingly) answered.

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[20] P. GAREGNANI, "The Classical Theory of Wages and the Role of Demand Schedules in the Determination of Relative Prices", *American Economic Review, Papers and Proceedings*, vol. 73, 1983, p. 309.

[21] For a presentation of the conceptual framework of temporary equilibrium stressing the importance of expectations in that concext, see J. M. GRANDMONT, "Temporary Equilibrium", *NPD*, vol. 4, pp. 620-623.

[22] J. ROBINSON, "Garegnani on Effective Demand", *Cambridge Journal of Economics*, vol. 3, 2, 1979, p. 180.