Revue européenne des sciences sociales, Tome XXXI, 1993, N° 96, pp. 47-83

Franco DONZELLI

THE INFLUENCE OF THE SOCIALIST CALCULATION DEBATE ON HAYEK'S VIEW OF GENERAL EQUILIBRIUM THEORY*

1. Introduction

The so-called Socialist Calculation Debate (henceforth SCD), which raged over the inter-war period, reaching its climax in the late 1930s, had a profound impact on the contemporary and subsequent developments of General Equilibrium Theory (henceforth GET). In fact, by revealing the existence of a serious ambiguity surrounding the interpretation and use of GET, the SCD forced economic theorists of different persuasions to probe more deeply into the nature of the general equilibrium approach and to specify more clearly the scope and significance of equilibrium analysis in economics and the social sciences in general.

The ambiguity referred to above was made apparent by the fact that, at least from a certain stage of the controversy onwards, both groups of participants in the SCD started to use alternative interpretations of what appeared to be the same formal apparatus (namely, the apparatus of GET, in the simple version then available) in order to support their opposing views about the best way to coordinate economic activities. Precisely, up through the third decade of this century, GET had essentially been viewed as an idealized representation of the functioning of a competitive market economy; moreover, in line with the suggestions originally made by the very founders of the general equilibrium approach, Walras and Pareto, the supporters of GET had typically used that theory to uphold their claim for the superiority of the «competitive market mechanism» over the alternative mechanisms (actually existing or merely conceivable) for allocating economic resources. But, dating from the late 1920s, various groups of «socialist» professional economists began to suggest that the analytical apparatus of GET, when suitably reinterpreted, could in effect be exploited to support the opposite claim for the superiority of some sort of «socialist

* I wish to thank the participants in the Conference for their penetrating comments and insightful remarks. This paper is partly based on previous research on related issues, which found expression in Donzelli (1988), (1990), and (1993). Financial support from the Italian Ministry for University and Scientific and Technological Research (MURST 40% research project on «Epistemology and Economic Theory») is gratefully acknowledged. Of course, all responsibility for the ideas expressed in the following is only mine.

planning» over the spontaneous order allegedly brought about by the «competitive market mechanism».

In the early 1930s, therefore, it became apparent that distinguished members of the economics profession could hold utterly contrasting views about the nature of GET and its implications for economic policy or social reform. The ensuing theoretical paradox could not escape the attention of those theorists who were at the time more directly involved in the work of development and appraisal of GET. Among the latter, a leading position was then held by Friedrich August von Hayek. Since the late 1920s, in fact, Hayek had embarked upon the very ambitious task of reconciling GET, which he then tended to regard as the core of pure economic theory, with the study of those «dynamic» phenomena that had piqued his interest from the very beginning of his scientific career (namely, the phenomena traditionally associated with the theory of money, capital, and the trade cycle). For this reason, therefore, Hayek was particularly sensitive to the methodological and theoretical issues concerning the meaning of the equilibrium concept and the significance of GET.

In the light of this, it is by no means surprising that, when Hayek eventually entered the SCD in 1935, his foremost concern should be to dispel the ambiguities beleaguering the contemporary use of the general equilibrium apparatus. To this end, he launched a frontal attack upon the then prevailing «socialist» reading of GET[1]. According to this position, which will be referred to in the following as the «first socialist interpretation of GET», a feasible method for tackling the economic calculation problem arising in a centrally planned economy would simply consist in numerically solving the system of simultaneous equations representing the (static) equilibrium conditions for a suitably specified Walrasian model of the economy. In criticizing this suggestion, which he regarded not only as unfeasible for practical reasons, but also as questionable on theoretical grounds and ultimately unfaithful to the true spirit of the general equilibrium approach, Hayek was led to open a new front of investigation, concerning the relationships between equilibrium analysis, on the one hand, and the social processes of formation and diffusion of knowledge, on the other. This new line of inquiry rapidly blended with his pre-existing research work, aimed at reconciling economic «dynamics» with general equilibrium analysis. This combined endeavor eventually resulted in Hayek's path-breaking paper, «Economics and Knowledge», where the author's own views (as of 1937) on the meaning of the equilibrium concept and the role of GET were first systematically stated.

As far as Hayek's involvement in the SCD is concerned, however, this was not to be the end of the story. In the second half of the 1930s, in fact, a new

[1] As a matter of fact, the actual sequence of the events occurring during the SCD was a bit more complex than it would appear from this sketchy presentation. This point will be taken up again in Sec. 4 below.

method for dealing with the economic calculation problem arising in a partially decentralized economy with some socialist traits was suggested by a group of economists, later called the «market socialists», advocating a milder form of «socialist planning». Their proposal turned on a novel, and much more sophisticated, reinterpretation of GET, which will be referred to in what follows as the «second socialist interpretation of GET». According to this position, a suitably reinterpreted Walrasian *tâtonnement* process could be employed to determine, by trial and error, the set of equilibrium prices to be used as a basis for the coordination of economic activities and the efficient allocation of resources in a partially decentralized socialist economy. As this proposal was alleged by its propounders to dodge most of the criticisms levelled at the previous «socialist» solution of the calculation problem, and as it raised, at the same time, some new questions in its own right, Hayek felt obliged to enter a new the theoretical debate. And again, in his 1940 paper specifically devoted to a critical discussion of the positions of the «market socialists», beyond pointing out the practical difficulties that would undermine any concrete attempt to apply the suggested method, Hayek made every effort to explain why the underlying interpretation of GET was to be regarded as misleading and theoretically unsound.

Now, while Hayek's criticisms of the proposals put forward by the economists on the «socialist» side during the SCD have been thoroughly discussed and duly appreciated in the literature, and while his objections to the «socialist» interpretations of GET have also received some attention by later commentators[2], one important effect of the SCD on the evolution of Hayek's own ideas has gone almost unnoticed. In fact, very little attention has been paid to the fact that in 1941, that is, immediately after the end of the historical Debate, Hayek dramatically changed his mind about the interpretation of the equilibrium concept and the nature of GET, by reversing his 1937 position on those issues in at least one crucial respect. Similarly, very few attempts have been made to analyze the connections between Hayek's participation in the SCD and his subsequent decision, taken in the early 1940s, to gradually abandon the field of economics in the strict sense and to replace the characteristic methods of economic theory, hinging on the general equilibrium concept, with a new theoretical approach to the study of social phenomena, resting on the alternative concept of a «spontaneous order»[3].

[2] In particular, a number of interesting insights on this issue can be found in three relatively recent (though not unbiased) reconstructions of the SCD, due to Vaughn (1980), Lavoie (1985), and Kirzner (1988), respectively.

[3] Two notable exceptions to the general trend prevailing in the literature are represented by Butos (1985) and Caldwell (1988). Butos's short paper, however, is not really concerned with Hayek's participation in the SCD (the only explicit reference to such Debate being contained in a brief footnote), so that it completely avoids discussing the questions raised in the text above. Such questions, instead, are explicitly addressed by Caldwell. Yet, his paper, while providing an excellent (though not entirely endorsable) reconstruction of the development of Hayek's ideas on GET in the inter-war period and beyond, is somewhat reticent on the specific issue of the influence of Hayek's participation in the SCD on such development, so that a few important points are still left unanswered.

The chief aim of this paper is to fill the gap which still exists in the literature in this respect. Precisely, it will be explained in which way, and to what extent, the evolution of Hayek's ideas about the meaning of the equilibrium concept and the role of equilibrium analysis in economics and the social sciences in general was influenced by his involvement in the SCD. Further, it will be argued that the change in Hayek's approach in the early 1940s, though contingently depending on a number of diverse factors, was speeded up and ultimately caused by the fact that, by reconsidering his own participation in the SCD, and by further reflecting on the nature and outcome of that debate, he eventually came to realize the existence of a number of difficulties and dangers intrinsic to GET that would have probably escaped his notice otherwise. The paper will be structured as follows. Sec. 2 will provide a concise presentation of the status of GET in the first quarter of this century, and especially in the mid-1920s, when Hayek entered the theoretical arena as a young professional economist. Sec. 3 will be devoted to a discussion of Hayek's early ideas on equilibrium and dynamics in the late 1920s and early 1930s. Hayek's 1935 criticism of the «first socialist interpretation of GET» will be examined in Sec. 4. Sec. 5 will discuss Hayek's more mature views about equilibrium analysis, as expounded in his 1937 paper «Economics and Knowledge». Hayek's 1940 criticism of the «second socialist interpretation of GET» will be reviewed in Sec. 6. The change in Hayek's ideas on GET in the early 1940s will be discussed in Section 7. Finally, Sec. 8 will contain a few concluding remarks.

2. The status of GET in the 1920s

When Hayek started his career as a professional economist, in the mid-1920s, his interests were chiefly directed at the field of monetary theory and policy. Soon after, in the wake of his mentor, Ludwig von Mises, he extended the scope of his inquiries to the study of economic fluctuations, a topic which was very much at the center of the theoretical debate going on in that period. Yet, like most young economists of his generation involved in the same line of research, he came quite soon to realize that the sort of analytical apparatus that was then typically employed to discuss the issues of monetary policy, price stabilization, output oscillations, and business cycles in general, was far from connected, or even consistent, with the kind of theoretical framework, usually referred to as «equilibrium theory», on which most economists used to rely when discussing the basic economic phenomena of price formation and resource allocation. In view of this, from the late 1920s Hayek's main concern became that of constructing a rigorously microfounded theory of the trade cycle, based on a fully integrated and logically consistent theory of money, prices, capital, and equilibrium. As he put it:

> We cannot superimpose upon the system of fundamental propositions comprised in the theory of equilibrium a Trade Cycle theory resting on unrelated logical foundations. All the phenomena observed in cyclical fluctuations, particularly price formation and its influence on the direction and the volume of production, have already been explained by the theory of equilibrium; they can only be integrated as an explanation of the totality of economic events by means of fundamentally similar constructions. [...]
> [T]he incorporation of cyclical phenomena into the system of economic equilibrium theory, with which they are in apparent contradiction, remains the crucial problem of Trade Cycle theory[4].

In order to correctly understand the nature of Hayek's research program, as outlined in the above passage, a more precise specification of what he meant by «equilibrium theory» would be needed. In this respect, Hayek himself appears to provide an important clue, by writing:

> By «equilibrium theory» we here primarily understand the modern theory of the general interdependence of all economic quantities, which has been most perfectly expressed by the Lausanne School of theoretical economics. The significant basic concept of this theory was contained in James Mill's and J.B. Say's *Théorie des Débouchés*[5].

At first sight, the first part of this passage would seem to suggest a precise identification of «equilibrium theory» with the Walrasian version of GET, as developed by the «Lausanne School of theoretical economics» (that is, by Walras, Pareto, and their immediate followers). Yet, as is made clear by the somewhat vague reference to James Mill and J.B. Say in the second part of the passage, as well as by the whole trend of the discussion in the same book and in all of Hayek's writings of the late 1920s, Hayek's knowledge of the Walrasian approach was at that time quite superficial, second-hand, and non-technical[6]. This limited familiarity with the Walrasian version of GET, however, was by no means a peculiarity of Hayek alone; rather, it was a feature shared in common by most theoretical economists doing research work in the third decade of this century. In that period, in fact, only a very restricted circle of economic theorists, essentially coinciding with Pareto's few direct followers, could boast a first-hand knowledge of the works of the founders of GET. Outside that small group, all that the economics profession at large knew of the Walrasian-Paretian approach, especially in the

[4] Hayek (1933a, 28-9, and 33, fn.). The above quotations are drawn from *Monetary Theory and the Trade Cycle*, which is the English translation of the German essay *Geldtheorie und Konjunkturtheorie*, published four years before (Hayek (1929)). The latter, in turn, is an expanded version of Hayek (1928a). Hence, the propositions quoted and discussed in the text should be considered as referring to the late 1920s, as that is the period in which they were originally conceived.

[5] Hayek (1933a, 42, fn.).

[6] It should be noted that the first part of the passage quoted above is reproduced by all the contemporary economists trying to establish, from different perspectives and with different purposes in mind, what sort of relationship actually existed in the second half of the 1920s between Hayek's approach and the Walrasian general equilibrium tradition; cf., in particular, Lucas (1983, 215), Butos (1985, 335), and Caldwell (1988, 523, fn. 11). It is a pity, however, that all of them fail to quote the second part of the passage which, as explained in the text, is much more revealing than the first as to the true state of Hayek's acquaintance with the Walrasian tradition in the late 1920s.

German-speaking world, amounted to the simplified version of Walras's theoretical system that could be found in Gustav Cassel's popular textbook *The Theory of Social Economy* (first printed in German in 1918).

In this regard, a point which is worth stressing is that Cassel, in vulgarizing Walras's *Eléments d'économie politique pure*, had not only drastically reduced the scope of the Walrasian system, by selecting for consideration only the simplest equilibrium model (namely, the model of a pure-flow competitive economy, without either capital or money) from the many models originally presented by Walras in his work, but had also significantly altered the meaning of the surviving part of the theory, by changing the interpretation of the equilibrium concept used therein. In fact, both Walras and Pareto had repeatedly suggested in their works that the equilibrium notions employed in their theoretical models should be given an *instantaneous* interpretation[7]. In his 1918 book, instead, Cassel had made it very clear that the simplified Walrasian model discussed therein, subsequently referred to in the literature as the «Walras-Cassel model», ought to be interpreted as a *stationary* equilibrium model[8].

Needless to say, the two ways of looking at the general equilibrium concept have quite different theoretical implications. While a general discussion of this issue would lead us too far away, a few remarks directly bearing on the subsequent development of our argument are in order. To this end, consider a general equilibrium model, whose supposed aim is to investigate the functioning of a given economy. Further, to avoid unnecessary complications, assume an equilibrium of the model to exist, and to be unique, for any admissible specification of the data characterizing the economy under theoretical investigation.

Consider now the instantaneous (temporary) interpretation of the equilibrium concept suggested by the founders of GET. According to such interpretation, any particular equilibrium of the model, corresponding to an admissible specification of the data, should be viewed as describing the equilibrium state instantaneously reached by the economy at that particular

[7] Cf. Walras (1874-7, esp. 301-2 and Lesson 35); Pareto (1896-7, Vol. II, 11-3), and (1906, 104-5). To be precise, one should recall at this point that, in the course of the historical evolution of GET, the instantaneous interpretation of the equilibrium concept has been specified in a number of different ways; hence, there currently exist in the literature many alternative equilibrium notions, all of the instantaneous type, but differing from one another in several respects. What we are concerned with here is that particular specification of the instantaneous equilibrium notion that was actually suggested by Walras and Pareto in their original writings. In today's language, such specific notion would be referred to as a «temporary equilibrium» of the so-called «bounded-rationality» variety (in the sense of Radner (1982, 940-2)). The term «temporary equilibrium», however, was never used by either Walras or Pareto. It was first introduced into the language of economics, in the sense in which it is presently understood, by Hicks in the first edition of *Value and Capital* (1939). A more detailed discussion of the nature of the equilibrium concept adopted by Walras and Pareto, as well as of the relationship between their theory and modern temporary equilibrium theory, can be found in Donzelli (1990).

[8] Cf. Cassel (1918, esp. Ch. I, Sec. 5, and Ch. IV).

instant (resp., unit period)[9] of its history at which the specified data are supposed to prevail. Moreover, under this interpretation, there is no reason why the data should be assumed to be unchanging over time; on the contrary, one should generally expect the data, hence the equilibria, to change as time elapses. This means, however, that the theorist embracing this interpretation is naturally led to describe the evolution of the economy over time by means of a chronologically ordered continuum (resp., sequence) of such instantaneous (temporary) equilibrium states, differing in general from one another[10].

Turning then to Cassel's stationary interpretation of the equilibrium concept, one finds an altogether different situation. According to such interpretation, in fact, any particular equilibrium of the model, corresponding to an admissible specification of the data, should be viewed as a stationary state of the economy under theoretical investigation; as such, it should not be taken to refer to any specific instant (resp., period) in the history of the economy, but rather to the whole continuum (resp., sequence) of instants (resp., periods) over which the data are supposed to be unchanging. This means, however, that this interpretation of the equilibrium concept, unlike the instantaneous one, forces the theorist to assume the stationarity of the data characterizing the economy over time.

In the light of the above considerations, one might find somewhat puzzling that Cassel, in almost literally reproducing one of Walras's original models, should have decided to bring about such a momentous change in the interpretation of the underlying equilibrium concept. But Cassel's decision will appear much less surprising as soon as one considers the theoretical context in which it was taken. The fact is that, dating from the last decade of the nineteenth century, all the leading economists belonging to all kinds of neoclassical schools, with the only exception of the members of the Lausanne school, had invariably suggested that their more or less formalized equilibrium models ought to be interpreted as stationary equilibrium models. As a consequence of this uniform trend, by the end of second decade of the twentieth century the stationary interpretation had conquered the economics profession in general, while the instantaneous one had almost

[9] The alternative between «instant» or «unit period» depends on whether time is regarded as a continuous or discrete variable. The present discussion, however, is unaffected by this choice.

[10] This is precisely what Walras and Pareto suggested to do (and actually did, albeit in a very limited way) in their writings. Moreover, both of them tended to regard the analysis of any such sequence of temporary equilibria, moving over time in accordance with the change in the data due to both exogenous and endogenous factors, as the only kind of «dynamic» analysis which is in effect compatible with the general equilibrium framework. To refer to this sort of analysis, Walras (1874-7, 74-5 and 301) indifferently used the expressions «variable or moving equilibrium» or «dynamic point of view»; Pareto (1896-7, Vol. II, 11-3) and (1906, 104-5) spoke instead of the «method of successive equilibria».

completely been forgotten[11]. In view of this, it was only natural for Cassel to superimpose the then prevailing stationary conception upon a theoretical system which had originally been formulated with a different interpretation in mind.

A number of factors concurred to foster the adoption, diffusion, and eventual supremacy of the stationary viewpoint over a period of almost half of a century. Here, however, we shall restrict our attention to one single factor, which is of particular significance for the subsequent discussion. The issue we want to single out for consideration has to do with the problem of the «empirical justification» of GET. In this respect, it should be noted that, since GET was formally developed in the mid-1870s, and for a long time thereafter, all the economists somehow concerned with general equilibrium analysis, independently of their particular affiliations or theoretical positions, shared in common the idea that the use of the general equilibrium apparatus ought to be «empirically justified» by showing that an adjustment mechanism is at work in the economy which is capable of driving it towards an equilibrium position, as defined by the theory.

Walras himself was the first to tackle this problem, as well as to provide a tentative solution to it. As is well-known, his suggested solution rested on the construct of the *tâtonnement*, which was conceived as an adjustment process applying to a competitive economy, where the motion of the state variables (both prices and output levels, in Walras's original formulation) is governed by a system of dynamic equations. The whole construct, however, was only discussed in a relatively informal way, so that its economic interpretation and analytic properties remained somewhat vague. While the general question of the interpretation and properties of the Walrasian *tâtonnement* will be taken up in Sec. 6 below, a specific point concerning Walras's own view of that construct calls for discussion now. Walras was convinced that the process of adjustment towards equilibrium taking place in the competitive markets of the real world is both very effective and extraordinarily quick[12]. On the basis of this conviction concerning empirical reality, he

[11] To give an idea of the overwhelming predominance of the stationary interpretation over the period 1890-1930, as well as of its persistence far into the 1930s, it may suffice to recall that, apart from Cassel himself ((1918) and (1925)), that interpretation was espoused by such outstanding theorists as Wicksell (1898) and (1901-6), J.B. Clark (1899) and (1907), Schumpeter (1911) and (1939), Knight (1921), Frisch (1935), and Pigou (1935). A basically stationary conception of the equilibrium construct can easily be seen to permeate the theoretical system of Marshall (1890) as well, even if the latter's analysis was typically couched in terms of a partial, rather than general, equilibrium framework, so that Marshall's acceptance of the stationary interpretation is not exactly comparable with the others'. Once again, a more detailed discussion of the stationary equilibrium approach within neoclassical economics can be found in Donzelli (1990).

[12] The following passage is quite explicit on this issue: «On voit clairment à présent ce qu'est le mécanisme de la concurrence sur le marché; c'est la solution pratique, et par hausse et baisse des prix, du problème de l'échange dont nous avons fourni la solution théorique et mathématique. On doit comprendre d'ailleurs que notre intention n'est aucunement de substituer une solution à l'autre. La solution pratique est d'une rapidité et d'une surêté qui ne laissent rien à désirer. On peut voir, sur de grands marchés fonctionnant même sans courtiers ni crieurs, le prix courant d'équilibre se déterminer en quelques minutes, et des quantités considérables de marchandise s'échanger à ce prix en deux ou trois quarts d'heure.» (Walras (1874-77, 65)).

deemed it legitimate to construct his theoretical model of the competitive adjustment process, that is, the *tâtonnement*, in such a way as to make it a purely virtual process, taking place in a sort of «logical» time (to be distinguished from the «real» time over which the economy evolves), and capable of bringing about its full effects without consuming any amount of «real» time[13]. By the same token, he was also led to justify the theoretical use of an instantaneous equilibrium notion (of the temporary type) as an empirically acceptable approximation; according to Walras, in fact, the «actual state» of a competitive economy at any moment, though never exactly coinciding with the «ideal equilibrium state» at that moment, is always sufficiently close to it as to validate the use of an instantaneous equilibrium notion for the purposes of the theory.

Walras's confidence in the speed and effectiveness of the real-world adjustment processes, however, was not shared by most neoclassical economists concerned with general equilibrium analysis (essentially of the competitive type) over the period 1890-1920. Of course, no such economist would have denied that an adjustment mechanism is actually at work in the competitive markets of the real world, nor would have disputed Walras's idea that such mechanism basically operates through changes in relative prices brought about by the so-called «law of supply and demand». Unlike Walras, however, most neoclassical economists of the first generations were convinced that the adjustment processes at work in real-world competitive markets would take a lot of time to carry their effects through. But, starting from this conviction, they were also forced to confront a difficult question. For, as soon as it is admitted that an adjustment process takes time, the very hope that such process may eventually approach a well-defined equilibrium position appears to depend in a crucial way on the data of the economy remaining unchanged over time. Hence, under the assumption of a time-consuming adjustment process, there seems to exist only one case in which the use of the equilibrium apparatus can be «empirically justified», at least in principle: namely, the case in which the economy under theoretical investigation is assumed to be characterized by an unchanging set of data. In conclusion, therefore, the preoccupation with the empirical relevance of equilibrium analysis, combined with the conviction that any real-world adjustment process is slow and time-consuming, was the chief reason behind the rapid

[13] That this is Walras's conception of the *tâtonnement* can be clearly seen from the two fundamental assumptions on which that construct (in its final formulation, as expounded in the fourth edition of the *Eléments*) is based: namely, the so-called «hypothèse des bons» and the assumption that the «phase des *tâtonnements* préliminaires» be completed before the «phase statique» can start (cf. Walras (1874-7, 301-2)). In fact, the first assumption, by ruling out any out-of-equilibrium behavior, turns the *tâtonnement* into a purely virtual process, where no observable phenomenon can occur; the second assumption, in turn, forces one to distinguish the «logical» time in which the *tâtonnement* process is supposed to take place (and, hopefully, to converge) from the «real» time in which the economy evolves.

diffusion and almost general adoption of the stationary interpretation of the equilibrium concept around the turn of the century and in the following years.

3. Hayek on equilibrium and dynamics in the late 1920s and early 1930s

Returning now to Hayek's position in the late 1920s, what emerges from his works of that period is that, in spite of the somewhat misleading reference to the Lausanne school, his conception of «equilibrium theory» was in effect moulded on the then prevailing stationary equilibrium approach. Hayek, however, came very soon to realize that to adopt an equilibrium theory of the stationary type as the general framework of the analysis would have made it extremely difficult, if not impossible, to adequately deal with that class of «dynamic» phenomena he was chiefly interested in analyzing (above all, the phenomena of the trade cycle). As he noted, in fact, the only kind of endogenous change in the economic variables which is *prima facie* compatible with stationary equilibrium theory is that kind of equilibrating change (particularly, in relative prices) through which the automatic process of adjustment towards a stationary equilibrium position is supposed to take place, under the assumption of unchanging economic data. Any other change in the economic variables has instead to be viewed as the effect of some exogenous (i.e., unexplained) change in the data. But then, when stationary equilibrium theory is taken as the basis of the analysis, no room is apparently left for developing a truly endogenous theory of the trade cycle, that is, a theory accounting for some endogenous change in the economic variables which is not of the equilibrating kind. As Hayek put it:

> There is a fundamental difficulty inherent in all Trade Cycle theories which take as their starting point an empirically ascertained disturbance of the equilibrium of the various branches of production. This difficulty arises because, in stating the effects of that disturbance, they have to make use of the logic of equilibrium theory. Yet this logic, properly followed through, can do no more than demonstrate that such disturbances can come only from outside — i.e. that they represent a change in the economic data — and that the economic system always reacts to such changes by its well known methods of adaptation, i.e. by the formation of a new equilibrium[14].

In order to overcome this difficulty, Hayek started to explore the possibility of replacing the inherited notion of stationary equilibrium with an

[14] Hayek (1933a, 42-3). (It should be recalled that these ideas were originally conceived in 1928-9.) Of course, the «fundamental difficulty» mentioned by Hayek in the above passage had already been perceived before by a number of economists working in the stationary equilibrium tradition and concerned with business cycle analysis. As a matter of fact, it had been precisely that «difficulty» that had prompted Schumpeter to write his *Theory of Economic Development* (including a theory of the business cycle) in 1912. It should be noted, however, that Schumpeter had taken a very conservative stance concerning equilibrium theory; in fact, he had made every effort to graft his new (endogenous) conception of economic «dynamics» onto a quite traditional stationary equilibrium framework.

alternative equilibrium notion, better suited to deal with a process of change over time. What he was looking for was an equilibrium concept according to which a non-stationary sequence of values of the economic variables (especially, prices) could be regarded as an equilibrium sequence — what was of course impossible within the traditional framework of stationary equilibrium theory. This line of inquiry found provisional expression in a paper written in German in 1928[15], where the new notion of «intertemporal equilibrium» was tentatively introduced. Such notion, though clearly satisfying the non-stationarity condition mentioned above, was not precisely defined, so that its exact meaning cannot be specified with absolute certainty; most probably, the notion that Hayek was striving to elaborate roughly corresponds to what would currently be termed a «temporary equilibrium with perfect foresight» (assuming a non-stationary, but perfectly foreseen, environment). In any case, Hayek's 1928 paper was a remarkable achievement: in fact, it was the first paper, written by an economist not belonging to the Lausanne school, where the stationary equilibrium approach was openly questioned and an instantaneous equilibrium notion was (albeit confusedly) suggested[16]. Apart from its historical merits, however, that paper had a limited impact on the development of economic ideas: not only did it remain practically unknown outside the German-speaking community, but it was paradoxically neglected by Hayek himself in the years immediately following its publication.

In fact, when Hayek presented his own version of the Austrian theory of the trade cycle in a series of guest-lectures delivered at the London School of Economics in February 1931[17], he made no use whatsoever of the new concept of «intertemporal equilibrium» he had «discovered» three years before; rather, he reverted to the stationary equilibrium approach, by adopting the simple stationary-equilibrium model put forward by Wicksell in *Interest and Prices*[18] as the starting point for his analysis. This conservative decision was most probably due to the following two reasons: first, the «intertemporal equilibrium» notion had not yet been adequately formulated and specified by Hayek, so that he did not dare construct his new ambitious theoretical endeavor on that basis; second, most of the theoretical debate concerning monetary issues which had been raging over the late 1920s had been couched

[15] Cf. Hayek (1928b), translated into English as «Intertemporal Price Equilibrium and Movements in the Value of Money» in Hayek (1984).

[16] In order to fully appreciate the relevance of Hayek's 1928 paper, one should consider that the first completely formalized model of «temporary equilibrium with perfect foresight» was put forward by Radner in 1972, that is, almost half-a-century after Hayek's article. On the distinction between the «perfect foresight approach» and the «bounded rationality approach» within temporary equilibrium theory, cf. Radner (1982, 940-2).

[17] Those lectures were printed in September of the same year as a small book with the title *Prices and Production* (Hayek (1931)).

[18] *Interest and Prices* is the English translation (appearing in 1936) of Wicksell (1898). The same model had also been used by Wicksell in the second part of his *Lectures on Political Economy*, devoted to monetary issues; cf. Wicksell (1901-6, Vol. II).

in terms of Wicksell's *Interest and Prices* model, so that, quite understandably, Hayek must have thought that to build his analysis upon that model would have made it easier to convey his message to the economics community. Yet, such choice, however reasonable in the short run, proved to be a serious mistake in the long run.

The problem with Wicksell's *Interest and Prices* model was that it was an essentially dichotomous, stationary-equilibrium model that had only been used by Wicksell to discuss the movements in the general price level supposedly brought about by a divergence between the so-called «market» and «natural» rate of interest, assuming all the fundamentals and the real variables of the economy (that is, preferences, production techniques, composition of output, relative prices, and the «natural» rate of interest) given and unchanging. As such, without significant alterations, Wicksell's model was wholly unsuitable to discuss those «dynamic» problems, characteristic of the trade cycle, that Hayek was chiefly interested in analyzing; in particular, it was incapable of supporting that kind of endogenous theory of the trade cycle, involving changes in the real structure of the economy, that he was striving to elaborate. Hence, being perfectly aware of the intrinsic limitations of Wicksell's original model, Hayek proceeded to modify and complicate its basic assumptions in order to make it consistent with his objectives. Thus, by taking seriously Böhm-Bawerk's theory of capital, which had been completely neglected by Wicksell in his *Interest and Prices* model, he recognized that any divergence between the «market» and the «natural» rate of interest, induced by the behavior of the banking system, would have caused a change in both the capital structure of the economy and the system of relative prices prevailing therein.

As soon became apparent, however, most of the conceptual framework originally adopted by Wicksell was inconsistent with the new and richer assumptions made by Hayek: in particular, the postulated dichotomy between the real and monetary part of the economy, the idea of a real stationary equilibrium unaffected by the monetary disturbances, and consequently the very notion of a «natural» rate of interest, on which Wicksell's analysis ultimately rested, became simply meaningless under the new assumptions. The existence of these difficulties was perceived by Hayek immediately after the publication of *Prices and Production*. Thus, the enormous success of that book notwithstanding, he decided to embark upon a huge process of revision of the received theories of money, capital, and equilibrium, in order to make the theoretical foundations of his trade cycle model more sound and consistent. This process was to last for about a decade. But, somewhat paradoxically, the extraordinary progress he was able to make in all the above-mentioned fields ended up by undermining the fundamental message he was trying to convey through his trade cycle theory. So that, in the early 1940s, after a number of unsuccessful attempts to improve the original structure of his *Prices and Production* model, he eventually dropped his long-lasting project concerning trade cycle theory, abandoning that field of inquiry altogether.

While an examination of the theoretical reasons explaining the eventual failure of Hayek's attempts to defend his trade cycle theory is beyond the scope of this paper[19], a related point which is directly relevant to our present purposes needs to be mentioned now. Among the many shortcomings of the original *Prices and Production* model, the most evident, and probably the most embarrassing to Hayek himself, was the use of a stationary equilibrium apparatus that was wholly at variance with those continual changes in relative prices, production techniques, and composition of output that constituted the distinctive feature of Hayek's trade cycle theory and the main object of its purported explanations. Hence, the first task Hayek set to himself in the early 1930s was to free GET from the shackles of the stationary equilibrium approach (that he then used to call the «traditional» or «timeless» equilibrium approach), thereby turning the equilibrium construct into a tool suitable for discussing those «dynamic» aspects that lay at the very center of his trade cycle theory. As he put it in 1933:

> The most characteristic feature of the work of our generation of economists is probably the general endeavor to apply the methods and results of the pure theory of equilibrium to the elucidation of more complicated «dynamic» phenomena.[...]
> Not very long ago I myself still believed that [...] the theory of the trade cycle at which we were aiming ought to be organically superimposed upon the existing theory of equilibrium. I am now more inclined to say that general theory itself ought to be developed so as to enable us to use it directly in the explanation of particular industrial fluctuations. [...]
> A great part of this work will certainly consist in the elaboration of particular chapters of general theory, especially of the theory of capital and the theory of money, in the direction of a more careful analysis of the processes resulting from any change in the data. It is, however, the common peculiarity of all such attempts to make the theory more realistic that they soon bring us back to the fundamental problem of all economic theory, that is to the question of the significance of the concept of equilibrium and its relevance to the explanation of a process which takes place in time. There can be no doubt that here some of the formulations of the theory of equilibrium prove to be of little use and that not only their particular content but also the idea of equilibrium as such which they use will require a certain amount of revision. [...]
> The main difficulty of the traditional approach is its complete abstraction from time. A concept of equilibrium which essentially was applicable only to an economic system conceived as timeless could not be of great value[20].

It should be added, at this point, that Hayek was by no means the only theorist to be concerned with «the question of the significance of the concept of equilibrium and its relevance to the explanation of a process which takes place in time»; on the contrary, quite similar concerns were shared by several young economists at work in the late 1920s and early 1930s, especially by the exponents of the so-called «Stockholm School» (namely, Lindahl,

[19] An extensive discussion of this issue can be found in Donzelli (1988, Sec. 4) and (1993, Sec. 3).

[20] The above quotations are drawn from the paper «Price Expectations, Monetary Disturbances and Malinvestments», in Hayek (1939, 135 and 138-9). This is the English translation of an article originally published in German as Hayek (1935a). The latter, in turn, was the edited version of a lecture delivered in Copenhagen in 1933.

Myrdal, Ohlin, and a few others) and by Hicks, who was then a colleague of Hayek at the London School of Economics. Exactly like Hayek, those economists were striving to develop an analytic method for dealing with «complicated 'dynamic' phenomena»; and, not differently from Hayek, they soon arrived at the conclusion that stationary equilibrium theory was unfit for that purpose and ought to be replaced by an alternative approach. So that, by following a similar path, they ended up by independently rediscovering that instantaneous equilibrium approach that had originally been suggested by Walras and Pareto, and much later tentatively reproposed by Hayek in his 1928 paper. In effect, Lindahl made use of a variety of instantaneous equilibrium notions (of the temporary type) in his well-known 1929 and 1930 papers, devoted to a discussion of a few controversial issues in the theory of capital and prices and to a reconsideration of the so-called «cumulative process» arising in a Wicksell-type model, respectively[21]; Hicks, instead, suggested the use of an instantaneous equilibrium notion (again of the temporary type) in a paper published in German in 1933, where he tried precisely to make sense of the «dynamic» process evoked (but not really analyzed) by Hayek in his *Prices and Production* model[22]. To complete the picture, it should only be added that, in the same year, Hayek himself finally unearthed his own «intertemporal equilibrium» notion (actually, a temporary equilibrium notion with perfect foresight), suggesting that it might represent the «correct» notion to be used in the analysis of a process taking place over time[23].

4. Hayek's criticism of the «first socialist interpretation of GET» (1935)

While Hayek was trying to improve his understanding of the equilibrium concept, and especially to reconcile equilibrium analysis with the study of «dynamic» economic phenomena, he became aware that the issue of the significance of equilibrium analysis had also arisen in a different theoretical context, namely, the context of the SCD, which was then well under way. And it was precisely this aspect of the controversy that attracted Hayek's attention, inducing him to enter the theoretical arena. Before considering

[21] Lindahl's 1929 and 1930 papers were later reprinted, in English translation, as Part III and Part II of Lindahl (1939), respectively. In particular, it should be stressed that Lindahl 1929 paper was a remarkable tour de force indeed: for in that paper, after criticizing the stationary equilibrium approach and rigorously specifying the very restrictive assumptions under which a stationary equilibrium notion could be consistently employed, Lindahl put forward in turn two temporary equilibrium models (one of the «perfect foresight» variety, and the other of the «bounded rationality» variety, in Radner's sense), and finally sketched the main lines of that «complete disequilibrium» approach that will become the distinctive trait of the «Stockholm School» in the 1930s.

[22] Cf. Hicks (1933).

[23] Cf. F.A. Hayek, «Price Expectations, Monetary Disturbances and Malinvestments», in Hayek (1939, 139-40). (It should be recalled that the original version of this paper can be traced back to 1933.)

Hayek's position on this issue, however, it is necessary to briefly review the preceding history of the SCD.

It is commonly agreed that the opening of the SCD, as it is presently understood, can be traced back to the publication of a famous article by Mises at the beginning of the 1920s[24]. The chief aim of that paper had been to refute a conception which was then very popular within the socialist movement, especially among the theorists of Marxist persuasion: namely, the view that the problem of value is exclusively associated with the existence of a capitalistic economic system, so that it is bound to disappear with the beginning of a new socialist era. For convenience of exposition, Mises's critical discussion of that conception can be subdivided into two parts.

In the first place, Mises had argued that any form of rational economic calculation, hence any attempt to rationally plan society as well, must rely on the price system, inasmuch as prices are the indicators of the relative scarcities of the goods and services produced and consumed in the economy. In the second place, he had maintained that prices are market phenomena and can consequently be determined by the market only. Hence, as socialism would seem to imply that at least the means of production be collectively owned, and as this entails the disappearance of at least the markets for capital goods, he had concluded that any sort rational economic calculation is impossible within a socialist economy.

Mises's critique provoked three kinds of answers, approximately corresponding to three successive stages of the SCD[25]. In a first stage, the ancient socialist theses about the irrelevance of value and prices in a non-capitalistic economy were reproposed and the old idea was reaffirmed that, under socialism, rational planning of production ought only to satisfy some technological or material requirements. But, while this stance continued for a long time to affect a sizable part of the socialist movement, as well as to condition the actual practice of most centrally planned economies, it rapidly fell into disrepute among the academic economists and was also dropped by those socialist economists who were better acquainted with the most recent developments of economic theory. From a certain point onwards, therefore, the chief aim of most socialist economists (at least in the Western countries) became that of counteracting Mises's objections on his own ground. Precisely, they accepted the first part of Mises's argument, by recognizing that prices are indeed indispensable for any sort of rational economic calculation; but then they tried to rebut the second part of his thesis, by showing that prices are not necessarily associated with a capitalistic market economy,

[24] Cf. Mises (1920).

[25] The idea that the SCD can be neatly subdivided into three successive stages was first suggested by Hayek (1940, Sec. 1). In effect, the proposed subdivision does not exactly correspond to the actual historical development, particularly because the dividing line between the last two stages of the Debate (as identified by Hayek) is somewhat blurred. However, as in this paper we are chiefly interested in Hayek's position during the SCD, we shall adopt his own «rational reconstruction» as a convenient expository device (cf. however footnote 28 below for a necessary qualification).

so that a price system can consistently be determined in a different economic context as well.

The general idea underlying the new socialist approach is well-known and can be concisely stated, in modern language, as follows[26]. For the sake of simplicity, consider an exchange and production economy where all the standard regularity conditions (that is, convexity of consumption and production sets, differentiability of utility and production functions, etc.) are assumed to hold. Under such assumptions, a unique set of marginal rates of substitution and/or transformation between pairs of goods and/or services can be associated to every Pareto-efficient allocation; such marginal rates can in turn be regarded as the rates of exchange between pairs of goods and/or services implicit in the allocation concerned; hence, a unique set of «shadow-prices» can be derived from any Pareto-efficient allocation. At this point, however, one might be led to conclude that, once a specific Pareto-efficient allocation has been chosen by the central authority of a hypothetical socialist economy, the set of «shadow-prices» implicit in that allocation can in principle be used by such authority to implement the chosen allocation. But then, granting that the above assumptions are accepted, the whole problem of socialist planning appears to boil down to the following: How can the central authority of a socialist economy come to know those «shadow-prices» that it ought to know in order to rationally plan the activities of an economy where at least some of the markets open in a capitalistic economy are missing?

It is precisely this question that the younger generation of socialist economists, acquainted with Mises's critique and conversant with «bourgeois» economics, tried to answer. Two alternative ways of tackling the problem were suggested in turn, the first in the late 1920s and early 1930s, and the second in the late 1930s. The two remaining stages of the SCD, in both of which Hayek took a very active part, revolved around the two proposed solutions, respectively[27]. While the first proposal, as well as Hayek's

[26] This idea is commonly traced back to an article by Barone (1908); it should be noted, however, that quite similar conceptions had already been put forward by Pareto (1896-7, Vol. II, 405 ff.). In this connection, two remarks are in order. First, as is well-known, the line of research originally outlined by Pareto and Barone, apart from affecting the stance taken by the socialist economists during the last two stages of the SCD, was also independently resumed and further developed in the late 1930s and subsequent years by a group of neoclassical economists, who turned Pareto's and Barone's original suggestions into a systematic branch of neoclassical economics which came to be known as «New Welfare Economics». Second, while it is beyond dispute that Pareto's and Barone's conceptions about equilibrium and efficiency exerted some influence on the socialist economists participating in the SCD, it is much more problematic to determine the precise nature of such influence, and especially to assess whether or not the ideas put forward by the socialist economists during the 1930s did really conform to those of their neoclassical predecessors. As will be seen, the latter question will become one of Hayek's central concerns from the last stage of the SCD onwards. Hence, to keep pace with the historical development of Hayek's ideas, we shall postpone the discussion of this issue to Sections 6 and 7 below.

[27] Hayek himself contributed three essays to the SCD: the first two constituted the introductory and concluding chapter, respectively, of the volume of collected papers *Collectivist*

criticism of it, will be examined in the remainder of this Section, discussion of the second one is postponed to Sec. 6 below.

The first socialist proposal, originally advanced by Taylor (1929) and Roper (1929) in the late 1920s, and later resumed by Dickinson (1933) and others in the early 1930s, rested on the idea that the calculation, as well as the allocation, problem arising in a centrally planned economy without markets of any sort might be solved by directly computing the numerical solution of a system of simultaneous equations defining the (static) equilibrium conditions of a suitably constructed competitive general equilibrium model, supposedly describing the structure of the economy concerned at a specified date. Or, as Hayek put it in 1935, the analyses of the supporters of that proposal «were directed to show that, on the assumption of a complete knowledge of all relevant data, the values and the quantities of the different commodities to be produced [in a centrally planned economy] might be determined by the application of the apparatus by which theoretical economics explains the formation of prices and the direction of production in a competitive system»[28].

As can be seen, the first socialist proposal is nothing but a straightforward application of that particular reading of the general equilibrium approach that has been called above the «first socialist interpretation of GET». Hence, it comes as no surprise that most of the objections raised by Hayek against the proposal concerned were in effect aimed at showing the untenability of the underlying interpretation of GET, according to which a general competitive equilibrium model of the economy can be viewed as a tool for computing the numerical values of the endogenous variables (essentially, prices and quantities of the various commodities to be traded and produced) at a certain moment of time. Four different kinds of objections

Economic Planning: Critical Studies on the Possibilities of Socialism, edited by Hayek himself in 1935 (in that volume the articles by Barone (1908) and Mises (1920) were also reprinted in English translation); the third appeared in *Economica*, 1940, under the title «Socialist Calculation: The Competitive 'Solution'». The three essays were later reprinted in Hayek (1949); in the following, they will be denoted as Hayek (1935c), (1935d), and (1940), respectively. While Hayek (1935d) put an end to the second stage of the SCD, the third and final stage was essentially concluded by Hayek (1940). After that date, of course, both Hayek and his socialist antagonists returned on a number of occasions on the themes discussed during the SCD, but the historical Debate as such was over. A few of Hayek's later contributions on this subject will be mentioned in the sequel.

[28] Hayek (1935d, 152). As a matter of fact, both Taylor (1929) and Dickinson (1933), beyond formulating the first socialist proposal along the lines specified in the text, also foreshadowed a few significant traits of the so-called «competitive solution», which however would not be formally spelled out before the second half of the 1930s. This is the reason why, as already remarked, the distinction between the second and third stage of the SCD is less clear-cut than it would appear to be from the «rational reconstruction» proposed by Hayek in his 1940 paper. In effect, in writing his 1935 contributions, Hayek himself was partly aware that the new socialist approach actually consisted of two distinct proposals. This can be seen from the fact that, after devoting the first part of Hayek (1935d) to a discussion of the first proposal, he reserved the last part of that paper (particularly, sections 8 through 10) to a critical review of a few «schemes» vaguely anticipating the «competitive solution» of the late 1930s.

were in effect put forward by Hayek (1935d), all of which revolved around the somewhat cryptic notion of «data» as employed by GET. We shall now take them up in turn, in order of increasing strength (from Hayek's own perspective).

The first criticism had to do with the actual computability of an equilibrium solution, under the most favorable assumption, provisionally made for the sake of discussion, that the central planning authority can somehow acquire that «complete knowledge of all relevant data» that is needed in order to write down a general equilibrium model which can, in principle, be computed. In this respect, Hayek simply pointed out that, if the degree of disaggregation of the model were to be even remotely comparable to that which can be seen to prevail in any real market economy, the number of the equations involved would be so large that, «with any of the means known at present [1935]», the task of actually computing a numerical solution and working out the concrete decisions which it implies «could not be carried out in a lifetime. Yet these decisions would not only have to be made continuously, but they would also have to be promptly conveyed to those who had to execute them»[29].

The second line of attack consisted in arguing that, for all practical purposes, the assumption provisionally made above is not to be taken seriously. According to Hayek, the crucial point to be stressed here is that the so-called «data», appearing in the equations of the general equilibrium model supposedly describing the structure of a given economy at a certain date, ultimately consist in the subjective knowledge of the individuals participating in that economy at that date. Hence, in practice, to assume that the central planning authority can get a «complete knowledge of all relevant data» is to suppose that there exists a way in which the subjective, fragmentary, and often inconsistent, knowledge dispersed among millions of different individual minds can be «concentrated in the heads of one or at best a very few people who actually formulate the equations to be worked out». But, in Hayek's opinion, «it is probably evident that the mere assembly of these data is a task beyond human capacity»[30].

As is well-known, the critical points made by Hayek in the two arguments summarized above were contemptuously dismissed by most of his socialist opponents, both during the SCD and afterwards, on the ground that those remarks merely pointed to a few «practical» difficulties, which could be confidently trusted to be overcome sooner or later, as a result of both the development of appropriate mathematical and statistical techniques, and the discovery and use of sufficiently powerful computing facilities. But, whatever one might think of this specific aspect of the controversy, it should

[29] Hayek (1935d, 156). It should be noted that Hayek did not even raise the question of the existence of an equilibrium solution, even if such question logically precedes the computability one. In effect, the existence problem was still a non-issue for most general equilibrium theorists in the mid-1930s.

[30] Hayek (1935d, 155-6).

be noted that Hayek (1935d) also singled out two further shortcomings of the «first socialist interpretation of GET», hence of the associated proposal, which cannot be easily played down as merely «practical» difficulties.

In effect, Hayek's third line of attack, though apparently starting as a simple re-elaboration of the second one, ended up by raising an essentially new question about the true nature of individual knowledge, hence about the interpretation of the notion of «data» typically implied by GET. In fact, in criticizing the idea that the dispersed knowledge available in society (especially, technical knowledge) can be concentrated into one single mind, he argued as follows:

> It is hardly necessary to emphasize that this is an absurd idea even in so far as that knowledge is concerned which can properly be said to «exist» at any moment of time. But much of the knowledge which is actually utilized is by no means «in existence» in this ready-made form. Most of it consists in a technique of thought which enables the individual engineer to find new solutions rapidly as soon as he is confronted with new constellations of circumstances. To assume the practicability of these mathematical solutions, we should have to assume that the concentration of knowledge at the central authority would also include a capacity to discover any improvement of detail of this sort[31].

As can be seen from the above passage, Hayek made in effect two distinct, though related, remarks. First, he explained that a significant part of the individuals' knowledge actually takes the form of «tacit» or «inarticulate» knowledge, that is, knowledge that is not consciously possessed even by those who do make use of it. But then, as the «data» of any general equilibrium model ought in principle to comprise also this sort of «tacit» knowledge, and as, on the other hand, «tacit» knowledge cannot be transmitted upon request, there is no way in which the corresponding part of the «data» can be made available to the central authority supposedly in charge of solving the equations of the model. Second, he stressed that «inarticulate» knowledge should in effect be viewed as a «technique of thought», by means of which concrete knowledge is progressively discovered under varying circumstances. This means, however, that the «flow» of concrete knowledge which is being discovered at any moment is at least as important as the «stock» of concrete knowledge which «can properly be said to exist» at that moment. But, by its very nature, GET is forced to insist on the «stock» part of concrete knowledge, to the prejudice of the «flow» part. Hence, by carrying the above reasoning to its extreme consequences (what Hayek was not yet prepared to do in 1935), one would be led to conclude that not only the «first socialist interpretation of GET», but GET itself, is bound to misrepresent the true nature and use of knowledge in society.

Finally, the fourth criticism levelled by Hayek at the «first socialist interpretation of GET» had to do with the fundamental problem of time and change in economics. In this respect Hayek pointed out that all the difficulties listed above (namely, the difficulty of computing a numerical solution,

[31] Hayek (1935d, 155).

of collecting the «data», and of making the people reveal their «tacit» knowledge) would be much less severe if one were allowed to suppose that the «data» of the economy will remain approximately unchanged over relatively long periods of time. Unfortunately, however, it is a basic fact of all modern economies that tastes, as well as technical knowledge and all the other so-called «data», «change from moment to moment»[32], so that the computation process would have to be continuously revised, thereby making the above difficulties insuperable in practice. But, once again, Hayek insisted that it would be wrong to play down the additional problem raised by the incessant change in the «data» as a merely «practical» difficulty. Rather, he suggested that the underestimation of that problem, characteristic of the supporters of the first socialist proposal, was the symptom of a deeper theoretical mistake, that is, of a serious misunderstanding of the true nature of GET; such misunderstanding, in turn, had been encouraged by the «excessive preoccupation with the conditions of a hypothetical state of stationary equilibrium», characteristic of «modern economists in general, especially those who propose this particular solution»[33]. In this way, Hayek's campaign against the «first socialist interpretation of GET» came to overlap with that attack against the stationary equilibrium approach (within GET) that he had started to develop a few years before.

5. Hayek's view of GET in «Economics and Knowledge» (1937)

As explained in the previous Section, in criticizing the «first socialist interpretation of GET» Hayek came to realize that the notion of «data», as had been traditionally employed in general equilibrium analysis, was very ambiguous and required further investigation. But, in view of the intimate connection between the «data» of GET and the knowledge of the individuals participating in the economy, Hayek's inquiries concerning the concept of «data» quickly developed into a study of the social processes through which the knowledge of the individual members of the economy is formed and changed over time. Thus, in the mid-1930s, the line of research inspired to Hayek by his involvement in the SCD most naturally merged with his preexisting concern about the relevance of equilibrium analysis to the explanation of processes taking place in time. By pursuing this joint research project, Hayek arrived at developing a new conception of GET, which was first systematically expounded in his 1937 paper «Economics and Knowledge».

Before discussing such new conception, it is convenient to briefly summarize Hayek's ideas on the scope and method of the social sciences in the strict sense of the word (also called the «social sciences proper»), as such

[32] Hayek (1935d, 155).

[33] Hayek (1935d, 167).

ideas underlie and shape the more strictly analytical parts of «Economics and Knowledge»[34].

As far as the social sciences proper are concerned, Hayek's fundamental methodological claim is that such sciences, owing to the peculiar nature of their subject-matter, are bound to adopt a distinctive method of their own (called the «compositive» or «individualistic» method), which partly differs from the method characteristic of some (though not all) of the natural sciences. According to Hayek, the peculiarity of the subject-matter of the social sciences proper lies in that they have to deal with two entirely different classes of phenomena, each of which raises its own specific problems: on the one hand, in fact, such sciences have to account for the conscious actions of the individual members of society, that is, they have to explain their intentional individual behavior; on the other, however, they have also to deal with the social results of the interactions taking place among many different individuals, that is, they have to explain those social processes which, being «the results of human action, but not of human design»[35], are «almost *ex definitione* not conscious»[36].

Given the twofold nature of their subject-matter, the theories of the social sciences proper are forced to use two different kinds of explanations. In particular, in dealing with intentional individual behavior they must necessarily start from the knowledge and beliefs of the individuals whose actions they try to explain, as «only what people know or believe can enter as a motive into their conscious action»[37]; for this reason, such theories cannot but be «subjectivistic», in the sense that their ultimate «data» must consist in the subjective opinions of the individual members of society, that

[34] Contrary to what has been suggested by Hutchison (1981) and a few other contemporary commentators, Hayek's methodological positions, unlike his theoretical positions, remained basically unaltered throughout the 1930s and 1940s. (For a refutation of Hutchison's strong discontinuity thesis, cf. Caldwell (1993).) Early statements of Hayek's views on the methodology of the social sciences can be found in Hayek (1933a) and (1933b). The ideas first synthetically expressed in those writings were then resumed in Hayek (1935c, esp. Sections 2, 3), and further developed in «Economics and Knowledge» two years later. Hayek's methodological positions were eventually given a systematic formulation in a long essay in three parts, which appeared in *Economica* in the period 1942-44; cf. Hayek (1942), (1943), and (1944). After that date, Hayek repeatedly came back to discuss methodological issues; but even his later contributions on this subject only brought a few minor qualifications to his previous conception, which was left substantially unchanged. As a thorough discussion of this part of Hayek's thought would exceed the scope of this paper, in the following we shall strictly confine our exposition to those specific aspects of Hayek's methodological stance that are necessary for understanding his theoretical positions in 1937 and subsequent years; in so doing, we shall occasionally use a few concepts and quotations drawn from Hayek's 1942-44 methodological essay, which more clearly express some ideas that were already present in his 1937 paper, though in a less explicit and definitive form.

[35] This sentence, drawn from Adam Ferguson's *Essay on the History of Civil Society* (1767), has been repeatedly used by Hayek in relatively recent times to summarize his interpretation of the nature of social formations. Cf., e.g., Hayek (1967b).

[36] Hayek (1944, 31).

[37] Hayek (1942, 284).

is, in that partial, specific, and possibly defective, knowledge on which the individuals found their intentional behavior. But, when it comes to explaining the overall processes taking place in the social world, a different sort of explanatory scheme is required: for, since the social processes are undesigned and often uncomprehended by anybody, any explanation based on the intentions of some individual member of society would be utterly out of place in this connection. Yet, the social sciences have to recognize that, independently of the will and intentions of any single individual, some sort of «spontaneous order» tends to emerge in most social processes: any such order consists in a stable pattern of relationships between the actions of the individuals participating in the process and, at the same time, represents the condition for the achievement of the ends at which those individuals aim. In most cases, however, such an order cannot be directly observed by the social scientist; it can only be theoretically reconstructed by a deliberate effort of directed thought, starting from the simple elements of which the comparatively more complex phenomenon is composed, that is, from the actions of the individuals participating in the process. According to Hayek, it is precisely this method of theoretically reconstructing the so-called «social wholes» from their constituting elements, called the «compositive» method, that represents the hallmark of the social sciences proper.

From this perspective, the main problem facing the social sciences is how to combine into a coherent theoretical framework the two kinds of explanatory schemes they have to use in order to account for both the intentional and the unintended phenomena falling under their jurisdiction. And it was precisely the question of whether, and to what extent, the social sciences proper had been able to cope with their fundamental problem that was addressed by Hayek in «Economics and Knowledge». In that paper, however, rather than discussing that question in its most general and abstract terms, he chose to turn his attention specifically to economics, that he then regarded as the most advanced among the social sciences proper.

By looking at the actual practice followed by past and contemporary theoretical economists, Hayek quickly arrived at the conclusion that most of their efforts had been directed to the preliminary task of analyzing the rational choices made by the individual members of the economy. He further remarked that, by taking a consistently «subjectivistic» stance, that branch of economic theory that is concerned with the individual process of rational choice — a branch that he called the «Pure Logic of Choice» in his 1937 paper, and will later rename «Economic Calculus» — had been able to make great progress in the recent past. But, according to Hayek, its achievements ought not to be unduly emphasized: for the Pure Logic of Choice is nothing but a «system of tautologies» which, taken by itself, is unable to explain any observable phenomenon, and cannot consequently be regarded as an empirical theory[38].

[38] Cf. Hayek (1937, 34-5 and 43, fn. 2). As a matter of fact, when Hayek spoke of the merely «tautological character of the Pure Logic of Choice», he had three different things in mind, which should be carefully distinguished (but were unfortunately mixed up in Hayek's own discussion). First, he wanted simply to point out that the part of economic theory dealing with the individual process of rational choice is an essentially deductive theoretical system, i.e., a system made up of statements which have the logical status of deductive arguments; hence, as any valid argument is a tautology from a logical point of view, to the extent that the Pure Logic of Choice actually makes use of valid arguments, it can trivially be said to consist of tautologies (Hayek (1937, 36)). Second, he apparently wanted to suggest that the general laws and assumptions (especially, the so-called rationality axiom) used in the premises of the arguments of which the Pure Logic of Choice consists are «*a priori* true», insofar as they «are facts which we know to be common to all human thought» (Hayek (1937, 46)). This is, in our opinion, the weakest part of Hayek's reasoning, the only part where the influence of Mises' aprioristic, praxeological ideas makes itself felt directly. But there is still a third sense in which Hayek spoke of the «tautological character of the Pure Logic of Choice»: what he wanted to stress, in this case, is that the corresponding part of economic theory, taken by itself, is definitely not an empirical theory. This third meaning, which is the most interesting of all, is the one on which we shall focus attention in the text.

What Hayek meant by this can be easily seen by considering the characteristic structure of the explanations offered by the Pure Logic of Choice. In any such explanation, the first thing the theorist has to do is to take as given the subjective knowledge and preferences of the individual whose choices are to be explained; then, starting from such «subjective data», he can proceed to logically deduce the plan (or plans) of action rationally chosen by that individual under the assumed conditions. But, if this is the typical structure of the explanations that the Pure Logic of Choice can provide, it should be clear that such theory can neither explain how the individuals come to possess the knowledge they are supposed to have, nor account for the individuals' observable behavior. For what any such explanation can at most account for is the plan of action that an individual would rationally choose to carry out under the postulated circumstances; but, as the chosen plan may well be unfeasible (given the subjective character of the individual's knowledge), that plan will not necessarily coincide with the individual's observable behavior. It follows that, if economics is to be a truly empirical science, the Pure Logic of Choice has to be supplemented by a different sort of theory, one aimed at analyzing the social processes through which a) knowledge is created and conveyed to the individual members of the economy, b) the plans of action rationally chosen by the various individuals are turned into observable actions, and c) individual choices are progressively revised in the light of experience.

It is at this point that, in Hayek's opinion, GET had to be brought explicitly into the picture. For, as he viewed it in 1937, among all the diverse theories of economics, GET was undoubtedly the one that had most seriously taken upon itself the task of combining the Pure Logic of Choice, characteristic of all modern (neoclassical) economics, with a study of the social interactions taking place among many different individuals in a market economy. But then the following question naturally arose: Had GET really been able to provide a satisfactory solution to the fundamental problem of the social sciences proper?

Before trying to answer this question, however, Hayek had to fulfill a preliminary task: precisely, he had to clarify the meaning of the equilibrium notion as employed by GET, what in turn could only be done by putting forward a general definition of the abstract equilibrium concept underlying all the particular models belonging to the general equilibrium approach, broadly conceived. This task was in effect accomplished in «Economics and Knowledge», where the abstract properties characterizing the equilibrium notion, as employed by GET, were clearly stated for the first time in the history of the subject: in fact, a general economic equilibrium was there defined as a particular state of the economy at a certain point of time, in which the subjective knowledge and beliefs of the various individuals participating in the economy are arranged in such a way that the plans of actions rationally chosen by all the individuals, on the basis of such knowledge and beliefs, are compatible with each other, as well as with the external constraints posed by the environment, so that they can all be carried out[39]. In the light of this interpretation of the equilibrium notion, Hayek could then proceed to evaluate the capability of GET to serve as a robust foundation for an empirical theory of society.

In this respect, one can easily see that GET, when interpreted as Hayek suggested, is in effect able to overcome one characteristic limitation of the Pure Logic of Choice: for, by assuming the economy to be in equilibrium, GET is no longer confined to merely explaining the plans of action rationally chosen by the individuals participating in it, but it is able, at least in principle, to explain their observed behavior as well. However, as Hayek did not fail to point out, this result can only be achieved by assuming an equilibrium to prevail in the economy, that is, by assuming the knowledge and intentions of the various individuals to be arranged in a very special way. But to assume this special arrangement of the individuals' knowledge and intentions, as is typically done in GET, is something altogether different from explaining the process through which such arrangement is, or can be, brought about. Yet,

[39] Cf. Hayek (1937, 39-41). With respect to this definition two remarks are in order. First, it should be noted that Hayek in effect provided two nested definitions of the equilibrium notion: a «stronger» one, which is the one given in the text, and a «weaker» one, requiring the mutual compatibility of all the individual plans, but not necessarily demanding the compatibility of such plans with the «objective» constraints posed by the external environment. We have opted here for the «stronger» definition, as only in this case can all the equilibrium plans be actually carried out, so that only with this definition can equilibrium theory be legitimately said to explain the individuals' observable behavior, and consequently to possess some empirical content as well (which, as will be seen, is very important from Hayek's own viewpoint). Second, it should be pointed out that Hayek's definition is in effect so general as to apply not only to the equilibrium concept as employed by GET, but also to the related, yet not identical, equilibrium concept that is characteristically used by Game Theory. In effect, there are a few passages in «Economics and Knowledge» where Hayek appears to lean towards a game-theoretic interpretation of the equilibrium notion (cf., e.g., Hayek (1937, 38)). However, we shall focus on the GET interpretation in the following, as (a) it seems to be the prevailing one on the whole, and (b) it is the one with which Hayek will almost exclusively be concerned in his subsequent writings.

it is precisely the latter kind of explanation that ought to be provided in order to turn General Equilibrium Theory into a truly empirical endeavor. As Hayek put it:

> We shall not get much further here unless we ask for the reasons for our concern with the admittedly fictitious state of equilibrium. Whatever may occasionally have been said by over-pure economists, there seems to be no possible doubt that the only justification for this is the supposed existence of a tendency towards equilibrium. It is only with this assertion that economics ceases to be an exercise in pure logic and becomes an empirical science [...].
> In the light of our analysis of the meaning of a state of equilibrium it should be easy to say what is the real content of the assertion that a tendency towards equilibrium exists. It can hardly mean anything but that under certain conditions the knowledge and intentions of the different members of society are supposed to come more and more into agreement [...]. The only trouble is that we are still pretty much in the dark about (a) the *conditions* under which this tendency is supposed to exist, and (b) the nature of the *process* by which individual knowledge is changed[40].

Of course, in writing his 1937 paper, Hayek was perfectly aware of the many attempts that had been made in the more or less recent past, within the framework of stationary equilibrium theory (chiefly of the competitive type), in order to account for the process supposedly driving the economy towards a stationary equilibrium state. Yet, as remarked in Sec. 3, in the early 1930s he had already arrived at the conclusion that such efforts had led economic theory to a dead end. In fact, as Hayek (together with Lindahl and Hicks) had come to realize a few years before, stationary equilibrium theory could only be consistently applied to a very limited set of economic phenomena, to which all the interesting «dynamic» phenomena at the center of the theoretical debate in the inter-war period did not belong. Moreover, in the mid-1930s, once again paralleling similar developments in Lindahl's and Hicks's thought, Hayek had begun to perceive that the unwarranted attempts to surreptitiously extend the scope of stationary equilibrium theory beyond its intrinsic limits had led to a number of mistakes that had seriously hampered the development of theoretical research, particularly in the crucial fields of money, capital, and interest. Finally, in his 1937 paper, Hayek struck a final blow to the stationary equilibrium approach, by explaining that, when the equilibrium notion is properly analyzed, «it becomes exceedingly difficult to say what exactly are the assumptions on the basis of which we assert that there will be a tendency towards equilibrium, and to claim that our analysis has an application to the real world»; what is sure, according to Hayek, is that the common assumption of the «constancy of the objective data is neither a necessary nor a sufficient condition [...] for the establishment of an equilibrium»[41], so that the traditional motivation for the adoption of a stationary equilibrium notion loses all raison d'être.

This is the reason why, in «Economics and Knowledge», Hayek explicitly and definitively rejected the still prevailing notion of a stationary equili-

[40] Hayek (1937, 43-4).

[41] Hayek (1937, 47).

brium, returning instead to that instantaneous interpretation of the general equilibrium concept (essentially, as explained above, a temporary equilibrium notion with perfect foresight) that he had originally suggested in 1928 and taken up again in 1933. In this regard he wrote:

> For a society then we *can* speak of a *state* of equilibrium at a point of time — but it means only that compatibility exists between the different plans which the individuals composing it have made for action in time. And equilibrium will continue, once it exists, so long as the external data correspond to the common expectations of all the members of the society. The continuance of a state of equilibrium in this sense is then not dependent on the objective data being constant in an absolute sense, and is not necessarily confined to a stationary process. Equilibrium analysis becomes in principle applicable to a progressive society and to those inter-temporal price relationships which have given us so much trouble in recent times[42].

Yet, while insisting that an equilibrium state should be referred to a given instant in the history of the economy, without implying any stationarity of either the environment or the economic variables, Hayek still cherished the hope that such instantaneous equilibrium approach might eventually be supplemented with the analysis of an equilibration process taking place in time, a step on which the empirical character of GET was made to depend in a crucial way.

6. Hayek's criticism of the «second socialist interpretation of GET» (1940)

While Hayek was busy at investigating the processes of diffusion of knowledge, with a view to specifying the conditions under which GET might be turned into an empirical theory, he became aware that a new proposal, allegedly solving the calculation problem arising in a socialist economy of some sort, had been advanced by a group of socialist economists, later called the «market socialists», whose best-known representatives were Lange (1936) and (1937), Lerner (1937) and (1938), and Dickinson (1939). Such a proposal, also known as the «competitive solution», was particularly interesting from Hayek's own viewpoint, for it rested on an apparently «realistic» reinterpretation of GET, and especially of the Walrasian *tâtonnement* process, and aimed to show that such a process, when suitably reinterpreted, could indeed be employed to convey the relevant knowledge to the individual members of the economy, thereby concretely effecting that tendency towards the establishment of a state of general equilibrium of the economy on which, according to Hayek, the empirical character of GET essentially turned.

[42] Hayek (1937, 41). To the above quoted passage the following footnote was appended: «This separation of the concept of equilibrium from that of a stationary state seems to me to be no more than the necessary outcome of a process which has been going on for a fairly long time. [...] The idea of a state of equilibrium which was not a stationary state was already inherent in my «Das intertemporale Gleichgewichtssystem ...» and is, of course, essential if we want to use the equilibrium apparatus for the explanation of any of the phenomena connected with 'investment' [...].»

As a matter of fact, the «market socialists» continued to use a simple general equilibrium model as their basic theoretical framework. But, unlike their socialist predecessors in the preceding stage of the SCD, they did not claim that an equilibrium solution could be directly computed by numerically solving the system of simultaneous equations defining the equilibrium conditions of the model; rather, they claimed that such a solution could be approached by trial and error, that is, by means of an iterative adjustment process which was nothing other than a «socialist» reinterpretation of the Walrasian *tâtonnement* process (with a few significant changes to which we shall return presently). In essence, the kind of economy they had in mind[43] was a sort of decentralized socialist economy, in which consumers' and workers' choices would be free, whereas production would be organized by State firms and/or industries run by managers abiding by competitive-like rules (in particular, the managers of individual plants, assumed to be single-product, would be instructed to choose the cost-minimizing technique for any level of output and to expand production until marginal costs are equal to price; the directors of the State industries would instead be required to meet more complex conditions concerning the optimal choice of the amount of equipment for each industry). Further, all prices (other than those of consumers' goods and labor services, to be determined in true competitive markets) would be quoted by a central authority, and changed by the same authority, according to some rule analogous to the so-called «law of supply and demand», whenever the excess demands occurring at the quoted prices were to differ from zero.

The «market socialists» were convinced that their models of decentralized planning would shun the objections previously levelled, by Hayek and others, at the socialist proposals of centralized planning. Moreover, they held that their «competitive solution» would allow one not only to ground the socialist organization of economic activities on a rational basis, but also to remove a number of problems pestering all capitalistic market economies (in particular, the so-called problem of the «anarchy of capitalistic production» and that of distributive inequalities).

As is well-known, Hayek rejected such claims as theoretically unsubstantiated and ultimately deceptive in his 1940 paper, specifically devoted to a critical discussion of the approach of the «market socialists». Yet, in that paper, before developing his critique of the «competitive solution», as well as of the «second socialist interpretation of GET» on which such «solution» rested, Hayek deemed it necessary to return to an unsettled question concerning the «first socialist interpretation of GET», a question which had been bothering him for a few years. What troubled Hayek was that a few socialist economists (including some of the «market socialists») had been

[43] In effect, the various plans put forward by the «market socialists» exhibited a few differences of detail as to the actual working of the socialist economy to be constructed. In the following we shall chiefly focus on Lange (1936) and (1937), who worked out the most comprehensive and consistent plan.

suggesting that, after all, the idea of using a general equilibrium model as a tool for directly computing the numerical values of the endogenous variables of the economy was already implicit in Pareto's and Barone's original discussions of the calculation problem, so that the «first socialist interpretation of GET» could ultimately be traced back to the very founders of the general equilibrium approach. That suggestion particularly disturbed Hayek as it implied a reconstruction of the history of GET (and, in the last analysis, of its significance as well) that was wholly at variance with what he had been maintaining in his 1935 paper and afterwards. In view of this, one can easily understand Hayek's intimate satisfaction when, in 1940, he was eventually able to disprove that historical reconstruction by quoting a long passage by Pareto (1906), where the latter, after dismissing as an «absurd hypothesis» the idea that one might try to numerically solve the equations of a general equilibrium model, concluded his discussion by saying that «if one really could know all these equations, the only means to solve them which is available to human powers is to observe the practical solution given by the market»[44].

After disposing of the question of the alleged conformity of the «first socialist interpretation of GET» to the ideas of the founders of the general equilibrium approach, Hayek was finally free to launch his attack against the «competitive solution» proper. In effect, in his 1940 paper, he raised two distinct kinds of objections against that «solution». In the first place, he pointed out that the implementation of the method devised by the «market socialists» for determining the equilibrium prices would, in effect, demand a lot of monitoring from the central authority. Due to the lack of incentives and sanctions automatically provided by the competitive market mechanism, the only way by which such authority might hope to force the State managers to behave according to the prescribed rules would be to subject their behavior to systematic inspection. But, in order to assess the conformity of the managers' behavior to the rules, the authority ought to possess a detailed knowledge of the special circumstances of time and place under which they operate. From this it follows, however, that any attempt to actually implement the «competitive solution» would inevitably raise the problem of centralizing all the dispersed and «tacit» knowledge of society in a single mind, so that, in the last analysis, the same criticism levelled at the first proposal would apply to the second one as well[45].

Apart from this, however, Hayek also developed a second line of attack, which is particularly interesting for our present purposes. In fact, in discussing the adjustment process envisaged especially by Lange, a process made up

[44] Cf. Hayek (1940, 181-2). A couple of years later, Hayek was also able to discover a passage by Cournot, making quite similar remarks, which he immediately appended to the already used quotation from Pareto in order to reinforce his thesis; cf. Hayek (1942, 291, fn. 1). In this regard, it should be noted that he might have strengthened his position further by citing the passage by Walras mentioned in footnote 12 above; but, evidently, he was unaware of its existence.

[45] Cf. Hayek (1940, esp. 189, 196-9, 201-3).

of successive revisions of prices carried out by the central authority and of consequent changes in plans effected by the production managers, he forcibly underlined the shortcomings and inconsistencies implicit in that otherwise ingenious attempt to provide a «realistic» reinterpretation of the artificial construct of the Walrasian *tâtonnement*.

It should be recalled, at this point, that the interpretation of the *tâtonnement* construct originally suggested by Walras (in the fourth edition of the *Eléments*) had been all but «realistic»: in effect, as remarked in Sec. 2 above, Walras had been led to interpret the *tâtonnement* process as a purely virtual process, taking place in a sort of «logical» time and consuming no amount of «real» time to carry its effects through. But, when Lange resurrected the Walrasian *tâtonnement* in the second half of the 1930s, he was forced, for obvious reasons, to adopt a much more concrete interpretation of that construct. Hence Lange, unlike Walras, had to envisage his «socialist» *tâtonnement* as a process taking place in «real» time. Not only that, but he had also to fill an apparent gap in Walras's original story: Walras, in fact, had refrained from specifying who should be in charge of changing the prices in the course of the process; Lange, instead, had to assign that task to a well-defined entity, the Central Planning Board, who was then to become the ancestor of a much more ethereal character playing a central role in all the later formalized versions of the *tâtonnement* story, namely, the «auctioneer». In conclusion, Lange's version of the *tâtonnement* represented the first, and in a sense also the last, attempt to provide a fully-fledged interpretation of the competitive adjustment process as a «real» process taking place in «real» time in a supposedly «real» economy (albeit a «socialist» one)[46]. In so doing, however, Lange unwillingly laid bare all the difficulties inherent in that construct, which could only be concealed in the artificial world originally devised by Walras.

Such difficulties were clearly grasped and criticized by Hayek. In essence, Lange's construction was to be blamed for the following: first, it was unable to explain why the production managers should «stupidly» behave as price-

[46] It should be noted, in this respect, that a few years before, in a relatively obscure article published in 1934, Hicks had been able to single out the only two sorts of situations (within the framework of a competitive, pure-exchange economy) in which the *tâtonnement* story makes sense from a logical point of view. As Hicks (1934, 343) put it: «Walras' system of prices will be reached, either if contracts are made provisionally or (a more important case) if people come on to the market on successive 'days' with the same dispositions to trade and there is no carry-over of stocks [...] from one day to the next.» As can be seen, Hicks's first case was nothing but Walras's original case of a purely virtual process in «logical» time, where no observable behavior is allowed to take place out of equilibrium. Hicks's second case, instead, represented the first explicit interpretation to be found in the literature of the Walrasian *tâtonnement* construct as a process taking place in «real» time. Hicks, however, made it very clear that such interpretation could only be consistently applied to a very special type of economy, that is, a pure-flow economy of the sort discussed in the so-called «Walras-Cassel model». The limitations intrinsic to any «realistic» interpretation of the *tâtonnement* story were instead completely passed over by Lange, with the consequence that the model put forward in his 1936-37 paper was logically inconsistent.

takers, while knowing that the prices would be changed by the Central Planning Board once out of equilibrium[47]; second, it was unable to account for the agents' disequilibrium behavior, though one ought honestly to recognize that, with the proposed method of price fixing and changing, disequilibrium would indeed be the norm in such an economy[48]; third, in order to make the equilibrium position determinate, it was forced to inconsistently assume away all changes in the «data», even those changes that would necessarily ensue from the agents' out-of-equilibrium behavior[49].

Once again, as he had already done five years before, Hayek suggested that the underestimation of all the above problems by the «market socialists» might be due to their «excessive preoccupation with problems of the pure theory of stationary equilibrium»[50]. But in this case, besides reiterating his by then customary criticism of the stationary equilibrium approach, he also added a few remarks cautiously revealing his mounting uneasiness with the whole of general equilibrium analysis. This is particularly evident in his pointing out that Lange's «excessive preoccupation with concepts of pure economic theory, [especially with] the concept of perfect competition», and his insistence on the so-called «parametric function of prices» had resulted in his «failure to understand the true function of the price mechanism»[51]: for, as is well-known, the «parametric function of prices», as well as the associated conception of «perfect competition», are by no means a distinctive feature of the stationary equilibrium approach alone; rather, they characterize the whole of general competitive analysis in the spirit of GET.

7. The change in Hayek's view of GET in the early 1940s

The critical conclusions arrived at by Hayek in commenting upon Lange's reinterpretation of the Walrasian *tâtonnement* as a process taking place in «real» time most probably contributed to shake his faith in the possibility of supplementing instantaneous equilibrium theory with an analysis of the dynamic processes that are supposed to bring an equilibrium state about. In any case, such faith had been entirely lost by 1941, when he finally published his great book *The Pure Theory of Capital*. Here, in fact, by completely reversing his 1937 position, and embracing instead that stance that he had sharply criticized in «Economics and Knowledge» as characteristic of a few «over-pure economists» uninterested in the empirical content of the theory, Hayek explicitly maintained that the general equilibrium concept ought to be viewed as a mere «intellectual tool». To this he added that

[47] Cf. Hayek (1940, 197-8).

[48] Cf. Hayek (1940, 193-4 and 197).

[49] Cf. Hayek (1940, 188).

[50] Hayek (1940, 188).

[51] Cf. Hayek (1940, 188, 191, and 193).

> it seems to be a weakness of the traditional use of the concept of equilibrium that it has been confined to cases where some specious 'reality' could be claimed for it. In order to derive full advantage from this technique we must abandon every pretence that it possesses reality, in the sense that we can state the conditions under which a particular state of equilibrium would come about[52].

Yet, though reduced to a «technique» devoid of empirical content, GET was still regarded by Hayek as an important intellectual achievement. As a matter of fact, in 1942, in the first instalment of his long essay (in three parts) on the methodological problems of the social sciences, he tried to specify what kind of knowledge one can legitimately expect to obtain from GET and what one cannot. To this end, he introduced a distinction between two different kinds of explanations, which might be called «explanations merely of the principle» and «explanations of the precise result», respectively. According to Hayek, while the latter type of explanations are used in all kinds of disciplines, the former are characteristic of the social sciences proper or, more generally, of all the empirical sciences dealing with intrinsically complex phenomena[53]. This is because

> the number of separate variables which in any particular social phenomenon will determine the result of a given change will as a rule be far too large for any human mind to master and manipulate them effectively. In consequence our knowledge of the principle by which these phenomena are produced will rarely if ever enable us to predict the precise result of any concrete situation. While we can explain the principle on which certain phenomena are produced and can from this knowledge exclude the possibility of certain results, e.g. of certain events occurring together, our knowledge will in a sense be only negative, i.e. it will merely enable us to preclude certain results but not enable us to narrow the range of possibilities sufficiently so that one remains.

To this Hayek added:

> The distinction between an explanation merely of the principle on which a phenomenon is produced and an explanation which enables us to predict the precise result is of great importance for the understanding of the theoretical methods of the social sciences. [...] The best illustration [of the first type of explanation] in the field of the social sciences is probably the general theory of prices as represented, e.g., by the Walrasian or Paretian system of equations. These systems show merely the principle of coherence between the prices of the various types of commodities of which the system is composed; but without knowledge of the numerical values of all the constants which occur in it and which we never do know, this does not enable us to predict the precise results which any particular change will have[54].

[52] Hayek, (1941, 28).

[53] Apparently, in 1942 Hayek was not yet fully aware that the use of the «explanations merely of the principle» does not only characterize the social sciences proper, but also those natural sciences that are concerned with complex phenomena and aim to explain the emergence of some sort of «order» in their respective fields of inquiry. This point, however, was clearly spelled out a few years later, especially in Hayek (1955) and (1964).

[54] Hayek (1942, 290-1). It should be noted that the above statements simply represent the methodological rationalization of the criticism that Hayek had raised a few years before against the «first socialist interpretation of GET». In effect, the already mentioned quotation from Pareto, where the latter forcefully denies all possibility of using GET as a computational device, was appended by Hayek as a footnote to the passage cited in the text.

But, even if GET could still be defended as an «explanation merely of the principle», the negative conclusions that Hayek had reached in 1941 about the empirical content of equilibrium analysis forced him very soon to face the following dilemma: on the one hand, there was pure economic theory, hinging on an abstract and «unrealistic» equilibrium construct for which no empirical justification could be offered; on the other, there were the all-important dynamic processes of creation and diffusion of knowledge, of coordination of individual plans and actions, etc., which had certainly represented the original justification for, and the implicit foundation of, General Equilibrium Theory, but could apparently not be accounted for within that framework, at least in a formal way. Confronted with this dilemma, Hayek gradually worked out a possible solution: precisely, he came to the conclusion that, if the dynamic processes of the real world could not be analyzed by means of the tools and methods of pure economic theory, one had to give up that theory, replacing it with an alternative theoretical system. Furthermore, since pure economic theory rested on the equilibrium concept, one also had to abandon that concept, using instead some other notion as the cornerstone of a new theory of society. The notion suggested for playing that ambitious role was the old notion of a «spontaneous order», a notion which had been embryonically present in Hayek's theoretical system since his early days, but could only be adopted as the organizing concept of his research activity after twenty years devoted to investigating the powers and shortcomings of General Equilibrium Theory.

As explained above, the first reason for Hayek's dissatisfaction with a theory centered on the equilibrium construct lay in his persuasion that such a theory would be unable to cope with the dynamic processes taking place in the social world, a persuasion he had eventually arrived at in the early 1940s after so many unsuccessful attempts to find a possible way-out. But, at about the same time, after much thought about the arguments put forward by the economists siding up with either party during the SCD, he must also have come to the conclusion that the equilibrium technique characteristic of economics was quite dangerous in two further respects, to which he had not paid enough attention before. In effect, Hayek has never been quite explicit on the reasons which led him to change his theoretical perspective in the early 1940s, and especially to abandon equilibrium analysis from the mid-1940s; so that the identification of the two further reasons which most probably convinced him to part company with the general equilibrium approach has necessarily to rest on a sort of «reconstruction from outside» of the development of his thought. We deem, however, that the reconstruction that will be suggested in the following is strongly supported by the available textual evidence[55].

[55] Hayek has also been very reticent on a fourth reason which most probably contributed, though to a lesser extent than the others, to explain his withdrawal from «technical economics» after the mid-1940s. In effect, in the early 1940s Hayek must have realized that his long-standing project of building a unified theory of the trade cycle, money, capital, and equilibrium, had ended up in a failure. But, even if that eventual failure almost certainly influenced Hayek's decisions as to the subsequent development of his scientific research, he never admitted it explicitly.

According to our reconstruction of Hayek's thought, the first dangerous implication of GET would follow from the very structure of that theory. As remarked above, during the SCD Hayek had convincingly refuted the «first socialist interpretation» of the Walrasian competitive equilibrium model as «an explanation of the precise result», arguing instead that the only reasonable interpretation of that model — which incidentally was the same as Walras's and Pareto's original one, as Hayek was able to prove in 1940 — is to view it as «an explanation merely of the principle». However, upon further reflection, he must have come to the conclusion that GET is in effect so structured as to allow for (or, at least, not to discourage) something like the «first socialist interpretation» of it. Such theory, in fact, is characterized by a potentially deterministic formal structure. That is, it is formalized in such a way as to suggest, albeit implicitly, that one can actually use it to associate an equilibrium configuration of the economic variables (quantitatively specified in its minutest details) to any given configuration of the «data» (similarly expressed in quantitative terms). But then the fact that Walras and Pareto had been far from regarding General Equilibrium Theory as a tool for computing equilibrium prices and quantities could not cancel the fact that they had built a theory which dangerously lent itself to being interpreted as an «explanation of the precise result», rather than as an «explanation merely of the principle». And, as Hayek came to think in the early 1940s, this possible confusion was in itself a sufficient reason for keeping one's distance from the equilibrium approach.

At about the same time Hayek became fully aware of a second danger ensuing from equilibrium analysis, a danger which he had already vaguely perceived before. Precisely, he began to be deeply concerned with the fact that, by overemphasizing the significance of the equilibrium positions of the economy, such analysis tended to direct the theorist's attention away from the more fundamental questions concerning the out-of-equilibrium behavior of the system. Once more, this issue had clearly emerged during the SCD. In fact, most economists taking part in that controversy on either side had essentially couched their arguments in terms of the properties of the equilibrium position to be eventually reached by either method: thus, many supporters of the «competitive market mechanism» had confined themselves to praising the efficiency properties of competitive equilibrium, whereas the advocates of socialism had tried to show that a competitive equilibrium could be more easily obtained by means of some sort of «socialist planning». But, as Hayek had first pointed out in his 1935 contribution, and later restated in his 1940 paper[56], the superiority of the «competitive market mechanism» over «socialist planning», or vice versa, would ultimately depend not on the properties of an equilibrium position, which would never be reached in either case, but rather on the disequilibrium

[56] Cf. Hayek (1935d, 156-7) and (1940, 188).

behavior of the economy. And, according to Hayek, it was in this latter sense, and in this sense only, that the claim for the superiority of the «competitive market mechanism» could be legitimately laid, owing to the special fitness of that «mechanism» for quickly reacting to the continual changes in the economic environment and the ensuing disequilibrium[57].

8. Concluding remarks

To sum up, the most important reasons explaining Hayek's change in perspective in the early 1940s, as well as his withdrawal from the field of pure economic theory, and especially of equilibrium analysis, from the mid-1940s, were essentially three, all of which strictly related to his participation in the SCD. First, his critical discussion of Lange's «realistic» reinterpretation of the Walrasian *tâtonnement* in the last stage of the Debate reinforced the suspicions he already had about the possibility of turning GET into a truly empirical theory, relevant to the analysis of the economic processes taking place in time. Second, in reconsidering the use which had been made of GET during the SCD, especially by the upholders of the «first socialist interpretation of GET», he came to perceive the dangers inherent in a fully deterministic theory, which however could not possibly keep the predictive (quantitative) promises which it apparently made. Third, in reviewing the whole trend of the discussion during the SCD, he came to realize that, by overstressing the relevance of the equilibrium construct, GET could seriously distort the economists' judgment as to what is really important and what is not in the economic process.

As a final remark, we want to mention a further argument which indirectly supports the above reconstruction. In effect, if one examines the new concept of a «spontaneous (economic) order», by which Hayek tried to replace the equilibrium concept in his theoretical research from the mid-1940s onwards, one immediately realizes that such concept was so devised as to dodge precisely those three shortcomings of the equilibrium concept, as employed by GET, that Hayek had come to discover through his involvement in the SCD. In the first place, in fact, a «spontaneous (economic) order», as defined by Hayek, is a qualitative relational structure, to which a number of different quantitative relationships may correspond: hence, no one can ever be misled into interpreting a theory based on such construct as «an explanation of the precise result», which instead may easily happen with GET. In the second place, as Hayek explicitly pointed out in some of his later writings[58], while it is possible to maintain that an order is «preserved throughout a process of change», the same cannot of course be said of an equilibrium state. This means, however, that the first concept,

[57] This was to become one of Hayek's central themes in later years. Cf., e.g., Hayek (1945), (1946), and (1968).

[58] Cf., e.g., Hayek (1968, 184).

unlike the second, can be employed for theoretically discussing those dynamic processes which, in Hayek's opinion, are so central to the social sciences. Finally, the notion of an «economic order», as defined by Hayek, is consistent with the existence of a disequilibrium situation in the economy; it may even be added that the occurrence of a certain amount of disequilibrium is a necessary condition for the preservation of an «economic order», an occurrence that cannot possibly be true of the equilibrium concept as employed by GET. Of course, what is gained in flexibility and fruitfulness by using the concept of a «spontaneous order», is lost in precision and determinateness. But since, according to Hayek, the deterministic character and predictive power of equilibrium analysis are ultimately deceptive, such a loss should not be really regretted.

Department of Economics
University of Milan

BIBLIOGRAPHY

Where two dates are given for a reference, the earlier date (in brackets) is the year of publication of the original edition, while the second date refers to the edition that is actually cited. The earlier date is always given in the text to highlight the historical timing of the contribution.

Barone E. (1908), «Il ministro della produzione nello stato collettivista», *Giornale degli economisti*, 2, 267-93, 391-414; translated into English as «Ministry of Production in the Collectivist State», in Hayek (1935b), Appendix.

Butos W.N. (1985), «Hayek and General Equilibrium Analysis», *Southern Economic Journal*, 52, October, 332-43.

Caldwell B.J. (1988), «Hayek's Transformation», *History of Political Economy*, 20, 4, 513-41.

— (1993), «Hayek the Falsificationist? A Refutation», *Research in the History of Economic Thought and Methodology*, forthcoming.

Cassel G. (1918), *The Theory of Social Economy*, Augustus M. Kelley, New York, 1967.

— (1925), *Fundamental Thoughts in Economics*, T.F. Unwin ltd., London.

Clark J.B. (1899), *The Distribution of Wealth. A Theory of Wages, Interest and Profit*, Augustus M. Kelley, New York, 1965.

— (1907), *Essentials of Economic Theory*, Macmillan, New York.

Dickinson H.D. (1933), «Price Formation in a Socialist Community», *Economic Journal*, 43, June, 237-50.

— (1939), *Economics of Socialism*, Oxford University Press, Oxford.

Donzelli F. (1988), «Introduzione», in Hayek (1988), 9-91.

— (1990), *The Concept of Equilibrium in Neoclassical Economic Theory. An Inquiry into the Evolution of General Competitive Analysis from Walras to the «Neo-Walrasian Research Programme»*, Unpublished Ph.D. Dissertation, University of Cambridge (U.K.).

— (1993), Friedrich August von Hayek (1899-1992), *Rivista Internazionale di Scienze Economiche e Commerciali*, 40, January, 1-29.

Frisch R. (1935), «On the Notion of Equilibrium and Disequilibrium», *Review of Economic Studies*, 3, 100-6.

Hayek F.A. (1928a), «Einige Bemerkungen über das Geldtheorie zur Konjunkturtheorie», *Schriften des Vereins für Sozialpolitik*. 173. 2. 247-94.

Hayek F.A. (1928b), «Das intertemporale Gleichgewichtssystem der Preise und die Bewegungen des 'Geldwertes'», *Weltwirtschafliches Archiv*, 28, 2, 33-76; translated into English as «Intertemporal Price Equilibrium and Movements in the Value of Money», in Hayek (1984), 71-117.
— (1929), *Geldtheorie und Konjunkturtheorie*, Hölder-Pichler-Tempsky, Wien & Leipzig.
— (1931), *Prices and Production*, Routledge & Sons, London (II enlarged edition 1935).
— (1933a), *Monetary Theory and the Trade Cycle*, Jonathan Cape, London; revised English edition of Hayek (1929).
— (1933b), «The Trend of Economic Thinking», *Economica*, 13, May, 121-37.
— (1935a), «Preiserwartungen, Monetäre Störungen und Fehlinvestitionen», *Nationalökonomisk Tidsskrift*, 73, 3; translated into English as «Price Expectations, Monetary Disturbances and Malinvestments», in Hayek (1939), 135-56.
— (1935b), (ed.), *Collectivist Economic Planning: Critical Studies on the Possibilities of Socialism*, George Routledge & Sons, London.
— (1935c), «The Nature and History of the Problem», in Hayek (1935b); as reprinted in Hayek (1949), 119-47.
— (1935d), «The Present State of the Debate», in Hayek (1935b); as reprinted in Hayek (1949), 148-80.
— (1937), «Economics and Knowledge», *Economica*, n.s., 4, February, 33-54.
— (1939), *Profits, Interest and Investment, and Other Essays on the Theory of Industrial Fluctuations*, Routledge and Kegan Paul, London.
— (1940), «Socialist Calculation: The Competitive 'Solution'», *Economica*, n.s., 7, 125-49; as reprinted in Hayek (1949), 181-208.
— (1941), *The Pure Theory of Capital*, Routledge and Kegan Paul, London.
— (1942), «Scientism and the Study of Society», Part I, *Economica*, n.s., 9, August, 267-91.
— (1943a), «Scientism and the Study of Society», Part II, *Economica*, n.s., 10, February, 34-63.
— (1944), «Scientism and the Study of Society», Part III, *Economica*, n.s., 11, February, 27-39.
— (1945), «The Use of Knowledge in Society», *American Economic Review*, 35, 4, 519-30.
— (1946), «The Meaning of Competition», Stafford Little Lecture, Princeton University; as reprinted in Hayek (1949), 92-106.
— (1949), *Individualism and Economic Order*, Routledge & Sons, London.
— (1955), «Degrees of Explanation», *The British Journal for the Philosophy of Science*, 6, 23, 209-25; as reprinted in Hayek (1967a), 3-21.
— (1964), «The Theory of Complex Phenomena», in Bunge M.A. (ed), *The Critical Approach to Science and Philosophy. Essays in Honor of Karl R. Popper*, The Free Press, New York; as reprinted in Hayek (1967a), 22-42.
— (1967a), *Studies in Philosophy, Politics and Economics*, Routledge & Kegan Paul, London.
— (1967b), «Résultats de l'action des hommes mais non de leurs desseins», in *Les fondements philosophiques des systèmes économiques. Textes de Jacques Rueff et essays rédigés en son honneur*, Paris; English version: «The Results of Human Action but not of Human Design», in Hayek (1967a), 96-105.
— (1968), «Der Wettbewerb als Entdeckungsverfahren», *Kieler Vorträge*, n.s., 56, Institut für Weltwirtschaft, Kiel; English version: «Competition as a Discovery Procedure», in Hayek (1978), 179-90.
— (1978), *New Studies in Philosophy, Politics, Economics and the History of Ideas*, Routledge & Kegan Paul, London.
— (1984), *Money, Capital and Fluctuations. Early Essays*, Routledge & Kegan Paul, London.
— (1988), *Conoscenza, mercato, pianificazione*, Il Mulino, Bologna.
Hicks J. (1933), «Gleichgewicht und Konjunktur», *Zeitschrift fur Nationalökonomie*, 4; translated into English as «Equilibrium and the Cycle», *Economic Inquiry*, 1980, 18, October, 523-34.
— (1934), «Léon Walras», *Econometrica*, 2, October, 338-48.
— (1939), *Value and Capital. An Inquiry into Some Fundamental Principles of Economic Theory*, Oxford University Press, Oxford.

Hutchison T.W. (1981), «Austrians on Philosophy and Method (since Menger)», in Hutchison T.W., *The Politics and Philosophy of Economics. Marxians, Keynesian and Austrians*, Basil Blackwell, Oxford, 202-32.
Kirzner I. (1988), «The Economic Calculation Debate: Lessons for Austrians», *The Review of Austrian Economics*, 2, 1-18.
Knight F. (1921), *Risk, Uncertainty, and Profit*, University of Chicago Press, Chicago and London, 1971.
Lange O. (1936), «On the Economic Theory of Socialism (Part I)», *Review of Economic Studies*, 4, 53-71.
Lange O. (1937), «On the Economic Theory of Socialism (Part II)», *Review of Economic Studies*, 5, 123-42.
Lavoie D. (1985), *Rivalry and Central Planning: The Socialist Calculation Debate Reconsidered*, Cambridge University Press, Cambridge.
Lerner A. (1937), «Statics and Dynamics in Socialist Economics», *Economic Journal*, 47, June, 251-70.
— (1938), «Theory and Practice in Socialist Economics», *Review of Economic Studies*, 6, October, 71-5.
Lindahl E. (1939), *Studies in the Theory of Money and Capital*, George Allen & Unwin Ltd., London.
Marshall A. (1890), *Principles of Economics*, Ninth (Variorum) Edition, Macmillan, London, 1961.
Mises L. von (1920), «Die Wirtschaftsrechnung im sozialistischen Gemeinwesen», *Archiv für Sozialwissenschaften*, 1; translated into English as «Economic Calculation in the Socialist Commonwealth», in Hayek (1935b), ch. 3.
Pareto V. (1896-7), *Corso di economia politica*, 2 Vols., Einaudi, Torino, 1953.
— (1906), *Manuel d'Economie politique*, Pichon, Paris; translated into English as *Manual of Political Economy*, Macmillan, London and Basinstoke, 1927.
Pigou A.C. (1935), *The Economics of Stationary States*, Macmillan, London.
Radner R. (1972), «Existence of Equilibrium of Plans, Prices, and Price Expectations in a Sequence of Markets», *Econometrica*, 40, 289-303.
— (1982), «Equilibrium under Uncertainty», in Arrow K.J. and Intriligator M.D. (eds), *Handbook of Mathematical Economics*, North-Holland, Amsterdam, 923-1006.
Roper W.C. (1929), *The Problem of Pricing in a Socialist State*, Harvard University Press, Cambridge, Mass.
Schumpeter J.A. (1912), *The Theory of Economic Development*, Harvard University Press, Cambridge, Mass., 1934.
— (1939), *Business Cycles*, 2 Vols., McGraw-Hill, New York, 1964.
Taylor F.M. (1929), «The Guidance of Production in a Socialist State», *American Economic Review*, 19, March, 1-8.
Vaughn K. (1980), «Economic Calculation Under Socialism: The Austrian Contribution», *Economic Inquiry*, 18, October, 535-54.
Walras L. (1874-7), *Eléments d'économie politique pure ou théorie de la richesse sociale*, édition définitive, R. Pichon & R. Durant-Auzias, Paris, 1952.
Wicksell K. (1898), *Geldzins und Güterpreise. Eine Studie über die den Tauschwert des Geldes bestimmenden Ursachen*, Gustav Fischer, Jena; translated into English as *Interest and Prices. A Study of the Causes Regulating the Value of Money*, Macmillan, London, 1936.
Wicksell K. (1901-06), *Forelasningar I Nationalekonomi*, C.W. Gleerups Vorlag, Lund; translated into English as *Lectures on Political Economy*, 2 Vols., George Routledge & Sons, London, 1934.