

THE EXPLANATORY ROLE OF GENERAL EQUILIBRIUM THEORY: AN OUTLINE ONTO A CRITIQUE OF NEOCLASSICAL ECONOMICS

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Abstract:

This paper is an attempt at exploring, and criticizing, the underlying ontology of the neoclassical economics, in the context of the general equilibrium theory, to be taken as the methodological "hard core" of this program. The reason for such an attempt is the conviction that the controversies over the "correct" methodology of mainstream economics, concentrating mainly on epistemological issues and accepting a form of "empirical realism," function to justify actual practices of economists themselves, rather than being prescriptive with respect to the correct "scientific" methodology. Such a defensive strategy however, creates in general a lacunae in the debates over methodological issues of economics. Based upon this conviction, it is argued in the paper that the adopted ontology of neoclassical economics, being an ontology of particulars implying methodological individualism, is the main problematic aspect of the whole approach, for it causes the analysis to fail to achieve what it promises to achieve: the explanation of the operation of the market system.

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Keywords: general equilibrium, positivism, instrumentalism, empirical realism, ontology, methodological individualism.

Anahtar Sözcükler: genel denge, pozitivizm, araççılık, deneysel gerçekçilik, ontoloji, yöntembilgisel bireycilik.

Özet:**Genel Denge Teorisinin Açıklayıcı Rolü: Neoklasik İktisadın Eleştirisi İçin Bir Taslak**

Bu çalışma, neoklasik iktisadın temel ontolojisini, bu programın "katı çekirdeği" olarak düşünülen genel denge kuramı bağlamında irdelemeye ve eleştirmeye yönelik bir çabadır. Böyle bir çabaya girilmesinin nedeni, yerleşik iktisadın "doğru" yöntembilgisinin ne olduğu konusundaki, büyük ölçüde epistemolojik sorunlar üzerinde yoğunlaşan ve bir tür "deneyci gerçekçilik" görüşünü temel alan anlaşmazlıkların, doğru "bilimsel" yöntemin ne olduğunu ortaya koymaktan çok, iktisatçıların kendi etkinliklerini haklı gösterme işlevini yerine getirdiği inancıdır. Ne var ki, böylesi bir savunuyu, iktisadın yöntembilgisine ilişkin tartışmaları havada bırakmaktadır. Bu inanç temelinde, çalışmada, neoklasik iktisadın benimsediği, yöntembilgisel bireyciliği içeren bir tekeller ontolojisinin, yaklaşımın gerçekleştirmeyi vaat ettiği hedefe, yani piyasa sisteminin isleyişinin açıklanması hedefine erişmesinde başarısızlığa uğramasına yol açan, temel sorunlu yönü olduğu ileri sürülmektedir.

Just as the tiniest error in navigation may lead to a landfall even on the wrong continent, so the acceptance of apparently innocuous principles can lead to doctrines which, if accepted, would render intellectual life as we practice it, and the world as we conceive it, impossible.

(Harré and Madden 1975: 1)

Introduction:

The methodology of the neoclassical economics has heavily been criticized both by economists belonging to the other traditions, such as the Classical or Keynesian schools, and by various philosophers who have different perspectives on the social sciences. In this regard, the three foundations of the neoclassical school; namely methodological individualism, rational choice theory, and the equilibrium method, have been subject to rigorous criticisms. According to these critics, neoclassical economics cannot provide a realistic picture of the economic phenomena, for its very foundations either cannot be tested (or falsified), or they are both empirically and analytically wrong in describing and explaining (or predicting) economic phenomena.

Although there is no agreement among the critics of the neoclassical economics, two main strands of criticism seem to emerge: first, whether the assumptions of the neoclassical economics are realistic enough to describe economic reality, that is, the market system, and second, what the "correct"

methodology of neoclassical economics is, that is, whether neoclassical economic theory is designed to explain or predict economic phenomena.

In this paper, I examine these two issues with reference to the general equilibrium theory, for this theory is taken to represent the very core of neoclassical economics. The paper is organized as follows: In the first section, I present a brief survey of the attempts on the part of economists to discuss the methodology of economics from the philosophy of science perspective. However, this survey is by no means exhaustive; it only intends to give a flavor of the controversies and to show that such an attempt has never been conclusive. With this aim in mind, I first discuss Milton Friedman's well-known "positive economics" briefly and evaluate the attempts to apply Thomas Kuhn's and Imre Lakatos's ideas to economics. With respect to Friedman, the main focus will naturally be his arguments about the status of the assumptions of economic theory. In this respect, two themes will appear. First, Friedman's views cannot be seen as a coherent methodological account, but instead his approach must be considered as a defense of the usual practice of the neoclassical economists, beginning with fictional or, more accurately, idealized assumptions, and proceeding as constructing deductive, axiomatized systems. Second, although he was not successful in his effort, Friedman was brave enough to stress "unrealisticness" of the assumptions of neoclassical theory, and he was one of the very few economists who tried to justify them from a methodological standpoint. On the other hand, with respect to Kuhn and Lakatos, it is argued that these two philosophies of science, among others, have generally been used by economists "opportunisticly"; that is, they too have been utilized to justify their actual practices.

The most important reason for Friedman's failure was his trying to defend an instrumentalist account, that theories are just instruments yielding predictions on the basis of "false" assumptions, for the economic theory, neoclassical or not, has actually been designed to explain the operation of the economic system, no matter how successful it may have been. In other words, I argue in the second section that the neoclassical economic theory can be treated as a realist approach which is concerned with the operation of the market system, and then have a word about why it fails in its own aim, stressing the problematic features of the methodological individualist framework used by the neoclassical school.

1. The Disarray in the Methodology of Economics

1.1. The "Reality" of the assumptions of Neoclassical Economics: Friedman Revisited

Historically speaking, from the very beginning of our "science," economists have constantly been embroiled in methodological issues characterizing analytical frameworks in which they practice.¹ Yet, at more recent times, with the development of philosophy of science as an independent discipline which is preoccupied with the demarcation of scientific enterprise from other, "non-scientific" cognitive activities (a fact roughly corresponding to the emergence of "Logical Positivism,"), the problem of determining what is, or what should be, the "correct" methodology of economics has become a prominent issue among economists as well. Therefore, since that time, a natural strategy that economists usually adopt has been to explain and defend the dominance of the Neoclassical framework by appealing to philosophy of science. The first response to this "identification problem," as is well known, was given by Milton Friedman's "positive economics" (Friedman 1953), presumably defending a "logical positivist/empiricist"² approach as the appropriate methodology of economics. Yet, the unique contribution of Friedman to the discussion of the methodology of economics lies in the so-called "F(riedman)-Twist" (Samuelson 1963: 232); he defends economics against the charge that the assumptions of neoclassical economics are unrealistic. As Caldwell (1980: 366) observes, although this paper has usually been reviewed negatively, "the methodological prescriptions advanced in this essay have been accepted by many working economists. And this has happened without Friedman ever having directly responded to his critics!"

In that paper, Friedman seems to hold two distinct theses: "instrumentalism" and the "F-twist"

1. *Instrumentalism:*

Positive economics is in principle independent of any particular ethical position or normative judgements.... Its task is to provide a system of generalizations that can be used to make correct predictions about the consequences of any change in circumstances. Its performance is to be judged by the precision, scope and conformity with experience of the predictions it yields.

...

The ultimate goal of a positive science of a "theory" or "hypotheses" that yields valid and meaningful (i.e., not truistic) predictions about phenomena not yet observed (Friedman 1953: 211).

2. The "F-Twist":

Truly important and significant hypotheses will be found to have "assumptions" that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions (in this sense). The reason is simple. A hypothesis is important if it "explains" much by little, that is, if it abstracts the common and crucial elements from the mass of complex and detailed circumstances surrounding the phenomena to be explained and permits valid predictions on the basis of them alone.

...the relevant question to ask about the "assumptions" of a theory is not whether they are descriptively "realistic," for they never are, but whether they are sufficiently good approximations for the purpose at hand. And this question can be answered only by seeing whether the theory works, which means whether it yields sufficiently accurate predictions. (Friedman 1953: 218)

Here, two things must be clarified; first, the role of assumptions, and, second, the appropriate methodology in economic theory. With respect to the first issue,

... "the assumptions of a theory" play three different, though related positive roles: (a) they are often an economical mode of describing or presenting a theory; (b) they sometimes facilitate an indirect test of the hypothesis by its implications; and (c), as already noted, they are sometimes a convenient means of specifying the conditions under which the theory is expected to be valid (Friedman 1953: 224).

Secondly, the appropriate method is the "*as if*" method:

A meaningful scientific hypothesis or theory typically asserts that certain forces are, and other forces are not, important in understanding a particular class of phenomena. It is frequently convenient to present such a hypothesis by stating that the phenomena it is desired to predict behave in the world of observation *as if* they occurred in a hypothetical and highly simplified world containing only the forces that the hypothesis asserts to be important. (Friedman 1953: 236)

As an example of this "*as if*" method, Friedman uses the example that the density of leaves of a tree can be explained on the basis of the assumption that leaves behave as if they seek to maximize sunlight (Friedman 1953: 224-25). Here, as Boland (1979: 513) stresses, "the individuals' *behavior* is not claimed to be *as if* they behaved as we assume, but rather it is the *effect* of their behavior that is claimed to be *as if* they behave according to our assumption."

With respect to the problem of "unrealisticness" and the "as if" methodology, Friedman seems to invoke Weber's "ideal type" concept: "The ideal types are not intended to be descriptive; they are designed to isolate the features that are crucial for a particular problem" (Friedman 1953: 233)³. For Weber (Manicas 1987: 134-136), we must distinguish between explanation via production of a valid law and explanation via the production of a teleological schema of rational action. The ideal-typical constructions of economics have no pretension at all to the general validity. The "laws" of economics are paradigmatic "teleological rationalizations," ideal-typical constructions which make sense of economic activity. The economist who understands what she is doing is not saying that her deduction explains, still less predicts, concrete acts or that the real actors are "rational." Weber asserts that the economist's schema makes acts intelligible because her theory is a subjectively and empirically adequate interpretation. On the other hand, if the "laws" are empirically valid generalizations, they cannot explain, since their causal interpretation is problematic.

However, this does not entail instrumentalism; On Weber's view, whatever happens is made intelligible by the theory, after it happens. Weber denies predictability as the goal of concrete science because of causal complexity. He takes a second best: the capacity to provide a "comprehensible interpretation." That is to say, Weber defends theoretical economics by restricting its claims (Manicas 1987: 135).

Yet, the most criticized aspect of Friedman's account has been the F-Twist rather than his instrumentalism. This is understandable because of the special status of the rationality assumption in economic theory. In other words, "to a philosopher or scientist, the F-Twist is of no great moment and its discussion might perhaps be bypassed. To present-day economics [however,] ... its validity would be of considerable moment" (Samuelson 1963: 233).

Responses to the F-Twist on the part of the philosophers of science have varied. For example, logical empiricist philosopher Ernest Nagel (1963) in his attempt "to save Milton Friedman from himself" (Samuelson 1963: 231), tries

to clarify some aspects of Friedman's account for he believes that "despite the inconclusiveness of his [Friedman's] argument his conclusion is sound" (Nagel 1963: 211). Nagel believes that Friedman fails to distinguish among three subgroups of statements belonging to a theory, namely, fundamental statements, those statements logically deducible from the fundamental ones, and observation statements (Nagel 1963: 212-13). Consequently, according to Nagel, Friedman is not explicit about the role of assumptions. If he asserts that theories are at best useful instruments for prediction but not to be viewed as genuine statements whose truth or falsity is at best irrelevant, he is right. However, if his assertion is that unrealistic theories are not only instruments but also explanations of various phenomena in terms of mechanisms involved in their occurrence, he is clearly wrong, for such a theory has no explanatory power. Yet, as can be expected, Nagel is inclined to interpret Friedman's position as maintaining the first assertion (Nagel 1963: 218). However, as we shall see below, this interpretation itself is hard to maintain.

Similarly, for Musgrave (1981) too, Friedman fails to distinguish among three kinds of assumptions, namely, *negligibility* assumptions (such as the absence of government in general equilibrium theory) whose unrealisticness does not entail instrumentalism; *domain* assumptions determining the domain of applicability of the theory (such as the absence of futures market or absence of money⁴ in the general equilibrium theory), the falseness of which implies that the theory can never be applied to any actual situation, and that it is untestable; and finally, the *heuristic* assumptions, i.e., the assumptions which are taken as negligible at first, but then are modified in the course of the model building process, whose falseness, again, creates significant problems for the theory (Musgrave 1981: 382).⁵

Therefore, it appears that Friedman's problem is his being not sufficiently clear about the concepts such as "assumption," "hypothesis," "theory" and "model" (Boland 1979: 513). Maybe worse than this is that he is not careful about different philosophies of science. For example, according to Maki (1990:39), Friedman has not provided a single coherent methodological view of economics; his position can be characterized as "a coherent combination of semantic commonsense realism and axiological instrumentalism (called the 'Friedmanian mixture')" (Maki 1990: 40). And this ambiguity leads Maki to assert that Friedman is actually a realist: "Friedman's text may be plausibly interpreted to imply subscription to several versions of realism, the range of these versions depending on how '(un)realisticness' is specified" (Maki 1990: 39) Maki seems to rely on this passage:

A fundamental hypothesis of science is that appearances are deceptive and that there is a way of looking at or interpreting or organizing the evidence that will reveal superficially disconnected and diverse phenomena to be manifestations of a more fundamental and relatively simple structure. (Friedman 1953: 231)

Nevertheless, the existence of realist elements in Friedman's paper does not prove that Friedman is a realist; the very same passage may point to another, something of a completely different direction as well: that of pragmatism (Hollis and Nell 1975: 196-201; Hollis 1994: 77-83). Although Friedman seems to believe that a theory just serves as "an analytical file system" (Friedman 1953: 213), i.e., as consisting only of "analytic" statements which have no "factual" content, he also seems to recognize theory as something more than a mere collection of analytic statements, for he maintains that "a theory is the way we perceive 'facts', and we cannot perceive facts without a theory" (Friedman 1953: 232). In other words, not only the "predictions" but also the "theory" as a "file system" may play an important role in the verification/confirmation of the theory as a whole, however indirectly.⁶ This possibility, in turn, makes W.v.O. Quine's (1953) famous thesis relevant for our discussion.

Quine's thesis regards three distinct propositions (Dancy 1985: 92-95; Losee 1993: 206-207): First, theory is underdetermined by data, that is, there will always be theories which explain and assimilate the data equally well. The second of these propositions is the problem stated first by Pierre Duhem (the Duhem thesis): individual non-observation statements cannot be conclusively verified, or falsified, by the evidence of our senses. The reason is that such statements are parts of complex statements, i.e., *ceteris paribus* clauses, in which case the theory under the test can be altered when a discrepancy arises at the observational level. Thus, experience can confirm theories as a whole, but it cannot confirm those sentences singly and directly. The third is the verification principle which states that the meaning of a statement is the difference that truth of it would make to the evidence of one's senses. By this principle, in specifying the observational consequences of a statement is taken as equivalent to the specification of it. However, Duhem's thesis states that no individual non-observation statement has its own observational consequences, which implies that no observational statement can have meaning on its own. That is to say, any statement in its relation to experience can be retained as true provided that sufficiently drastic adjustments are made elsewhere in the system. Therefore, since no statement is finally immune to revision, meaning belongs to theories rather than to individual statements; at the non-observational level, sentential meaning is indeterminate. Given this conclusion, one can argue that most sentences are made true by a

combination of what they mean and how the world is, that is they are synthetic. Therefore, positivists' strict distinction between "analytic" and "synthetic" statements does not hold (Dancy 1985: 95). Yet in empiricism, the verification principle must be retained with the holistic claim that the unit of the empirical significance is the whole of science:

Total science is like a field of force whose boundary conditions are experience. A conflict with the experience at the periphery occasions readjustments in the interior of the field. Truth values have to be redistributed over some of our statements. Reevaluation of some statements entails reevaluation of others, because of their interconnections ... But the total field is so underdetermined by its boundary conditions, experience, that there is much latitude of choice as to what statements to reevaluate in the light of any single contrary experience. No particular experiences are linked with any particular statements in the interior of the field, except indirectly through considerations of equilibrium affecting the field as a whole. (Quine 1953: 42-43)

If Quine's thesis to be maintained, and every statement is open to revision, we should acknowledge that there may be more ways of ordering than we attempt to do (Hollis 1994: 83). That is, the real "tribunal" to decide whether our theory is consistent with experience is ourselves (Hollis 1994: 82), implying a form of Pragmatism which insists that "the mind is always active in deciding what counts as knowledge" (Hollis 1994: 77). Returning to Friedman, it can be asserted that Friedman in effect takes the theory not as simply a "filing system" but as something more than this: even if the predictions produced by the theory are to be verified by the experience, this also implies that it is the whole theory, not the implications taken individually, which is confronted with the empirical evidence. It can also be added that since the assumptions gain their meaning only within the whole, their "unrealisticness" individually should not matter much. In short, Friedman seems to depart from the "dogmas" of the logical empiricist program by positing his own "intermixture." Yet, in my view, such an "intermixture" also shows the relevance of Rosenberg's (1992: 25) thesis that economists are not positivists, and the association between economic theory and positivism, since Friedman, is just a defensive strategy adopted by economists against the charge that its predictive record is not convincing and its boundary conditions are never realized, and his attempt actually shows the predictive weakness of the theory⁷. This is true, as we shall see shortly, in the case of other philosophies of science as well, a fact which makes Rosenberg's thesis that economists "tailored their tastes in epistemologies to fit the actual character of their disciplines, as they see them" (Rosenberg 1992: 20) plausible.

1.2. Paradigms, Scientific Research Programs and Economic Theory

With the decline of positivism, economists turned their attention to Popper, Kuhn, and Lakatos. However, this attitude has had its own difficulties. For example, with respect to Popper, as Blaug correctly argues, "a dogmatic application of Popper to economics would leave virtually nothing standing," for "analytical elegance, economy of theoretical means, and generality obtained by ever more 'heroic' assumptions have always meant more to economists than relevance and predictability" (Blaug, 1976: 369).

On the other hand there are two problems in applying Kuhn's account into economics. First, Kuhn's theory has been criticized as implying an irrationalist and a relativist position, for it leads to the "incommensurability problem": paradigms are incommensurable, that is, there is no rational basis by which theories can be compared and hence it is impossible to find a neutral language in order to make comparisons. Moreover, the new paradigm need not necessarily better explain the world than the old one. That is to say, Kuhn challenges the notion that science is not only rational but also a continuous, cumulative activity. A final point is that scientific knowledge is the collective opinion of the scientific community. These ideas are quite disturbing for a "positive" science⁸. On the other hand, its application to economic theory is quite impractical: First, economists often use the term "paradigm" interchangeably with "world view" (*weltanschauung*) which is not the intended usage of that term. Second, with Kuhn's account, it is hard to explain the plurality of "paradigms" in the history of economic thought: almost for every ("great") economist, we can define a different paradigm. Maybe it is possible to define two broad categories as the Neoclassical "paradigm," dominated by the general equilibrium theory, and the Keynesian "paradigm," which stresses market "imperfections." However, neither the Neoclassical nor the Keynesian "paradigms" can constitute coherent ones; and, worst of all, neither Keynes, nor most of the Keynesians, hardly ever questioned the rationality postulate or methodological individualism on which the Neoclassical approach rests. In short, Blaug seems right when he proposes that "the term 'paradigm' ought to be banished from economic literature, unless surrounded by inverted commas" (Blaug 1976: 361).

Then, a possible candidate could be Lakatos' the Methodology of Scientific Research Programs (MSRP). For example, Blaug (1976) argues that the MSRP is a useful account in history of economics, and Weintraub (1985a,b) argues that Neoclassical economics is a "progressive" research program. However, as Lakatos posits, since the "normal science" with its accepted

paradigm is nothing but a research program that has achieved monopoly (Redman 1991: 145), then the same problems in the application of Lakatos's account are bound to appear.⁹

For instance, according to Blaug (1976:370), the "hard core" of the neoclassical program consists of the "assumptions" of competitive theory, namely, rational economic calculations, constant tastes, independence of decision-making, perfect knowledge, perfect certainty, perfect mobility of factors, etc. Whereas the "positive heuristic" consists of such practical advice as (i) divide markets into buyers and sellers, or producers and consumers; (ii) specify the market structure, (iii) create "ideal type" definitions of the behavioral assumptions so as to get sharp results; (iv) set out the relevant *ceteris paribus* conditions, (v) translate the situation into an extreme problem and examine first-and second-order conditions, etc.

On the other hand, according to Weintraub (1985a: 26; 1985b: 109),¹⁰ the "hard core" can be characterized as

- HC1. there exists economic agents;
- HC2. agents have preferences over outcomes;
- HC3. agents independently optimize subject to constraints;
- HC4. choices are made in interrelated markets;
- HC5. agents have full relevant knowledge;
- HC6. observable economic outcomes are coordinated, so they must be discussed with reference to equilibrium states.

the positive heuristic:

- PH1. go forth and construct theories in which economic agents optimize;
- PH2. construct theories that make predictions about changes in equilibrium states;

and the "negative heuristic";

- NH1. do not construct theories in which irrational behavior plays any role;
- NH2. do not construct theories in which equilibrium has no meaning;
- NH3. do not test the hard core propositions; etc.

Facing with this ambiguity, it is possible to reformulate Blaug's claim, as Redman (1991: 145) does: "Blaug was wrong: *paradigm and program* should be banished from economic literature once and for all."¹¹

Therefore, Rosenberg seems once again right to argue that "the entire motivation for appeal to Lakatos is the absence of strongly confirming empirical data." (1992: 102). In other words, the fascination with Popper, then Kuhn, and finally Lakatos represents a chronological succession that mirrors the developments in the philosophy of science (Redman 1991: 143), for economists are indiscriminately employing different views on methodology as "immunizing stratagems."

However, there may be another reason for why economists prefer Popper's and Lakatos's accounts as appropriate characterizations of their positions. According to Bhaskar (1989b), these philosophers, even including Kuhn and Feyerabend, implicitly adopts an empiricist *ontology* implying methodological atomism/individualism. Given this claim, then it is no wonder why economists find these philosophers attractive, for they share the same ontology. But before drawing this conclusion, we must first discuss the essential methodological ingredients of the neoclassical economics.

In this regard, it appears that the problem of whether the assumptions of the general equilibrium theory, especially the rationality postulate, are empirically realistic is of a secondary issue. The reason for this is the belief that the smallest unit of appraisal is "theory," and hence we must assess theories as a whole on the basis of their explanatory power. This, on the other hand, carries us directly to the principle of "emergence" claiming that there are different levels of reality that should be distinguished from each other. Therefore, in the next section, I will examine the reasons for the use of general equilibrium framework, and then assess whether general equilibrium theory is a realist one or not. Yet, it will also be emphasized that the point is not merely whether this theory is a realist one or not, but what kind of realism it posits.

2. Is General Equilibrium Theory Explanatory?

2.1. General Equilibrium Theory

Methodologically speaking, General Equilibrium theory constitutes the "hard core" of the neoclassical "program," concerning both Walras's own version and its Arrow-Debreu variant. According to this latter version (Hahn 1984: 47-48), goods are distinguished from one another by their physical property, by their location in space and in time, and by the state of the world. This implies that an Arrow-Debreu equilibrium can be interpreted as a state of affairs where (a) all actions are decided upon only at one instant of time and (b) actions always contain contingent elements (Hahn 1984: 52). This means

certainty and perfect knowledge, for we assume the existence of futures and contingent markets. A price is defined for each good. There are two kinds of agents: households and firms. By solving their optimization problems households and firms decide how much to consume and produce respectively. An equilibrium is then a triple: a non-negative price vector, a vector of demand, and a vector of supply, such that (a) the demand vector is the vector sum of household action at these prices, (b) the supply vector is the vector sum of firms' actions at these prices, and (c) for no good does demand exceed supply. The most important assumption in this framework is the rationality postulate. General equilibrium theory characterizes the agent independently of his environment; the agent has preferences and the domain of these preferences is the set of consequences of his possible actions.¹² Although most, if not all, of the assumptions in this framework either appear as practically false, such as the existence of futures market for most of the goods, or they are not empirically testable or falsifiable, in reference to the above discussion, it is still possible to discern some methodological principles guiding the general theoretical framework at a more fundamental level. Even from this brief sketch, it can be seen that general equilibrium theory is founded upon three methodological principles, as one of the leading general equilibrium theorists, Frank Hahn, explains his inclination toward Neoclassical economics (Hahn 1984: 1-2):

- (1) I am a reductionist in that I attempt to locate explanations in the actions of individual agents.
- (2) In theorizing about the agent I look for some axioms of rationality.
- (3) I hold that some notion of equilibrium is required and that the study of equilibrium states is useful.

These elements, namely, (methodological) individualism, rationality, and equilibrium, can be said to characterize the Neoclassical school correctly.¹³ Naturally, all these three elements have their own share of criticisms.

The rationality postulate has been criticized from various points. Above all, we can assert that for three reasons it has no explanatory value in analyzing human behavior. First, it requires the assumption of perfect information and certainty, which is plainly false.¹⁴ Second, human beings are not capable of undertaking necessary calculations to solve their optimization problems. Third, and a more important criticism is that rationality is not backed by some psychological mechanism that provide causal explanations for preferences/expectations, explanatory variables in economic theory, a problem which seems unsolvable because of the "problem of intentionality." A belief (or preference, expectation etc.) is a relation between a human being and a statement, and it consists in the statement it contains, and hence any attempt to

explain beliefs leads to infinite regress problem. Therefore, it must be denied that "the intervening variables linking goals to stimuli to behavior are propositional organized mental states" (Rosenberg 1992: 141, 239).¹⁵

Nevertheless, these criticisms can be said to be irrelevant for the general equilibrium theory.¹⁶ According to Boland (1981: 1034) the neoclassical premise that "for all decision makers there is something they maximize" is a kind of "All-and-some" statement which is neither verifiable nor refutable.¹⁷ Second, rationality statement is actually a "metaphysical" statement whose "status is a result of *how* it is used in a research program. Metaphysical statements can be false but we may never know because they are the assumptions of a research program which are *deliberately put beyond question*" (Boland 1981: 1034). Therefore, "even if one could prove that a consumer is not maximizing utility or a producer is not maximizing profit, this would not constitute a refutation of the neoclassical hypothesis" (Boland 1981: 1034).

I think this is a sound interpretation. The postulate of rational, optimizing behavior is in fact a normative prescription rather than being an explanatory statement: to say that people are rational does not explain *what* they do, but only at best *how* they should do it. (Bhaskar 1989a: 30) But rationality, by itself, can explain nothing, even if it is designed to explain "everything." Rationality is an *a priori* presupposition of investigation and neoclassical economic theory is, at best, a normative theory of efficient action which generates a set of techniques for achieving given ends. In other words, rationality is an idealized, normative statement which belongs to the realm of what Schumpeter calls "preanalytic vision" (Schumpeter 1949; 1954: 41-42). Preanalytic vision is a mixture of perceptions and prescientific analysis of the researcher. According to Schumpeter,

... in order to be able to posit to ourselves any problems at all, we should first have to visualize a distinct set of coherent phenomena as a worth-while object of our analytic efforts. In other words, analytic effort is of necessity preceded by a preanalytic cognitive act that supplies the raw material for the analytic effort. (1954: 41)

For him, scientific procedure "starts from the perception of a set of related phenomena which we wish to analyze and ends up ... with a scientific model in which these phenomena are conceptualized" (Schumpeter 1949: 265). That is to say, this "prescientific cognitive act," i.e., the vision, is not only the source of our ideologies, but even more importantly,

also the prerequisite of our scientific work. No new departure in any science is possible without it. Through it we acquire new material for our scientific endeavors and something to formulate, to defend, to attack. Our stock of facts and tools grows and rejuvenates itself in the process. And so -though we proceed slowly because of our ideologies, we might not proceed at all without them. (Schumpeter 1949: 274).

Based upon this notion, it is possible to assert that the vision of any economist defines the intellectual foundations dictating her conception of prices and hence social and moral significance of market allocation; in short, it is a set of views about the ultimate nature of reality (Hunt 1983: 334).

Different preanalytic visions lead to different views on social-economic problems because this vision determines the ethics and epistemology of the analyst. In this regard, "the preanalytical vision of Neoclassical economics is so extremely individualistic that the only way in which human sociality appears at all is in the individual's need for other entities with whom to exchange" (Hunt 1983: 335). Social relations play no part in this model; and this model applies as much as to Cruseo as to socialized human beings which implies that "mankind is much the same at all times and places," Hume's dictum, revealing its ahistorical and a priori biases (Bhaskar 1989a: 29).

This claim directly leads us to the second element of the neoclassical "vision," namely, methodological individualism. The rationality postulate by itself is rather weak; to make it yield interesting implications we must rely on individualistic principles. While it is possible in principle to adopt a methodological individualist approach without adopting the rational choice framework, in practice methodological individualist social theory has almost always adopted the assumption of rational optimizing behavior. (Hunt 1992:93)

Methodological individualism is the doctrine that the facts about societies, and social phenomena generally must be explained solely in terms of individuals (Bhaskar 1989a: 27; Little 1991: 183). In this doctrine, social institutions are just abstract models based on the facts about individuals. This approach consists of three related but distinct theses; namely, the *ontological thesis* stating that all social entities are reducible without remainder to logical compounds of individuals; the *meaning thesis* stating that social concepts must be *definable* in terms of concepts that refer only to individuals and their relations and behavior; and the *explanation thesis*, stating that there are no autonomous social explanations; instead all social facts and regularities must ultimately be explicable in terms of facts about individuals -their motives, powers, beliefs, and capacities (Little 1991: 183-188). Even though the

ontological thesis is true, that is, society is made up or consists of people and the material presence of social effects consists only in changes in people and changes brought about by people on other material things (Bhaskar 1989a:30), we can also assert that individuals and society (or social structures) are ontologically distinct from and irreducible to each other. Yet in neither case the ontological thesis implies the theses about meaning and explanation (Little 1991: 200). The meaning thesis, on the other hand, makes sense if the facts refer only to individuals and their psychological properties. But there is no reason to think that such a reduction is possible. To begin with, facts about individuals always make reference to social contexts. The predicates designating properties special to persons all presuppose a social context for their employment. Secondly, the facts about individuals are not necessarily either more observable or easier to understand than social facts, and the facts applicable to individuals are not necessarily either clearer or easier to define than those that designate social phenomena (Bhaskar 1989a: 28). Returning to the explanation thesis, we can assert that there are some *emergent* properties of societies irreducible to the dynamics of individuals. We can see that methodological individualism is a special case of the view known as "reductionism." Reductionism asserts that 1) it is possible to provide a rigorous specification of a hierarchy of entities, from higher to lower ones, and hence rank any pair of domains, and 2) the entities and laws of higher levels can be reduced to facts about entities and laws at lower levels (Little 1991: 191). In this framework, then, some higher order entities, properties or powers can be based on or explained by some lower order (atomistic) ones. However, reductionism as a research strategy in social sciences is likely to fail, because a successful example of a reduction (in the sense of explaining an entity with a lower order one), such as the reduction of chemistry to physics, requires a prior existence of a well developed body of knowledge in the domain of the to-be-reduced science. However, in human sciences such body of knowledge generally does not exist (Bhaskar 1989a: 98-99). As we will see below, this is exactly the failure of the neoclassical school.

2.2. The Ontology of General Equilibrium Theory

Therefore, from a methodological point of view, it is possible to distinguish among three levels relevant to the general equilibrium framework (Hollis 1994: 64). First, the *ontology* that this framework adopts is of particulars, existing independently of the theory, mostly taken as individual objects (including human beings). Second, the *methodology* aims at identifying regularities in the behavior of particulars. And finally, the *epistemology* is a simple version of empiricism stating that claims to knowledge can only be

justified through experience. These three levels are dependent on one another such that both the methodology and epistemology are founded upon an implicit ontology of particulars.

From an ontological point of view, In the general equilibrium framework, in order to obtain equilibrium, one must begin with individual preferences and proceed from utility functions to a multi-market setting by aggregating individual demand and supply functions. In this regard, the relations between the properties of the parts (individuals) and the whole (market mechanism) are “*additive*” (Harré 1984: 164). In other words, the general equilibrium framework uses “*linear*” models having two properties (West 1985: 5): *proportionality*; the response of the action of each separate factor is proportional to its value (that is, the output of any operation is directly proportional to the inputs); and *independence*; total response to an action is equal to the sum of the results of the values of the separate factors. Therefore, methodological individualism is an important ingredient in the general equilibrium framework; it even determines the “*mathematization*” of economics to a large extent.

Now the relevant question is this: why do economists need this framework? I am inclined to maintain that neoclassical economic theory and the general equilibrium framework are designed to *explain*, rather than to predict, economic phenomena. For example, for both Hausman (1981) and Rosenberg (1992), general equilibrium theory seems to have explanatory arguments where the “*explanandum*” is the existence of economic equilibrium, on the basis of “*lawlike*” statements (Hausman, 1981: 345), some of which can actually be characterized as “*postulates*” rather than lawlike statements. However, for both of them, there is no equilibrium in the actual world to be explained (Hausman 1981: 349; Rosenberg 1992: 212). Instead, as Rosenberg argues, the explanandum is the *stability of the price system*, in which case equilibrium seems to be an explanan rather than an explanandum.

Historically speaking, the challenging question for economists, since A. Smith, has been the problem of why intentionally self-interested behavior of individuals do not lead to chaos but instead lead to order in society; that is, to explain capitalism or the market "mechanism." The operation of this mechanism, which is unobservable, manifests itself as causal "laws." In this regard, it becomes possible to formulate an argument. First, as Hausman (1981: 350-540) suggests, economics is an "inexact" science, a term coined by J.S. Mill, whose explanation is different from the "deductive-nomological" model for explanation. For example, according to Hahn (1984: 47), the construction of general equilibrium theory

makes no formal or explicit causal claims at all. For instance it contains no presumption that a sequence of actual economic states will terminate in an equilibrium state. However it is motivated by a very weak causal proposition. This is that no plausible sequence of economic states will terminate, if it does so at all, in a state which is not an equilibrium. The argument is straightforward; agents will not continue in actions in states in which preferred or more profitable ones are available to them nor will mutually inconsistent actions allow given prices to persist.

The explanatory mechanism in the general equilibrium theory is competition which forces prices to adjust when there is a change in demand conditions. This mechanism is conceptualized by Walras' *tatonnement*, or Edgeworth's *recontracting* processes which are not radically different from each other. According to *tatonnement*, the market price of a commodity will rise when there is an excess demand, and fall when there is an excess supply. Therefore, just like an auction which is guided by an auctioneer, all markets, with the assumption of perfect mobility of factors of production, will come to equilibrium which is defined by zero excess demands.¹⁸ The absence of an "auctioneer" or a central coordination agency in the real world is not important because unobservable competitive forces drive the system towards equilibrium, just like "as if" there is an "Invisible Hand of the Providence," though the real dynamics of the adjustment process were not understood clearly enough.¹⁹

With respect to the operation of the market "mechanism," economic "laws" always have been regarded as "tendencies"²⁰ since Mill and Marshall. For example, in the 'law' of demand, the direction of the change in quantity demanded when price changes is not certain because of the existence of *ceteris paribus* clauses (tastes, income etc. are held constant), and second, for some groups of commodities (such as Giffen goods) it is likely to display a positive relationship. Therefore, we need an explanation as to why this "prediction"

does not hold. Likewise, for the whole economy, repercussions occurred in different markets may create complications for the "Walras' Law." Therefore, the complexity of the economic phenomena requires a conception of laws as tendencies. Here tendency may carry two distinct senses (Manicas 1987:41): First, a tendency is the existence of a cause which, if operating unimpeded, would produce some constant result (an actual or existing state, if not prevented, will produce some result). In other words, it is a *causal power of the system*. There may be more than one cause and other causes may prevent the expected effect. The tendency is real, but its outcome is not manifest. Second, the existence of such a state of things that a result *may be expected to take place* (a reliable pattern). However, it seems that it is the first meaning most economists regard tendencies.

This conception is clearly a realist one; but this is an "empirical realist" position which is based on Humean conception of lawlike statements as constant conjunctions between atomistic events. The empirical realist view fails to distinguish among the three ontologically distinct levels, namely, the domain of the "real," referring to the generative mechanisms and structures behind the appearances; the domain of the "actual," referring to the events that these mechanisms or structures generate; and the domain of the "empirical," referring to experiences of these events. In other words, these three domains collapse into one. The reason for this is that empirical realism always assumes the existence of *closed* systems, referring to Humean theory of causal laws which assumes the existence of constant conjunctions of discrete, atomistic events (Bhaskar 1975: 12). Since causal laws are considered as empirical regularities, they are reduced to sequence of events, and the events to experiences (Bhaskar 1989a: 15). Such a methodology, which is based upon an implicit ontology of constant conjunctions of discrete, atomistic events, implies a particular conception of human beings: they are to be seen as passive sensors of given facts and recorders of their constant conjunctions, rather than active agents in a complex world (Bhaskar 1975: 198). An extension of this view, especially with respect to social science, is the methodological individualism.²¹

As Hahn and Hollis (1979:13) observe, "economists have been greatly under the influence of a majestic image of the laws of physics and have sometimes been led to hope for a set of Laws of Economic Motion as forceful as Newton's Laws." Behind this view, what Harré (1984, ch. 5) calls "the corpuscularian inheritance," and along the same lines, what Bhaskar calls "classical paradigm of action," lies (Bhaskar 1975: 79). This paradigm adopts a *corpuscularian* or atomistic view of matter and a *mechanical* view of causality in which all causes are regarded as efficient and external to the thing in which

change occurs. The essential features of the classical corpuscularian/mechanical world view are (Bhaskar 1975: 83);

1. Causation is external to matter,
2. Effects are immediate and matter is passive,
3. Fundamental entities (whether corpuscles, events or sense data) are atoms,
4. There is no complex internal structure,
5. There is no pre-formation or material continuity,
6. There is no objective basis for transformation and variety in nature (they are "secondary qualities").

These views defines a "limit condition" of a "closure," that is, the constant conjunctions between atomistic events (Bhaskar 1975: 79). In this paradigm, atomicity is perceived as either a physical, identified by size, or an epistemological, identified by simplicity, entity; and these atoms are the basic building blocks of knowledge, implying methodological individualism for the social sciences (Bhaskar 1975: 82).

However, the individualist foundation is the dominant factor which is responsible for the failure of the general equilibrium framework in its aim at explaining reality. Since it uses the "linear" model explicated above, it cannot consider the multiplicity of causes or the "stratified" ontological status of reality and knowledge (Harré 1984: 183), for it cannot take account of the emergent properties of social structures, such as the market structure (Harré 1984: 164; Little 1991:187) In this sense, general equilibrium theory cannot explain the existence of aggregate relations which are incompatible with the relations characterizing individual behavior. That is to say, general equilibrium theory cannot take into account the principle of *emergence* which

may be defined as the relationship between two terms such that one diachronically, or perhaps synchronically, arises out of the other, but is capable of reacting back on the first and is in any event causally and taxonomically irreducible to it, as society is to nature or mind to matter. It is thus a figure of constellationality, pivots on the openness of being and the falsity of actualism. (Bhaskar 1994: 73)²²

The principle of emergence can be encountered in many instances in the context of economics. The most well known example for this is the famous "fallacy of composition" which states that what may be true for the individual level need not necessarily be true for the aggregate level. However, this principle is not relevant solely to the macro-micro distinction. As Kregel

(1985:35) notes, even in microeconomics, in a general equilibrium setting, entrepreneurs find that when they maximize their profits, in the limit, their profits will be driven to zero! This fact shows, according to Kregel, that the general equilibrium theory cannot recognize “the difference between the simple summation of individual behavior and the process of abstraction by which the economist is able to formulate by means of a set of simple relations, the substance of what occurs in the real world” (37). That is to say, the aggregation process by which the individual intentions are preserved through a simple summation of individual behavior is inconsistent with the realization of this intentions, or with the unintended effects of individual behavior, at different levels of abstraction. Therefore, the “linearity” property and the “invisible hand” argument, according to Kregel, is inconsistent with each other (38).²³

A good example for the “emergent” properties that appear in general equilibrium framework is Kenneth Arrow’s famous “(im)possibility theorem.” According to this theorem (Arrow 1979: 121-26), it is impossible to find an appropriate (i.e. complete, reflexive, and transitive) social preference function, which defines collective rationality, on the basis of individual preferences.²⁴ But despite Arrow’s proof, economists still think in terms of individual preferences and are unable to solve the contradiction between the individual level and the social level.²⁵

Therefore, as an explanatory framework, the general equilibrium theory fails in its own aims, for it is based on strict individualism which tries to explain aggregate (social?) relations on the basis of individual behavior. What it forgets is the fact that almost no individual behavior can be explained, or even be defined, without the context within which that behavior occurs. By ignoring this (social) context it cannot even see those “emergent” properties.

Conclusion:

If the argument of this paper that the neoclassical economic theory is to be interpreted as a realist framework is correct, then Friedman’s defense of the theory is irrelevant, for he, together with most of the neoclassical economists, confuses “explanation” and “prediction.” One reason for this “symmetry” between explanation and prediction may be the influence of Hempel’s positivist theory of explanation which maintains that symmetry. However, it is argued in this paper that the “vision” of the neoclassical economic theory that perceives reality as a “uni-layered,” “unstratified” entity, thus being unable to distinguish between the “real,” the “actual” and the “empirical” levels, a vision it shares with the positivist account, is the main factor in explaining this confusion, as

even before Hempel, "Marshall used to say that 'explanation is prediction written backwards'" (Blaug 1976: 376). Such an ontology, in turn, causes the theory to fail to consider the discontinuity between the level of the individual, characterized by individual behavior, and the level of the "social," characterized by the social relations or structures as outcomes of individual behavior. As a result, even though this would not be seen incompatible from the standpoint of a realist account, Neoclassical theory, due to its alleged misconceived ontology, clearly fails in its own aims; that is, explaining general equilibrium.

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Notes:

1. For a useful anthology collecting different economists' views on methodological problems, see Hausman (1984).

2. For the basic tenets of the logical positivist/empiricist approach, with its connection to the empiricist epistemology, see Hollis and Nell (1975: 1-10).

3. According to Weber, "an ideal type is formed by the one-sided *accentuation* of one or more points of view and by the synthesis of a great many diffuses, discrete, more or less present and occasionally absent *concrete individual* phenomena, which are arranged according to those one-sidedly emphasized viewpoints into a unified *analytical* construct (*Gedankenbild*). In its conceptual purity, this mental construct (*Gedankenbild*) cannot be found empirically anywhere in reality. It is a *utopia*" (Weber 1949: 111).

4. One problem of the general equilibrium (GE) theory is its being a "barter" system; this theory is not capable of explaining the demand for money even as a medium of exchange. Therefore, it is possible that the introduction of money may completely destroy the conclusions of the GE theory. In fact, some "Keynesian" economists such as Robert Clower asserts that this is the case.

5. Musgrave also adds that criticism may change the status of an assumption; what in youth was a negligibility assumption may be reduced in middle-age to a domain assumption, and decline in old-age into a mere heuristic assumption (Musgrave 1981: 385). For example, "Assume that the budget is balanced" can be formulated as "Whether or not the budget is balanced makes no detectable difference to the phenomena being investigated" (negligibility) or can be written "If the budget is balanced, then ..." (domain), or "Assume for the moment that the budget is balanced (we will relax this assumption shortly)" (heuristic) (Musgrave 1981: 385-86). In this case, we should be careful about the distinction between them.

6. The problem here is concerned with the relationship between the "empirical laws," i.e., those statements that can be confirmed directly by observation and the "theoretical laws," i.e., those statements that are concerned with nonobservable elements, and cannot be derived from empirical laws (Carnap 1966: 133). With regard to confirmation of theories, therefore, we need sentences or rules which connect observables (such as the temperature of a gas) and nonobservables (such as the kinetic energy of molecules) which are called by Carnap as *correspondence rules* (Carnap 1966: 137). On this view, theories are primarily "syntactic" objects whose terms and claims are interpreted by means of these correspondence rules. Theories are collections of lawlike statements and form deductive systems in which the relationship between lawlike statements are provided by correspondence rules (Hausman 1992: 297). However, such a defense for positivism is shown to be untenable, as we are about to see.

7. Of course, this can also be said for the explanatory power of economic theory. However, an effect of positivism on economics was a contribution to economic theory in its becoming a closed, technical discipline conceived as independent of any moral judgement (Hahn and Hollis 1979:1). A quick inference from this might be that it is more than a mere coincidence that the "perfected" form of the GE framework has been

achieved in the 1950s, a period characterized by the dominance of positivism, though such a claim requires more extensive work. But this fact does not undermine the claim that positivism has been used as a defensive strategy by economists.

8. Yet, interestingly, most "Post-Keynesians" seem to claim the validity of Kuhn's account (e.g., Dow 1985). The reason may be that they see their position as a promising candidate for the replacement of the dominant paradigm. Unfortunately, it is usually emphasized that the Post Keynesian "school" has never been a coherent approach (Walters and Young 1997).

9. Perhaps for this reason, Feyerabend has dedicated his *Against Method* (Feyerabend 1975) to Lakatos with the words "fellow anarchist."

10. Weintraub's attempt to apply MSRP to economics has drawn many criticisms. For a debate over Weintraub's position, see Rosenberg (1992: 91-111); Backhouse (1991); Salanti (1991, 1993a,b) and Weintraub (1991).

11. Recently, Blaug (1992: 168) seems to have abandoned his insistence on MSRP, for he argues that Weintraub's attempt to characterize neoclassical economics as a research program misrepresents the developments in neoclassical economics from 1930s to the present.

12. Formally, if the domain characterizes the alternative bundles of goods x , y and z , and R is a preference relation such that xRy is read as "x is at least as good as y," then rationality requires that R is (a) *complete* (for all bundles x and y , either xRy or yRx or both); (b) *reflexive* (for all bundles x , xRx); and (c) *transitive* (for all bundles x , y and z , if xRy and yRz , then xRz). And additional assumptions for consumption sets are; (a) *continuity* of preferences, (b) *monotonicity* which means the more is better, (c) non-satiation meaning one can always do a little bit better, and (d) *convexity* of preferences which corresponds the diminishing marginal utility principle. On the basis of these assumptions we can assign ordinal numbers to each bundle, that is, utility functions, which in turn can be used to derive individual demand functions. By making corresponding assumptions about production sets we can derive supply functions and hence we can get equilibrium (see Varian 1992 ch. 7). On the basis of these assumptions, the three important problems concerning equilibrium are the existence, uniqueness and stability of equilibrium. As a result of this analysis, the two "Fundamental Theorems of Welfare economics" can be proven: 1) Any Walrasian equilibrium is Pareto-efficient, and 2) Under favorable conditions (convexity etc.) any Pareto-efficient allocation is a Walrasian equilibrium.

13. Nevertheless, it is quite interesting to see that these three elements also characterize the school of thought known as "Analytical Marxism." According to Hunt (1992: 92-93), for example, there are three common elements unifying this school: (1) Methodological individualism, (2) the axiom that all human activity can be reduced to rational, utility maximizing exchanges, and (3) an ahistorical acceptance of certain

institutional features of capitalism as the natural, eternal, and ever-present framework within which rational choices occur.

14. Almost no economist would deny the existence of uncertainty and imperfect information. However, since there is no "correct" definition of imperfect information, this assumption seems to be a "last resort" strategy; any problem can be attributed to imperfect information. In other words, it is just as arbitrary as the rationality postulate itself.

15. In fact, Rosenberg's criticism amounts to asserting that, rationality assumption, unless it is backed by some psychological mechanism, is inconsistent with the presumed reductionism of the whole framework. That is to say, this theory presupposes a "local" reductionism, which requires further explanation about why reductionism is to stop at the individual level.

16. For a good exposition of the weaknesses of these criticisms, see Hahn and Hollis (1979: 8-11).

17. Yet, for a different view concerning the falsifiability of "All-and-some" statements, see Mongin (1986).

18. Here the equilibrium is achieved by price adjustments. However, if prices are not flexible this equilibrium cannot be achieved. For some "Keynesian" economists such as Robert Clower and Axel Leijonhufvud, adjustments are made by quantities, since prices are fixed. These economists have been criticized as they lack of an explanation of why prices are fixed, for rationality implies perfect flexibility. Yet, this quantity adjustments idea does not question the very framework of GE theory, with its rationality and individualism.

19. GE theory is actually a "timeless" setting, that is, there is no "historical time" in this framework. In this theory, comparative static analysis in which two distinct states of the economy are compared without explaining actual adjustment process between these two states is used.

20. This conception roughly corresponds to what Rosenberg calls "generic predictions" (Rosenberg 1992: 69).

21. Therefore, according to Bhaskar, positivist approach is based on a "trinity": Empirical realism, which is based on Humean causality view, *epistemic fallacy* which assumes that statements about ontology (about being) can always be reduced into statements about epistemology (about our knowledge of being) (Bhaskar 1975:16) and sociological/methodological individualism.

22. As a matter of fact, Duhem and Quine's above-mentioned thesis is an instance of the principle of emergence, prevalent at the level of the theory. Such a principle, on the

other hand, implies that the notion of causality is not to be taken as a mechanistic one, presupposing atomistic events, but, on the contrary, it should be taken as a "holistic" notion implying: "(a) the totality, i.e. the form or structure of the combination, causally determines the elements; and (b) the form or structure of the elements causally codetermine each other and so causally codetermine the whole" (Bhaskar 1994: 77).

23. Such an inconsistency between the individual behavior and the unintended consequences is, in the context of Menger's writings, shown in Özel (1998).

24. According to this theorem, in order for such a social preference ordering to be found, five conditions must be met simultaneously: *collective rationality*, which can be subsumed under *unrestricted domain*, stating that given the individual orderings, the social choice from the available alternatives must be determined by an ordering, i.e., the social ordering must meet the same requirements of rationality imposed on individual orderings (it must be complete, and transitive); *Pareto Principle*, stating that if every individual prefers x to y, then society must prefer x to y; *independence of irrelevant alternatives*, stating that the society's choice from all feasible alternatives must depend on only on the individual orderings of those social states, and only variations in preferences over the relevant alternatives are allowed to have any influence. That is, if one social state becomes irrelevant or infeasible, then the final outcome must be unchanged as long as individual orderings over the remaining feasible alternatives do not change; and finally, such an ordering must be *non-dictatorial*, i.e., there should be no individual whose preference orderings automatically becomes society's preferences, regardless of what all the other members of the society want (Bonner 1986: 56-63). Arrow shows that these assumptions are too strong, and only if we allow, for example, a dictatorial social ordering, then the other assumptions can hold too. As a matter of fact, for our purposes, if we allow the principle of emergence to work, the assumption of the "independence of irrelevant alternatives" becomes meaningless, for if one alternative becomes irrelevant, this may change the other alternatives as well.

25. An excuse for this might be that Welfare Economics is the only "non-positive" component of the neoclassical economics which depends on value judgments, and hence such a problem does not affect the conclusions of the GE theory. However, as can be seen from the "Fundamental Theorems of Welfare Economics" mentioned in the note (12) above, one can easily have the impression that the whole GE framework is developed in order to support Welfare implications, especially with respect to the issue of the role of government in actual economies.

