

## Methodology of Economics: A Survey of Matters and Methods

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### **Abstract**

In spite of some pessimistic attitudes towards methodology of economics, it has succeeded as a central theme in numerous books, refereed journals, international conferences and symposiums whose volume is increasing at an unprecedented rate. The proliferation of methodological literature also is accompanied by a proliferation of issues that concern: the goals of economics, realism of economics, mathematics and economics, history and economics, ontology and complexity. Economics usually is associated with the hypothetical deductive method. But short-term and cycle analysis of macroeconomics and classical analysis of long-term development do not depend on this method, instead they are carried out within the methodological frameworks of historical-deductive, empirical-inductive or dialectics. This paper focuses on the status and diversity of methodology in economics.

**Keywords:** Methodology of Economics, Economics & Ontology, Economics and Mathematics, Goals of Economics, Complexity, Realism in Economics.

### **1- Introduction**

There is an episode in economics that says: no reputable economist would probably ever risk her (his) reputation by writing about methodology of economics (henceforth briefed as ME) in professional journals or speaking about

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it in professional gatherings (Koopmans, 1957:130-132, Boland, 1997:284-285). There is another episode that says: methodology of economics is a central theme in numerous books, refereed Journals, international conferences and symposiums, graduate courses and research projects whose number is increasing at an unprecedented rate (Hausman, ed., 1995, pp. 40). Some of the more recent period additions to the stream of literature that treat methodology of economics as their prime concern include: Methodology of Economics by Blaug (1991)<sup>1</sup>, Facts and Fiction in economics: Models, Realism and Social Construction by Maki, ed.( 1991 ), Methodology of Macroeconomic Thought, by Dow (1996), The Economy as an Evolving Complex System II, by Brian, et al, ed. (1997), Methodological Foundations of Macroeconomics: Keynes & Lucas, by Alessandro Vercelli (1991), The New Evolutionary Microeconomics: Complexity, Competence and Adaptive Behavior, by Potts (1991) and Reorienting Economics, by Lawson (2003). In addition, we now have the Journal of Economic Methodology, the Socio-economic Journal, and the International Network for Economic Method, the Association of Heterodox Economics, and many more Journals and professional societies that consider methodological questions as their main concern.

The proliferation of methodological literature also is accompanied by a proliferation of questions that generally fall under epistemology (the study of the nature of economic knowledge) or under methodology (the study of the method(s) of constructing economic knowledge). Some of these questions are related to the goal of economics, the degree of realism of economics- whether realism of assumptions or conclusions drawn from these assumptions; others are related to the “mathematization” of economics, economics’ indifference to history, ontological failures of economics and so the story goes on. The main issue at stake is that “monism” does not seem do the job any more.

Economics usually is associated with the hypothetical-deductive or apriori methodology and from this follows the proposition that economics is a

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1- Originally published in 1980 with 10<sup>th</sup> reprint in 1991.

hypothetical science and detached from the real world ( Hausman , ed., pp.1-2 Blaug, 1991 , pp. 77- 78, Rosenberg,1983, in Hausman, ed., 1995, pp. 376-394 and Lawson, 1997, Chapter one). But, it is not quite accurate to associate economics exclusively with the a-priori method. Short-term and cycle analysis of macroeconomics and classical analysis of long-term development do not depend on this method, instead, they are carried out within the methodological frameworks of historical-deductive (Kalecki, 1971, Bresser-Pereira, 1996, 2003), empirical-inductive (Sims, 1995, Simkins, 1999) and dialectics (Ozawa, 2004). Each of these methods has its own application and to reduce economic methodology to any one of them would be an oversimplification that is not readily justifiable.

To proceed with methodological matters and diversity in economics<sup>1</sup> which constitutes the theme of this paper, we first begin with some matters concerning: the status of ME, the goals of economics and the methodology most appropriate to each goal. Then, we will have brief surveys of the methods used in each of micro, macro and development economics. Advantages and disadvantages of the use of mathematics in economics also are discussed? We also will attend questions concerning realism of economics and how economics fares with ontology and complexity.

The central proposition of this paper is that methodology of economics is not limited to hypothetical deductive or any other single method, even if some economists might still support methodological monism, i.e., the use of a single method<sup>2</sup>. The use of a single method limits the range of problems that economics can handle successfully. For example, economic models based on hypothetical deductive method are too reduced and too narrow to face complexities and

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1- The existence of several methods in economics does not mean the same thing as the existence of several truths. Truth remains undivided. It is just that there exist several approaches to Truth.

2- Most standard microeconomic textbooks follow a hypothetical deductive methodology, but economics is not confined to microeconomics and even in microeconomics, there are topics such as game theory that do not depend on the hypothetical deductive methodology.



problems such as volatilities and disparities. These problems can be handled more effectively within different methodological settings, such as ones employing assumptions derived from historical facts.

So I first begin with some observation about the status of economic methodology. This will be Section II after this introductory section I. Section III deals with goals of economics and how might they make a difference to the choice of methodology. Section IV, V and VI will deal with methodology of microeconomics, macroeconomics and development economics, respectively. Sections VII, VIII and VIII will treat mathematics and ME, Ontology and ME and Complexity and ME, respectively. The last section (X) will summarize and bring the paper to its conclusions.

## **II- The Status of ME**

Traditionally, economists have not been very accommodating to methodological questions and although now this is changing, there still are some reservations especially among orthodox economists to explicitly consider methodological questions that according to them do not belong to economics but to philosophy or even to speculative arts. For one example, Harrod writes:

“Speculations upon methodology are famous for platitudes and prolixity. They offer the greatest opportunity for internecine strife: the claims of the contending factions are subject to no agreed check, and a victory, even if it could be established, is thought to yield no manifest benefit to the science itself. The barrenness of methodological conclusions is often a fitting complement to the weariness entailed by the process of reaching them.

“Exposed as a bore, the methodologist cannot take refuge behind a cloak of modesty. On the contrary he stands ready by his own claim to give advise on all and sundry, to criticize the work of others, which whether valuable or not, at least attempts to be constructive; He sets himself up as the final interpreter of the past and dictator of future efforts”( Harrod, 1938, pp. 384-412 as quoted by Blaug, 1991, p. xi)

For another example, Koopmans, an Harrod contemporary, observes:

“If methods of scaling are ever applied to measure the relative prestige of various topics in economic research, methodological discussion will undoubtedly be found to rank near the low end of the scale” (Koopmans, 1957, p. 129).

For a third example, Hahn says:

“Methodology like original sin won’t go away . . . What I really wanted to advise the young on was to avoid spending much time and thought on it. As for them learning philosophy, what next?” (Hahn, 1973 as quoted by Lawson, 1997, p. 12).

Yet for a fourth, and more recent example, one may cite Samuelson who says:

“I rather shy away from discussions of Methodology with a capital M. To paraphrase Shaw: Those who can, do science; those who cannot, brattle about its methodology” (Samuelson, 1993, in Szenberg, ed., 1993, p. 240).

Hence, doing methodology, according to the implications of these quotations, is in the least a much less valuable enterprise than doing economics. Economists are advised to avoid methodological inquiries because such inquiries do not yield any manifest benefit to the science of economics. Methodological conclusions are unproductive because the reasoning by which they are reached is weary.

It is doubtful that these incredulous statements can be considered typical of a profession-economic- that owes a great deal to economists such as John Stuart Mill and his famous methodological essay: “On the Definition of Economy and the Method of Investigation Proper to It”(1836), who apparently felt no embracement to write on methodology. Nevertheless, it is probing to see why methodology might have scored such bad marks as it has.<sup>1</sup> For one thing, economic methodologists since the late 19<sup>th</sup> century have hardly said anything that directly bears on economic literature in general and what they have said bears more on philosophy than on economics (Boland, 1983).

For another thing, methodologists often take the general equilibrium theory as a prototypical model in economic analysis and generalize its philosophical, social, and biological deficiencies to the whole of economics. But general equilibrium theory is only one sub- field within the field of economics, not the whole field. It is true that the general equilibrium model is used in understanding

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1- According to Wallerstein and his followers at the World-System Network, “economics” can not be separated from philosophy and social sciences such as sociology and political sciences. There also are many who would argue that economics is bound up with culture, believes, ethics and institutions.



the abstract workings of a competitive market, but this does not mean that economic analysis is limited to the analysis based on the general equilibrium model. Economic analysis also includes other topics such as various market imperfections, asymmetric information, risk and uncertainty. Even in general equilibrium theory there is a whole lot of applications such as: international trade, employment and income distribution, that methodologists often ignore (Variant, 1993: 4).

For a third thing, some of those who have spoken on methodology have done so out of an impetus that calls for some form of deliberate management of economies. This includes economists that fall under the umbrella of Socialists, Institutionalists, Neo-Keynesians, and so on, whose philosophies are not the same as that of “free market capitalism”. It also includes methodologists opposed to Friedman’s “The Methodology of Positive Economics”, because of Friedman’s alleged association with a particular (mostly Chicago) school of neoclassical economics that carries strong neo-liberal political inclinations, allegedly responsible for the recent slow down in world economic performance including the growing income disparities. Josef Stiglitz’s position on this issue and his quibble with the World Bank and IMF is too well known to be repeated here. But of course Friedman’s methodological theory is one thing and his alleged association with the Chicago School of Economics is another and methodologists who think that they must begin venturing into methodology by choosing sides with or against Friedman seem to be mixing methodology and ideology (Boland, 1983).

A fourth reason may be because economists are so busy developing the technical tools of economic measurement and analysis that they do not find the time to deal with other methodological issues. In fact, of the five components of economic theories, namely: (a) the behavioral assumptions about the basic goals of the actor, (b) the auxiliary assumption specific to the problem at hand, (c) the rule that the actor employs to achieve his goal, (d) the constraints faced by the actor and finally (e) confronting predictions drawn from these elements with the data to see how well they explain them, the progress made in the last four components (technical mostly) has been substantially more than the progress made in the first, so that investigating the behavioral assumptions seem to have been an enterprise of last resort (Mueller, 1992, as reprinted in Hausman, 1995, P.300).

Still a fifth but less likely reason has to do with a presumption that the better-established economists may become accustomed to specific habits of mind and, therefore, resistant to any change in the *status qua*. This complicated socio-psychological proposition seeks to explain how methodological foundations of an economic orthodoxy may become institutionalized and resistant to change (Frey, 2000).

Criticizing the works of others certainly is not a modest enterprise and it takes a lot of audacity to claim true knowledge of how others have come to their knowledge and also of where others' knowledge construction scheme might have been flawed. Indeed, if the only task of methodologists were to criticize the works of others, and if methodologists only were to issue prescriptions for how economics should be pursued, then methodologists would probably stand on the same level as dictators. But the main project of methodologists really is not issuing prescriptions on how economics should be done but rather providing extra facilities for a better understanding of economics and its limitations.

Any way, methodology is here to stay. The unprecedented recent record of more and more economists turning to methodological questions without any apparent embarrassment may itself be taken as a testimony that ME has a better status now than it had in earlier periods and than some orthodox economists might think. Many universities around the world have introduced methodology as an independent course or even as a separate specialization within the field of economics. As mentioned before, the number of books, journals and conference proceedings published in recent periods has increased dramatically.

It is likely that the efforts expended on methodological discussions will continue to rise and methodology will come to the forefront of economic literature. This optimism is based on several reasons: The simplest one is that economics needs to come into terms with its own critics. For example:

"The two methods of inference-from sufficient reason [as in economics] and from efficient cause [as in some physical sciences]-are out of touch with one another and there is no transition from one to the other" (Veblen, 1909, as reprinted in Hausman, ed., 1995, p.147).

It is part of the mission of economic science to have a open eye on how it is being reviewed. Admittedly, economics is a peculiar subject. Many of its premises, as Hausman observes, are either platitudes- such as: individuals can rank options or they chose options that they most prefer- or simplifications- such as: commodities are perfectly divisible or individuals' information is perfect.



From such platitudes and simplifications, economists have concluded an immensely mathematical edifice that although not “necessarily erroneous” are nonetheless often inapplicable. Furthermore, in order to explain why economic agents do what they do, economists often invoke reasons, not causes. Now there is an argument in philosophy of science that says explanation based on reasons is not the same as explanations based on causes. In fact explanations based on reasons may not be explanations at all (Hausman, ed., 1995, p. 2, also see : Blaug, 1991, preface).

Another reason is the increasing pressure from economic dissidents for opening up the doors of economics to diverse methodological views (Monaghan, 2003). The argument is that economics will, in fact, benefit from diverse methodological views such as: historicism, inductivism, institutionalism, Systemism and computer based simulations. Economics’ sole dependence on hypothetical deductive method, it is argued, renders it less able in explaining phenomenon (Lawson, 2003).

Yet a third reason is what Redman and Machlup brief nicely in the following passages, respectively :

“Methodology is here to stay. Perhaps natural scientists have less use for methodology because they can experiment and control their environment, but economists need methodology because the substance of their science is argument” (Redman, 1993, p.102).

“The danger of vacuous chatter is great if one engages in methodological discourse without previous work on substantive problems. On the other hand, even a lifetime of scientific research does not generate, let alone guarantee, comprehension of methodological problems” (Mach up 1978, in Redman, 1993, p.102)

Even If not for any of these sakes a number of economists would still choose methodology because they feel “a strong inner urge” to say something (Koopmans, 1957, p. 130). As a conjecture, I might say that keeping the doors open to alternative methodological views will provide a forum for critical and progressive thought in economics.

### **III- The Goals of Economics and ME**

The expression: “methodology of economics”, to quote Blaug, conjures some ambiguity: it may refer to the methods or technical procedures of economics. It may also denote an examination of concepts, theories and basic



principles of reasoning of economics. It may be interpreted as philosophy of science applied to economics meaning that it investigates the process by which economists explain. Questions of methodological nature obviously involve questions of the scientific credential of economics (Blaug, 1980, preface). But what is science? In the words of Nagel, a prominent philosopher of science:

“It is the desire for explanation that are at once systematic and controlled by factual evidence that generates science; and it is the organization and classification of knowledge on the basis of explanatory principles that is the distinctive goal of sciences” (Nagel, 1961: 4, as quoted by Blaug, 1980 :preface).

Since economics provides numerous examples of explanations that are at once systematic and controlled by factual evidence, there remains no reason why we should doubt the scientific nature of economics, particularly, because there is a “general consensus [among philosophers] that we have no notion of science good enough to measure candidates against”. (Roseburg, 1983, as reprinted in Hausman, ed., 1995, p. 376). Hence, since there are no definitive and widely accepted criteria by which we can demarcate science from non- science, there remains no strong reason for denying economics the scientific status that it deserves.

Economists, however, divide on what they consider to be the primary objective of economics: Is it explanation or is it prediction and control? Those who believe that the primary objective of economics is to explain fall under the banner generally called “realists”. Those who believe that the primary objective of economics is to predict and control fall under the group conveniently called “instrumentalists” or “pragmatists”. There are also economists that have a foot in both of these categories, arguing that the predictive and explanatory objectives of economics do not necessarily have different logical foundations. Their story is that explanation and prediction use the same logical rules of inference so that what constitutes explanation may at the same constitute prediction. To them, “*explanation is simply prediction written backwards*”. In explanation one starts with a statement of what is to be explained and finds at least one universal law that together with a set of initial conditions logically imply the statement of what is to be explained. In prediction, on the other hand, one starts with a universal law plus a set of initial conditions and from them one deduces a statement about an unknown event (Blaug, 1980, pp. 3-4, Hausman, ed., 1995, p.6).

This, so-called *symmetry thesis*, is attributed to Hempel's deductive nomological (D-N; "nomo" means law in Greek) model of explanation. As it says: all truly scientific explanations involve at least one universal law (general regularities as opposed to accidental generalizations) of the kind whenever event x, then event y, plus a statement of relevant initial conditions that together constitute *enplanes* or *premises* from which an *explanandum*, a statement about some particular regularity (event, phenomenon) that we are seeking to explain, is deduced by the rules of deductive logic ( Hausman, ed., 1995 : 6-8 and Blaug, 1980 :3-5, Lawson, 1997, pp.17-18). This can be represented schematically as follows:

Statement of initial or boundary conditions

Laws or general regularities of the type whenever x, then y

Statement of a particular regularity (event) whose explanation we are seeking

(The "line" represents a deductive inference).

This scheme is intended to show that the particular regularity that we are seeking to explain is an instance of some broader regularity by deducing a statement of the particular regularity from those broader regularities and other true statements.

An example, borrowed from Hausman, can illustrate the point. Suppose we wish to explain why more computers are sold now than six years ago. We can explain this by deduction from the true statement that the price of computers is lower now than it was six years ago plus the demand law that says a change in the price of a commodity will change the quantity demanded of that commodity in the opposite direction. This, however, is only an account of a deterministic explanation. If we only have a statistical regularity then we will not be able to deduce what is to be explained, though we may be able to show what is probable, which is what Hamper's inductive statistical (I-S) model requires. In the case of prediction on the other hand we start with the law of demand plus the fact that computers are cheaper now and from these premises deduce that more computers are sold now compared to six years ago. Here, prediction is typically



used to see whether the demand law is in fact upheld (Hausman, ed., 1995, pp. 7-8)<sup>1</sup>.

But prediction need not always imply explanation, nor need explanation always imply prediction. For example, a moving average model of a time series in economics can yield predictions without necessarily entailing any explanatory implication. Evidently, explanation requires more than establishing a relation between variables; it requires some intertwining causal mechanism that can account for the existence of such a relationship. The events regularities present in the D-M model are of the form whenever x occurs, and then y occurs. Here causality is present only in a Humean sense: as the constant conjecture of two events that happen in time and space, without any indication of the presence of any intertwining causal mechanism. This type of causality does not seem to lie within the methodological preview of a “realist”.

Now a “realist” would probably define economic methodology as that branch of knowledge that deals with the question of how economists explain, or how economists come to know the truth (the reality). In other words when some economist claims that she(he) has arrived at some knowledge of social reality, a realist methodologist would ask how does that economist know that she (he) has come to know the truth (reality) and nothing but the truth (reality). This question cannot be answered, a realist would say, without reference to ontology, which will be taken up further down in this paper.

But, instead, if the objective of economics is not to know the truth, but is limited to prediction<sup>2</sup> and control, then the relevant definition of economic methodology would probably be: the study of how economists come to construct theories (models) that can predict and possibly be used to control phenomenon. This is the position usually known as “instrumentalism” or “pragmatism”. If this

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1- For a different example see Lawson(1997).

2- Here, we are using prediction in the sense: if event (a) occurs, then event (b) will occur. This is different from the sense we use the word prediction to mean “forecast”. Prediction involves “logical time” while forecast involves “historical time”.



were the case then a realist would lose ground for raising the issue of realism with an instrumentalist whose objective is not to explain reality but to predict it. The issue that an instrumentalist could be held accountable for would be whether or not economics were successful in doing its prediction job.

A realist (in the sense of scientific realism not philosophical realism) would probably say "that there are ultimate objects of scientific investigation, and that these exist for the most part quite independent of, or at least prior to, our investigation of them" (Lawson, 2000: 10). In other words there exist a material and social world, which does not depend on any individual consciousness, but whose knowledge is accessible to human consciousness and whose nature can be encapsulated within some true theoretical system. It is the knowledge or theories of this kind that a realist would aim at.

An instrumentalist would probably (since there are different versions of instrumentalism) assert that the objective of economics is predictive not explanatory. Seen from this perspective, theories may even be false to the reality they are purported to explain. But this does not distract from the usefulness of false theories because they nevertheless preserve their predictive ability. To an instrumentalist, arriving at theories with predictive ability is all that is required.

Occasionally, the realist/instrumentalist debate centers on philosophical questions about the treatment of "unobservable", an issue that does not seem to concern economic theories, because economic theories hardly postulate the existence of unobservable entities or properties other than variants of everyday observable such as beliefs and desires. If so when a methodologist seeks a theory of prediction, she (he) at the same time is seeking a theory of explanation (Van Fraassen, 1980 and Boyd, 1984, in Hausman, 2003, p.14).

It should be said that the philosophical roots of "realism", as Stairs has noted, vary. For example, an Aristotelian would probably be interested to know what reality is like at the most general level -- what kinds of things there are and how they are related. The *physiologic* of pre-Socratic era took various elements -- water, air, and fire -- to be the basic stuff of reality. The Pythagoreans seem to have believed that number -- or perhaps the Limited and the Unlimited -- were at the root of being. For the atomists, it was atoms and the Void. Plato believed that abstract, eternal "ideas" or *forms* were the truly real (Stairs, 1998). Certain "critical realists" such as Lawson, 1997 and Bahaskar, 1993, employ a concept

of reality that seems close to the Aristotelian concept of ontology, which involves inquiries into the nature of existence. As Maki writes:

“One can be a realist about the world and about theories of that world. Take *T* to be a theory, model, or assumption related to chunk *S* of the world. One is a realist about *S* in relation to *T* if one believes that *S* exists independently of accepting, believing, or uttering *T*. One is a realist about *T* in relation to *S* if one thinks that *T* and its constituents refer to *S* or that *T* in addition truly represents or should truly represent *S* – where truth is likewise independent of whether *T* is accepted, believed, or uttered. These definition sketches imply that, for example, the observability of an object and the testability of a theory are conceptually unconnected to realism” (Maki, ed., 1991: 8).

Other “realists” follow a methodology that depends on taking account of the type of evolutionary changes that a deductivist methodology sidesteps. Some of the so-called heterodox strands of economic thought and the Santa-Fe approach fit this category.

Nor has the term “instrumentalism” always had a unanimously accepted interpretation. For example, is Friedman’s methodological position, in his well known 1956 paper: “The Methodology of Positive Economics”, that of an instrumentalist? If so, then his position would probably be to defend the propositional constituent of theories that predict adequately well without being necessarily true. However, there are definite limits to the extent that this position can be defended at level of economic actors and markets. To paraphrase Simon: the proximate propositional components of the relevant theory at the actor level are: 1) *x*- businessmen seek to maximize profits, and 2) *y*- businessmen have the ability and the will to identify the profit maximization course of action. The relevant theory at the market level can be summed up as: 3) *z*- prices and quantities observed at those levels that assure maximum profits for firms in the market. An instrumentalist defense of the theory consisting of *x*, *y* and *z* is that it does not matter if *x* and *y* are false, provided *z* is true. This schema of taking *x* and *y* as premises and *z* as a conclusion fits one common if implicit notion of explanation so that one explains the “macroscopic” by the “microscopic” plus some composition law. In other words, one explains the market by the actors while believing that individual actors are simple components of the complex market and therefore proper explanatory elements. The *x* and *y* elements, plus the composition laws, allow one to derive other propositions, for example about shifting of taxes, at the market level, which one is not able to test by direct



observation. But there is no evidence that the proposition of  $z$  is a tested one; “No one has, in fact, observed, whether the actual position of the business firms are the profit maximizing ones. Nor has anyone suggested a method for testing this proposition by direct observation”. So, if  $x$  and  $y$  are not empirically valid assumptions, and  $z$  follows from  $x$  and  $y$  while  $z$  is untested, how then can one establish the validity of the  $z$  theory, i.e. the theory that says there is a price quantity vector that is consistent with maximum profit for firms in the market? (Simon, 1963, as reprinted in Hausman ed., 1995, p. 215)

Many, if not most, economists would probably embrace both objectives for economics, i.e. prediction and explanation. In that case, the type of causal relations used in economics once again matters and the question of whether economics does indeed have a satisfactory explanatory apparatus or not, once again, becomes a pertinent one. One way to get valid implications out of the deductive logic of reasoning in an explanatory apparatus is to replace the  $x$  and  $y$  propositions with the more realistic propositions of  $\hat{x}$  and  $\hat{y}$  with the latter based on historical facts, which constitutes the cornerstone of “the historical fact method” proposed by Bresser-Pereira(2003)

#### **IV- Methodology of Microeconomics**

During the interwar years (1920's to 1950's), microeconomics was under the heavy influence of “logical positivism” and its philosophical prejudice, which considers only scientific activity to be ‘meaningful’ and all else (including philosophy and methodology itself) to be ‘meaningless’. “(Boland,1983, p. 1). The popularity of logical positivism is somewhat in the wane these days, but micro economic theory still retains its essential positivist character. It mimics closely the science of physics with an eye on detectable empirical regularities that can be generalized into laws.

Paraphrasing Koopmans, one can discern the reasoning or the logical structure of microeconomic theories from underneath economists’ polished prose. It involves a chain that starts from certain premises called “postulates” (basic premises). Each postulate contains certain “terms” that represent the postulates in the analysis of persons, organizations, things, actions or states existing in real world or the world of experience. The terms in turn are made more insightful by the use of certain key words such as consumer, worker, entrepreneur, commodity, production processes, output consumption, probability and climate. Definitions, statements or descriptions are used to establish the link



between the terms and observable phenomenon. These descriptions, also called “interpretations”, provide relevance and economic meaning to the postulates. The pure logical structure of microeconomic theories, however, may form an “uninterrupted” system. In other words, from the view of the logic of reasoning, the interpretations are detachable: the only thing that matters is the logical contents of the postulates. Once a set of postulates is specified, it becomes a matter of logical or mathematical reasoning to draw implications that are verifiable or in some ways interesting. Often it becomes necessary to aid the reasoning process by introduction of additional terms such as: utility, price, income, capital, savings, and efficiency strategy, through definitions that use terms already in use. Some of these terms may be “primitive” in other pieces of analysis. The reasoning may prove that postulates are in contradiction with each other. Or it may reveal that the postulates are not sufficiently specific or numerous to have the kind of implications we are looking for (Koopmans, 1957: 131-133, also see: Hutchison, 1960).

Lionel Robbins, as noted by Koopmans, seems to attach a quality of immediate obviousness to the basic postulates of microeconomic theories. But once these postulates are spelled out literary and in details, the limits on their obviousness become apparent. Borrowing Koopmans’ example, consider the postulate that each consumer orders his preferences for all commodity bundles that he has to consume and compare it with our direct knowledge of how consumption is decided upon. In one interpretation, the postulate is used to explain how consumption is determined by given circumstances. It becomes immediately obvious that this interpretation deprives the consumer from the joy of random variability in consumption as well as consumption habits resistant to the change in circumstances (Robbins, 1935, as noted by Koopmans, 1957, p. 139).

Friedman) seems to believe that having an opportunity for direct verification of postulates is irrelevant to the objective of economics as a predictive science (1953). But as mentioned above, Simon and many others (not mentioned above) including Hausman does not find Friedman’s argument particularly compelling (Hausman, 1992, reprinted in Hausman, ed., 1995, pp. 217-221). Any way Friedman’s position and Robbins’ position, as Koopmans notes, are at odds and their defense does not make microeconomic theories any less fallible. The truth is that microeconomic theories are built on a few premises

that approximately represent an otherwise complicated reality and the fact that these theories may yield relevant policy implications does not render them any less fallible (Koopmans, 1957, p.141).

But fallible or infallible, microeconomics still retains its adherents if only under a more up to date assessment of Friedman's position. Thus, Lucas says:

"To observe that [microeconomics] is based on a superficial view of individual and social behavior does not seem to me to be much of an insight. I think it is exactly this superficiality that gives [microeconomics] much of the power that it has: its ability to predict human behavior without knowing very much about the make up and lives of the people whose behavior we are trying to understand" (Lucas, 1986, p. 425, as quoted by Lawson, 2000, p. 6).

From the standpoint of the position taken by Lucas, it seems that the objective of economics is strictly predictive and the question of how much it purports to illuminate on the realities of the social world, aside from depending on "a superficial view of individual" and on abstraction, is of secondary importance. Assumptions are there to serve as instruments only; they are not, as Hahn says, to be taken "descriptively":

"When a [micro economist] assumes that there is a three good economy lasting two periods, or agents are infinitely lived (particularly because they value the utility of their descendents which they know!), everyone can see that we are not dealing with an actual economy. The assumptions are there to enable certain results to emerge and not because they are to be taken descriptively" (Hahn, 1985, p. 15, as quoted by Lawson, 2000, p. 7).

But, as Lawson (2000, p.9) says: even if we accept that [microeconomic] theories can be "*interpreted as instruments successful at accommodating the data*" (italics are original) there still remains an issue "with the failure of the theory to fit the data in practice". Adherents of a deductive view would probably respond that refining or revising models with poor empirical performance usually resolves this issue. That is may be why economists continue to build models after models. As a matter of fact, new efforts in microeconomic model building include models that, as Kreps by Maki says, relax 'contextual' assumptions such as 'large numbers and anonymity of agents, shared information and static analyses and replace them by 'small numbers interaction, asymmetric information and non-trivial dynamics'. Some other models include more drastic steps in the form of relaxing assumptions such as far sighted



rationality, purposeful greed and equilibrium (Kreps, 1997, in Maki, 1991, p. 7). Still others include even more drastic steps in the form of “starting at the very foundations of [microeconomics] and systematically building up a vision of what an evolutionary [microeconomics] should encompass, taking up graph theory, systems theory and complexity theory along the way”(Wakeley, 2002, p.279, reviewing: Potts, 2000 ).

## **V- Methodology of Macroeconomics**

John Menard Keynes in a preface to the *General Theory* wrote:

“I have called my theory a *general* theory. I mean by this that I am chiefly concerned with the behavior of the economic system as a whole ...[a]nd I argue that important mistakes have been made through extending to the system as a whole conclusions which have correctly been arrived at in respect of part of it taken in isolation” ( Keynes, 1973, p. xxxii, quoted by Denis, 2003, p. 4).

Hence, according to Keynes, it would not be correct to extend the conclusions drawn from analysis of the behavior of parts of the economic system- firms and individuals acting in single markets- to the behavior of the economic system as a whole. In other words Keynes economics seem to purport the “system view” that says the sum is not a simple addition of its parts.

Lucas on the other hand favors an individualistic methodology and argues for ‘the reincorporating of aggregate problems such as inflation and the business cycle within the general framework of ‘microeconomic theory’ (Lucas, 1987, p.107, as quoted by Bunge, 2003, p. 4). He does not regard macro phenomena as ‘the province of [Keynesian] ‘macroeconomics’. Now, this is clearly a statement in support of a reductionist approach.

But reducing macroeconomics to its microeconomic foundations is a complicated and tenuous effort, because methodology of macroeconomics primarily uses historical analysis, whereas that of microeconomics primarily uses hypothetical deductive analysis (Bresser-Pereira, 2003, 1996, Simkins, 1999). Using Bresser- Pereira’s words:

“The historical- deductive method...that Keynesian macroeconomists originally employed to explain the cyclical fluctuations, started from the observation of new historical facts which involve ruptures and discontinuities, with the acknowledgement of the new resultant regularities, and with the analysis of logical connections between variables”(Bresser-Pereira, 2003, p. 10.).



Vercelli, as noted by Bresser –Pereira, compares the macroeconomic models of Keynes and Lucas. Whereas Lucas develops a project that ends up being merely reductionist – a reduction of macroeconomics to Walrasian microeconomics – Keynes developed a new method or a new way of thinking economic problems that founded macroeconomics and “gave it a permanently autonomous nature”. The heuristic model developed by Keynes in the *General Theory* starts from the acknowledgement of the enormous complexity and non-homogeneity of a modern monetary economy, and leads to the development of models characterized by a basic indetermination and by a structural instability (Vercelli, 1991, pp. 232-233, as noted by Bresser-Pereira, 2003, p. 12).

Furthermore, according to Dow, as noted by Bresser-Pereira, a modern monetary economy, is an ‘open system’, that is, a system in which ‘not all the constituent variables and structural relationships are known or knowable, and thus the boundaries of the system are not known or knowable’ (Dow, 1996, p. 14, as noted by Bresser- Pereira, 2003, p. 12). According to Bresser-Pereira, the method that is able to produce models for the kind of system, in which instability and uncertainty are heavily present, is the “historical deductive method”– the same method as Keynes used. When macroeconomists or policymakers use this method they don't work with certainties, they do not apply ready-made models, but take into account all the variables and models available, and only then make decisions, that is, choose in a setting of uncertainty (2003. p.12).

Borrowing Bresser-Pereira's example: so far as economists adhered to the assumptions of full employment, downward flexibility of wage, and supply creating its own demand, they failed to understand the nature of the 1930's Great Depression. But Keynes abandoned these assumptions whilst he also observed the new facts of the structural changes in the world economic system that were brought about by The First World War and the collapse of the Gold Standard before he wrote his General Theory. Once the general theory was in place, it served as new insights into problems that hitherto had remained unresolved because of the failure to observe the fresh historical facts. Keynes' model of macroeconomics was a model well geared to the reality of his time a model that opened a whole new field of economic policy (p.18).

Simkins gives a historical account of the deductive/ inductive or a-historical/ historical debate that hanged over macroeconomics thought of the 20<sup>th</sup> century:

The “measurement without theory” and “theory without measurement” debate “has its roots in the empirical-inductive approach to business cycle analysis developed by Wesley Clair Mitchell in the early 1900s. Mitchell’s quantitative economics... ultimately provided a wealth of descriptive information about the workings of market-based economies... Branded as ‘measurement without theory’ by Tjalling Koopmans... Mitchell’s empirical methodology was largely supplanted by... large-scale structural macro econometric models which formed the foundation of macroeconomic forecasting and policymaking in the 1960s and 70s. During the 1970s, statistical modeling techniques developed by Christopher Sims, were influential in reviving interest in empirical analysis of macroeconomic behavior... [E]mpirical macroeconomic researchers have progressively added more theoretical structure to Sims’ a theoretical statistical methodology during the last twenty years. At the same time, theoretical macroeconomic modelers [notably: Edward Prescott] have more closely tied their models to real world data, using observed behavior to suggest model parameter values and validate the business cycle properties of their models. The result has been a gradual but steady movement toward closer agreement between the theoretical and empirical approaches to macroeconomic research” (Simkins, 1999, p. 1).

So the debate in macroeconomic methodology centers on several interrelated issues that include: econometric vs. formal statistical procedures, deductive vs. inductive reasoning, historical vs. a-historical methods, empirical vs. theoretical and abstract vs. realism. Although these methodological procedures provide considerable insight into understanding the workings of a modern economy, but none is too far apart from the insights that Keynes himself (though not necessarily Keynesians) provided into understanding the working of a modern capitalist economic system.

Actually, as Fontana says: “in the past tow decades, there has been a flourishing of writings on the methodological approach of Keynes... One of the main outcomes of that research is the idea that Keynes during his career.... adopted a particular method of investigation based on the close connection between theory and practice” (Fontana, 2001). Hicks as quoted by Fontana says:



‘one has to talk about Keynes's methods, in the plural, since there are so many of them. It is not merely that there were changes of method between his three main books on money--the Tract on Monetary Reform of 1923, the Treatise on Money of 1930, and the General Theory of 1936. Even in the General Theory itself, the main method is a hybrid, a combination of two, which it is useful to distinguish. And there are the beginnings of other methods also’ (Hicks, 1985: 52, quoted in Fontana, 2001).

Harcourt and Sardoni, as noted by Fontana, have restated the same idea arguing, ‘Keynes's philosophical attitudes meant that in his economics he never liked to stray very far from actual happenings, from concrete situations and the use of language and concepts and practices which were grounded in them’. ‘Keynes's method represents the main legacy for the development of modern economics’ (Harcourt and Sardoni 1994, p. 134, in Fontana, 2001).

At the danger of exaggerating the intellectual performance of Keynes, it might be said, that Keynes's achievement in neoclassical economics matches Hegel's achievement in German classical philosophy. Remaining well within the confines of economics, faithfully abstaining from considering the socioeconomic processes as a whole, the Keynes's analysis advanced to the very limits of classical economic reasoning. Indeed, it bended the conventional economic knowledge to the admission that volatility, predisposition to stagnation, chronic underutilization of human material and human resources, are intrinsic in the capitalist system. It implicitly rebutted the zealously guarded “purity” of academic economics by illuminating the superseding importance for the comprehension of economic process of the structure of society, the role of the state and the other exogenous factors. Behind this lies Keynes's particular regard for what he observed in real world - the facts of his day so to speak. As a complement to this one may cite the Post Keynesian Robinson who says:

“The victory of Keynes theory over the orthodoxy of sound finance, for example, was not due to his superior logic but to the presence of the events in the world. Perhaps we shall finally owe the defeat of neoclassical complacency to the public indignation at the devastating accidents which highly probable technology is always bringing about” (Robinson, 1980, p. 119).



## VI- Methodology of Development Economics

As Ormazabal notes, the first economists who studied development were Adam Smith and Ricardo. Ricardo, for example, began his analysis of the workings of a capitalist economy by employing the concept of value in exchange of labor that he took over from Adam Smith. The ultimate aim of both economists is to account for profit, but profit is surplus value in exchange. In order to understand profit, Smith and Ricardo say, we first must understand value in exchange, and, on this basis, we will be able to understand profit or surplus exchange value. "Profit is the name of the game in the classical economics, simply because it is understood as the name of the game in capitalistic economy" (2003, p. 16). It is the driving force in the development of capitalism. Yet, It may be added that the labor theory of value, depending as it does on physical labor alone and not including human capital, including knowledge, culture and spirituality does not explain the different forms of labor that have evolved in different epochs of time.

There also are stage theories of development with Josef Schumpeter (1883-1950), W.W. Rostow and Marx (1818-1883) each expressing their own version. Albeit their differences, they all seem to share in the observation that development proceeds in stages. Of these stage theorists, Rostow stands in prominence in the literature on mainstream economic development. Rostow identifies five stages of growth in the history of the world development, which he then uses to explain the "major discontinuities" of economic development in the now-industrialized nations (Taylor, 2004: Chapter 14).

Summarizing Taylor, the first stage Rostow identifies is a pre-industrial stage that he labels *traditional society*. But development does not actually commence until the preconditions for take-off are met. This involves an agricultural sector that it can feed a growing population of non-farmers; an infrastructure build up in the form of roads, canals or railroads; and a growing elitist group willing and able to lead the country into industrialization. Once savings of 10 to 15% of GDP are reached and invested on a regular basis in one or more manufacturing industries, the point of self-sustaining growth begins with leading industries working out their forward and backward linkages. The next stage is Rostow's drive to maturity where forward and backward linkages are exploited to their full advantage. The final stage or what Rostow calls "the

age of high mass consumption”, begins when rising wages lead to the increased consumption of new consumer goods.

Marx’s stages, however, might proceed through “catastrophic” disruptions from capitalism to some forms of socialism. According to Marx, as cited by Taylor, the expansion of capitalism will not proceed without conflict. The spread of capitalism to feudal or other pre-capitalist societies will evoke tensions between the invading capitalists and the landowning or other members of the older ruling class. If labor is abundant, there may be some temporary alliance between the capitalists and the old ruling class. But conflict will eventually arise and the industrial capitalists would triumph. Capitalism, by its very nature, is a world system. Capitalists will continuously seek out the world for new markets, cheaper sources of raw material and cheaper labor. This will spread the capitalist mode of production to all parts of the globe.

Marx and later Schumpeter also are among the founders of the cycle theories of economic development. Cycles come in different patterns. The shortest one is called the *Kitchin cycle* or the inventory cycle with duration of 3-5 years between the upper and the lower turning points that arise from an alternate buildup and depletion of business inventories. The second shortest is the *Juglar cycle* with a duration of 7-10 years between the turning points. These cycles are not understood well yet. Third comes the *Building (or Kuznets) cycle* that appears to last between 15-25 years and runs from the construction of buildings facilities until the time when they depreciate and must be replaced. The fourth one is the *Kondratieff cycle* with a length of 30-50 years that is associated with major technological innovations such as railroads and automobiles. Whilst Marx saw cycles as occasions for overthrow of capitalism, Schumpeter, saw them simply as an integral part of the capitalist system, though not an occasion for an overthrow of capitalism (Bronfenbrenner et al, 1990, pp. 176-178).

Ozawa identifies a dialectic methodology in the evolutionary approach of theorists from Karl Marx to Friedrich Engels and to Schumpeter, in the analytical approach of David Hume and David Ricardo and in the product “cycle theory of trade” and the ‘flying-geese’ paradigm of industrial upgrading. Ozawa’s version of dialectics “refers to a dynamic tension within a given system and the process by which change occurs on the basis of that tension and resultant conflict”. This derives its roots, as Ozawa says, from the Hegelian dialectics



according to which, “ideas evolve through a dialectical process—that is, a new idea (namely a thesis) gives rise to its opposite (antithesis), and in the wake of resultant contradictions and tension, a third entity (the synthesis) emerges”. According to Ozawa, this sequence that Hegel has labeled, as thesis-antithesis-synthesis is quite relevant for what is called “evolutionary economics” (Ozawa, 2004, p. 1). In Ozawa’s own words: “... there are *always* some elements or repercussions of the Hegelian dialectic in any evolutionary economic theory since an evolution of economic structure itself is a perpetual process of change with constant contradictions, self-transformation, and self-organization, a never-ending process that is stimulated and driven by opposing economic forces and tendencies”. At any given point in time, “an economic situation may be in a chaotic state, but eventually settles down to some stable temporary conditions to be disturbed again by new contradictory forces in a constant state of evolution” (p. 1).

Equally relevant to this description is Immanuel Kant’s (1724-1804) “transcendental dialectic”. Kant observed the contradiction between our being simultaneously subject to laws of nature (man being part of nature) in the *sensible* world and free (human autonomy) in the supersensible world. He resolved this by differentiating a ‘phenomenal man’ from a ‘noumenal man’ (Hunt, 1993, as cited by Ozawa, 2004). The phenomenal man is subject to the law of nature and determined by prior events, but the noumenal man can be autonomous without being predetermined in reasoning. And as the Oxford Dictionary says: ‘Noumenon is an ‘object of intellectual intuition devoid of all phenomenal attributes’; “antithesis to phenomenon”(Ozawa, 2004, p. 1, footnote).

The so-called “product cycle” theory of trade, using Ozawa’s description, refers to the phenomenon that a developed country initially exporting a commodity subsequently ends up in importing that commodity so that “the nation transforms itself from an exporter to an importer of the very product it had initially innovated at home”. The “flying-geese” refers to the paradigm that the growth of the upgrading of certain industries as evidenced in Japan followed a pattern “similar to a flying formation of geese”. This type of inverted V- or U-shaped pattern traced out the development sequence of *imports, domestic production, and exports* with given time lags as Japan pursued import-substitution-cum-export promotion in these industries. Here again we see

dialectic reasoning at work along the Hegelian lines of thesis-antithesis and synthesis (Ozawa, 2004, pp.5-7).

In other developments, according to Ray, that there is a need to move away “from a traditional preoccupation with the notion of *convergence*”. “This is the basic notion.... that leads to (a) a limited depth in the way we ask development questions, and (b) a certain type of policy bias”(Ray, 1998, p. 2).

Theories based on the notion of multiple equilibria, according to Ray, can help us stay away “from the determinism inherent in the convergence idea”. Some equilibria, Ray notes, are determined mainly by “..historical conditions. That is, given a particular historical experience, the outcome that results is fully pinned down, but the influence of that historical experience persists through time in observed outcomes. In either case, there is no presumption of convergence or a- historicity”(p.2).

Perroux adds a spiritual dimension to the methodology of development that according to him is best understood within the framework of the philosophy of action proposed by the works of Maurice Blondel. “In Blondel’s view”, he says: “It is impossible...to ‘abstain or stand aloof’; I am incapable of ‘self satisfaction, self-sufficiency and *self liberation*’. The individual is not a natural product of the universe, nor a successful achievement of life, but an act whose conscious comes ‘as it were from above’; it emerges from the Universe”. This doctrine, according to Perroux, implies faith in God; it sees a “religious dimension in all thought and the elements of thought in action” (Perroux, 1983: 115).

Hence, three dimensions may be added to methodology of development, which are dialecticism, disequilibrium and spiritualism. The common element in the first two dimensions seems to be their regards for history and they differ by their emphasis on inductive\ deductive or dialectic reasoning. The third is spiritual that deserves a wider space than the space of this paper permits.

## **VII- Mathematics and ME**

In recent periods a number of economists have expressed deep concerns with the rising trend in mathematization of economics. For example, according to Klammer & Colander, mathematics has flooded the curriculum in leading US universities and graduate schools. Only 3 per cent of graduate students surveyed on top US economics programmes perceived ‘having a thorough knowledge of the economy’ to be ‘very important’ for professional success, while 65 per cent



thought that 'being smart in the sense of problem-solving' is what matters, and 57 per cent believed that 'excellence in mathematics' was very important (Klamer and Colander, 1990, p. 18, as cited by Hodgson, 2004). And according to Mark Blaug who perhaps is one of the foremost methodologists of the mainstream economics: "[e]conomists have converted the subject into a sort of social mathematics in which analytical rigor is everything and practical relevance in nothing" (1997, p. 3).

The well known John Maynard Keynes is no less concerned with the dangers that the mathematization of economics might pose. Using Keynes's own words:

"It is a great fault of symbolic pseudo-mathematical methods of formalizing a system of economic analysis ... that they expressly assume strict independence between the factors involved ... ; whereas, in ordinary discourse ... we can keep "at the back of our heads" the necessary reserves and qualifications ... in a way in which we cannot keep complicated partial differentials "at the back" of several pages of algebra which assume that they all vanish" (Keynes, 1973: 297-298, as quoted by Fontana, 2001 ).

Of course, economics is not the only science that uses advanced mathematics. The so-called basic sciences like biology and physics also use advanced mathematics. But, compared to other social sciences such as sociology and political sciences, economics is perhaps alone in depending so much on mathematical reasoning. Most graduate programs in economics now require students to take courses in a sequence of quantitative method courses, including: mathematical economics, econometrics, linear and non-linear programming and even statistics. Some of the topics in mathematical economics like differential calculus and set theory, and some in econometrics/statistics such as "vector auto regression" (VAR) could get troublesome to students who lack undergraduate training in mathematics, statistics, biometrics, optimization techniques and the like. In fact those undergraduate economic students who have their undergraduate degrees in mathematics, engineering and perhaps even statistics have an advantage in performance over the students who do not have such backgrounds. A good background in mathematics and quantitative methods is not only advantages to graduate students in economics, but also to "would be professors" in that the latter get their tenure and promotions in a much shorter time and surer way than those who do not have any mathematical (technical)

background. It is therefore not a coincidence that Norman in a preface to his book says:

“My experience in the military and in the aerospace industry made me keenly aware of the importance of technological change in society...Deciding while moon walks were interesting, they would do little to alleviate social problems here on earth, I endeavored to become an economist and use my math and computer skills in development economics. In pursuing my PhD degree I was stunned to find that although earlier economists such as Marx and Schumpeter were deeply interested in technology, the mainstream economics taught by the faculty assumed tastes and technology as given and thus had almost nothing to say about how society promoted and adjusted to technological change. .... As an academic, I decided to write the minimum number of technical articles in prestigious journals to get tenure and to devote the majority of my energies to constructing a utopian design for emerging microbionic technology” (Norman, 1993, p. ix).

The implications of what Norman is saying in this passage are interesting: first, a mathematical (technical) background can suffice for studying economics, even without having to go through the full undergraduate program of economics- as a social science and second, one with a mathematical background gets a faster tenure by writing a “minimum number” of “technical” papers in “prestigious Journals” than one with no mathematical background. Actually, Journal standards for screening submitted papers often include the criteria of rigor and parsimony that, according to some Journal editors, only mathematical papers can afford. So economic papers nowadays use more mathematics than ordinary language.

With the exception of the mathematical overtone of the marginalist revolution and some highly mathematical economic papers such as “A mathematical theory of savings” by Ramsey, which belongs to late 1800, and 1920’s, the intensive mathematization of economics began in the second half of the 20<sup>th</sup> century. Both Marshall and Keynes kept mathematics out of their works even though they both had a background in mathematics. A sub-sample of economic paper topics that reflect the craze for presenting economics in mathematical form in the 1950’s and 60’s, include: “*On a theorem of von Newman*” by Loomis, L. H. (1946), “*The Dornan- Samuelson turnpike theorem*” by McKenzie, L. W. (1963), “*Separation theorem for convex sets*” by Debreu, G. (1959), and “*On the Stability of the competitive equilibrium*” by



Arrow, K. & H. L. (1958). The enthusiasm for mathematization of economics has continued to the more recent times with some well known works by Allen, R.G.D, (1967), Samuelson, P. A. (1976) and, Varian, Hall R. (1978) among many others .

Those who have tried to learn economics by reading these works know very well that doing that obviously requires a high degree of training in mathematical skills; not, however, that this, by itself, can detract from the value of these works as excellent pieces of scientific work. The extensive use of mathematics in these works seem to couple well with the type of questions and problems that their authors try to handle. In fact, if one is trying to find out an optimal saving rate or the conditions under which a general equilibrium of a hypothetical economy exists in a unique and stable form, the steady state solution of a Solow's growth model, a single price output combination of a firm at which the profit of that firm is maximized, or the exact multiple effect on output and employment of a given change in the money supply, one may not be able find a more efficient and economical way other than by way of mathematics (or geometry). Nor is there any other faster way of finding the average annual Iranian GDP growth for the past one hundred years, for example, other than by employing a growth formula.

Therefore, As Kirman says:

"The argument that the root of the problem ... [is] that we are confined by a mathematical strait jacket which allows us no escape, does not seem very persuasive. That the mathematical frameworks that we have used made the task of changing or at least modifying our paradigm hard, is undeniable but it is difficult to believe that had a clear well-formulated new approach been suggested then we would not have adopted the appropriate mathematical tools"(Kirman, 1989:137, as quoted by Lawson, 2003: 10).

So the problem seems not to be with the use of mathematical tools in economics per se; it rather is the coupling of a certain type of mathematical reasoning with deductive logic that yields results, which do not match well the reality around us. For example, as Robinson Says:

"The Marshallian method of exposition is to attempt to trace the effects over the future of a particular event happening "today" by the one-at-a-time method, that is to say by assuming that we know what would have happened over that particular period of future time if this event had not occurred . . . . [But Marshall] knows that other things in fact will not be equal--history marches on--but he supposes that it is possible to trace the

effects of a single specified event as though it was the only change that occurred at a particular date”(Robinson, 1980, p. 93).

It may also be that the type of questions that the more mathematically minded economists usually deal with are questions of minor practical relevance. Many economists in the applied sphere, and many among the well-informed public, often get weary of economists searching for solutions to theoretical (abstract) problems whilst what they see is growing instabilities, insecurities, and disparities (See for example, Goodwin, 2003, Harris, 2001). Actually, mathematical deductive systems seem to offer little insights into the workings of the actual economy. To cope with practical problems, we might need a different theoretical edifice, one built on more appropriate mathematical foundation that takes account of the relevant historical variables and offers clear policy guidelines.\*

A particular burden on economics is to prove it self in the face of what the general public, particularly well informed public, expects of economics. This burden does not have the same weight when it comes to the public expectations of other sciences, like physics, chemistry and biology. The proof of these other sciences is in the reality of new products and new technologies that are being regularly produced. But what evidence is there to convince the public that economics is making progress in finding solutions to economic problems that we often see all around us.

At any rate, it is quite possible to base economics on a less mathematical, foundation and still have a science of economics. As Lawson says:

“My Cambridge colleague Professor Amartya Sen was correct when recently in *Le Monde* (31/10/2000) he observed that mathematics is not a unique foundation of economic science. In fact it is not a foundation of economics -as-science at all” (Lawson, undated).

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\* - See Perroux(1983) for examples of more appropriate mathematics, i.e. the types of mathematics that he has brought under that title of “mathematics of general interpretation”.



### VIII- Ontology and ME

Ontology, according to Ingarden, is the science of what is, of the kinds and structures of objects, properties, events, processes and relations in every area of reality. 'Ontology' sometimes is used as a synonym for 'metaphysics' (a label meaning literally: 'what comes after the *physics*'). The early students of Aristotle according to Ingarden, used the term metaphysics to refer to what Aristotle called 'first philosophy'. It is, sometimes, used to refer to the study of what *might* exist; 'metaphysics' is then used for the study of which of the various alternative possible ontologies is in fact true of reality. Ontology seeks to provide a definitive and exhaustive taxonomy of entities in all spheres of being. The taxonomy should be definitive in the sense that it can serve as an answer to such questions as: what classes of entities are needed for a complete description and explanation of all the goings-on in the universe? Or: What classes of entities are needed to give an account of what makes true all truths? It should be exhaustive in the sense that all types of entities should be included in the classification, including also the types of relations by which entities are tied together to form larger wholes (Ingarden, 1964, in Barry, 2003).

To Lawson, ontology is "the study (or a theory) of being or existence, a concern with the nature and structure of the 'stuff' of reality" (2000, p. 2). Now, it may be said, paraphrasing Lawson, that all methods have ontological presuppositions that are conditions under which their usage is appropriate. To use any research method is immediately to presuppose a worldview of sorts. It seems to be the case, however, that the ontological presuppositions of the methods of mathematical modeling used by economists are rarely questioned or even acknowledged, at least not in any systematic or sustained way. As a result, the possibility of a lack of ontological fit (a mismatching of the presuppositions of these modeling methods with the nature of those features of social reality being investigated) is not considered. Yet, it may be noted that the methods of mathematical-deductivist modeling, like all methods, might indeed have ontological presuppositions. But the preconditions of mathematical-deductivist methods appear not to arise very often in the social realm (2000, pp. 12-13).

And why are these preconditions not very likely to arise in the social realm? Because to the extent that human beings as well as society are, in reality, complex, evolving and open, a methodology which necessitates that the subject-matter addressed is everywhere atomistic and isolated is likely very often to

throw up accounts of human individual and collective behavior that are fictitious and rather superficial, to say the least (Fleetwood, 2002: 2)

Having said that let me emphasize here with Lawson that “the possibility of closures of the causal sequence kind, i.e. of the sort pursued by modern mainstream economists cannot be ruled out a priori”. Ontology usually does not rule out entirely the possibility of regularities of events standing in causal sequence in the social realm. But it does “render the practice of universalizing a priori the sorts of mathematical-deductivist methods economists wield somewhat risky if not foolhardy, requiring or presupposing, as it does, that social event regularities of the relevant sort are ubiquitous” (Lawson, 2000:17).

### **VIII- The Complexity Approach and ME**

According to Clark (2002), the Santa Fe Institute collection of papers on economics outlines a “Santa Fe approach” to economics that might have “profound implications for the foundations of economic theory and for the way in which theoretical problems are cast and solved. These implications, according to the words of Brian et al., ed, are:

Cognitive foundations. Agents are seen “as having to cognitively structure the problems they face -as having to ‘make sense’ of their problems-as much as solve them ... To ‘make sense,’ to learn, and to adapt, agents use a variety of cognitive processes. The very categories agents use to convert information about the world into action emerges from experience ... Agents therefore inhabit a world that they must cognitively interpret”.

Structural foundations. “First ... economic functionality is both constrained and carried by networks defined by recurring patterns of interaction among agents. Second, economic action is structured by emergent social roles and socially supported procedures-that is, institutions. Third, economic entities have a recursive structure: they are themselves composed of entities ... [T] he fundamental principle of organization is the idea that units at one level combine to produce units at the next higher level.”

What counts as a problem and a solution? “The only descriptions that can matter in such a world [of perpetual novelty] are about transient phenomena-about process and emergent structures” These can be characterized “as seeking emergent structures arising in interaction processes, in which the interacting entities anticipate the future through cognitive procedures that themselves



involve interactions taking place in multi-level structures (Brian et al., ed.,1997:2-4)".

And using Clark's words:

"Complex Systems (CS) explains how the world works-recursive structure, learning, adaptation, novelty; process and emergence. CS may develop as a terminal mathematical economics, a social scientific 'end of history', in which agents of all sizes and powers navigate a rugged landscape of perpetual novelty, which includes everything from economic cycles to wars to environmental disasters"(Clark, 2002:6).

As an economist once put it, says Clark, "you can understand complex systems, but you can't control them".

But history, notes Clark, can catch up and make us willful. The era of economic neo-liberalism-of deregulation, tax cutting, privatization and diminished government-begun with such insistence 25+ years ago is coming to amazing fruition. The crash of stock markets, the colossal waste in deregulated industry, the epic business frauds and bankruptcies, the medieval social inequality, are a rerun of the Gilded Age and the Roaring Twenties. The reversal of interventionist attitudes and reforms is perhaps threatening us with another Depression.

According to Clark, the Complex System approach would treat social forces in historically specific, purposeful, ameliorative ways. Narratives of historical explanation might be constructed from history, socioeconomic taxonomy, and simulation and "adaptive mechanics" analysis of complex system.

It should be added that the Complex System approach is a multifaceted (and in a way pluralistic) approach whose exact meaning is still under construction. In one version, it considers economic systems as evolving complex systems with agents that decide rationally to increase their satisfaction. These decisions, however, are masked by a cloud of uncertainty depending on expectations that agents form about likely future states of the world. This world is a complex non-stationary world which makes the solution of decision problems quite difficult. Therefore, models purporting to explain agents' behavior need to come into terms with an uncertain environment and an imperfect (bounded, for example) rationality.

**X- Summery and Conclusion.**

To summarize, I have tried to show that economic methodology has been acquiring, thanks to the persistence of methodologists, a better and better status in spite of the pessimism expressed by some orthodox economists, and more and more economists have come to realize (for good reasons) that methodological discussions are inevitable and illuminating. Meanwhile, in spite of what some critics of economics think, economics does not depend solely on a single method, certainly not on hypothetical deductive method alone. In fact, short-term fluctuations in macroeconomic variables and long term analysis of development of capitalism are more effectively analyzed within the framework of historical-deductive, statistical inductive or dialectic methods. The complex system methodology of the Santa-Fe approach seems to have the potential to overcome some of the difficulties usually associated with the deductive hypothetical method.

Mathematical methods do not seem to have any across- the -board advantage over other, non- mathematical methods, such as the use of rhetoric, except that the use of mathematical methods may become inevitable depending on the problem at hand. The ontological concerns of critical realists such as Lawson seem close enough to those echoed by some Keynesian and classical development economists in that they both heed to what reality can teach us.

It is fairly evident that the new heterodox thinking is paying increasing heed to the possibility of doing economics within a more diverse methodological setting, including: ways that do not depend on so much mathematics, ways that take explicit account of historical events, ways that take account of complexities and ways that are more commensurate to the realty at hand. It seems that a decisive test of any method would be how much it can yield theories with policy implication that can guide us in finding solutions to the actual economic problems. But this does not mean that the desire for reaching the truth taken by itself would be uninteresting.

There still remains one question whose answer awaits the future course of intellectual events: whether or not economic textbooks would be reframed to assimilate the type of methodological diversity that is promoted by heterodox economists. How can we remodel the text books of economics to tell us more about rhetoric, complexity, historicism, network analysis, computer simulation and system analysis, for example? How can we allow for more diverse



approaches to economic analysis in our intellectual communication and still meet the criterion of parsimony and rigor. This paper provided some of the answers, though somewhat incomplete and tentative, by reviewing the major methodological issues and procedures. As an ending remark, I should say that even though economists sometimes have sharp disagreements over methodological issues, but in the final analysis they all seem to agree that they can learn from each other.

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