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Invisible Hand in the Process of Making Economics or on the Method and Scope of Economics

Summary: As a social science, economics cannot be reduced to simply an a priori science or an ideology. In addition economics cannot be solely an empirical or a historical science. Economics is a research field which studies only one dimension of human behavior, with the four fields of mathematics, econometrics, ethics and history intersecting one another. The purpose of this paper is to discuss the two parts of the proposition above, in connection with the controversies surrounding the method and the scope of economics: economics as an applied mathematics and economics as a predictive/empirical science.

Key words: Invisible hand, Scope and method in economics, Economics as an applied mathematics, Economics as an empirical science, Economics as ideology.

JEL: B41, B23.

“If self-interested individual behaviour can (as if by an ‘invisible hand’) generate economic efficiency, then why couldn’t the self-or-professionally-interested behaviour of scientists also (given the right institutions) produce epistemic efficiency?”

(Wade D. Hands 2001, p. 56)

The scope and method of economics are separate, but related, fields of economics as a social science. Economists often define their discipline as “*economics is what economists do*”. Over the course of its two hundred and fifty year long history, economics has provided explanation and prediction about human behavior and offered policy prescriptions. Throughout this development economics has experienced several shifts in its scope and its subject matter.

During this period *the methodology of economics*, which can be defined as the study of setting up the rules (protocol) to decide upon economic thoughts or theories, the objects of economics, and if such theories are scientific or not, has exerted an ongoing development. While normative methodological approach in economics has inquired on how economists must/should make economics, positive methodological approach has questioned how economists actually make economics. In brief, the de-

velopment of the relation between the scope and the method of economics is an inter-related historical process.

The main hypothesis of this paper argues that as a social science economics cannot be reduced to only an *a priori* science or ideology. Moreover, economics is neither a purely empirical science nor solely a historical study. Economics is a research field which studies only one dimension of human behavior where these four fields, mathematics, econometrics, ethics and history intersect one another each other. This hypothesis is based on the view that the “judicious pluralism” approach, which has emerged from the recent developments in the methodology of economics, is applicable to the scope of economics as well. In other words, especially in recent years, as the recent literature on the economics of *science* is expanding, the application of Adam Smith’s famous “invisible hand” to the process of “making economics” can potentially be very fruitful. What we need is necessary is only professional ethics; that is, everyone is attempting to make his/her best. The pre-condition for the increased level of interaction of economics with other disciplines is first to constitute a tradition of communication and discussion among economists specialized in different fields of economics.

From the 1970s to the 2000s, there have been two major strands of criticisms against mainstream economics: the critics of formalism and the Post Autistic Economics. The foundations underlying the Post Autistic Economics, which mainly criticizes the “education of economics”, in recent years are closely related to the criticisms raised against the so-called formalist revolution during the 1940s and 1950s that have changed the way of “making economics” (Terence Hutchison 1992; Mark Blaug 2003; Edward Fullbrook 2004). The neoclassical or mainstream economics, with its structure that uses excessive abstract and inner-consistent mathematical models, does not help us to understand or to explain the real world. Also, Post Autistic Economics never suggests an alternative economics but advocates *empiricism instead of priorism* and *critical thinking instead of ideology* (Fullbrook 2004, pp. 4-5). These are the fundamental points which must be discussed by economists. Is it possible to purify economics from *a priori* qualifications and transform it into an empirical science? How can we interpret the idea of economics as a critical approach instead of the idea of economics as an ideology? Can we say that “neoclassical economics is equivalent to ideology?” If the answer to the last question is “yes”, is this due to a priori nature of economics? If every economic approach is an ideology, then where do we go from there? Several questions can be raised similar to these.

Naturally, attempting to address all of the questions raised above would push the limits of a journal article. Therefore, in this paper we focus only on the relationship between economics and mathematics and econometrics/statistics: Can economics be reduced to applied mathematics or merely a predictive/empirical science? Or to put it differently, can economists abandon mathematics and/or econometrics/statistics?¹

¹ In this article, we do not consider the relationship between economics and ideology (or ethics/political philosophy) and also the relationship between economics and history.

If we attempted to answer these questions without properly taking time perspective into account, we would miss an important point: *There is no word unspoken under the blue sky*. In other words, any discussion of methodological aspects of making economics, without regard to historical perspective associated with the practice of economics, will ultimately fail. For this reason, in this paper we will attempt to apply the principle stated by (borrowing from Philip Mirowski 1989, p. 8) Emile Meyerson: “*the best way to talk about science is to examine how scientist have done it*” to making economics and attempt to answer the questions above in a historical and methodological perspective.

The plan of the paper is as follows; Section one covers a general evaluation of the methodological controversies in economics for the last thirty years. The following two sections deal with the place of mathematics and econometrics in economics respectively.

1. Developments in the Methodology of Economics since the 1980s

The methodology of economics has experienced important changes, transformations and developments in the period from the 1980s to the present, which is now called the *Renaissance of the methodology of economics*. In addition, to expanding scope of economics and increasing number of scholars in the field, there have been two changes related to the purpose of this paper. The first one is the transformation in the nature of economic methodology. The second one is the rising importance of economic approach in methodological discussions.

1.1 Changes in the Nature of Economic Methodology

Prior to the 1980s the methodological problems which economists meet in practice used to be solved by the application of certain methodological approaches, for example, the logical positivism or to Karl Popper. In that period, the fundamental problem of the methodology of economics was to find the true and unique set of rules for making economics more scientific. However today, the attitude of the majority of economists on this approach is to give up on these *monist approaches*, which try to find a true unique rule to decide whether theories of economics are scientific or not, or which is the best theory among several available economic theories. We can say that, the idea that scientific theories can be compared and contrasted in connection with a unique rule without subjective value judgments (i.e. Popperian falsificationism) has been falsified since the Duhem-Quinn thesis (see Rod Cross 1994). This implication is valid not only for economics but also for science in general, or philosophy of science (Mirowski 1989, p. 356; Sheila C. Dow 2001; Hands 2001).

Since the 1980s, an important transformation has been observed in the literature of economic methodology. While the *descriptive-positive approaches* have been adopted over the *prescriptive-normative approaches*, various methodological approaches have been brought forward. These include the ideas of Imre Lakatos, Thomas Kuhn and Paul Feyerabend, the deductive methodology of John Stuart Mill (Hausman), as well as the different interpretations of Karl Popper (Blaug or Boland),

Deirdre N. McCloskey's rhetorical approach to the critical realism and the different versions of apriorism (Misesian or M.Hollis-E.Nell' approaches). In other words, the reaction of the methodologists of economics to the foundationalism which advocates that the practice of making economics must be based on the methodological rules suggested by philosophers of science, has swung them to the other extreme: *relativism* or using the phrase from Feyerabend, "*anything goes*". Although during the beginning years of this transformation the tolerance suggested by the post-modern approach has been particularly attractive for scientists, after some time, it has been suggested that pure relativism is not only useful but also dangerous for the development of science. Because of this, the majority of methodologists of economics define their own place/position in methodological discussions as a place between foundationalism and relativism (Dow 2001; Hands 2001).

According to this approach, which we call it as *judicious pluralism*, a scientist chooses one of the available approaches (paradigms or research programs) subjectively and narrows his or her research field. In this regard, the important point is that every approach must be aware of other approaches studying in the same research field. In our opinion the interactions among various approaches and their *de facto* or potential competitions would increase the productivities of these approaches and would lead to the improvement of science. In this framework, what is expected from methodology of economics is to determine the reasons for the diversities among various approaches and to determine how we arrived to the available state. By this means, economists from different areas of specialization will have an opportunity to evaluate the merits of their own approach and others' approaches, and see the advantages and disadvantages associated with them. This process of interaction, communication and discussion will ultimately result in leaping progress in science.

1.2 Increased Importance of Economic Approach in Methodology and Philosophy of Science

Since the 1980s, the underlying forces behind the positive growth trend of the literature on the methodology of economics can be depicted as follows:

- Failure of the neoclassical synthesis approach of macroeconomics to solve the stagflation crisis of the 1970s in the real world has led to the crisis in the world realm of theoretical economics. In the wake of this crisis, hoping to unveil the causes, economists have turned to the theoretical and methodological origins of their approach and studied the ideas of the leading philosophers of science like Popper, Kuhn, Lakatos and Feyerabend in a relatively short period. Thus, with the help of ideas of philosophers of science, they have tried to understand the crisis of economics, and to solve the controversies among various alternative schools in economics, as well as to explain the development trend of the history of economic thought;
- In this context, upon closer examination of the positivist and Popperian prescriptive approaches, economists have arrived to the conclusion that some economists in the past have had similar and important methodological ideas (for example Joseph A. Schumpeter) and hence there is no need

to seek the help of the philosophy of science; [McCloskey's rhetorical economics and Hausman's (Millian) economics as inexact science]

- The historical studies on methodology and philosophy of science have shown that economists had already taken part at the start of methodological controversies, as opposed to joining some time later. For example, there were many economists or social scientists in the Vienna Circle which laid the foundation of positivism in the 20th century (Oscar Morgenstern 1976; Gerhard Schwödiauer 1987; Thomas E. Uebel 1991; David Edmonds and John Eidinow 2004; Giandomenica Becchio 2005);
- Finally, although social scientists have tried to explain the process of scientific knowledge by the sociology of science (Kuhn), rather than imitating natural sciences for a long period, they have recently begun to explain this process by the economics of science (Paula E. Stephan 1996; Mirowski and Esther M. Sent 2002).

The implications from the recent developments in the methodology of economics summarized above can be stated as follows: The approach suggested for the methodology of economics, “*judicious pluralism*”, can also be equally applied to the scope of economics. The scope of economics is extremely diverse, where although some economists deal with mathematical economics, some others deal with empirical economics. While a group of economists questions the ethical foundations of welfare economics, yet another group studies economic history. In the framework of recent philosophy of science, it is not possible to determine the superiority of one study (or area) to another. For example, how do we compare a game theoretical study on human behavior with a pure econometric study or a study on economic history? We believe that, rather than focusing only one or two dimensions (among mathematical economics, econometrics, welfare economics and economic history) and excluding others, each branch in economics, instead, should attempt to achieve the best within its boundaries and then discuss, interact and cooperate with the other fields of study. This would ultimately help make economics a more rigorous and more reliable social science. Furthermore, to support this proposition we need to focus on the history (or archeology) of economics and to determine how we have got here.

2. Economics as an *A Priori* Science or the Relationship between Economics and Mathematics

First we must emphasize that an *a priori* science does not mean mathematical economics. After the acceptance of the view that Popperian methodology is not proper for economics by the majority of economists, several approaches advocating that economics could not be a science, such as physics, have gained importance: Misesian apriorism (Ludwig von Mises 1962, 1998, 2003), deductive approach of Martin Hollis and Edward Nell (1975) and Daniel M. Hausman's (1992) “economics as an inexact science”.

Our specific aim is to question the process of the formalization, or the transformation of economics to *an applied mathematics*. Thus, the main question which we will try to answer can be stated as follows: If formalization of economics means

the abstraction = axiomatization = mathematization = being rigorous science, why are the majority of economists disturbed? (Roy E. Weinraub 1999, p. 72)

When we look at the literature related with our discussion we see many proponents and opponents of mathematization of economics. Although mathematical economist Paul A. Samuelson (1994, p. 267 and 272) states that *“It is confirmed that what I predicted more than four decades ago-that the virus of mathematics would spread in economics and cause grave psychological discomfort in those scholars who lag behind the external-margin frontier of its extreme cultivation (...) Science advances funeral by funeral”*. We consider believe that his statement must be considered with regards to the two sides: While there are mathematical economists thinking like Samuelson, there are also several mathematical economists (Wassily Leontief 1971; Nicholas Georgescu-Rogen 1979; Michio Morishima 1991) who do not agree with him. The question of “how much mathematics should be in economics” cannot be handled in a healthy and constructive way if we disregard the historical perspective.² We have to find the answers to three questions: What are the criticisms against mathematization of economics? What are the main ideas of the proponents of mathematization/formalization of economics? How can we evaluate the formalization of economics from the perspective of economic methodology?

During the twentieth century at least seven major criticisms to the mathematization of economics have been raised. These can be summarized as follows (Herbert G. Grubel and Lawrence A. Boland 1986; Clive Beed and Owen Kane 1991):

- The axioms of mathematical economics do not correspond with real world behavior;
- The number of empirically testable hypotheses generated by mathematical economics is small compared with the volume of mathematical economic analysis;
- Some /much of economics is not naturally quantitative and therefore does not lend itself to mathematical exposition;
- The translation of the description of economic processes from a natural language (such as English) to mathematics can be naïve and illegitimate;
- There is no objective way to gauge whether mathematical economics is more precise than less mathematical economics;

² It would be fruitless to evaluate the merits of mathematization of economics without a historical perspective. In their survey Munir Quddus and Salim Rashid (1994) cite the views of a famous mathematical economist, P. Samuelson. Samuelson thinks that most economists are uncomfortable with the spread of the virus of mathematics in economics and he advocates the view that “economists do not understand mathematics” is wrong. Thus, he arrives the conclusion that the opponents of high mathematization of economics are those who do not understand it. On the other hand, in reply to O. Morgenstern’s question of how he finds the mathematics of Samuelson, the founder of game theory and one of the leading mathematicians of the 20th century von Neuman replies “[Samuelson] has murky ideas about stability. He is no mathematician and one should not credit him with analysis. And even in 30 years he won’t absorb game theory” (Robert J. Leonard 1994, p. 494). Furthermore, Deirdre N. McCloskey (1985) says that mathematics in economics is mostly rhetoric and this makes economics very unrealistic, thus advocates that the individual in economics should be replaced with someone real like, for example, the novel heroine Madam Bovary. In response to McCloskey, Mirowski (1988), borrowing from Julian Barnes says: “Madam Bovary is not real a novel heroine, a fictional character not existing in reality.” Then, where is the truth? Beyond fiction how do we produce scientific knowledge about human behavior?

- There is *no one* “best” system of mathematical logic;
- Because of all the above problems, mathematics is often an unnecessary adornment to economic discovery about the real world, but serves other purposes.

On the other hand, we can summarize the ideas of economists (Samuelson, Kenneth J. Arrow and Gerard Debreu) who have made significant contributions to the mathematization of economics in the 20th century as follows:

According to Samuelson (1952, p. 52), mathematics is a language, a communication tool. All of the scientific disciplines need deduction because it has the modest linguistic role of translating certain empirical hypotheses into their “logical equivalents”:

“What is not always so clearly understood is that a literary statement (...) has its complete equivalent in the symbolism of mathematical logic. If we write out (our propositions) in such symbolism, we may save paper, ink and time”³

But the more important function of mathematics argued by Samuelson is that (1952, p. 61):

“not to produce a truth by mathematics that could not have been proved by words; (but) to produce one that has not, as a matter of historical fact, been previously produced by words. I suggest that a careful review of the literature since the 1870’s will show that a significant part of all truths since arrived at have in fact been product of theorists who use symbolic techniques.”

Economist Arrow argues that two propositions of social scientists on mathematics seem to be based on nothing more than a misunderstanding: First, although mathematical/quantitative analysis is useful for physical sciences, social sciences call for qualitative analysis. Second, the judgment and intuition of the skilled investigator are fundamentally more useful in the social sciences than mathematical formulas based on quantitative observations. The answer of Arrow to these propositions can be stated as follows: Mathematics with all branches is not quantitative in nature, especially mathematical or symbolic logic is purely qualitative and can be used in social sciences. There are two advantages of using mathematical methods in social sciences. In the first place, it provides clarity of thought. Secondly, the explicit formulation of theories in mathematical terms assists the empirical verification of theories (Arrow 1951).

According to Arrow a mathematical model of social behavior can be constructed via an individualistic activity function which represents behavior and impulse of an economic agent to other individuals/agents, and changes in external conditions. One can handle the rationality of an individual, one of the pillars of economics, in an optimization framework or in a strategical model or game theory frame-

³ Again in Samuelson (1952, p. 58) words “I should hate to put six monkeys in the British Museum and wait until they had typed out in words the equivalent of the mathematical formulas involved in Whitehead and Russell’s *Mathematical Principia*. But if we were to wait to wait long enough it could be done.”

work as in the works of von Neumann and Morgenstern in which every social and individual outcome is dependent on the others' decisions (Arrow 1951).

Economist Debreu (1984) lists several benefits of axiomatization of economic theory:

- Making the assumptions of an economic model as explicit as possible enables us to evaluate the extent to which it applies in specific situations;
- Axiomatization may provide ready answers to new questions when a novel interpretation is discovered;
- By stressing mathematical rigor axiomatization leads to deeper understanding of the problems economists are studying;
- It provides a firm basis from which new approaches can be established;
- Due to simplicity and generality it makes theories more aesthetic, easily understandable and applicable to a broad class of problems;
- Furthermore, being an efficient means of communication axiomatization enables economists to communicate with each other easily and effectively.

According to Debreu the assessment of the state of economic theory and the critiques of mathematization of economics requires a detailed analysis of how we got in here in the first place. In this regard the process of mathematization of economics after the World War II consists of several "accidents" (Debreu 1986). The main factor behind the mathematization of economics is that theoretical physics was set as the unapproachable ideal before this transformation process. According to Debreu, the interaction of physics and mathematics enabled physics to find ready answers to its real-world problems from the abstract world of mathematics. Physics was able to resist the pressure of mathematization by resorting to natural experiments and factual observations so that it established a balance whereas economics was not able to follow the same path. Since the experimental/empirical part of economics is not sufficiently sound, economists tend to put more weight on internal consistency and logical structure of their models. Thus, mathematics plays an important role in reducing the potential errors in deductive reasoning of economics to a minimum.

2.1 Controversy on Formalization of Economics

The first complaint about the formalism in economics is related with the image of economics in public opinion. First of all, formalism has created an obstacle for non-economists and students willing to communicate with economics. This non-existence of communication originates not only from the difficulties caused by mathematical techniques in economics but also the transition of economics to a form which has no relation with real economy (Dow 1998, p. 1826). Similarly, criticisms associated with formalization of economics after World War II have focused on the transition of economics into a science which includes inner-consistent models like mathematics but which does not include testable hypotheses and inferences (Leontief 1971; Hutchison 1992).

The second important criticism is related with the rhetoric of mathematical formalism of economics. McCloskey is not against the use of mathematics but to the quality of mathematics in economics. According to McCloskey, "*Economics has*

made progress without mathematics, but has made faster progress with it. Mathematics has brought transparency to many hundreds of economic arguments. The ideas of economics –the metaphor of the production function, the story of economic growth, the logic of competition, and the facts of labor-force participation– would rapidly become muddled without mathematical expression.” (McCloskey 1994, p. 128). McCloskey who takes formal mathematics as “*abstract and pure mathematics*” stated that economists must choose the kind of mathematics suitable for themselves: In this regard, not mathematics (and its pure mathematics) but physics (and its applied mathematics) must be taken by economists as a model for themselves (McCloskey 1985). According to Weinraub, this criticism of McCloskey is based on his unawareness of the changing meaning of a good mathematical proposal (Weinraub 2002, pp. 73-74).

The point above stated by Weinraub directs us to the history of interaction between economics and mathematics in order to understand the process of formalization of economics. This process, at the same time, falsifies the view stated by Debreu that the formalization of economics is a natural conclusion of the fact that the two main concepts in economics, price and quantity are measurable (Debreu 1984, 1991).

First of all, the view that “*the more formalized, more abstracted and more axiomatic economics is more reliable and rigorous and the opposite of this is not rigorous*” is not a true or accepted proposition for all times. It is a historical (not ahistorical) proposition (Weinraub 1998, p. 1840; Weinraub 2002):

“For most economic theorists today to formalize means to axiomatise, and thereby to make rigorous. My main assertion (...) is that an argument is ahistorical, and therefore probably wrong on most interpretations.”

In the beginning of twentieth century, for the applied mathematical theoreticians like Vito Volterra, Henry Poincare, Albert Einstein, the meaning of “*a rigorous mathematical model of a phenomena*” was that a mathematical model must have direct and obvious implications based on empirical facts and must have testable implications. The opposite of “*being a rigorous*” or “*formal*” model was not being informal but rather “*unconstrained*”. This approach, which is based on Newtonian physics in the beginning of the 20th century, was also adopted by important economists in France (like Dupuit and Cournot) and England (Jevons Marshall, Edgeworth).

However, in order to better understand the formalization of economics it will be useful to look at the real history of the developments in mathematics, physics and philosophy of science. In this context, one must mention the *Vienna Circle* which was founded by a group of mathematicians, physicists, philosophers and social scientists. The Vienna Circle, founded by Moritz Schlick (philosopher), Hans Hahn (mathematician) and Otto Neurath (sociologist, social scientist) had very influential members in almost every discipline that shaped the intellectual and scientific world in the twentieth century: Rudolf Carnap, Philipp Frank, Friedrich Wiseman, Kurt Gödel, Abraham Wald, Karl Menger, Victor Kraft, Herbert Feigl, Felix Kaufmann. Its members have named themselves as “*Scientific World Concept: the Vienna Circle*”, and they have tried to demarcate physics (science) from metaphysics (non-science) and intended to combine logic and empiricism in order to provide logical

base for science. In other words, it was a “*Unified Science System/Encyclopedia*” project. Two of the most important philosophers of the 20th century, Karl Popper and Ludwig Wittgenstein have been closely connected with the members of the Vienna Circle (Sigmund 1995; Edmonds and Eidinow 2004).

During this period, while the members of the Vienna Circle were assembling philosophical meetings under the chairmanship of Schlick; Karl Menger, who was a member of the Circle and son of Carl Menger - the founder of Austrian School of Economics Carl Menger, was organizing conferences on mathematics (Karl Menger’s Colloquium). Vienna was so famous that it had been called *Mecca of mathematical logic*. In these meetings many mathematicians and logicians from the entire world have given lectures: from Poland Alfred Tarski, American Willard van Orman Quine, German Carl Hempel (Sigmund 1995), etc. At the same time, the Vienna Circle which proudly named Albert Einstein, Ludwig Wittgenstein and Bertrand Russell as intellectual fathers, was in touch with several famous mathematicians of this period, like German (from Gottingen University) David Hilbert, Dutch (from Amsterdam University) L.E.J. Brouwer.

On the other hand, this period was also the crisis period of crisis for mathematics. There were three mathematical approaches in competition (Weinraub 1998, 2002):

- Logicians: Attempted to show the importance and the power of logical and critical approach by making irrevocable deductions from certain axioms and definitions. According to mathematician Frege, numbers were logical objects that exist independent of human definition and it was possible to reduce the laws of numbers to logic. Moreover, *Principia Mathematica* of Whitehead and Russell was the most significant attempt for the reducibility of mathematics into logic.
- Formalists: Were known for their skill to tackle a given problem in a formal way and to reformulate it as a concrete algorithm. According to D. Hilbert of Gottingen University, who may be noted as the founder of the approach, mathematics consists of formal objects that can be interpreted and reformulated as one may wish. Hilbert contended that formalization as axiomatization was directly linked with discovering and establishing mathematical and scientific knowledge. The purpose of formalization, modeling or constructing theories for a given phenomenon was not to establish the link between the model and experimental data, but to develop a series of inner-consistent logical chains and thus to discover new scientific knowledge.
- Intuitionists: Leader of this approach, which who emphasized the importance of geometric intuition in every branch of mathematics, was Dutch mathematician L.E.J Brouwer from Amsterdam University. He argued that mathematical objects should be constructed and perceived as well-defined and finitistic structures.

It is interesting to note that towards the end of 19th century, the mathematization of economics in order to form a scientific foundation for economics, was per-

ceived by economists as imaging of rational mechanics or as an effort to constitute resemblance between structure of economics and the concepts of mechanics like power, energy, equilibrium and stability and their empirical counterparts. But in the first quarter of the 20th century, while the form of the making mathematics was changing, the relativity theory and quantum physics superseded the physics of 19th century. These developments changed both the form of making/producing scientific knowledge and the relationship between mathematics and natural sciences.

According to mathematical economist Weinraub (2002), the architect of this change was German mathematician David Hilbert. The aims of formalists in the 1920s, has been found in the two parts of the Hilbert's Formalist Program: *Finitistic Program for the Foundations of Arithmetic* and *Axiomatic Approach*.

The first part of the Hilbert's Program was related to the form of making mathematics, or the perception of mathematics, and was defined as establishment of consistent system, logic or set theory. In other words, this part of the program is related to, firstly, the problem of the solubility of every mathematical problem, and secondly, the problem of the quality of a mathematical proof and the determination of the rules of mathematical provability. Furthermore, according to Hilbert, mathematical knowledge as a whole, with the help of a few axioms, can be produced as an axiomatic system which is both inner-consistent and complete and the system can be reformulated as a whole of verified propositions by adding a new consistent axiom. A system formed in this manner is either true or false, otherwise it is not complete. In other words, the essence of formalization/axiomatization of mathematics relies on the idea that "truth of every system implies consistency of the system as a whole". For example, in a two-person game, theory will be arithmetically true (consistent) due to deterministic logic (as the outcome will be true if the assumptions are true). In a sense, this means that the truth of a model is related to its inner-consistency.

According to Hilbert not only mathematical propositions, but also development of an idea or a theory, which that can be an object of scientific thought require its foundations to be based on axiomatic method, i.e., mathematics. At the same time this idea is the basis for the second part of Hilbert's program: *Axiomatic Approach*. According to this approach, mathematicians should develop mathematical structures which can be suitable basis for models in applied sciences (electrodynamics, radiation, thermodynamics, gravity, quantum theory, etc.). In this way, instead of scientific knowledge process which is perceived as constructing models reduced to mechanical structures in the 19th century, a process of model construction reduced to mathematical structures was being advocated.

The first part of Hilbert's program, the project to prove that "*mathematics is a consistent and complete axiomatic system*", was reputed by "*inconsistency*" and "*incompleteness*" (or impossibility) theorems published in 1931 by Czech mathematician and a member of Vienna circle Kurt Godel (Jerry P. King 2004, pp. 36-37). While Godel's theorems caused disappointment in Russel's logicist-mathematicist approach in which mathematics is perceived as the totality of logical deductions, they led to the defeat of Hilbert's first program and the removal of logic from "*the center of mathematician's world*" (Weinraub 2002, p. 94; King 2004, p. 37).

Furthermore, Godel's theorems did not affect the second part of the Hilbert's Program (axiomatic approach), a project aiming to prove that all sciences can be constructed based on axiomatic-mathematical foundations. However, as mathematics and therefore scientific knowledge is not the only way to establish reliability, accuracy and truth, it eventually has led to the widespread adoption of the idea that mathematical truth changes with mathematical approach: mathematical relativism.

The reflection of these developments in the history of mathematics on economics could perhaps not be stated better than Professor Weinraub (2002, p. 100):

*“Thus we have the split, looking ahead to today from the early decades of this century, between those who would argue that mathematical rigor (and scientific knowledge) must develop not from axioms but from observations (about the economy) and (economic) data, so that the truth of a theory or model may be tested or confirmed by reality-like Volterra, Pareto and Edgeworth- and those who would claim that mathematical (economic) models are rigorous (and “true” in the only useful scientific sense of the world) if they are built on a cogent axiom base –like von Neumann and Morgenstern, and Debreu. The arguments about formalism in economics thus recapitulate divergent views about, and changing meanings of, scientific knowledge. Our archaeology of formalism in economics unearths increasingly energetic and successful challenges to certain more or less traditional or standard views about scientific truth/knowledge, and the development of more or less successful alternatives in various quarters: the strata are the emergent re-conceptualizations of both science and knowledge. In concrete terms, there is indeed a disjunction between Debreu's *The Theory of Value* and Friedman and Schwartz's *A Monetary History of the United States 1867-1960*: although both are mathematically rigorous, the latter is rigorous in an older sense, the former in the newer sense. This is one source of the divergence between econometrics and mathematical economics.”*

In this framework, following Debreu and Weinraub, we can point some remarkable developments in the formalization process of economics.

If the “symbolic” birth year of mathematical economics was the published year of the book, *An Investigation on the Mathematical Principles of Wealth Theory* by A. Cournot, the symbolic starting year of the contemporary mathematical period in economics is 1944 when *The Theory of Games and Economic Behavior* was published. While the important names of the first wave were Leon Walras (1834-1910) Vilfredo Pareto (1848-1923) and Francis Y. Edgeworth (1845-1926), the names and studies of the second wave, which triggered the deep and important changes in economics, were as follows: In addition to the book written by von Neumann and Morgenstern, the input-output analysis of W. Leontief, *Foundations of Economic Analysis* of Paul Samuelson, activity analysis of production of T. Koopmans and simplex algorithm of G. Dantzing. Of course the workings of Cowles Commission must be added to the list. During the mathematization and formalization process of economics which began in the 1940s the Cowles Commission, which can be considered as the organic continuation of the Vienna Circle in Austria, was very influential. Both of the members of the group who studied on general equilibrium theory (Oscar Lange, Jacop Mosak, Ragnar Frisch, R.G.D. Allen, Abba Lerner, Leonid Marschak, Tjalling Koopmans, Kenneth Arrow, Gerard Debreu, Edmond Malinvaud, Lionel Mc Kenzie,

Nicholas Georgeescu-Roegen) and the group who studied on econometrics (Ragnar Frisch, Gerhard Tintner, Trygve Haavelmo, Lawrence Klein and Abraham Wald) played primary role in this process (Debreu 1984; Carl F. Christ 1994).

Main motives of these studies were the investigation of the general equilibrium theory, its transformation to more strengthened, generalized and simplified forms, and the extension to new directions. These include solving numerous problems in the theories of choice, utility and demand and using new mathematical techniques. The aim of the Walras's mathematical theory was to explain the equilibrium between the actions of agents participating in commodity markets as interconnected with each other and the vector of prices. In equilibrium, while every producer maximizes his profit according to the price vector in his production set, every consumer satisfies his preference under the budget constraint and total demand equals total supply for every good. Walras and his followers have thought that the model would be incomplete without a proposal which support the existence of equilibrium and they have tried to fill this gap with the mathematical assumption that the number of equations equals number of variables.

The important cornerstones in the process of replacement of the proof of Arrow-Debreu model instead of above assumption can be stated as follows:

- The first model which proposes solution to the existence of equilibrium was the 1936-1937 model of mathematician Abraham Wald;
- The 1937 growth model by John von Neumann in which the fixed point theorem of Shizuo Kakutani, a student of mathematician Hilbert, was used;
- The arrival of Oscar Morgenstern to Princeton University who was a member of the Vienna Circle and has been studying business cycles, statistics and the idea that perfect foresight was a paradox in general equilibrium theory and the publishing of the *Theory of Games and Economic Behavior* (1944), coauthored with von Neumann, which laid the foundations of Game Theory;
- The joining of G. Debreu to Cowles Commission who was trained in French Burbaki School of Mathematics which adopted the formalist mathematical approach of Hilbert and the proofs basic theorems of Welfare economics together with K.J. Arrow;
- The publishing of 1950 article of John Nash who widened the Kakutani theorem to the proof of the "Equilibrium Points in n-Person Games";
- And finally, the proof of general equilibrium by Arrow-Debreu in 1954 by generalizing Nash's results.

In the beginning of the 1950s, according to Debreu, it was time to provide a formal proof of general equilibrium. Independent from Arrow-Debreu, Lionel McKenzie and David Gale proved the existence of equilibrium (for details on this topic see Debreu 1984, 1986, 1991; Weinraub 2002).

In fact, fundamental problematic of general equilibrium should be interpreted as an attempt by mathematical economists to provide a theoretical framework for A. Smith's invisible hand (independent rational decision making of agents in an economy will not create chaos but contribute to achieve social optimum) and likewise for

several basic economic problems such as efficient allocation of resources, noncentral decision making, incentives of decision makers and the nature and the role of information in economy (Arrow 1972; Debreu 1984).

According to Debreu, economics is far from creating a **Grand Unified Theory**⁴ but instead it appears as an entirety of small theories each focusing on certain economic problems one by one. Nevertheless, it provided solutions to a wide range of aspects of the economic system. One of the leading was theoretical explanations developed for the functions of prices, where solutions are found for the function of efficient allocation of resources using results from convex analysis, for the function of equating demand and supply using results from fixed point theorems and the function of avoiding the formation of destabilizing coalitions (or instabilities) using results from integration theory and non-standard analysis (Debreu 1991, p. 3).

On the other hand, mathematical economists argue that developments in game theory have led to significant improvements in the scope of economics, where economics has evolved from a social science focusing on the problem of production and allocation of resources, into a science studying the analysis of incentives of individuals in every social institution. In this regard, non-cooperative game theory, in particular, was seen as an important turning point in the evolution of economics and social sciences. Beginning with A. Smith economic theory has developed a sound formal analytical structure using linear algebra methods for the problem of price-quantity in commodity allocation space and with Nash it has further expanded into the analysis of individual incentives (rational competitive behavior) in market as well as non-market institutions (Roger B. Myerson 1999, p. 1080). Expansion of the research domain of economics took place together with the change in the functional purpose of social sciences: economics as a social science aiming at studying social institutions and evaluation of institutional reform proposals instead of abstract intuition associated with human behavior (Myerson 1999, p. 1069).

3. Economics as an Empirical Science

“Empirical macroeconomists are engaged in several promising lines of work. They are also engaged in making strained analogies between their work and the natural sciences and in classifying work in styles other than their own as outdated or mistaken based on its methods, not its substance. Since there is also a tendency in the profession to turn away from all technically demanding forms of theorizing and data analysis to focus a lot of negative energy on each other. All the lines of work described (...) are potentially useful, and the lines of work show some tendency to converge. We would be better off if we spent more time in reading each others' work and less in thinking up grand excuses for ignoring it.” (Christopher A. Sims 1996, p. 119)

The place of econometrics and statistical methods in economics is closely related to the history of the interaction of economics with physics. Furthermore, undoubtedly an explicit indicator of this close interaction between economics and phys-

⁴ Here, although it is not clear whether Debreu meant Unified Scientific System (or Encyclopedia) advocated by Vienna Circle or Hilbert's axiomatic approach, it will be reasonable to accept that he meant the latter.

ics manifests itself in the transformation of the name of the discipline from “political economy” to “economics”. This change can be seen as an effort to transform economics into a science with a higher degree of scientific legitimacy by establishing a value-free system of constructing economic propositions, testable hypotheses and inferences. The origins of this effort can be traced back to the *Unified Scientific System of the Vienna Circle* which was an empirical rationalism project directly aiming to distinguish science from non-science and to provide logical base to scientific arguments. Moreover, since the 1930s methodological discussions associated with applicability of Popper’s falsification criterion to economics together with significant developments in econometrics can be thought of as an effort to imitate the methodology of physics.

The relationship between economics and physics is closely associated with controversies on the relationship between economics and mathematics. In the previous section we saw that opponents to formalization of economics argue that mathematical physics, rather than pure mathematics, must be chosen as a model for economics. In other words, foundations and implications of economic theories must be empirically testable. This takes us to the ultimate question: can economics be a science like physics? This important and difficult question may be answered satisfactorily by making “*archeology of economics*”, hoisting stratoms of economic thought.

Moreover, following Mirowski (1989), we may argue that since Adam Smith the history of economic thought, especially the evolution of neoclassical economics since the 1870s; can be evaluated as “imitation” of physics, not only its methodology but also its content, problematic and conceptual framework (Mirowski 1989, p. 357).

“The neoclassicals opted to become scientific by ignoring what the physicists and the philosophers of science preached, and to cut the Gordian knot by directly copying what the physicists did. There can be no more pragmatic definition of science than this: Imitate success.”

Nevertheless, according to Mirowski, although the imitation process (imitation of Newtonian physics) was not without problems to begin with in the first place until the late nineteenth century, it was almost impossible following for economics to follow several innovations in physics (thermodynamics, quantum mechanics, general relativity and chaos theory) during the first three decades of the 20th century. These new scientific concepts and innovations, some of which are irreversibility of time, explanation of the macroscopic behavior of systems without appeal to underlying causes at the micro level, the possibility of universe with no scarcity, are not reconcilable with the “hard-core” of neoclassical economics (Mirowski 1989, pp. 386-394).

Even though Terence Hutchison (in the 1930s) and Milton Friedman (in the 1950s) are known as the main representatives of the view that economics must be purified from the political and ideological value judgments in order to become a positive science and must develop, corroborate and falsify theories by adopting methods of physics, the origins of this view can be traced back to the foundation years of neoclassical economics, to W.S. Jevons (Jan R. Magnus 1999, p. 55):

“The deductive science of Economy must be verified and rendered useful by the purely inductive science of Statistics. Theory must be invested with the reality and life of fact. But the difficulties of this union are immensely great.”

Jevons's emphasis 130 years ago on the necessity of improving empirical-inductive dimension and combining it with theoretical-deductive dimension in economics must be accepted as an important prediction on the difficulty of this process. Sixty years later the Econometric Society was founded in USA aiming to develop empirical foundations of economics (or to advance economic theory in its relation to statistics and mathematics). One of the main reasons behind the foundation of the Econometric Society was that *“economics is a science and it has an important quantitative side”* (Schumpeter 1933, p. 5). Econometric studies initiated in 1933 have shown important developments especially in the 1940s and the 1950s by the contributions of the Cowles Commission. Even if econometrics which is generally defined as the combination of collecting and analyzing economic data (data analysis and measurement), estimating parameters (modeling) and hypothesis testing processes and techniques (evaluation and testing) has made significant progress in the last fifty years, this development level has been criticized by many econometricians as being unsatisfactory (see, David F. Hendry 1980; Sims 1980; Edward E. Leamer 1983).

Indeed, it is possible to determine some implications about ongoing developments in econometrics which can be described as a positivist project that aims to produce the objective knowledge of truth about human behavior.

First of all, even if economics is one of the disciplines which have the most quantitative characteristics, it is a science in which conducting controlled-experiments are impossible or very difficult. While in physics and chemistry testing a hypothesis is possible by appropriately designed laboratory experiments, economists have to rely on mostly non-experimental data (which are generally historical data collected by public or private institutions for other purposes) when testing and comparing hypotheses. In this sense, there are crucial differences between physics and economics regarding the nature of the data they use. Furthermore, economics is different not only from experimental sciences, but also from observational sciences (archeology, astrophysics, biology and psychology) with respect to the relevant process of data collection. While in these disciplines the person who makes observation or collects data is the same person who analyzes the data, in economics the two are generally different (Zvi Griliches 1985).

According to Professor Hendry, although econometrics is a main innovation to improve estimation and forecasting procedures designed for non-experimental data, it must prove itself as a science rather than “alchemy” in order to overcome some problems such as the lack of experimentation (which precludes reproducible knowledge) and producing passive forecasts based on extrapolative methods. As a result of this, economists have to be aware of the fact that statistical significance is not always equivalent to economic significance and have to be aware of the criticisms of Keynes in 1940 on Professor Tinbergen’s approach (these criticisms are known as “problems of linear regression model”) (Hendry 1980):

“using an incomplete set of determining factors (omitted variables bias); building models with unobservable variables (such as expectations), estimated from badly measured data based on index numbers (Keynes calls this the “frightful inadequacy of most of the statistics”); obtaining “spurious” correlations from the use of “proxy” variables and simultaneity; being unable to separate the distinct effects of multicollinear variables; assuming linear functional forms not knowing the appropriate dimensions of the regressors; mis-specifying the dynamic reactions and lag lengths; incorrectly pre-filtering the data; invalidly inferring “causes” from correlations; predicting inaccurately (non constant parameters); confusing statistical with economic “significance” of results and failing to relate economic theory to econometrics.”

In order to overcome the accusation of alchemy, without forgetting the criticisms raised by Keynes, econometrics should bring out more improved test procedures to evaluate the empirical outcomes (Hendry 1980, p. 403):

“The three golden rules of econometrics are test, test and test; that all three rules are broken regularly in empirical applications is fortunately easily remedied. Rigorously tested models, which adequately described the available data, encompassed previous findings and were derived from well based theories, would greatly enhance any claim to be scientific.”

Economist Leamer also argues that the state of empirical economics is not so promising (Leamer 1983, p. 37):

“This is sad and decidedly unscientific state of affairs we find ourselves in. Hardly anyone takes data analyses seriously. Or perhaps more accurately, hardly anyone takes anyone else’s data analyses seriously. Like elaborately plumed birds that have long since lost the ability to procreate but not the desire, we preen and strut and display our t-values”

Although the credibility issue of econometricians is related to historical, rather than experimental character of data, it is more related with both econometricians’ not being transparent about data analysis and estimation procedure and with the *“myth that scientific inference is objective, and free of personal prejudice”*. Professor Leamer suggests that *“this myth which economists inherited from the physical sciences is utter nonsense”*. In addition;

“The false idol of objectivity has done great damage to economic science. Theoretical econometricians have interpreted scientific objectivity to mean that an economist must identify exactly the variables in the model, the functional form and the distribution of the errors. Given these assumptions and given a data set, the econometric method produce an objective inference from a data set, unencumbered by the subjective opinions of the researcher. This advice could be treated as ludicrous, except that it fills all the econometric textbook. Fortunately, it is ignored by applied econometricians. (Leamer 1983, p. 36).

According to Leamer, endeavors to develop an econometric model are not suitable to the axioms of statistical theory. However, in econometric studies statisti-

cal theory is used to test the hypotheses of the model. Modeling efforts show that a small change in the model or in definition of data changes empirical inferences significantly; in other words, empirical results are very fragile. Since economists cannot escape from their intuition, opinion and presuppositions they need to emphasize the distinction between their beliefs and model results in a more obvious way. On account of this Professor Leamer suggests that we need to find “more complete” but “still economical” way to report the “mapping of assumptions into inferences”. He proposes to “develop a correspondence between regions in the assumption space and regions in inference space.” By this way, one can report that “all assumptions in a certain set lead to same inference or some assumptions in the set lead to radically different inferences.” (Leamer 1983, p. 38) According to Professor Leamer unless this attitude becomes widespread among econometricians, the practice of econometric modeling will not be able to get rid of the reputation of being “fragile and easily breakable”.

Econometrician Magnus (1999, p. 58) also summarizes the current state of econometrics as follows:

“Most econometricians today are more ambitious. They believe that the main objective of applied econometrics is the confrontation of economic theories with observable phenomena. This involves theory testing, for example testing monetarism or rational consumer behavior. The econometrician’s task would be to find out whether a particular economic theory is true or not, using economic data and statistical tools. Nobody would say that this is easy.”

And he asks and (answers):

“But is it possible? (...) [Is there any published paper that contains a test which, (...) significantly changed the way economists think about some economic proposition? Such a paper, if it existed, would be an example of a successful theory test. [But there is no such a paper and] such is the state of current econometrics.”

Another controversial subject in econometrics is related with the use of data and modelling. In the first half of the twentieth century, the first approach related with business cycles which was developed by Mitchell and Burns, and adopted by NBER was *descriptive data analysis* (correct measurement of variables, choice of indicator and graphical demonstration). This approach has focused on the inductive-empirical side of models or “measurement problem” and has been criticized by the Cowles Commission (by Koopmans) as “measurement without theory, which was active at Chicago University in the 1940s (Scott Simkin 1999). On the other hand, the Cowles Commission, with its “*measurement with theory*” approach, emphasized the importance of theory in econometrical analysis, developed simultaneous structural equations models which were based on statistical probability theory (Christ 1994). In this way, theories developed to explain economic structure have been tested using statistical data, with the help of methods based on probability theory. The prediction failures of these models, especially in the 1970s, led to development of the general equilibrium or calibration models (Peter L. Hansen and James J. Heckman

1989; Charles I. Plosser 1989). These models emphasize the difference between observational data and incentives which determine the economic behavior. In real business cycles models which are also called “*theory without measurement*”, the conjunctural developments have been calibrated as “stylized facts” and determined the relationship between variables and conjunctures. Against the calibration models Professor Sims has developed VAR models (measurement without theory). In these models while the necessity of the models as a good description of data has been defended, it has been stated that the implications of models which developed as appropriate to theory will be invalid (Sims 1980, 1996). Of course this approach was not exempt from critics: These criticisms have led to the development of structural VAR models and dynamic stochastic general equilibrium models.

4. Conclusion

From the perspective of 200-250 years of history of economic thought, which may well be described as a history of making its theories more acceptable and closer to the truth one can definitely reach the conclusion that economics has reached a certain level of maturity. Two-hundred and fifty years of history of economic thought may well be described as a history of making economic theories more acceptable and closer to the truth. From this perspective one can conclude that economics has reached a certain level of maturity. Economics has formed a concrete theoretical core; it is the only social science awarded the Nobel Prize and its theories cannot be underrated from the viewpoint of mathematical rigor and reliability. Moreover, for a long time now, the interaction of economics with mathematics and philosophy (of science and of morals) has become bidirectional, not one-sided.

In this respect, we can say that instead of the hegemony of one of the mathematical, empirical (and of course moral or historical which we did not focus in this paper) aspects in the process of economic knowledge production, it would be more fruitful for economics if economists attempt to simultaneously improve each of these aspects. In a sense, this means freeing science (P. Feyerabend) and applying A. Smith’s *invisible hand* to the process of producing science.

We conclude this paper by stating our approach associated with ontological roots of economics which recently have been frequently emphasized:

Economics, as one of the social sciences dealing with various aspects of human behavior, should be constructed on three fundamental bases (or relationship dimensions): *human-nature* relationship, *human-human* relationship and *human-institutions* relationship.

The first essential ontological root of economics, the *human-nature* relationship, is best expressed by the traditional definition of economics stated by Lionel Robbins (1932, p. 16) in the beginning of the 1930s (which defines economics as science studying human behaviour as a relationship between ends and scarce means which have alternative uses). Although this definition is generally criticized as reducing human to an *a-social* (Robinson Crusoe) *maximizing individual*, its most important aspect is its emphasis on the scarce resources of nature. It is not possible to remove the phenomenon of scarcity from economics: considering the availability of the stocks of basic energy resources such as petroleum and natural gas in the concrete

world that we live in, it should not be a significant prediction to say that the human-nature relationship will gain more importance for economics in the foreseeable future within the context of the phenomenon of “scarcity”.

The second ontological root of economics, the *human-human* relationship, first of all expresses human as a social entity (people are born in a society and a family). It is neither possible nor meaningful for a person to provide own needs from nature alone. In this sense, purposefully or not, the specialization of people in certain areas and the division of labor socialize production and distribution. Besides the allocation of given material resources, this dimension of relationship enlarges the scope of economics to the problems of production (reproduction) and distribution (redistribution) (see Hollis and Nell 1975).

The third ontological root of economics, the human-environment or human-institutions relationship emphasizes that the economic decision or activity environment (institutional structure) of an individual is important and people both affect and are affected by this structure (Douglass N. North 1990; Ronald Coase 1998). People make economic decisions and perform their economic activities in certain institutions or through the mediation of certain institutions and this affects the end results. Here, institutions should be thought in general terms including specific institutional structures, rules, customs and traditions.

We can say that, without these three dimensions, the scope and analysis of economics will remain incomplete.

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