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Michele Gori and Vinicio Guidi

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Dipartimento di Scienze Economiche, Università degli Studi di Firenze
Via delle Pandette 9, 50127 Firenze, Italia
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# Rhetoric and conceptual problems in economics: the case of general equilibrium theory

Michele Gori
Dipartimento di Matematica per le Decisioni, Università degli Studi di Firenze
Via Lombroso 6/17, Firenze, Italy
e-mail: michele.gori@unifi.it

Vinicio Guidi
Dipartimento di Scienze Economiche, Università degli Studi Firenze
Via delle Pandette 9, Firenze, Italy
e-mail: vinicio.guidi@unifi.it

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**Abstract** The rhetorical perspective of science claims that scientific truths are what scientists agree is true on the basis of intellectual standards socially determined by the scientific tradition. According to that perspective, models are just rhetorical arguments used by economists in order to affect beliefs of the reference scientific community. Then, looking at the economic practice, we undertake a preliminary analysis of the criteria used by economists to assess and compare economic models. In particular, we argue that economists attach a great importance to the capability of models to solve problems of conceptual type. That allows to better understand how important is the role that general equilibrium theory has had and still has in economics and to rebut some criticisms made to the theory.

**JEL Classification** A12, B41, D50.

**Keywords** scientific rhetoric, general equilibrium theory, economic models.

## 1 INTRODUCTION

Since Adam Smith the economic thought has undergone a long process which has led to conceive what are nowadays the fundamental concepts of the economic discourse. Equilibrium, welfare, efficiency, money, production, utility and growth are some of them. At the same time, economists have proposed many relations and links among those concepts in order to suggest more and more suitable economic policies. Providing economic models where such relations are necessarily deduced has always been the way most economists have followed to defend their ideas. Models are the main tools economists use in order to understand and explain economic facts. As Keynes pointed out, 'economics is a science of thinking in terms of models joined to the art of choosing which models are relevant to the contemporary world.'[1] As a consequence, understanding economic models, their role, and their meaning is a necessary step for understanding the way economics develops.

In the present paper we provide an interpretation of economic models on the basis of the epistemological viewpoint of the scientific rhetoric. The rhetorical perspective claims that the truth of scientific laws (both in social and in hard sciences) is what scientists agree is true according to shared intellectual standards socially determined by the scientific tradition. Thus, scientist work is to create new beliefs or change old ones in the scientific community during a concrete debate and through arguments that have to satisfy specific constraints fixed by the reference scientific community.

Such a perspective implies that economic models are rhetorical arguments economists may use in order to affect her colleagues' opinions about certain economic propositions. Each model has in turn to be supported by further arguments that serve to prove it satisfies the required standards and then it can be accepted as a 'strong' argument by the economic community. In other words, when a model is proposed it has to be shown it fits the values that economists recognize as important. Another

[1]Keynes (1973).

consequence of the rhetorical approach to science is to provide a particular interpretation of philosophers' activity. In fact, the conviction of the impossibility to determine universal standards guiding scientific research implies that philosophers have to avoid normative claims and only observe scientists at work.

Then, looking at the economic practice, in the present paper we undertake a preliminary analysis of the criteria used by economists to judge the strength of their main cognitive arguments, i.e., economic models. In particular, analysing general equilibrium models, we point out the importance overtly or covertly attached by the economists to the capability of models to solve problems of conceptual type. That allows to clarify the role and the impact that general equilibrium theory has had and still has in economics and to show how some important criticisms made to the theory stem from the fact that they ignore its main virtues and only focus on empirical aspects and factual arguments.

## 2 RHETORIC IN SCIENCE AND ECONOMICS

The twentieth-century modernism is characterized by the belief on the existence of a method that allows to distinguish science from any other intellectual discipline and whose application guarantees to reach the aim of science. The determination of such a method has driven large part of the methodological literature and Popper, Lakatos, Laudan, and many other authors, despite of the great differences among the methodologies they propose, carry on the same project and are moved by the same conviction that 'if science possessed no method, it would not be a cognitive and rational endeavour.'[2] At the same time, however, fundamental contributions, for instance, by Feyerabend, Latour, Rorty, Foucault have strengthened the idea that the existence of universal standards is not very realistic and can even have pernicious effect on the progress of science.

Even though we agree with those authors claiming that the scientific method is a kind of myth and then its search should be abandoned, we also believe that science remains a rational activity as its rationality resides in the fact that it can and does rely on conveniently qualified rhetorical arguments to justify its decisions. Indeed, assuming there is no method does not lead 'either to [abolish] the constraints of science by saying that "anything goes", or [assimilate] a scientific discussion to a political or ethical conversation. Rhetoric does not necessarily lead to these two conclusions: neither to the former because rhetoric *does have* constraints; nor to the latter because scientific rhetoric has *specific* constraints.'[3]

That viewpoint is far from being original as many authors have emphasized the role of rhetoric in science. A very effective metaphor describing the rhetorical interpretation of the scientific inquiry is proposed by Pera (1994, p.11):

> 'The methodological model views science as a game between two players: the researcher proposes, and nature - with its ringingly "yes" or "no" - disposes. In the counter-methodological model, the situation is the same, the only difference being that nature's voice is so weak that it is drowned out by the researcher's, who ultimately becomes nature's ventriloquist, providing the desired answer. The [rhetorical] model is different: it requires three players: the proposer who asks questions, nature that answers, and a community of competent interlocutors which, after a debate hinging on various factors, comes to an agreement upon what is to be taken as nature's official voice. In this model nature does not speak out alone. It only speaks *within* the debate and *through* the debate.'

According to the considered epistemological framework, what all scientists can do is just to strengthen or induce a change in belief in the scientific community during a concrete debate, on the basis of rhetorical arguments that, in order to be considered strong and reasonable, have to follow rules that are implicitly or explicitly established by the scientific community itself. Indeed, 'perhaps there is no scientific method, no global strategy for all of sciences; still there are scientific methods, the aggregate of agreed-upon procedures.'[4] Thus, from a rhetorical perspective, the truth of scientific laws is what

[2]Such a statement is what Pera calls *Cartesian syndrome* (Pera 1994, p.4).
[3]Pera (1994, p.57).
[4]Gross (1990, p.32).

scientists agree is true according to certain shared procedures determined by scientific tradition and specific to each scientific debate. Those socially determined intellectual standards operate so in social sciences and humanities, as in hard sciences. Even in mathematics, 'the line between complete and incomplete proof is always somewhat fuzzy, and often controversial'[5] and the 'mathematical truth, like other kinds of truth, is fallible, corrigible, tentative, and evolving, as is every other kind of human knowledge.'[6]

The role that rhetoric plays in the specific area of economic science has been deeply analysed, as well. McCloskey, the main representative of the rhetorical economics, claims that accepting an economic theory as scientific does not depend on the correspondence with fixed epistemological rules as 'the many official methodologies are apparently not the grounds for [economists] scientific conviction.'[7] In her view, the acceptance of an economic theory relies instead only on the persuasiveness of theory's author. Thus, rhetoric is the study of how people persuade and its main task is to articulate the shared norms of good conversation and to make economists be aware of the rhetorical features characterizing their conversations leaving 'no space for the traditional concern with methodological principles and rules.'[8] Indeed, 'the only relevant and significant criteria for assessing the practices and products of a discipline are those accepted by the practitioners. Apart from a few general standards such as honesty and a willingness to listen to criticisms, the only justifiable criteria for any conversation are those of the participants.'[9] The rhetorical perspective is then different from an 'anything goes' relativism as the conversational view of intellectual life requires that scientists make their conversation overlap and that very overlapping provides specific standards. As Klamer (2001, p.72) states:

> 'My current response is that even though economics lacks unambiguous standards, clearly not anything goes. Just try to reason that people buy more because God tells them to, or argue that economics is all rhetoric, and see what happens. If not constrained by some objective standard, economic discourse is social and hence is constrained socially.'

The rhetorical model of economics emphasizes the social dimensions of economic science and 'offers an alternative to the modernist extreme of hard-nosed science and soft-hearted relativism, both of which inhibit conversation and learning.'[10] Of course, assuming that science is persuasion rather than demonstration implies the need to carefully point out the main factors in terms of which the rhetorical arguments can be appraised.

For instance, showing that a theory has the capability to predict phenomena is surely the most effective argument used by scientists to support it. Particularly in natural sciences, there are lots of theories that have numerous empirical confirmations, that is, using Laudan's terminology, solve empirical problems. In general, such factual arguments can make theories be largely accepted and be considered worth trusting. Yet, as Kuhn crucially pointed out, the idea that if a theory makes a correct prediction, then it gets stronger while if a theory fails a prediction, then it is rejected, is only a mere simplification of the matter. In fact, empirical researches have never been able to determine theoretical choices without the analysis of further factors. For instance, when an empirical test is done, at least two things are needed to assess it. First, it has to be decided whether the test deals with a significant phenomenon. Then, it has to be decided whether the test is successful or not. Both these issues can be evaluated only through non-factual arguments and may lead to very long discussions between supporters of different theories, as shown by a lot of examples in the history of science.[11] In other words, as empirical evidence is rarely sufficient to reject a theory or to decide among different rival theories, the assessment of those theories has to consider their capability to solve, using again Laudan's terminology, conceptual problems.

Analysing paradigmatic examples of scientific practice, Pera (1994, chap. 4) points out some

[5]Davis and Hersh (1981, p.34).
[6]Davis and Hersh (1981, p.406).
[7]McCloskey (1983, p.482).
[8]Mäki (2008, p.549).
[9]Hausman (2008, sec. 4.2).
[10]Garnett (2004, p.240).
[11]Gross (1990, p.4) states: 'the "brute facts" themselves mean nothing; only statements have meaning, and of the truth of those statements we must be persuaded. These processes, by which problems are chosen and results interpreted, are essentially rhetorical: only through persuasion are importance and meaning established.'

factors that are significant to assess theories and that do not involve empirical evidence. Among them we have, for instance, the relation to accepted theories; the ontological interpretation of the world (say, interpretative theory) supporting the theory; regulative values, such as simplicity, harmony, elegance, and falsifiability; the related scientific tradition working as a principle of preference among different values; presumptions, that are those assumptions that are true until they are proved to the contrary.

The rhetorical arguments used by scientists are built by taking into consideration the factors above described and maybe others. In addition, the way arguments are presented, that is, the style used to convey them is not merely ornamental but plays a fundamental role in persuading people. Indeed, the work of McCloskey and Klamer on the rhetoric of economics is strongly characterized by the analysis of the economic writings under a stylistic viewpoint due to their idea that the best understanding of the social standards used to evaluate economic theories can be reached by performing more and more sophisticated rhetorical analysis of economic literature. Their study led then to 'the identification of various rhetorical ploys and textual strategies used by economists, such as the use of attractive metaphors, and appeals to authority and mathematical brilliance.'[12]

We strongly believe that such kind of analysis is valuable as it may contribute to make economists be aware of how their way of writing is linked to their success and to the acceptance of the results of their research. However, in our opinion, not all is style. On the contrary, following Pera's approach and due to our conviction that 'there is no external viewpoint from which one can judge which statements, theories, or methods are sound and which are not,'[13] we think we could get a deeper insight into the way economics develops by avoiding normative claims, directly looking at the economic practice, and explicitly determining the criteria used by economists to judge and compare theories and models.

Of course, the task is difficult and represents an out-and-out research program. In what follows, we attempt a first analysis of the topic restricting our view to economic models – the main economists' tools. Yet, first of all, we need to clarify what an economic model is under a rhetorical perspective.

## 3 ECONOMIC MODELS

The philosophical evaluation of theories and models has changed radically over time. For many years the setting was monopolized by the 'received view', which basically defines theories as formal-syntactic calculi and attaches a particular importance to the language.[14] It ran into difficulties as, interpreting only the part of language relative to the observables, it had to be committed to correspondence rules.[15] Yet, considering those rules as part of the theory would have caused the theory to be modified by their change, while the theory would have had no content otherwise. Besides, it was stressed also the naive interpretation of theories as direct representations of the world by ignoring idealizations, ceteris paribus, simplifications and approximations.[16]

That explains why over the last four decades the 'semantic view' has become the orthodox view of models and theories. Its adherents define a theory simply as a class of models satisfying certain formal relations, called 'structures'. But even in this case we have no satisfactory answer to the problem of the representation of reality.[17] That does not happen in a particular version of the semantic view, called 'structuralism', which was very active in the seventies and eighties. In fact, this version axiomatizes theories which become 'mathematical structures' and whose assumptions are intended as open sentences. As a consequence, theories are not constrained to represent reality but, at the same time, they have no truth-value.[18]

[12]Mäki (2008, p.549).
[13]Kölmann (2008, p.594).
[14]There are different versions of the 'received view'. For an exposition see, inter alia, Suppe (1979), (1989), Salmon (1989), Morgan and Morrison (1999), and Henry and Psillos (2007).
[15]Originating so the well-known problems of partial interpretation, the supposed dichotomy between observational and theoretical terms and the distinction between analytic and synthetic terms.
[16]The issue of idealization is masterly analyzed in Nowak (1992), Hausman (1992) and Morgan and Knuuttila (2012).
[17]See, for instance, Frigg (2006, pp.50-54).
[18]Some of them were particularly interested in economic theories. For a detailed exposition see Klein (1998).

Both conceptions of theory share the same setting. In fact, both aim largely to a formal, logical analysis of scientific theories in order to suggest research rules instead of understanding what scientists are exactly doing. Differently from that position, some philosophers have instead started analysing how scientists build and use models based on the idea that models, even if tied to a theory, do not totally depend on it and play an autonomous role and 'the processes of constructing models and manipulating them are crucial in gaining information about the world, theories and the model itself.'[19]

There is a wide literature on models in science[20] which ranges over a wide variety of dimensions such as their representational function (semantics), their essence (ontology) and the way we can learn from them (epistemology). This is true also for economic models and we can trace multiple often contrasting approaches to their nature and epistemic function. For instance, we have Blaug's popperian legacy, Mongin's deductive-nomological approach, Hausman's lawlike generalizations of equilibrium theory qualified with ceteris paribus clauses, Weintraub's lakatosian account of global theories and, recently, Marchionni's and Hindriks' contrastive explanation.[21]

Modal relations and properties are distinguishing features of neoclassical economic models (for instance, existence of equilibrium). This clarifies why Rappaport (1998) has labeled as 'modal view' his approach to the epistemology of economic models which are identified 'with a set K of definitions and non-definitional assumptions together with their deductive consequences, plus a set L of hypothetical objects described by the statements in K.'[22] Even thought about hypothetical objects, in the modal view the axioms of a theoretical model are truths. This contrasts with the structuralist approach according to which the axioms included in the definition of a predicate which is an economic model lack a truth value, unless a realization of the model is supplied. Rappaport's approach shares some aspects with the analysis of Gibbard and Varian (1978), and is substantially equivalent to one of 'possible worlds', originally developed by some philosophers such as, for instance, David Lewis and Saul Kripke, to solve certain problems in formal semantics and later applied to narrative and literary theory.[23] Yet, the open question is about the cognitive role of models and, in particular, the relation between the model and the actual world. The analysis is complicated by the coexistence in a model of objects that are easily associated with real objects, and fictional objects, which exist in the model (that is, in a possible world), and only under certain conditions they could be traced in the actual world.

Sudgen (2000) justifies the induction to the actual world only in 'credible worlds' and makes narrative themes appear on the economic scene, as well. Sudgen's notion of credible model parallels that one of 'realistic novel' and requires that model assumptions must be coherent at two different levels. First, beyond logical coherence, they have to fit naturally together in the working of the model, (for instance, ad hoc hypotheses should be excluded). Moreover, they need to be coherent with the known casual processes in the real world.[24] In Sudgen's opinion, the credibility of a model justifies the inferences from it to the actual world and the more we can understand the model as a description of the world, the more we can trust those inferences.

Grune-Yanoff (2008) partially criticizes Sudgen's view. In particular, he disagrees with Sudgen's claim 'that the inference goes from claims about a particular model to a general hypothesis about the world,'[25] and stresses that inferences should be indeed thought as going from particular imaginary situations to particular actual world situations. Moreover, as the parallelism between the actual world and a plausible one can never be complete, he emphasizes also the problem to better justify why judging a model to be credible allow to infer from the model claims about the actual world.

Nevertheless, Grune-Yanoff believes that it is possible to learn from credible models, even from the minimal ones, that is, models which 'are assumed to lack any similarity, isomorphism resemblance relations, to be unconstrained by natural laws or structural identity, and not to isolate any real factor.'[26]

[19]Morgan and Morrison (1999, p. 8).
[20]Frigg and Hartmann (2006).
[21]Blaug (1992), Mongin (2002), Hausman (1992), Weintraub (1985), Marchionni (2006), Hindriks (2008).
[22]Rappaport (1998, p.167). The same approach, used also by Guidi (1999), was introduced first by Handler (1982).
[23]Ryan (1991).
[24]Sugden (2000 p.25) states: 'It describes a state of affairs that is credible, given that we know (or think we know) about the general laws governing the world.'
[25]Grune-Yanoff (2009, p.93).
[26]Grune-Yanoff (2009, p.83).

In fact, even though credible minimal models do not support neither general nor particular claims about the world, they may affect our belief about what is impossible or necessary in the real world and may play a heuristic role in developing such claims. In line with Sudgen's analogy between credible models and credible fictions, Grune-Yanoff states also that judging a model to be credible 'is a consequence of what scientists do with models: they imagine a world that the model describes, they manipulate that situation in various ways, and they investigate that world's internal coherence with our intuitions. Crucially, these intuitions often do not exist independently of the imagined world.'[27] We finally stress that, as the assessment of a model as credible cannot be separated by the principles of the reference paradigm, Sudgen and Grune-Yanoff's approaches seem to us to share some aspects with ours.

In fact, according to the rhetorical perspective considered in the present paper, we simply interpret an economic model as a rhetorical argument. In our opinion, presenting an economic model to the economic community, convincing people that the whole set of assumptions made are reasonable and coherent, showing what are the consequences of those assumptions by means of deductive reasoning, and providing effective interpretations in the actual world of those consequences is one of the possible arguments economists may use in order to make people believe or reject certain propositions about the actual world. Thus, as also stressed by Kuhn (1977), a model is a strong argument that can discard or corroborate already established concepts, provided it fit the scientific standards fixed by the reference scientific community (that is, it can be supported in turn by other arguments showing it should really considered strong).

Economic models work as thought experiments[28] where formal objects are associated with suitable economic concepts and mathematical relations have particular economic interpretations which may suggest new links among actual economic entities, cast doubts on some commonly believed economic propositions and increase the confidence in some others.[29] Yet, as highlighted by Davies, 'as in the case of standard fictional narratives, we are prohibited from taking as background understanding everything we believe to be true in the world. (...) Further, and again analogously, the understandings required to make sense of a [thought experiment] will be those things taken to be true of the world (...) by the community of scientists or thinkers to whom the [thought experiment] is addressed.'[30] A community which shares similar principles and beliefs, uses the same tools, expresses ideas and concepts in the same language (generally of a mathematical type) and has to decide the ultimate scientific destiny of models.

## 4 ECONOMIC MODELS AND EMPIRICAL PROBLEMS

As the ultimate purpose of economics is to describe the actual world, the correspondence to empirical evidence is surely an important factor able to make a model be accepted. In fact, empirical research is nowadays an important part of the economic inquiry and, in particular, econometrics and experimental economics aim to give empirical content to economic relations in order to test economic theories, forecast, examine many of the neoclassical paradigm issues and, especially for econometrics, evaluate policy decisions.

There is a long and fertile tradition in empirical analysis in economics going back, at least, to the 'political arithmetic' of William Petty, Gregory King and Charles Davenant in the 16th century. However, 'the period of emergence of econometrics began in the late of nineteenth century, when econometrics was merely a glint in the eye of commentators (…) and goes on to the time of the establishment of the Econometric Society in 1931 with its confident presidential address by Joseph Schumpeter.'[31] As it was

[27]Grune-Yanoff (2009, p.94).

[28]Brown and Fehige (2010), Ylikoski (2003), Davies (2007), Vosnadiou (2002). In particular, Kornai (1971, p.17) states that 'equilibrium theory is merely an intellectual experiment.'

[29]Brown and Fehige (2010, sec.2) stress that, in order to be effective, thought experiments having the purpose to cast doubts do not need to be very plausible and propose the following interesting analogy: 'In a court of law, the jury will convict provided guilt is established "beyond a reasonable doubt." A common defence strategy is to provide an alternative account of the evidence that has just enough plausibility to put the prosecution's case into some measure of doubt. That is sufficient to undermine it. A good "counter thought experiment" need only do that much to be effective.' Interesting analogies between a scientist and a judge are presented in Pera (1994, sec.2.4).

[30]Davies (2007, p.35). A similar statement as regards the language is found in Kuhn (1989, pp.12-13).

[31]Hendry and Morgan (1995, p.4).

clearly stated by Frisch in the editorial of the first issue of Econometrica, the purpose of econometrics was 'to promote studies that aim at a unification of the theoretical-quantitative and the empirical-quantitative approach to economic problems'[32] with statistical and mathematical techniques. The unification of measurement with theory in economics was seen as possible, given the quantitative nature of economic relationships.

In the twenty years after 1950, a number of important innovations both in the technological and in the theoretical ground made econometrics become the main method of applied economics. On the technological side, there was an exponential growth of economic data sets, and better and faster computers increased the capability to quickly manipulate them. On the theoretical one, thanks to Cowles Commission, the primary task of econometrics was considered the estimation of structural parameters of a system of simultaneous stochastic equations. In the next decades, important results and methodological innovations were realized as regards, for instance, micro and macroeconometrics, financial time series and economic policy.[33]

However, despite of those developments, a deep-seated skepticism about econometrics has always been present among theoretical economists. Such critical attitude towards that discipline was already emphasized by Keynes (1940, p.156), who stated:

> 'No one could be more frank, more painstaking, more free from subjective bias or parti pris than Professor Tinbergen. There is no one, therefore, so far as human qualities go, whom it would be safer to trust with black magic. That there is anyone I would trust with it at the present stage, or that this brand of statistical alchemy is ripe to become a science, I am not yet persuaded.'[34]

We believe that econometrics can provide important contributions to the understanding of the economic processes. At the same time, as stressed by Hendry, 'it is difficult to provide a convincing case for the defence against Keynes' accusation almost 40 years ago that econometrics is statistical alchemy since many of his criticisms remain apposite.'[35] Indeed, if we take into consideration the plague of data mining;[36] the possibility of spurious correlations; the existence of multiple criteria implying 'that rival and inconsistent models might proliferate; that design criteria might reveal the prejudices of the modeller, not the underlying economic structure; that parameter estimates might be biased; and that test sizes might be misleading,'[37] we can understand why suspicion is still quite common among theoreticians.

So it is not surprising that for some economists 'successful empirical research has been characterized by attempts to gauge the strength of associations rather than to estimate structural parameters (...) and the skilful use of carefully natural experiments rather than sophisticated statistical technique to achieve identification.'[38] In other words, some authors believe that successful empirical research is the one referring to the other empirical side of economics, that is, experimental economics.

Indeed, 'from the 80's onwards, there has been an explosive growth in the use of experimental methods in economics'[39] and it is unquestionable that economics has undergone so deep changes in the last years to induce some economists to speak of a revolution.[40] The success of experimental economics was made possible not only by the technological factors previously described, but also by some important changes such as the birth of game and expected utility theories; the sunset of general equilibrium theory as the fundamental basin of attraction of theoretical economics; the increasing dissatisfaction with the strong assumption of rationality; the ignorance of the social dimension and the psychological aspects in economic agents behaviour as regards the process of forming expectations and modelling preferences.

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[32]Frisch (1933, p.1).

[33]For these and other aspects, see Geweke, Horowitz and Pesaran (2008).

[34] The quotation refers to the Keynes's comment closing the debate about its review of Tinbergen's essay *Statistical Testing of Businne-Cycle Theories*.

[35]Hendry (1980, p.402).

[36]Leamer and Leonard (1983, p.306) state that collective data mining renders standard inference meaningless.

[37]Du Plessis (2009, p. 224).

[38]Summers (1991, p. 130).

[39]Barsdley, Cubitt, Loomes, Moffatt, Starmer and Sudgen (2010, p.1).

[40]Kreps (1997, p.73), Barsdley et al. (2010, p.2), Guala (2005, p.3).

Undoubtedly experiments were particularly useful in order to isolate 'effects of the rules of the game by which markets are organized' or to understand 'when equilibrium predictions will be descriptive and when they will not.'[41] Other positive effects were produced in the field of coordination, in the replacement of the constant rate of discount with a hyperbolic function, in the elaboration of the value function which captured the higher concern of people for losses than for gains, and in recognizing behavioural regularity according to which 'people seem to respond to perceived gains or losses rather than to their hypothetical end states.'[42] Through experiments, it is possible to generate new data, correcting so a drawback of econometric techniques and opening the avenue to a collaboration between the two fields.

However, the conventional wisdom is that, given the great complexity of economic subjects, laboratory experiments could offer little to our science.[43] For instance, among criticisms we have that 'experimental situations often project a game-like atmosphere in which a "subject" may see himself as "matching wits" against the experimenter designer of the game.'[44] Moreover, experimental subjects are cast in the role of seller or monopolist without any education and neglecting the role of asymmetric information and learning processes. It is also dubious that some students or volunteers can be a representative sample of the universe. But another cause making economists be unconvinced by the results of experimental economics is that, at least up to now, no set of methodological principles has been recognized and accepted for the discipline. In particular, 'economists can and do use experimental methods in their own work while rejecting the different methods used by other experimenters. They can and do recognize the value of some programs of experimental economics while expressing skepticism about, or even hostility toward, others.'[45]

So the experimental turn has not implied a 'shift in the centre of gravity of the discipline. Economists have not flocked en masse to new subjects, nor have they abandoned the old.'[46] The problem to understand what economics can really learn from laboratory experiments and how traditional theory should be modified by experimental results is still under debate, as clearly expressed by Levitt and List (2007, pp.153-154):

> 'a critical assumption underlying the interpretation of data from many laboratory experiments is that the insights gained in the lab can be extrapolated to the world beyond, a principle we denote as generalizability. For physical laws and processes like gravity, photosynthesis, and mitosis, the evidence supports the idea that what happens in the lab is equally valid in the broader world. The basic strategy underlying laboratory experiments in the physical sciences and economics is similar, but the fact that humans are the object of study in the latter raises special questions about the ability to extrapolate experimental findings beyond the lab, questions that do not arise in the physical sciences. While few scientists would argue that observation influences whether Uranium239 would emit beta particles and turn into Neptunium, many economists agree on the fact that human behavior may be sensitive to a variety of factors that systematically vary between the lab and the outside world.'

As all other scientists, economists deal with phenomena whose description requires many simplifications and in which many interferences may be supposed to influence them. However, differently from natural sciences, economics still lacks something like crucial experiments.[47] For such reasons, Hausman concludes that economists trust more the implications deduced by models' assumptions than the results which may emerge from empirical testing, 'and confidence in the implications of economics derives from confidence in its axioms rather than from testing their implications.'[48] As clearly stated by Rubinstein (2001, p.619), explaining how a theorist think the role of experiments is:

[41]McKinney and Roth (2010, p.2).
[42]Thaler (1987, p.105).
[43]In Guala (2005, pp.2-3) you can find skeptical statements of well-known economists.
[44]Cross (1980, p.404).
[45]Barsdley et al. (2010, p.2).
[46]Kreps (1997, p. 67).
[47]Beker (2005, p.4).
[48]Hausman (1992, p.1).

> 'Experiments serve as a test of the plausibility of assumptions and not conclusions. When experimental economics feeds economic models it can suggest new ideas about human reasoning in economic situations. In any case, experimental economics should relate to the plausibility of assumptions we make on human reasoning rather than trying to accurately predict human behavior. Experimental economics provides us with a safeguard which protect us from mistaken intuitions.'

Then, by now, we can state that despite of the great effort in making economics strictly related to the empirical evidence there is no economic model which can be considered strongly confirmed by observations and only few economic models have been rejected because of an apparent empirical disconfirmation.[49] In other words, even though most economists think that predictive success is important and 'the standards of predictive success which lead one to have qualms about economics are already standards that many economists accept,'[50] factual arguments have never been able to be decisive to solve a controversy in economics. As a consequence, economists take into account and use a lot of arguments that are not properly factual and important criteria to evaluate economic models are on the theoretical and conceptual side.

In the next section, we specialize on general equilibrium theory, which represents a paradigmatic example of economic investigation whose development is mainly linked to conceptual aspects rather than empirical, and we highlight some reasons why the conceptual analysis, beyond the empirical one, is important in economics. As a by-product, we clarify the role that general equilibrium theory has in economics describing some main virtues of its and providing an answer to certain important criticisms made to that theory.

## 5 GENERAL EQUILIBRIUM MODELS AND CONCEPTUAL PROBLEMS

In a broad sense, general equilibrium (GE) theory[51] is that part of economics whose ultimate purpose is to give a precise mathematical description of the whole economy and to provide a formal statement of the celebrated Adam Smith's metaphor of the 'invisible hand', that is, the proposition stating that in a decentralized economy, the choices of many economic agents acting for their own interest and guided by price signals are coherent with a desirable social distribution of resources. Under the label of GE theory we can collect a large amount of mathematical models, each of them focusing on different features of the market and characterized by a specific definition of economic equilibrium suitable for the considered framework.

As already explained, factual arguments are seldom proposed for defending a GE model, due to the difficulties to provide for this kind of models any reasonable empirical confirmation. Certainly, GE theorists think economic models must ultimately serve to describe and explain reality, as also shown by certain branches of GE theory directly referring to empirical applications like the Applied GE theory, whose models are used for the policy evaluation,[52] and the Dynamic Stochastic GE theory[53] with the delicate issue of calibration. But looking at GE theory as a whole and its scholars community, that is, the sociological reference group generating the system of values used to assess new models, empirical confirmation is neither the basic issue to be considered nor the most compelling. Most of the persuasiveness of such models comes from other factors that go beyond empirical evidence and prediction.

GE theory was for many years at the heart of economics, starting from Arrow and Debreu (1954), and it is interpreted in different ways by both philosophers and economists. For instance,

[49]Hahn (1987, p.110) says: 'it is not easy to think of a proposition in economics that all reasonable economists agree to have been decisively falsified by the evidence'.
[50]Hausman (2008, sec. 4.2).
[51]According to Hausman the fundamental theory of neoclassical economics is Equilibrium Theory, while 'General Equilibrium Theories augment equilibrium with simplifications or specifications concerning the situation to be studied' (Hausman 1992, p.273). See also Hausman (1981, p. 21). For a different position that we share, see Guidi (1999).
[52]Shoven and Whalley (1992), Kehoe, Srinivasan and Whalley (2005). Varian (1989, p.2) says: 'Given my view that economics is a policy science if I want to defend a practice in economics , then I must defend it from a policy perspective.'
[53]Bukhard and Herr (2005). On the calibration issue, see also Morgan and Knuuttila (2012).

Hausman argues that GE theory lacks of explanatory value but 'may be of great heuristic value,'[54] Rosenberg evaluates it as applied mathematics while Weintraub, with others, cuts out its important role of hard core in the lakatosian perspective of the neoclassical economic research program. Further, the epistemology of the father founders Walras and Pareto is set forth in a keen way by Montesano,[55] while the long process of refinements leading to the existence of a competitive equilibrium is very well stated by Weintraub (1985).

Just focusing on the fact that GE models lack of empirical content, some authors stress that the mathematization process implies some risks as, in particular, it makes theories lose contact with reality. Blaug, for instance, committing himself to a popperian program, considers GE theory a complete failure as a representation of the world and attributes no scientific merit to it because, after the paper by Arrow and Debreu, its leading characteristic has become 'the endless formalization of purely logical problems without the slightest regard for the production of falsifiable theorems about actual economic behavior.'[56] Backhouse stresses that the modern incarnation of Adam Smith's proposition about invisible hand, that is, the Pareto-efficiency of competitive equilibrium in the Arrow-Debreu general equilibrium model, is very different from the original as its empirical content has become lost. In his view, that proposition is not about the real world and only deals with properties of an abstract mathematical model. He thinks instead that 'formalism, as the term has come to be understood in economics, must be tempered by an understanding of how theoretical concepts might, or might not, be related to the real world.'[57] McCloskey recognizes that mathematics has allowed economics to make faster progresses than before its use but, at the same time, she thinks that 'the economists are in love with the wrong mathematics, the pure rather than the applied.'[58] In her opinion, that has led to abandon any kind of quantitative analysis in favour of 'a search through the hyperspace of conceivable assumptions.'[59] Propositions and, in particular, existence theorems of GE theory are not quantitative. They are only qualitative results and definitely not surprising in a qualitative sense. In her opinion, 'the prestige of mathematical argument led economists to believe, contrary to their discipline, that the economist could get something intellectually for nothing, proving or disproving great social truth by writing on a blackboard.'[60]

Even though the lack of significant empirical content can surely be considered a limit of these models, GE theorists and us think that looking at them as conceptual tools they can make us understand something useful about the economic world though. In fact, as we are going to discuss later, the above considered criticisms ignore some important virtues of GE models.

As discussed before, we think that economic models and, in particular GE models, are rhetorical arguments which economists may use to support their ideas and modify the beliefs of their colleagues.[61] Such arguments are written using a mathematical language and most economists believe that the mathematization of economics carried on by GE theorists has led to progress of their discipline due to several reasons.

First, GE models have been fundamental to provide a clarification, via the mathematical formalization, of those concepts and relations that the economic community recognizes as fundamental and, consequently, to solve many pseudo-problems making economists discuss about a problem having in mind the same things.[62] Moreover, practitioners of GE theory have stressed the heuristic role of the models as useful benchmarks suggesting new hypotheses and new relations among the objects described in the models. In fact, sometimes the mathematical objects introduced in a GE model have more properties than we explicitly attribute to them, and finding out those properties may produce new results that allow to gain a deeper economic insight. For instance, the first neoclassical economists (the

[54]Hausman (1981, p.26).
[55]As there are many papers of the author on the topic, we limit to Montesano (1995). Other references can be found in Guidi (1999).
[56]Blaug (1992, p.169).
[57]Backhouse (1998, p.1857).
[58]McCloskey (1991, p.12).
[59]McCloskey (1991, p.10).
[60]McCloskey (1991, p.10).
[61]Hausman (1981, p.25) states: 'it seems to me most appropriate to assess general equilibrium theories with respect to models of explanatory arguments, since, as existence proofs, they are arguments.'
[62]See, in particular, Guidi (1999) and Köllmann (2008).

marginalists) were criticized by psychologists for using a cardinal utility. Later, Edgeworth and Pareto, studying the consumer problem, realized that the equality between the marginal rates of substitution and the relative prices would not change multiplying the marginal utilities of goods by the same positive constant (the derivative of a strictly increasing function), showing so that the maximization of utility under the budget constraint was consistent with an ordinal utility, as well.

Finally, the use of mathematical reasoning is recognized to have forced researchers to be more precise, enrich understanding, uncover contradictions, eliminate ambiguities, reveal hidden assumptions, suggest generalizations and 'has enabled researchers to build on the work of their predecessors and to accelerate the cumulative process in which they are participating.'[63]

As documented by Weintraub (1985), many economists and mathematicians have contributed to the development of GE theory along the just described lines, and many contributions to the economic theory arise from their conceptual speculations. In a series of remarkable papers Arrow and Debreu were decisive to provide unambiguous definitions (at least within the model they consider) of the intuitive notions of equilibrium, efficiency, rationality and so on and showing 'the viability and the efficiency of the market system (...) in a model completely faithful to the neoclassical methodological premises of individual rationality, market clearing and rational expectations.'[64] Indeed, in the Arrow-Debreu's framework it is possible to prove, in particular, the so called 'fundamental theorems of welfare economics', that is, that every equilibrium allocation is Pareto optimal and that every Pareto optimal allocation is an equilibrium allocation for a suitable price system. The first of these theorems was interpreted as one of the possible mathematical formalizations of Adam Smith's metaphor for self-organization of the market and thanks to that result the limits of the invisible hand were considered better understood, even in absence of empirical tests.[65] The second theorem suggested that, in the ideal competitive economy, a state intervention in the economy should be limited to the redistribution of initial endowments without interfering on prices. But it was soon clear that this kind intervention requires an unrealistic amount of information on the economy. Thus, economists who believe in a possible allocation channel different from the market were forced to find other and more substantial reasons, such as 'market failure' situations, for justifying their view. As Krugman (1993, p.28) says,

> 'Arrow-Debreu model (...) is indeed a wonderful model, not because its assumptions are remotely plausible but because it helps us think more clearly about the nature of economic efficiency and the prospects for achieving efficiency under a market system. It is actually a piece of inspired marvelous silliness. What I believe is that the age of creative silliness is not past. (...) If a new set of assumptions seems to yield a valuable set of insights, then never mind if they seem strange.'

Arrow and Debreu show further that, suitably specifying the characteristics of commodities according to the geographic location, the temporal availability, the state of nature, the agent who causes the external effects and the one who is subjected to those effects, it is possible to include in the same apparatus capital theory, optimal allocation of risks, transportation costs and public goods. In particular, the formal equivalence between the static allocation mechanism and the intertemporal allocation mechanism under uncertainty is surely astonishing as it implies that uncertainty does not matter. 'But the insight is valuable. Arrow and Debreu produced a rigorous, consistent general theory of markets under uncertainty which inherits the most important properties of markets without uncertainty. In doing so, they forced us to clarify what is intrinsically different about uncertainty.'[66] It is thank to this clarity that economists realize that in that model markets for present and future commodities are concentrated in one single instant of time, even in presence of rational expectations, and so the model make money, insurances and financial instruments have no role to coordinate the use of resources either in time or state of nature.

[63]Debreu (1991, p.3).
[64]Majumdar and Radner (2008, p.1).
[65]Arrow and Hahn (1971, pp.VI-VII) state that, in order to criticise Arrow-Debreu model, 'it is not sufficient to assert that, while it is possible to invent a world in which the claims made on behalf of the "invisible hand" are true, these claims fail in the actual world. (...) In attempting to answer the question "Could be true?" we learn a good deal about why it might not be true.'
[66]Chichilnisky (2010, p.77).

A more general setting involving uncertainty is represented by the GE models with incomplete financial markets (GEI-models) which assume the reopening of spot markets, and therefore a sequence of market systems and allocations on time, and the possibility that agents cannot transfer income in all states of nature or in all the dates because of an insufficient number of financial assets. In this framework the first welfare theorem does not hold true anymore as almost all equilibria are inefficient. Then economic policies can produce positive effects and that has urged economic research to elaborate the important concept of Pareto Improvement.[67] Moreover, such models have suggested the need to encompass in a satisfactory theory the presence of restrictions on the participation in financial markets, the presence of collaterals and the possibility of default and bankruptcy, the issues of asymmetric information and moral hazard. Besides, limitations of rationality seem more compelling to be analysed in these models.[68]

In the nineties, the Arrow-Debreu framework was generalized also to infinite periods of time encompassing the absence of a terminal date for economic activity (judged not completely reasonable in presence of capital goods) and the coexistence of different generations (the Samuelsonian overlapping generation models[69]), where fiscal and monetary policies can play an important role. This kind of extensions[70] raised doubts, inter alia, on the standard role of prices as in these models it is not always possible to solve the maximization problems of the economic agents.[71] Some authors tried to relax the assumptions of a complete system of prices and rational expectations. That caused to take into serious consideration the expectation process and the information structure of the agents. It is thanks to Radner that 'by a simple reinterpretation of production possibility set and consumption possibility set, we can extend the theory of the Arrow-Debreu economy to allow for differences in information among the economic agents.'[72]

Another recent significant approach to uncertainty is the GE model proposed by Chichilnisky.[73] It is known that the classical model cannot encompass catastrophic risks such as climate warming, extinction of a species, financial crisis and so on, that is, 'rare events with major effects.'[74] That is due to the traditional axioms of choice under uncertainty which allow to represent the individual ordering of preferences via a Von Neumann-Morgenstern utility function. Chichilnisky's axioms permit to generalize the old apparatus and establish important conceptual connections in a framework where it is impossible to learn through experiments because of the presence of some risks producing irreversible effects on a large scale.

As regards macroeconomics, we agree with Kölmann that GE theory was useful to tidy-up the 'unfortunate terminological choices and conceptual vagaries' of Keynes' *General Theory* as 'those applying the framework of general equilibrium to the reconstruction of Keynes could in turn profit from the more conceptual work of Debreu and others.'[75] In fact, statements as the ones by Keynes surely cast light on economic relations both among concepts already present in the economic tradition and even new ones. Those deep intuitions suggested bases for the construction of sophisticated economic theories. But in their own they are not unambiguous statements and mathematical formalism contributed to clarify their meaning. We cannot even ignore Lucas' conceptual contribution referring to the Philips Curve, i.e., the statistical "trade-off" between the rate of unemployment and the inflation rate whose statistical evidence suggested the presence of involuntary unemployment and the rightness of

[67]Geanakoplos and Polemarchakis (1986), Citanna, Kajii and Villanacci (1998).

[68]It is not reasonable to require anymore 'that the agents possess capabilities of imagination and calculation that exceed reality by many orders of magnitude.' (Majumdar and Radner 2008, p.3).

[69]Hausman (1992, ch. 8).

[70]Aliprantis, Brown and Burkinshaw (1990).

[71]Similar doubts were raised even by the Edgeworth box, a useful device in textbooks to introduce some issues of general equilibrium theory to students. In fact, in that model equilibria guided by prices are a strict subset of the contract curve which is the set of outcomes of the rational agents trade in ideal and abstract conditions (absence of transaction and information costs etc.).

[72]Majumdar and Radner (2008, p.3).

[73]See, for instance, Chichilnisky (2009, 2010).

[74]Chichilnisky (2009, p.130).

[75]Kölmann (2008, p.590). Hahn (2005, p.11) also says: 'the General Theory is an attempt to answer that question (i.e., involuntary unemployment, authors note) but the question would not have been asked in this form if it had not for Neo-classical Economics.'

Keynesian policies. Lucas (1972) showed how a similar result could arise in a perfect competitive GE model with uncertainty on which active policies have no role.[76]

All the considered examples show only a small part of the theoretical contributions of GE theory to the economic theory. We strongly believe that such contributions are far from being inessential and it is not possible to deny the truth of the following words of Sen (1991, p.70), a great intellectual who is not certainly sympathetic with GE view:

> 'If general equilibrium theory has been at least in some respects valuable (as, I tend to believe, it has been), this is not because its results have been anything verified, nor because the theory is free from serious distortions of reality. The claims lie elsewhere - in the insights that this theory have provided about how a class of crucial economic interrelations work. That claim is not easy to dismiss, given the way general equilibrium relations are invoked - often implicitly - even in very practical debates about prices and markets.'

Certainly, we are left with the problem to explain why some GE models are preferred to others and become more influential. In other words, an analysis of the criteria not referring to the empirical content used by the economists to assess and compare GE models is needed. In what follows, we try to enlighten some aspect of that very difficult issue.

First of all, as GE models are mathematical models, some of the factors economists use to assess them comes just from mathematics. In fact, having a rigorous axiomatic structure and being coherent under a logical viewpoint are very important properties GE theorists require the models to satisfy. As Debreu (1991, p.2) explains:

> 'being denied a sufficiently secure empirical base, economic theory has had to adhere to the rules of logical discourse and must renounce the facility of internal inconsistency.'

The influence of the axiomatic approach characterizing GE theory has been strong in economics. Indeed, 'it is taken for granted that a model is not properly defined unless it has been proved to be logically consistent. Much of the clamour for "microeconomic foundations of macroeconomics", for example, is a desire to see an axiomatic clarity similar to the Arrow-Debreu model applied to other areas of economics.'[77] Among other things, logical internal coherence of GE models is related to the capability of the model to guarantee the existence of an equilibrium for the whole set, or at least a large set of, the exogenous parameters defining the economy under consideration. In other words, one of the main coherence testes is related to the possibility to prove in the model a general enough 'existence theorem'. In fact, the model works as a possible mathematical world where starting from certain properties of equilibria we infer that these same properties (or better, suitable interpretation of them) might hold in the actual world. It is then important that the objects we are speaking about in the model exist in a mathematical sense.

Of course, the idea that in economics internal logical coherence can always compensate the inadequacy of empirical evidence is pernicious and we think that no economist really believes it. Indeed, economists judge very important also forms of coherence of different nature other than mathematical. For instance, replying to the question 'What criteria would you use to evaluate the soundness of an alternative theory?', Arrow (1987, pp.69-74) emphasizes what is significant for him beyond empirical content:

> 'Persuasiveness. Does it correspond to our understanding of the economic world? I think it is foolish to say that we rely on hard empirical evidence completely. A very important part of it is just our perception of the economic world. If you find a new concept, the question is does it illuminate your perception? Do you feel you understand what is going on in everyday life? Of course, whether it fits empirical and other tests is also important.'

Among the values to assess a GE model and decide whether it 'illuminates perceptions' there is surely the 'realism' of model assumptions, that is, whether those assumptions are a suitable interpretation of the actual world. Koopmans emphasizes the role of realism too as he looks upon economic theory 'as a

[76]See also the remarkable exposition in Boumans (1998, pp.84-90).
[77]Geanakoplos (2008, p.5).

sequence of conceptual models that seeks to express in simplified form different aspects of an always complicated reality. At first these aspects are formalized as much as feasible in isolation, then in combinations of increasing realism.'[78] In fact, GE theorists seem to be guided in building their models by the desire to encompass in their model not yet explored combinations of economic concepts described by 'realistic' enough and not tested mathematical assumptions in order to find out new links among the concepts under consideration.

But this point needs to be deepened. In fact, mostly for mathematical reasons, each GE model is characterized by a large number of very unrealistic assumptions (infinite divisibility of commodities, continuity and convexity of agents' preferences, perfect foresight, continuum of agents)[79] and most of them are in fact accepted because of mathematical convenience (for instance, to have convex preferences is crucial to get existence of equilibria). Moreover, we could state that there is no reason to assume that we can improve a model necessarily introducing de-idealizing modifications of the assumptions[80] and, in fact, it is not the way followed by GE theorists. Therefore, even though 'realism' of assumptions is fundamental in assessing a model, that assessment is always done 'cum grano salis', and two models are rarely compared looking at their single assumptions but looking at them as a whole.

As already stated, in our opinion, the conceptual work of GE theorists in clarifying new and old economic concepts and relations, creating new effective patterns of reasoning and providing enlightening insight of the economic world is valuable and nowadays still deserves to be carried on. Moreover, in the light of the above observations, we can strengthen our viewpoint rebutting the criticisms to GE theory proposed at the beginning of the section. In fact, those criticisms simply ignore the importance economists give to the capability of GE models to solve problems of conceptual type.

For sure, Blaug, Backhouse and McCloskey are definitely right stating that Arrow-Debreu model has no direct contact with reality. Yet, it is not clear how we can figure Adam Smith's proposition be connected with reality. In fact, notions like equilibrium, selfish behaviour, social welfare and efficiency are not unambiguous at all. How can economists discuss about the validity of Adam Smith's intuition without finding a previous agreement on the fundamental objects involved? How can we figure to test the validity of that proposition? The different formalizations proposed by GE theorists have instead provided common frameworks to discuss on the topic and, despite the already stressed lack of direct empirical content of most GE models, GE theory has provided the formal language to build many of the empirical theories in economics. In other words, GE theory has shown to have sort of an 'indirect' empirical content. Moreover, McCloskey's criticism about the qualitative meaning of GE theorems does not consider their actual cognitive role. In particular, she does not realize the importance that economists attach to the internal logical coherence in order to better justify the possibility to learn from models, and to what extent such a coherence relies on existence theorems.

We conclude by analysing a further criticism moved to GE theory. As well-known, in the 1970s theorists reached quite strong, and almost entirely negative, conclusions about both the uniqueness and the stability of equilibria in the Arrow-Debreu exchange economy model.[81] Moving from those results and from the fact that, as pointed out by Ingrao and Israel in their historical analysis of GE theory,[82] those properties of equilibria have always been considered important since the beginning of neoclassical economics, Ackerman (2002, pp.132-133) states:

> 'General equilibrium is still dead. Exactly 100 years after the 1874 publication of Walras' most important work, the [Sonnenschein-Mantel-Debreu] theorem proved that there was no hope of showing that stability is a generic property of market systems. More than a quarter-century of additional research has found no way to sneak around this result, no reason to declare instability an improbable event. These negative findings should challenge the foundations of economic theory.'

[78]Koopmans (1957, p.142).
[79]Hindricks (2008, p.335) considering a model with unrealistic assumptions speaks of false models.
[80]It seems that a kind of second best problem arises in philosophy of science. On the topic, see Morgan and Knuuttila (2012, pp.14-18), Frigg and Hartmann (2006, p.14). Hindriks (2008, p.358) states that 'the process of relaxing unrealistic assumptions did not serve to get from a falsehood to a truth.'
[81]Sonnenschein (1973), Debreu (1974), Mantel (1974).
[82]Ingrao and Israel (1990).

First of all, we note that, under a logical viewpoint, the quoted results certainly do not imply that for every GE model and every conceivable price adjustment mechanism, uniqueness and stability of equilibria fail. That could only suggest the need to search for different models and alternative mechanisms to determine prices, not necessarily to abandon the neoclassical paradigm. However, regardless of that and according to our rhetorical approach to economics, we believe that GE theory has never been dead as GE theorists have never had reasons to make it die. Indeed, despite the above discussed theoretical problems about uniqueness and stability of equilibria for the Arrow-Debreu model and probably for most GE models, virtues of GE theory still keep surviving. As already said, GE models are arguments describing possible mathematical worlds that are built in order to make explicit and clear certain relations among significant economic concepts (like, for instance, equilibrium, efficiency and even uniqueness and stability) and need certain coherence properties to be considered convincing arguments. Perhaps, proving existence, uniqueness and stability of equilibria might contribute to make a model be perceived as more convincing,[83] but having all these results together is not strictly needed to make a model useful to learn. And, indeed, GE theorists do not believe so as proved by the enormous amount of literature not involving uniqueness and stability issues.


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## REFERENCES


Ackerman, F. (2002), 'Still dead after all these years: interpreting the failure of general equilibrium theory', *Journal of Economic Methodology*, 9,119-139.

Aliprantis, C.D, Brown, D.J., and Burkinshaw, O., (1990), *Existence and Optimality of Competitive Equilibria*, New York: Springer.

Arrow, K., and Debreu, G. (1954), 'Existence of an Equilibrium for a Competitive Economy', *Econometrica*, 22, 265-290.

Arrow, J.K, and Hahn, F. (1971), *General Competitive Analysis*, Edimburgh: Oliver&Boyd.

Backhouse, R.E. (1998), 'If Mathematics is informal, then perhaps we should accept that Economics must be informal too', *The Economic Journal*, 108, 1848-1858.

Barsdley, N., Cubitt, R., Loomes, G., Moffatt, P., Starmer, C., and Sudgen, R. (2010), *Experimental Economics*, *Rethinking the Rules*, Princeton: Princeton University Press.

Beker, V. (2005), 'Is Economics a Science? A discussion of some methodological issues', available at SSRN: http://ssrn.com/abstract=839307.

Blaug, M. (1992), *The methodology of economics or how economists explain (2nd Edition),* Cambridge: Cambridge University Press.

Boumans, M. (1998), 'Built-in justification', in *Models as Mediators. Perspectives on Natural and Social Science,* eds. M. Morgan and M. Morrison, Cambridge: Cambridge University Press,pp.66-98.

Brown, J.R., and Fehige, Y., 'Thought Experiments', *Stanford Encyclopedia of Philosophy*.

Bukhard, H., and Herr, A.M. (2005), *Dynamic general equilibrium modeling: computational methods and applicat*ions, Berlin: Springer.

Chichilnisky, G. (2009), 'Catastrophic risks', *International Journal of Green Economics*, 3, 130-141.

Chichilnisky, G. (2010), 'Arrow Kenneth', in *Wiley: Encyclopedia of Quantitative Finance*, ed. R. Cont, Chichester: John Wiley and Sons.

Citanna, A., Kajii, A., and Villanacci, A. (1998), 'Constrained suboptimality in incomplete markets: A general approach and two applications', *Economic Theory*, 11, 495-521.

Cross, J.C. (1980), 'Some Comments on the Papers by Kagel and Battalio and by Smith', in *The Evaluation of Econometric Models,* eds. J. Kmenta and J.B. Ramsey, New York: Academic Press.

Davies, D. (2007), 'Thought Experiments and Fictional Narratives', *Croatian Journal Of Philosophy*, VII, 29-45.

Davis, P., and Hersh, R. (1981). *The Mathematical Experience*, Boston: Birkhäuser.

Debreu, G. (1974), 'Excess demand functions', *Journal of Mathematical Economics*, 1, 15-21.

Debreu, G. (1991). 'The mathematization of economic theory', *American Economic Review*, 81, 1-7.

Du Plessis, S. (2009), 'The Miracle of The Septuagint and The Promise of Data Mining in Economics', in *The Oxford Handbook of Philosophy in Economics,* eds. H. Kincaid and D Ross, Oxford: Oxford University Press.

Frigg, R. (2006), Scientific Representation and the Semantic View of Theories, *Theoria*, 55, 49-65.

Frigg, R., and Hartmann, S. (2006), 'Models in Science', *Stanford Encyclopedia of Philosophy*.

Frisch, R. (1933), 'Editorial', *Econometrica*, 1, 1-4.


[83]We stress, however, that not all economists think that uniqueness of equilibria is really a desirable property of a model. In particular, a continuum of equilibria is sometimes seen as a more realistic feature of the model by those economists who believe that in actual world there is room for guiding the economy towards the choice of a particular equilibrium with desirable social properties without any disruptive change.

Garnett, R.F. Jr. (2004), 'Rhetoric and Postmodernism in Economics', in *Elgar Companion in Economics and Philosophy,* eds. J. Davis, A. Marciano and J. Runde, Cheltenham: Edward Elgar, 231-259.
Geanakoplos, J. (2008), 'Arrow-Debreu model of general equilibrium', in *The New Palgrave Dictionary of Economics. Second Edition*, eds. N.S. Durlauf and L.E. Blume, Palgrave Macmillan.
Geanakoplos, J., and Polemarchakis, H.M. (1986), 'Existence Regularity and Constrained Suboptimality of Competitive Allocations when the Asset Markets is Incomplete', in *Essays in Honour of J.K. Arrow,* vol. 3, eds. W.P. Heller, D. Starrett and R. Starr, Cambridge: Cambrige University Press.
Geweke, J., Horowitz, J., and Pesaran, H. (2008), 'Econometrics', in *The New Palgrave Dictionary of Economics. Second Edition*, eds. N.S. Durlauf and L.E. Blume, Palgrave Macmillan.
Gibbard, A., and Varian, H. (1978), 'Economic Models', *The Journal of Philosophy*, 75, 664-677.
Gross, A.G. (1990), *The Rhetoric of Science*, Cambridge: Harvard University Press.
Grüne-Yanoff, T. (2009), 'Learning from minimal models', *Erkenntnis*, 70, 81-99.
Guala, F. (2005), *The Methodology of Experimental Economics*, Cambridge: Cambridge University Press.
Guidi, V. (1999), 'Debreu: Assiomatizzazione e equilibrio economico generale', *Studi Economici*, 69, 5-32.
Hahn, F. (1987), 'Review of McCloskey, The rethoric of economics', *Journal of Economic Literature*, 25, 110-111.
Hahn, F. (2005), 'General Equilibrium for Intellectual Historians', *Storia del Pensiero Economico*, 2, 5-12.
Handler, E.W. (1982), 'The Evolution of Economic Theories: A Formal Approach', *Erkenntnis,* 18, 65-96.
Hausman, D.M. (1981), 'Are General Equilibrium Theories Explanatory', in *Philosophy in Economics,* ed. J.C. Pitt, Boston: D. Reidel Publishing Company.
Hausman, D.M. (1992), *The inexact and separate science of economics*. Cambridge: Cambridge University Press.
Hausman, D.M. (2008), 'Philosophy of Economics', *Stanford Encyclopedia of Philosophy*.
Hendry, D.F. (1980), 'Econometrics-Alchemy or Science?', *Economica*, 47, 387-406.
Hendry, D.F., and Morgan, M.S. (1995), *The Foundations of Econometric Analysis*. Cambridge: Cambridge university Press.
Hindriks, F. (2008), 'False Models as Explanatory Engines', *Philosophy of the Social Sciences*, 38, 334-360.
Ingrao, B., and Israel, G. (Ian McGilvray, trans.) (1990), *The Invisible Hand: Economic Equilibrium in the History of Science,* Cambridge, Mass, and London: MIT Press.
Kehoe, T.J., Srinivasan, T.N., and Whalley, J. (2005), *Frontiers in Applied general equilibrium modeling*, Cambridge: Cambridge University Press.
Keynes, J.M. (1940), 'On a method of statistical business-cycle research: a comment' ,*Economic Journal*, 50, 154-156.
Keynes, J.M. (1973), 'Letter to Harrod July 4, 1938', in *The General Theory and After, Defence and Development, Collected Wirtings, vol XIV,* ed. D. Moggridge, Cambdrige: Cambridge University Press.
Klamer, A. (2001), 'Making sense of economists: from falsification to rhetoric and beyond', *Journal of Economic Methodology*, 8, 69-75.
Klein, E. (1998), *Economic Theories and their relational structures: a model-theoretic characterization*, London: MacMillan.
Köllmann, C. (2008), 'General Equilibrium Theory and the Rationality of Economics', *Analyse&Kritik,* 30, 575-599.
Koopmans, T.C. (1957), *Three Essays on the State of Economic Science*, New York. Mc Graw Hill.
Kornai, J. (1971), *Anti-Equilibrium*, Amsterdam: North Holland.
Kreps, D. (1997), 'Economics: The Current Position', *Daedalus*, 126, 59-85.
Krugman, P. (1993), 'How I work', *American Economist*, 37, 25-31.
Kuhn, T.S (1977), 'A Function for Thought Experiments', in *The Essential Tension*, ed. T.S. Kuhn, Chicago: University of Chicago Press.
Kuhn, T.S. (1989), 'Possible Worlds in History of Science', in *Possible Worlds in Humanities, Arts and Sciences, Proceedings of Nobel Symposium*, ed. S. Turen, Research in Text Theory, vol.14, Berlin: Walter De Gruyt & Co.
Laudan, L. (1975), *Progress and its Problems. Towards a theory of Scientific Growth*, Berkeley: University of California Press.
Leamer, E., and Leonard, H. (1983), 'Reporting the Fragility of Regression Estimates', *The Review of Economics and Statistics*, 65, 306-317.
Levitt, S.D., and List, J.A. (2007), 'What Do Laboratory Experiments Measuring Social Preferences Reveal About the Real World?', *Journal of Economic Perspectives*, 2, 153–174.
Lucas, R.E. (1972), 'Expectations and the Neutrality of Money', *Journal of Economic Theory*, 4, 103-124.
Majumdar, M., and Radner, R. (2008), 'Uncertainty and general equilibrium', in *The New Palgrave Dictionary of Economics. Second Edition*, eds. N.S. Durlauf and L.E. Blume, Palgrave Macmillan.
Mäki, U. (2008), 'Philosophy of economics', in *Routledge Companion to the Philosophy of Science,* eds. M. Curd and S. Psillos, London: Routledge.
Mantel, R. (1974), 'On the characterization of aggregate excess demand', *Journal ofEconomic Theory,* 7, 348-353.
Marchionni, C. (2006), 'Contrastive explanation and unrealistic models: The case of the new economic geography', *Journal of Economic Methodology*, 13, 425-446.
McCloskey, D.N. (1983), 'The rhetoric of economics', *Journal of Economic Literature*, 31, 434-461.
McCloskey, D.N. (1991), 'Has formalization in economics gone too far?', *Methodus*, 3, 6-16.
McCloskey, D.N. (1994), 'How economists persuade', *Journal of Economic ;Methodology*, 1, 15-32.
McCloskey, D.N. (1998), *The Rhetoric of Economics* (2nd edn.), Madison: University of Wisconsin Press.
McKinney, N.C., and Roth, A.E. (2010), 'Experimental Economics', in *Social Science Encyclopedia,* eds. A. Kuper and J. Kuper, London: Routledge.

Mongin, P.(2002), 'La conception deductive de l'explication scientifique et l'economie', *Information sur les Sciences Sociales-Social Science Information***,** 41, 139-165.
Montesano, A. (1995), 'Approccio descrittivo-esplicativo ed approccio assiomatico nella teoria dell'equilibrio economico generale', in *Economia al Bivio*, ed. G. Sabattini, Milano: Franco Angeli.
Morgan, M.S. (1990), *The History of Econometric Ideas*. Cambridge: Cambridge university Press.
Morgan, M.S., and Morrison, M. (eds.) (1999), *Models as Mediators, Perspectives on Natural and Social Science*, Cambridge: Cambridge University Press.
Morgan, M., and Knuuttila, T. (2012), 'Models and Modelling in Economics', in *Handbook of the Philosophy of Economics,* ed. U. Mäki (a volume of *Handbook of the Philosophy of Science*, eds. D. Gabbay, P. Thargard and J. Woods).
Nowak, L. (1992), 'The Idealizational Approach to Science', in *Idealization III: Approximation and Truth*, eds. J.Brzeziński and L. Nowak, Amsterdam: Rodopi, 9-63.
Pera, M. (1994), *The Discourses of Science*, Chicago: Chicago University Press.
Rappaport, S. (1998), *Models and reality in Economics*, Cheltenham: Edward Elgar.
Rosenberg, A. (1983), 'If Economics Isn't Science, What Is It?', *Philosophical Forum*, 14, 296-314.
Rubinstein, A. (2001), 'A theorist's view of experiments', *European Economic Review*, 45, 615-628.
Ryan, M.L. (1991), 'Possible Worlds and Accessibility Relations: A Semantic Typology of Fiction', *Poetics Today*, 12, 553-576.
Salmon, W.(1990), *Four decades of scientific explanation*, Minneapolis: University of Minnesota Press.
Sen, A. (1991), 'Economic methodology: heterogeneity and Relevance', *Methodus*, 3, 67-80.
Shoven J.B., and Whalley, J. (1992), *Applying General Equilibrium*, Cambridge: Cambridge University Press.
Sonnenschein, H. (1973), 'Market excess Demand Functions', *Econometrica*, 40, 549-563.
Sugden, R. (2000), 'Credible worlds: the status of theoretical models in economics', *Journal of Economic Methodology*, 7, 1-31.
Summers, L.H. (1991), 'The Scientific Illusion in Empirical Macroeconomics', *Scandinavian Journal of Economics*, 93, 129-148.
Suppe, F. (ed.) (1977), *The Structure of Scientific Theories*, Urbana: University of Illinois Press.
Suppe, F. (1989), *The Semantic Conception of Theories and Scientific Realism*, Urbana: University of Illinois Press.
Thaler, R. (1987), 'The psychology of choice and the assumptions of economics', in *Laboratory experimentation in economics, six points of view,* ed. A.E. Roth, Cambridge: Cambridge University Press.
Varian, H. (1989), 'What Use is Economic Theory?', avalaible at http://www.ischool.berkeley.edu/people/faculty/halvarian
Vosnadiou, S. (2002), 'Mental Models in Conceptual Development', in *Model-Based reasoning: Science, Technology, Values*, eds. L. Magnani and N. Nersessian, New York: Kluwer Academic Press, available at http://www.cs.phs.uoa.gr/en/staff/vosniadou.html
Ylikoski, P. (2003), 'Thought Experiments in science studies', *Philosophica*, 72, 35-59.