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Are economists "rational"?
Models in economics: parables, fables, fictions, stories.

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“We essentially play with toys called models.”
(Rubinstein 2006: 865).

Abstract

Mathematical modeling is the dominant linguistic instrument in contemporary economics, and the canon to assess the scientific character of theories. Since the 1940s, Olympian rationality, to use Simon's phrasing, was the prevailing assumption in theoretical models in both microeconomics and macroeconomics, although the validity of the assumption is questioned in ample literature on bounded rationality and behavioural economics. In current research practice in the discipline, mathematical models are conceived as born out of the economist's imagination, and flexibly adaptable for reasons of interpretation, or technical convenience. Each specific model is the free, arbitrary formalization of some fictional Wonderland that may be broadly interpreted as linked to economic subjects and events. The authors, who introduce a model into theory, conceive the creation of their imagination as open to multiple adaptations and interpretations, within the logic of its basic formal structure. Today, in economics mathematical modeling has for ever lost the pretence of truth; the criteria used to associate models to the reading of economic events are loose, and freely adjusted by each scholar according to his or her own epistemology. No unique canon exists to validate or reject the significance of a model. A. Rubinstein, questioning the meaning of his work as a game theorists, wrote: “We essentially play with toys called models.”(Rubinstein 2006: 865). Even the criteria of empirical validation have lost appeal; academic communities validate models by the presumed robustness of their mathematical results, by their novelty, or because of fashion, ideology, power. New currents in contemporary economic methodology theorised the current practice; mathematical models in economics are theorized as being fables, fictions, stories, thought experiments, loose analogies providing insights. The paper questions the paradoxical coexistence in economics of the stringent requirement of agents' rationality versus fuzzy ideas of models as fictional narrations, adopted both in methodology and in research practice.
1. Mathematical language and the quest for scientific truth.

In 1844, writing on commercial crises, J.S. Mill noted that "men miscalculate" and "the passion of gain (as in gamblers)" may "over-ride" their calculations (Mill 1844[1967]). Mill attributed a myopic vision to traders in evaluating improvident speculations, and he suggested that young persons being employed in trade should be properly trained, to better understand the fundamentals in the trend of prices. His approach was, in this regard, both elitist and enlightened. The philosopher and social scholar does not share the myopic vision of the merchant, as far as the general overview of economic life is concerned. Smith had expressed a similar judgement on the narrowly focused mind of the common trader at the core of its invisible hand passage, where he denied that ordinary businessmen should express sound judgements on the welfare of society.

In the 19th century, the great social scholars, who pursued the mathematicalization of economics aimed at conquering scientific truth. Mathematical language was an instrument of rigorous arguing to discover and express the inner laws of markets. They conceived the necessary abstractions and simplifications that mathematical modelling imposed in economic theory, as the quest for ideal schemata of market phenomena. The ideal relations portrayed in mathematical models of markets could abstract from frictions, or even be conceived in their frictionless purity as normative models to which approximate market realities. Within a variety of epistemological approaches, Cournot, Walras, Jevons, or Pareto, to quote just some outstanding pioneers of mathematical modeling in economics since the early 19th century to the first quarter of the 20th century, pursued the quest for scientific truth.

Cournot was crystal clear that the abstract idea of value in exchange in the mathematical theory of wealth should not be too far from "the actual objects" to which it should be applied, if it should not become an "idle speculation". 1 In 1838 in his Recherches, and again in later works, he argued that the abstract idea of wealth could be studied as a mathematical object, maintaining that contemporary societies were moving towards increasing rationalization in exchange relations. In contemporary societies, real markets were moving towards the reduction of frictions by progressive rational measurement. Cournot excluded utility from his mathematical reasoning because he saw the idea as not amenable to precise measurement. 3

Walras, who staunchly argued that mathematical reasoning is the "rational method", not empiricism, proposed the scientific procedure to start from the real types (types réels) of

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1 "The Theory of Wealth, according to the idea we are trying to give, would doubtless only be an idle speculation, if the abstract idea of wealth or value in exchange, on which it is founded, were too far from corresponding with the actual objects which make up wealth in the existing social status. The same would be true of hydrostatics if the character of ordinary fluids should be too far removed from the hypothesis of perfect fluidity. However, as we have already said, the influence of a progressive civilization constantly tends to bring actual and variable relations nearer and nearer to the absolute relation, which we attain to from abstract considerations. In such matters everything becomes more and more easily valued, and consequently more easily measured. The steps towards finding a market resolve themselves into brokerage, losses of time into discounts, chances of loss into insurance charges, and so on. The progress of the gregarious tendency and of the institutions related to it, and the modifications which have taken place in our civil institutions, all cooperate towards this mobility, which we would neither apologize for nor detract from, but on which the application of theory to social facts is founded." (Cournot 1838 [1971]: 17).

2 "The progress of nations in the commercial system is a fact in the face of which all discussion of its desirability becomes idle; our part is to observe, and not to criticize, the irresistible laws of nature. Whoever can measure, calculate, and systematize, ultimately becomes the object of measurement, calculation, and system. Wherever fixed relations can replace indeterminate, the substitution finally takes place. It is thus that the sciences and all human institutions are organized." (Cournot 1838 [1971]: 10).

3 "The abstract idea of wealth or of value in exchange, a definite idea, and consequently susceptible of rigorous treatment in combinations, must be carefully distinguished from the accessory ideas of utility, scarcity, and suitability to the needs and enjoyments of mankind, which the word wealth still suggests in common speech. These ideas are variable, and by nature indeterminate, and consequently ill suited for the foundation of a scientific theory." (Cournot 1838 [1971]: 10).

4 "La methode mathématique n'est pas la methode experimentale, c'est la méthode rationelle."(Walras 1900: ).
It is interesting to read the letter that Pierre Duhem sent to an unknown correspondent on the 21rst of September 1901, about the dissertation written by Albert Aupetit. Aupetit (Walras's follower and friend) had sent it to the great physicist in the hope of a positive evaluation. In this letter Duhem, although recognising that the mathematical expression of a scientific theory reduces the complexities of reality into an abstract and summary representation, called attention to the necessary "conformity" between the consequences of the assumptions in theory and empirical evidence that Aupetit had underlined.

M. Aupetit indique clairement que la légitimité de ces hypothèses est établie seulement par la conformité entre leurs conséquences et l'expérience; de plus il sait et dit nettement que cette concordance ne peut pas être parfaite, que la théorie mathématique ne donne de la réalité concrète et complexe qu'une représentation abstraite et abrégée." (Letter 1665, 21rst of September 1901, pp. 338).

The hypotheses of which Duhem spoke were those, which permit to substitute a specific function (or functional form) in the mathematical representation of the phenomenon that the scholar aims at representing in mathematical language. It is the procedure that permits to substitute an "algebraic combination" to a "subjective element" or a "datum of perception". As Duhem deals with it in the letter of which above, the reference to empirical evidence is the ultimate check of the appropriateness of the relevant mathematical theory, although the mathematical representation is conceived as abstract and summary.

Pareto and Marshall, although writing from diverse epistemological perspectives, took great care to explore and clarify the connections between mathematical modeling and economic reality. In 1896-97[1964], in his first treatise, Pareto declared that his aim was "(...) to offer an outline of economic science considered as a natural science and founded solely on facts." (Pareto 1896-97[1964], I: III). He defended the principle of successive approximations, and all life long, he pursued the search for approaching empirical evidence, going from pure and applied economics into sociology to capture the complexities of social life. He was thorn by the double, strict commitment to pure science in mathematical form, and the search for the comprehensive understanding of men in social life that only the synthesis offered by sociology could offer. He was perfectly aware of the

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5 "Pure political economy corresponds to rational mechanics; it takes abstraction to the extreme, studying a skeleton of economic operations; men are reduced to simple hedonistic molecules, as in rational mechanics solid bodies are reduced to material points. Applied economics, or, rather, the various types of applied economics flesh out the skeleton a little; man remains homo oeconomicus, but no longer reduced to a hedonistic molecule; account is taken of certain of his affections, prejudices, etc. Carrying through this work of synthesis, applied economics could eventually take into consideration all the characteristics of the real man and become sociology, just as applied mechanics could eventually
unrealistic assumptions in the image of the *homo oeconomicus*, in pure economics. Although defending their role as a first step in theoretical research, he aimed at going beyond their constraints, venturing into applied economics and sociology. 

Interdependence is to be considered not only in the case of economic phenomena but also for social phenomena, be they economic or sociological. There is hardly any real problem that can be said to be purely economic, and not both economic and sociological at the same time. Indeed, very often it is the sociological element that prevails over the economic, as in the issue of free trade or protection, many monetary matters and almost all the problems of taxes and such like. Without leaving the experimental field, we can separate matters like barter and production, and devise separate theories on them, but if we mean to remain in the realm of the concrete phenomena we must then produce a synthesis of the results obtained. Similarly, we are not abandoning the experimental field when we study the economic and sociological parts separately, but we would be if we did not then recombine those parts to obtain that whole that alone has experimental existence, and that we have arbitrarily broken down into abstract parts simply to facilitate study. (Pareto 1918 [1982]: 636).

As for Alfred Marshall, his cautious attitude towards mathematization is well known, though it is controversial whether it changed through the course of his life. The position he expressed in a letter to Edgeworth is very well known. Let us remind here what he wrote in another well known letter at the close of the 19th century.

"The fact is I am the dull mean man, who holds Economics to be an organic whole, & has as little respect for pure theory (otherwise than as a branch of mathematics or the science of numbers), as for that crude collection & interpretation of facts without the become physics, and even chemistry or physiology. (...) And since it seems useful to give a name to the science that achieves this synthesis, we may, if there are no objections, call it sociology(…).(Pareto 1899[1980]: 170).

"We will study the many logical, repeated actions which men perform to procure the things which satisfy their tastes. (...) Moreover, we will simplify the problem still more by assuming that the subjective fact conforms perfectly to the objective fact. (...) Thus by considering only one part of man’s actions and, in addition, by assigning certain characteristics to them, we have simplified the problem enormously. The study of these actions makes up the subject of political economy." (Pareto 1909[1971], chap. III: 103).

"Pure political economy corresponds to rational mechanics; it takes abstraction to the extreme, studying a skeleton of economic operations; men are reduced to simple hedonistic molecules, as in rational mechanics solid bodies are reduced to material points. Applied economics, or, rather, the various types of applied economics flesh out the skeleton a little; man remains *homo oeconomicus*, but no longer reduced to a hedonistic molecule; account is taken of certain of his affections, prejudices, etc. Carrying through this work of synthesis, applied economics could eventually take into consideration all the characteristics of the real man and become sociology, just as applied mechanics could eventually become physics, and even chemistry or physiology. (...) In the present case applied economics should find its limit where the man it considers is no longer, or practically no longer, a *homo oeconomicus*. To go any further, help is required of other sciences: ethics, the science of religions, politics, etc. And since it seems useful to give a name to the science that achieves this synthesis, we may, if there are no objections, call it sociology(…)."(Pareto 1899[1980]: 170).

"In my view ‘Theory’ is essential. No one gets any real grip of economic problems unless he will work at it. But I conceive no more calamitous notion than that abstract, or general, or ‘theoretical’ economics was economics ‘proper’. It seems to me an essential but a very small part of economics proper: and by itself sometimes even - well, not a very good occupation of time. The key-note of my Plea is that the work of the economist is ‘to disentangle the interwoven effects of complex causes’; and that for this, general reasoning is essential, but a wide and thorough study of facts is equally essential, and that a combination of the two sides of the work is *alone* economics proper. Economic theory is, in my opinion, as mischievous an impostor when it claims to be economics proper as is mere crude unanalysed history." (Letter to Edgeworth, the 28th of August 1902 in A.C. Pigou ed. 1925: 437).
aid of high analysis which sometimes claims to be a part of economic history. (Letter to W.A.S. Hewins, the 12th of October 1899 quoted in Coase 1975[1993]: 413).

Irving Fisher, one of the great pioneers of mathematical economics and econometrics, and the presumed representative of neoclassical theory, certainly not aiming at subverting the rational foundations of neoclassical economics, devoted a whole book to explain how common people, and even the majority of people involved in business, do not correctly understand the notion of the general price level and its movements (Fisher 1928). They are subject to money illusion, and to making serious mistakes in times of monetary instability. After failing to anticipate the collapse of stock prices in October 1929, he devoted the early 1930s to build anew a business cycle theory that could account for the severity of major recessions (Fisher 1933). In such theory, Fisher carefully distinguished the "tendencies", that is the economic links that theoretical analysis could isolate by abstract reasoning on business cycles, from the global explanation of each cycle in historical reconstruction. In his book Booms and Depressions, he was preoccupied with theoretical explanations that might fit the facts, and he devoted large part of the narration to the examination in historical perspective of what was going on in the U.S. economy (Fisher 1932).

All these scholars, each on his own methodological premises, shared the idea that economists as learned scholars could reach more enlightened intelligence in assessing the overall picture of the economy than the single trader in the ordinary business of economic life. They put effort in designing the appropriate policies to be undertaken, and did not refrain from taking full responsibility for the consequences they derived from their theoretical intelligence of market economies and economic events.

2. Pluralism and anarchy in contemporary economic theory.

In contemporary mathematical economics, the quest for scientific truth is almost dead. We may like or not this crude fact, but a crude fact it is. No scholar aims at discovering the laws of phenomena, as they were conceived in 19th or early 20th century theorising, according to one of the conceptions we summarized above, in the effort at the time scholars did to place mathematical economics on the same scientific podium as physics. It was a complex process of transformation with wide cultural roots, which we cannot discuss here, not even in summary9. Let us look just at the contemporary point of arrival.

Contemporary economic theory is going through a phase of fragmentation and anarchy. No family of mathematical models exists, which had been built with the ambition to offer a global vision of the working of markets. Mathematical models, and the accompanying interpretations, are fragmented, dealing with specific issues, or separate fields of research. Economic theory is composed by a set of specialized disciplines, each using specific procedures and linguistic codes. In 2004, Colander, Holt, and Rosser Jr. convincingly argued that the set of mainstream theories in the discipline, as they are validated or explored at the edge of research by outstanding academic communities, form a complex system of evolving ideas10. These scholars introduced the idea of a flexible, mobile core of innovative research "that signals the future direction of change in economics and how the profession eventually comes to be viewed and understood by its elite." (Colander, Holt, Rosser Jr. 2004: 486-487). They concluded that there is no more any unique, dominant paradigm of set ideas that might constitute a stable, invariant core of economic theory11.

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9 In his last, highly scholarly book, G. Israel surveyed the evolution of mathematical modeling in the natural sciences, much as in economics, with critical comments on open questions and epistemological turns (Israel 2015). For his previous assessment, see Israel 1996.


11 "For example, mainstream economists today such as William Baumol, George Akerlof, Thomas Schelling, Truman Bewley, and Paul Krugman, in important aspects of their thinking, are working outside of what is generally considered the orthodoxy of the profession. Yet, their ideas are widely accepted and discussed within the mainstream of economics.
The scholars, who build new theories at the edge, may be fully conscious of the pitfalls in some of the 'mainstream' mathematical models, and freely explore new directions of research.

Standard classifications also emphasize a fairly narrow orthodox core of the profession and convey a picture of all conventional economists accepting this core. The reality is more complicated; conventional economists often hold a variety of views simultaneously. If the variance of views increases, while the core remains relatively unchanged, the static characterization of the profession will not change, but its dynamic characterization will. (Colander, Holt, Rosser Jr. 2004: 486-487).

Notwithstanding the fluidity of the core, according to these scholars the "evolving set of ideas", is "loosely held together by its modeling approach to economic problems", that is by the shared reference to mathematical language as the language of economic theory par excellence. In a further joint comment they defined the mainstream in economics as a "complex, adaptive system" (Colander, Holt, Rosser Jr. 2007: 308). Amidst the variety of hypotheses, procedures and interpretations admitted within such fluid, adaptive system, validated by powerful academic communities, mathematical language is mandatory as the standard requirement of scientific approach in economics. Thus, the empirical credibility of the theory of rational choice as inspired by Olympian rationality has been discredited by research in experimental economics12; the success of models of bounded rationality favoured the opening of new fields of study, but it did not discredit the many families of mathematical models still surviving, who assume perfectly rational, forward looking agents. Whichever the complexity might be of the evolving core in its dynamic changes, economic theory properly should speak in the language of mathematical models and statistical techniques. In the commonly shared practices of contemporary economic research, mathematical models are families of mathematical objects, flexibly evolving under the joint pressure of the effort at producing robust analytical results, and the effort at connecting in interpretation the mathematical skeleton to the conceptualization of economic phenomena, or to presumed regularities observed in data. The criteria adopted to associate mathematical models to interpretations are loose, and different authors freely adjust them to their own epistemological vision. This difficult passage will be critically dealt in this paper.

Models, theories, representations...Let us begin by clarifying the terminology we shall adopt in what follows. An economic model has a mathematical skeleton, being a mathematical object subject to a number of logical requirements, as far as the overall coherence of its primitive concepts, assumptions, equations, or functional forms are concerned. The mathematical skeleton per se becomes an economic model properly, when names and meanings are attached to its variables, equations, and functional forms, which are understood as building a world of imaginary, conceptual interactions, involving fictional agents or variables, whose behavioural rules are specified imposing specific structure on the mathematical skeleton. Two sets of meanings are involved: the mathematical object per se moulded by both mathematical forms and specific constraints; the reading of the mathematical object thus built as being the conceptual picture of some fictional space of social interaction, notably some fictional Wonderland economy. Finally, at the third step, the imaginary characters and situations moving in the Wonderland theatre are transfigured into characters and situations, which are related to actors and events in real economies. This last step requires the narration of some story about the possible interpretations of the Wonderland world in the model, if any such interpretation is explicitly suggested by the scholar, who first conceived it, or by other scholars using, developing, or criticising the model in question.

It is such work that has increased the variance of acceptable views in the profession." (Colander, Holt, Rosser Jr. 2004: 486-487).

12 Herbert Simon named Olympian rationality, the idea of rationality with no limits or costs in information gathering, in opposition to the idea of bounded rationality, or procedural rational intelligence under informational constraints.
In 2001, Mary Morgan argued that the narration which accompanies an economic model is not just a rhetoric or heuristic device; it is an essential procedure of the cognitive process that permits to use the model to explore reality, and to attribute meaning to its use (Morgan 2001: 361). An economic model, as such, combines deductive and narrative reasoning; in narrative reasoning narrative sequences are explored according to narrative language and rules. Even in a static model, deductive logic on the inner structure of the mathematical model has to be integrated by answers and questions articulated in narrative sequences to account for possible sequences of events within the fictional world the model depicts, their dynamic evolution, their relation, if any, to aspects of reality (Morgan 2001: 369). According to Morgan the stories narrated on a mathematical model establish connections with some aspects of the real world, even if only by exploring a specific or typical case.\footnote{13}

First, typically these sequences of changes, prompted to answer the question, have the structure of a narrative. The question proposes an event, which changes something in the model, which suggests that something else happens (and maybe another round of changes results) and then we arrive at a final outcome (different from the starting point) or a new ‘solution’ point. Secondly, the elements in the interpretation are related, with implied, rather than overt, causal connections. Thirdly, whenever we ask and answer these questions, we tend to bring in the interpretative level and discuss the changes in the elements in terms of the things in the world we have represented in our model. (Morgan 2001: 369).

The stories narrated on models are, thus, an integral part in the cognitive process of building economic theories, as Morgan effectively diagnosed. They are at the core of our inquiry in this paper. Their status is controversial, being exceedingly controversial how and if these stories are more or less related to occurrences in the world of markets, in past history or in contemporary societies. In the current practice of research, scholars conceive mathematical models as conceptual tools born out of the economist's imagination, which may be flexibly moulded for analytical reasons or for purposes of interpretation. The very notion of theory is in question: is an economic theory a coherent set of substantive hypotheses about what is going on in the world, or is it just a coherent set of assumptions and theorems on the fictional interactions built in the imaginary world of some models? Each specific model may be transformed into further multiple versions of its basic structure; no economist thinks that it has the character of truth, or it aims at being a theory of general validity. Moreover, no consensus exists in the profession on a shared canon to validate models, as far as their credibility, or utility, to build and assess theories are concerned. Families of models are explored, or abandoned, according to the conventions prevailing in diverse academic communities, notably on account of their presumed productivity in terms of analytical results. The dominant canon of scientific procedure is a linguistic one: an economic theory, however it might be conceived, should be formulated in the language of mathematical modeling, while the constraints in terms of evidence and interpretation are flexible, loose, fuzzy, or even not relevant at all.

In conformity with what Tom Wolfe called the high tide of the "scientificalization” in the humanities, the dictate is to show up as hard scientists, to acquire the proper stars in the world of science and academia. “Get hard! Whatever you do, make it sound scientific!” (Wolfe 2016: 87). Scientificalization biased advanced research towards mathematical complexity per se, as the easiest and safest way to acquire academic prominence, as Paul Krugman lamented.\footnote{14} We shall explore the following question: are indeed contemporary model builders in economics hard scientists on the

\footnote{13} “But models also relate to the world via their interpretations, where I have argued we tell stories in answering questions and in doing so we make points about concrete cases (specific or typical, but not necessarily real) which have or which might have occurred in the world.” (Morgan 2001: 377).

\footnote{14} “First, success in academic economics came from publishing ‘hard’ paper, meaning papers that used rigorous and preferably difficult mathematics. This in itself biased publication toward equilibrium business cycle models, as opposed to the ad hoc modeling typical of what I consider useful macroeconomics.” (Krugman, 2011: 311).
basis of their own methodological criteria, or are they just story tellers, much as fiction writers or other scholars in the humanities? On their own criteria of Olympian rationality, are they rational scholars, or are they searching as the inferior agents of bounded rationality, or even as Keynesian people forced to adopt conventions to face radical uncertainty?

3. Building castles in the air?

Empirical evidence is commonly considered a requirement of theories in the natural sciences. How far are historical evidence or predictive power relevant for mathematical modeling in economics? According to the criteria Milton Friedman proposed in his essay "The Methodology of Positive Economics", the significance of an economic theory should be tested by its predictive power, not by the conformity to realism of each of the specific assumptions adopted in building it.

Viewed as a body of substantive hypotheses, theory is to be judged by its predictive power for the class of phenomena which it is intended to "explain." Only factual evidence can show whether it is "right" or "wrong" or, better, tentatively "accepted" as valid or "rejected." As I shall argue at greater length below, the only relevant test of the validity of a hypothesis is comparison of its predictions with experience. The hypothesis is rejected if its predictions are contradicted ("frequently" or more often than predictions from an alternative hypothesis); it is accepted if its predictions are not contradicted; great confidence is attached to it if it has survived many opportunities for contradiction. Factual evidence can never "prove" a hypothesis; it can only fail to disprove it, which is what we generally mean when we say, somewhat inexacty, that the hypothesis has been "confirmed" by experience. (Friedman 1953: 5).

The attentive reading of this essay suggests that Friedman held a substantive idea of theory, the anchorage of theory to reality being stronger in his vision than today it is commonly held to be the anchorage of economic models to economic events. He downplayed the role for logical consistency, as relatively secondary. Theory is "a body of substantive hypotheses", which have to be tested with the criterion that they should be "sufficiently good approximations for the purpose in hand". A hypothesis is always descriptively false, because it abstracts some crucial element from a mass of circumstances...it abstracts, however, from the complexities of observed phenomena; it abstracts within the constraint of reaching a good approximation for the interpretation the scholar has in mind. Predictive power is the ultimate criterion of the effectiveness of each theory for the specific issue dealt with. If this vision is at far distance from Walras's dream of discovering the

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15 "The choice among alternative hypotheses equally consistent with the available evidence must to some extent be arbitrary, though there is general agreement that relevant considerations are suggested by the criteria “simplicity” and “fruitfulness,” themselves notions that defy completely objective specification. A theory is “simpler” the less the initial knowledge needed to make a prediction within a given field of phenomena; it is more “fruitful” the more precise the resulting prediction, the wider the area within which the theory yields predictions, and the more additional lines for further research it suggests. Logical completeness and consistency are relevant but play a subsidiary role; their function is to assure that the hypothesis says what it is intended to say and does so alike for all users - they play the same role here as checks for arithmetical accuracy do in statistical computations." (Friedman 1953: 5-6).

16 "A hypothesis is important if it “explains” much by little, that is, if it abstracts the common and crucial elements from the mass of complex and detailed circumstances surrounding the phenomena to be explained and permits valid predictions on the basis of them alone. To be important, therefore, a hypothesis must be descriptively false in its assumptions; it takes account of, and accounts for, none of the many other attendant circumstances, since its very success shows them to be irrelevant for the phenomena to be explained. To put this point less paradoxically, the relevant question to ask about the “assumptions” of a theory is not whether they are descriptively “realistic,” for they never are, but whether they are sufficiently good approximations for the purpose in hand. And this question can be answered only by seeing whether the theory works, which means whether it yields sufficiently accurate predictions. The two supposedly independent tests thus reduce to one test." (Friedman 1953: 8-9).
both normative and scientific laws of markets, the building of theory is still conceived as the building of substantive hypotheses to be empirically tested.

At the beginning of the 1980s, Robert Lucas advanced much looser requirements about realism, or predictive power, as regards mathematical modeling in macroeconomics. According to Lucas to build a model is to build some fully artificial, system created by scientific imagination. The model builder provides a set of instructions that put together some clockwork economy, that is an engineering device somewhat mimicking the time series of actual economies. The requirement is no more the predictive power of the abstract theory as a body of substantive hypotheses; it is the mimicking power the clockwork system might exhibit ex-post, by showing the conformity of the trends of its crucial variables in simulations to the overall trends of past time series.

Any model which is well enough articulated to give clear answers to the questions we put to it, it will necessarily be artificial, abstract, patently 'unreal'. On this general view of the nature of economic theory then, a 'theory' is not a collection of assertions about the behaviour of the actual economy but rather an explicit set of instructions for building a parallel or analogue system - a mechanical imitation economy. A 'good' model, from this point of view, will not be exactly more 'real' than a poor one, but will provide better imitations. Of course, what one means by 'better imitation' will depend on the particular questions to which one wishes answers. (Lucas 1980: 697).

It should be noticed that the mimicking power requires that the scholar should be able to create a correspondence between some artificial variable in the analogue, mechanical system and the relevant variables in time series; but the correspondence is not constrained by any, strict conditions about the similarity between the artificial, analogue system and the real world of economic events. In the 1980s, the family of real business cycle models that followed the first generation of R.E. macro models were built on extreme "as if" assumptions. They depicted Wonderlands of hermit economies that had no pretence to be sufficiently good approximations of complex economic realities. Applying the new technique of calibration, their mimicking power was checked on assigned values of parameters, whose legitimate source in microdata and whose empirical validity were questioned in heated controversies. This was the situation in macroeconomics at the end of the 1980s. Meanwhile, the conception of mathematical models in economics had been deeply changing. Already in 1982, Sargent, describing his joint work with Wallace, spoke of building a clockwork economy, conceived as a "spectacular" example, that is fictionally built to show a patently extreme, artificial behaviour, imagined for the purpose of a thought experiment. Mathematical models were conceived as conceptual sets of instructions to build logical structures for the mere purpose of thought experiment exploring fictional Wonderlands. Mathematical models are not even analogue, clockwork systems; they are fictional castles in the air explicitly built for thought experiment. Thought experiment on fictional situations of interaction is the explicit foundation of game theoretic models in economics, a trend of research quickly gaining space and prominence in economic theory since the 1980s.

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17 "One exhibits understanding of business cycles by constructing a model in the most literal sense: a fully articulated, artificial economy, which behaves through time so as to imitate closely the time series behavior of actual economies." (Lucas R.E. Jr, 1977: 11).

18 For a summary of the main controversies on real business cycle models, see De Vroey 2016, chap. 16-17.

19 "This meant that when one conducted a thought experiment involving a change in one of the exogenous laws of motion, some or all of the behavioral equations - decision rules - of the model would change. This fact called for a thoroughgoing change in the formal methods that macroeconomists had used to think about choosing desirable laws of motion for government policy variables." (Sargent T.J. 1982: 387).

20 "Neil Wallace and I (1975) tried to draw attention to this situation by constructing an example economy in which the behavioral rules changed in such a drastic way with changes in the law of motion for the money supply that it frustrated any possibility for successful systematic countercyclical monetary policy. The example was, as intended, a spectacular one (...)." (Sargent T.J. 1982: 382).
G. Debreu proposed another vision of extreme modelization freedom, by disconnecting the mathematical structure of axiomatic models from interpretations. He gave priority to the axiomatic mathematical skeleton, as a set of primitive concepts and a coherent system of axioms, on which theorems may be proven. The axiomatic theory in its bare mathematical skeleton may be applied to address various conceptual questions and imaginary environments, which may be read as referring to specific economic reality.

An axiomatized theory firsts elects its primitive concepts and represents each one of them by a mathematical object (…) Next assumptions on the objects representing the primitive concepts are specified, and consequences are mathematically derived from them. The economic interpretation of the theorems so obtained is the last step of the analysis (Debreu, 1986: 1265).

This axiomatic conception is silent about the source of primitive concepts, and the why of the constraints the scholar should impose by the assumptions adopted in the mathematical object. Concepts in economic theory arise from deep roots in the history of economic ideas, which in turn have deep roots and connections with philosophical ideas, or the humanities at large. They cannot be severed from these roots and connections, if they have to keep meaning. Ideas such as price, good, choice, competition, welfare, equity, growth, development, to give but a few exempla, cannot be properly understood when severed from the wider cultural heritage to which they belong, much as the interpretations involving narrations, which might use them in speaking about events in the real world. Conceptualization is a complex task, which requires coherence and specialized language much as the proof of theorems on mathematical objects.

Is predictive power still relevant for mathematical modeling in economics today? Or with losser requirement is a model's mimicking power relevant in constraining the acceptable range of models admitted in theory? Indeed, both the predictive, or the mimicking requirements imposed on models are far from being a constraint in economic modeling practices. In ample fields of economic theory, no evidence requirements whatsoever are imposed on modeling, neither by mimicking nor by whichever conformity to observed evidence, ex ante on assumptions or ex post on interpretations. Economists feel free to build mathematical castles in the air. Extreme freedom in modeling is the dominant practice in game theory, in much of macroeconomics, and in so many fields in microeconomics. Freedom in modeling strategies admits whichever number of imaginary Wonderland economies, created in imagination by the most various "set of instructions" as regards assumptions and structure, with almost no constraints in terms of correspondence to actual behaviour, or to evidence checked from historical events, or to trends of variables in historical time series, accurate predictions, and so on and so forth. Almost infinite freedom is admitted in conceiving structures picturing imaginary worlds attached to mathematical skeletons, Wonderlands which, for one reason or another, seem to offer good opportunities for rigorous analysis, and proofs.

The constraint that was more often accepted or imposed is the strict requirement of forward looking rationality by each and every puppet agent moving in fictional Wonderlands. However, the assumption of rationality is no more predicated as a substantial hypothesis about the behaviour of real economic actors, as in Muth's original paper in 1961. It was downgraded to a technical model building principle, whose ultimate reason is the economy of means in scientific discourse; or it is not explained at all. To paraphrase Mitja Karamazov's thorny question, without Olympian rationality, "it means everything is permitted now, one can do anything" in the economic Wonderlands? The rationality assumption stands, because, so to speak, out of the realm of rationality, anything might happen in the imaginary worlds of economic models.

22 "Without God, and the future life? It means everything is permitted now, one can do anything?", Brothers Karamazov, Part IV, Book 11, chapter 4.
In current practice in advanced research in economics, models are built, developed or thrown away in full freedom and with no regrets, according to academic fashion, promises of further results, analytical dead ends, or rising hopes for new interpretation. In 1996, at the close of his Nobel Lecture, R.J. Lucas quickly moved away from earlier R.E. equilibrium and business cycle models\textsuperscript{23} to the new family of real business cycle models. He declared that they had provided precious insights, but they were far from offering a satisfactory theory of business cycles. It was time to look at the new edge of research: real business cycle models.

But I think it is clear that none of the specific models that captured this distinction\textsuperscript{24} in the 1970s can now be viewed as a satisfactory theory of business cycles. Perhaps in part as a response to the difficulties with the monetary-based business cycle models of the 1970s, much recent research has followed the lead of Kydland and Prescott (1982) and emphasized the effects of purely real forces on employment and production. This research has shown how general equilibrium reasoning can add discipline to the study of an economy's distributed lag response to shocks, as well as to the study of the nature of the shocks themselves. More recently, many have tried to reintroduce monetary features into these models, and I expect much future work in this direction. But who can say how the macroeconomic theory of the future will develop, any more than anyone in 1960 could have foreseen the developments I have described in this lecture? (Lucas 1996: 679-680).

The disowned models had been at the origin of a vocal revolution in macroeconomics, marketed as a radical turn, and a major theoretical innovation. They were dismissed in a few lines.

4. Epistemological debates on the use of models in contemporary economic theory

In 1997, Solow, while discussing methodological questions in macroeconomics, underlined "the model-building philosophy that motivates and guides economics", a specific aspect that differentiates economics among other social sciences (Solow 1997: 55). In his description, Solow proposed a simplified vision of the approach that in methodological debate is named the isolationist view. He anchored mathematical modeling to the strategic simplifications of crucial aspects of economic reality, with the ultimate aim of illuminating what goes on in the world.

A model is a deliberately simplified representation of a much more complicated situation. (...) The idea is to focus on one or two causal or conditioning factors, exclude everything else, and hope to understand how just these aspects of reality work and interact. (...) modern mainstream economics consists of little else but examples of this process. (Solow 1997: 43).

A good model makes the right strategic simplifications. In fact, a really good model is one that generates a lot of understanding from focusing on a very small number of causal arrows. Model-building is not a mechanical process(...) Economic models are usually stated mathematically, but they do not have to be. They can be described in words, as I have been doing, or in diagrammatic form, or in computer flow charts, for that matter. But mathematics turn out to be a very efficient way to express the structure of a simplified model and it is, of course, a marvellous tool for discovering the implications of a particular model. (Solow 1997: 46).

The isolationist vision underlines the nature of models as simplified representations that succeed in isolating crucial aspects of the economic world in an abstract theoretical scaffolding

\textsuperscript{23} The R.E. macro models with misperceptions and monetary surprises built in the 1970s.
\textsuperscript{24} The distinction between anticipated and unanticipated monetary policy.
Isolation by idealization is the experimental moment in an important and large class of models in science. What this involves is control for noise so as to isolate some important fact, dependency relation, causal factor or mechanism. The controls are implemented causally in material experiments, while in theoretical modelling other things are neutralized by way of idealizing assumptions and silent omissions. Idealizing assumptions are often false, but this is not necessarily a reason for concern, since many such assumptions are employed and manipulated as strategic falsehoods that serve the purpose of isolation. In assuming that certain potentially efficacious factors are absent, constant, or in normal or otherwise suitable states, the modeller isolates whatever seems worthy of close attention from those other things. (Maki 2009: 30).

According to Maki's summary explanation, the model builder builds a surrogate system that aims at representing some outside target system. In the surrogate system, scientific representation does not impose strict requirements of similarity, since it admits, and requires, the procedure of isolating the causal factors, or mechanisms, that appear most relevant. Unfortunately, many problems are open in the isolationist vision, when applied to explanation and presumed scientific procedures in the social sciences. In social life, the causes may not be separable, or they cannot be neatly isolated as in laboratory experiments. The assumptions adopted in mathematical economics are often justified by mathematical convenience, more than by isolation requirements, since the mathematical object that is the skeleton of the model should provide theorems with the available mathematical techniques, in terms e.g. of existence, or uniqueness of solutions, restrictions about possible solutions, dynamic convergence or non explosive behaviour, and so on and so forth. In principle, the mathematical object should be structured in such a way that the derived results should be robust to minor variations in the mathematical structure. In his comment on the issue, Knuuttilla effectively underlined that the mathematical model in economics is a complex construction that fails to be inspired in its scaffolding by the neat isolation of some causal factors.

The problem is that the model assumptions do not merely neutralize the effect of the other causal factors. They do much more: they construct the modelled situation in such a way that that it can be conveniently mathematically modelled. Moreover, in this process such properties are attributed to the modelled entities and their behaviour that the model starts to look like an intricate construction rather than a neat experiment involving isolations to 'seal off' the influence of other confounding factors. (Knuuttilla 2009: 60)

25 "Models represent in two ways, both of which require the model to be a model of something else. First, to say that a model is a representative of some target is to say that it stands for that target as its surrogate. A model M represents a target system R in the sense that M is a representative of R: in other words, M is R’s surrogate system. The epistemic function of models as representatives or surrogates is that one does not seek to acquire information about target R by examining R directly, one rather examines M directly, engaging in an inquiry into its properties and behaviour, with a view to indirectly acquiring information about R. Secondly, one could only hope to learn about target R by examining model M, if M represented R in the second sense: M resembles, or corresponds to, the target system R in suitable respects and sufficient degrees. This second aspect of representation enables models to serve a useful purpose as representatives: by examining them as surrogate systems one can learn about the systems they represent. However, one might also fail to learn about the target by examining the model, but this should not be taken to imply that there is no representation. Representation does not require resemblance: it only requires issues of resemblance to potentially arise. (Maki 2009: 32).
However neat and appealing the isolationist vision seems in its apparent similarity to procedures in the natural sciences, it does not portray how today economists work at the edge of research in economics. Mathematical economists do not work by isolating the most relevant causal factors or mechanisms, and notably so scholars in advanced macroeconomic theory. The dominant idea remains deeply rooted that mathematical modeling is the proper, scientific language of economic theory; but the prevailing paradigm admits almost no restrictions on assumptions and structure, beyond basic requirements in terms of logical coherence and robust theorems. Methodological debates reflected the change of focus, and the isolationist interpretation had to face the constructionist approach that became dominant. The exact nature of the divergence between the two visions was the object of controversy, but the main outlines are clear. The constructionist approach signals the artificial nature of models, comparing them to narrations born out of imagination, such as fables, parables, novels. Model builders creatively make up parallel, mind worlds that they freely explores in imagination, much as do writers and readers of literary fiction.

On the one hand economic models have been conceived of as surrogate systems through which we can obtain knowledge if they succeed in isolating or abstracting some causal mechanisms or tendencies correctly (Cartwright 1989; Maki 1992, 2009 b), while on the other hand it has been suggested that they are rather like pure constructions or fictional entities that nevertheless license different kinds of inferences (Sugden 2002, 2009). In what follows I will examine the two alternatives in an attempt to locate where their differences lie. I will argue that whereas the isolationist view is still tied to the representational understanding of models that takes the model-target dyad as the basic unit of analysis, the fictional and constructionist perspective can better accommodate economic modelling as it is actually practised, including the specific modelling styles and the very discourses of which the models are a part. (Knuuttila 2009: 60).

According to the constructionist interpretation, models are built to explore ‘self-contained imaginary worlds’, with no pretence to symbolically represent simplified pictures, or schemata of aspects of reality, not even in highly abstract, symbolic language. Economic models are thus compared to literary narrations, where fictional worlds are created in imagination that tell us something about life. According to different authors, the references were to parables, fables, stories, novels; and they were proposed either by economists dealing with their own work, or by both economists and methodologists in debates on method (Gibbard, Varian 1978, Morgan 2001, 2007, Rubinstein 2006, Grüne-Yanoff, Schweinzer 2008, Sugden R. 2000, 2009). Let us remind some views on the issue; in this paper, we shall not deal with the differences between literary narration and mathematical models, which are poorly accounted for in such comparisons (Ingrao 2015).

Among scholars defending the isolationist view, the ambition prevails that models should tell stories about the real world. The fictional story is not the world; but the idea is that it narrates something about what is going on in the real world. Or, more loosely, a story is attached to the model that helps to understand what is going on in the real world. However, even the more nuanced version of the isolationist vision is rejected by authoritative mathematical economists, and it is not at all pursued among model builders at large. Ariel Rubinstein, an outstanding scientist building game theoretic models, openly argued that in economic models economists narrate fables (Rubinstein 2006). In 2012, he published a book with the title Economic Fables (Rubinstein 2012).

The word ‘model’ sounds more scientific than ‘fable’ or ‘fairy tale’ although I do not see much difference between them. The author of a fable draws a parallel to a situation in real life. He has some moral he wishes to impart on the reader. (...) We do exactly the same in economic theory. A good model in economic theory, like a good fable, identifies a number

26 See the debates in the number 7 of Erkenntis in 2009.
of themes and elucidates them. We perform thought exercises that are only loosely connected to reality and that have been stripped of most of their real-life characteristics. However, in a good model, as in a good fable, something significant remains (Rubinstein 2006: 881).

An economic fable is a short story in common language that describes the imaginary situation in which fantasy characters interact in a fantasy context; the economic fable deals with their interaction in behaviour as they are moved by the aim of pursuing their selfish interest. The story relies on the model's mathematical skeleton; thanks to intuitive judgement, the economic fable will offer some teaching or insight about situations in the real world, which the interpreters (model builders or readers) might happen to face or be involved in. Rubinstein fails to mention the distinction between fables and fairy tales, on which ample debate exists in literary studies, much as he fails to indicate how and by which cognitive procedures the message in the fable should be decoded, if and whether a single decoding should be applied, or a plurality of messages is admitted. In conclusion, the presumed, scientific messages derived from mathematical models are downgraded at the same level of the pearls of wisdom, which a secular tradition in narration attached to fables.

Or are they upgraded? Fables treasure meanings, which were, and are decoded with the multiple clues offered by the evolving, cultural heritage of literary language, custom, ethics...And so fairy tales, or other fantasy stories. Fables tell stories about imaginary characters; the reader has not to believe that speaking turtles and hares are competing in a run, to enjoy them, and appreciate their messages. To stand on safer ground, R. Sugden established the comparison between the imaginary models of economics, and the realistic novels, which narrate what happens in imaginary, but credible worlds, making them credible to the readers, although both fiction writers and fiction readers well know that no Madame Bovary in France, or Anna Karenina in Russia, committed suicide in the occurrences narrated in the respective novels.

Credibility in models is, I think, rather like credibility in ‘realistic’ novels. In a realistic novel, the characters and locations are imaginary, but the author has to convince us that they are credible – that there could be people and places like those in the novel. As events occur in the novel, we should have the sense that these are natural outcomes of the way the characters think and behave, and of the way the world works. (...) Simplification and isolation are allowed, of course; we do not expect to be told everything that the characters do or think. But what is being simplified is not the world of actual events, but the world imagined by the author. (Sugden 2000: 25).

According to his approach, mathematical models in economics are thus "credible counterfactual worlds". (Sugden 2000: 28).

(...) the model world is a construction of the modeller, with no claim to be anything other than this. Its specification is just whatever the modeller has chosen it to be. In particular, there is no claim that it has been constructed by stripping out some features of the real world and describing what remains. (Sugden 2009: 17).

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27 "A description of an economic model is like the introduction in a tale, presenting the heroes, their interests and the setting in which they operate. An array of rules by which the model is allowed to develop from its beginning to its end is called a solution concept." (Rubinstein 2012: 19).

28 "We will take the tale’s message with us when we return from the world of fantasy to the real world, and apply it judiciously when we encounter situations similar to those portrayed in the tale." (Rubinstein 2012: 16).
Other authors spoke of case-based knowledge, maintaining that in economic theory there is no pretence at offering substantive hypotheses, general rules, or conjectures of general validity. Models in economic theory explore thought experiments about fictional cases. The knowledge acquired by building economic models should be conceived as case-based reasoning, from which by inductive inference some conclusions about similar cases in reality, if any, might be inferred (Gilboa, Postlewaute, Samuelson, Schmeidler 2014).

In particular, we agree that: economic models are often viewed differently than models in the other sciences; economic theory seems to value generality and simplicity at the cost of accuracy; models are expected to convey a message much more than to describe a well-defined reality; these models are often akin to observations, or to gedankenexperiments; and the economic theorist is typically not required to clearly specify where his model might be applicable and how. Most importantly, we agree that economic models are used to suggest conclusions about real situations by means of inductive inference. In this article, we attempt to take this view one step further, focusing on case-to-case induction, or on case-based reasoning, as a model of the way economists expect (some of) their models to be used. (Gilboa, Postlewaute, Samuelson, Schmeidler 2014: 516).

This vision fits well to account for game theory, where the theorist builds fictional contexts of interaction, freely defining fantasy characters and fantasy rules of interaction, just for the purpose to explore what will happen in their static or dynamic stories. Economic theorists expect that some of their models might be used by other people for case-based reasoning, inferring conclusions about cases occurring in the real world. The inference will be based on the assessment of similarity to the theoretical cases; but economic theorists are not required to propose any such inference when they are building the mathematical models born out of their creative imagination. They are, so to speak, spared the effort at inductive inference, whatever that might be. The audience of practitioners makes the inference, if they like so; as Sugden suggested, the readers will take care of the inference, if that is the case. The economic theorist may fail to specify by which cognitive instruments, or specific procedures, the audience should build a similarity function to connect the fictional case to the interpretation of real occurrences. As it seems, the validation of a canon for inductive inference procedures lies outside the economic theorists' expertise and tasks.

Neither the theorist nor the experimentalist is expected to state a priori which real-life problems belong to the same category as their case. Their job is only to contribute these cases as additions to the literature and to leave similarity judgments to the practitioners who might use these cases in real-life problems. An economic theorist who offers a model prepares the ground for a practitioner who should employ his judgment in using this model; but the theorist’s contribution falls short of a testable prediction. (Gilboa, Postlewaute, Samuelson, Schmeidler 2014: 519).

If my grandma had wheels, she might be a bike. Or a motorcycle? Or a bus, perhaps? Does my grandma have wheels? Most probably not, unless her legs are too weak, and she has to go

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29 "As stated, the example can be viewed as the claim, ‘I have observed a case in which idealised agents, maximizing expected utility, with the following utility functions and the following information structure, behaved in such and such a way’. The relevance of this observation for prediction will depend on the perceived similarity between the idealised agents and the real agents one is concerned with, the similarity between the situation of the former and that of the latter, and so forth. An economist who is interested in real agents would therefore have to judge to what extent the situation he studies resembles the idealised situation in the ‘case’ reported by Akerlof; see Sugden (2009), who argues that it is the reader who must make the mapping between a model and the reality it models." (Gilboa, Postlewaute, Samuelson, Schmeidler 2014: 518).

30 "The common practice in economic theory is to use models without a clear specification of the similarity function that should be used to apply them to concrete problems." (Gilboa, Postlewaute, Samuelson, Schmeidler 2014: 519).
around on an electric scooter...a fact that does not make her become a bike, or motorcycle anyhow. Perhaps she is already dead, and she died without ever going on an electric scooter. The model narrates a hypothetical case, as the story of the grasshopper and the ant, or Frodo and the ring, Yoda and Darth Vader. A story is never wrong...well, this is not true, indeed, in literary narration. Writers take a lot of care to write coherent stories, building evolving and complex, but coherent characters. The overall coherence of stories and characters is a major task in literary narration, as it is produced by writers and appreciated by both common readers and literary critics. On the contrary, as far as economic models are concerned, the authors above suggest that the practitioners or the readers freely decide whether the fictional story told in the each case-model fits the events they are trying to face and understand, whatever they may be. How? By employing their judgement; and what is this faculty of judgement? Is it innate? Is it subject to rational, procedural checks? Were readers or practitioners trained to reach sound similarity assessments? Who are the scholars, if any, who might train audiences to infer sound similarity conclusions? We do not know. We just understand that is not a task for economic theorists, who are models' builders.

The grasshopper will die because it has no food during winter: this is the theorem in the fable. Should the audience infer the message that it is good to save for the winter, or that to save for the winter is mandatory to survive? Does the theorem mean that our neighbour will die, if we are not compassionate? Does is suggest that songs give much joy, but not food?

In a more covert way, the idea of case-based knowledge is still evocative of fables or fairy tales, since the theoretical cases born out of the theorist's imagination should convey a message, just because they might be spectacular, or exemplar cases, although plainly unreal ones. By applying our own judgement, as it seems, to the ideal cases, we readers may infer the deductions we think appropriate to infer, as to its significance, and its value of truth for what is going on in real life. Is it the preoccupation of trained, academic economists to establish a rigorous procedure to derive conjectures from the fictional cases explored in economic models? Not at all, provided that those cases are well written, in rigorous mathematical language.

5. The controversial bridge: one or many languages in economics?

Let us remind that R. Sugden proposed the crucial question very neatly: do models build a bridge towards understanding the real world?

Economic theorists construct highly abstract models. If interpreted as representations of the real world, these models appear absurdly unrealistic; yet economists claim to find them useful in understanding real economic phenomena. This prompts the question: Do these models really help us to understand the world, and if so, how? (Sugden 2009: 3).

In conformity with the constructionist interpretation, in attempting to answer this crucial question, this scholar was not surprised that the gap between the mathematical models of economic theory and the real world could be open and wide, and difficult to bridge in rigorous ways. He noted that economists often tend to hide the gap by "rhetorical devices". Sugden trusted the credibility property of models as being a warrant towards good inductive inferences from the credible, counterfactual worlds to the real world (Sugden 2000: 28).

Somehow, a transition has to be made from a particular hypothesis, which has been shown to be true in the model world, to a general hypothesis, which we can expect to be true in the real world too. (Sugden 2000: 19).

The practice of sweeping the dust under the carpet is not a recommended scientific procedure. The crucial passage calls into question the intuitive insight or message that theoretical models convey, and the cognitive processes by inductive inference, comparison or analogy that
should build a bridge from the infinite variety of Wonderlands rigorously defined in theoretical models, towards the stories narrated on them, which might have some reference to the world of economic events as experienced in past history or contemporary societies. On this cognitive process the meaning and significance of building mathematical models depend; but its rational procedures, its recommended steps, checks, or constraints remain vague. Neither shared practices are validated in the profession, as sound scientific ones, nor critical attention has been devoted to this crucial issue\textsuperscript{31}. In a number of cases, the scholars skip the passage, or confine it to a different sphere of language, other than scientific language properly. The rigorous reasoning that mathematical language permits is about the imaginary Wonderland built over the nude mathematical skeleton, and on its inner logic. It has nothing to do with the world. Let us read the answer proposed by R. Weintraub to the criticism by T. Lawson, in a polemical discussion in 2005. Weintraub wrote:

In general, both equilibrium and stability (in the sense that any trajectory of the dynamic system initially "near" equilibrium converges to equilibrium under the system's dynamic laws) are features of the model. And as Dorfman et al. (1958, p. 351) said so directly, "It is the model we are analyzing, not the world." So much for terminology. (Weintraub 2005: 448).

The bridge should be neither built nor crossed: it is, to paraphrase Wittgenstein, unspeakable, and thus not to be spoken of. A disturbing conclusion follows: the economist building a mathematical model somewhat pertaining to the discipline of economics has the right to disclaim any responsibility for proposing interpretations, which should connect it to aspects of reality, as a cognitive device relevant for reading what is going on in the world. According to this approach, the model-builders are kind of Pilatus. They send for a bowl of water and wash their hands: "We are innocent of the vulgar interpretations of our mathematical theories!". Back in 1986, Lucas predicated his innocence of the vulgar interpretations about economic policy that audiences might figure out as being inferred from his models. He might sketch some "connections" that came to his mind, and that's all. No pretence at logical rigour, not to speak of scientific truth, when dealing with the understanding, if any, people might get from economic models about what was going on around them, in contemporary history or economic policy. No strict presumption to have something to say on economic policy in the real world based on robust, rational arguing anchored in the Wonderlands narrated in mathematical models, although the final aim of macroeconomic theory was marketed to be to propose ideas on crucial policy issues.

At the beginning of this talk, I said that my main purpose was to discuss connections between policy in the model I was about to set out and policy in the United States, today. My experience is that an economic model, if it is concrete enough to be visualized, has a life of his own, and people will draw such analogies between it and 'reality' as they find helpful, quite independently of how one might wish or try to direct them. I will sketch the connections that seem clearest to me, but with the understanding that they cannot be established on the same logical level at which we can understand the internal workings of the model itself. (Lucas R.E.Jr. 1986: 129).

As we saw above, Gilboa, Postlewaute, Samuelson, and Schmeidler suggested that theoretical economists should not be primarily concerned with the significance of the theoretical, cases they explore in the mathematical skeleton of their models, based on assumptions which might

\textsuperscript{31} In economics (and perhaps, as the example from biology suggests, in other sciences too) there seems to be a convention that modellers need not be explicit about what their models tell us about the real world. Given this convention, it is hardly surprising that the question of what role models play in economics is controversial among methodologists. (Sugden 2009: 16).
be wrong, when related to some external economic reality. Models, as self-contained Wonderlands, are never wrong, provided internal consistency and analytical rigour are respected.

First, one need not wonder why economists feel that they gain insights and understand economics better using models, whose assumptions are wrong. In the case-based approach, models cannot be wrong. As long as the mathematical analysis is correct, a theoretical case is valid, the same way that an empirical or experimental case is valid as long as it is reported honestly and accurately. Cases do not make any claim to generality, and therefore they cannot be wrong. (Gilboa, Postlewaute, Samuelson, Schmeidler 2014: 520).

Should economists assume full responsibility for the theories they predicate in the mathematical models they disseminate? The answer should arguably be: Yes; but it is often: No. In line of principle, economists as model builders speak rigorously about specific models conceived as self-contained imaginary worlds or theoretical cases, and that all. Should economists be held responsible of building sound interpretations of the mathematical models they create, as regards the relevant insights they might convey on real occurrences, on what went on in economic history, or which economic policies are appropriate in which circumstances? The answer should arguably be: Yes; but it is: No, in line of principle. The theoretical economist speaks rigorously about what is going on in the artificial Wonderland or theoretical case built by his or her imagination. The rest is silence; or it is talk, with no pretence to rigour. We reach the diqueting deduction that according to these methodological premises, professional economists should abdicate any pretension to offer suggestions on the basis of a deeper, rational knowledge than the laymen may have. Their intelligence of the world is not improved by the use of rigorous mathematical models, or, if it is, that happens by mysterios ways, and not by transparent, shared procedures of scientific or learned discourse, which are discussed, tested, and validated in the discipline. In principle, theoretical economists do not know how much of their "expertise" as model builders might be "transferable to the conduct of policy in the world of today", to use Lucas's words. How to address such "difficult question"? What is the precise meaning of the analogy reasoning that might render artificial, fictional economies (if economies they are meant to be in the stories told) interesting for understanding the real world? M. De Vroey, who searched through Lucas's archives, found interesting notes; but he concluded: "However, in his published papers, Lucas gave no clue as to what he meant exactly." (De Vroey 2016: 178).

On these methodological premises, theoretical economists should not pretend to know how to cross the bridge towards reality, although they might be predcating the criterion of Olympian rationality as foundational in economics, in innumerable fictional worlds created by their imagination. May we assume that they are more learned than audiences, readers, practitioners, much as Smith, Mill, Cournot, Walras, Marshall, Fisher thought to be more learned than the laymen as far the understanding of the economic world is concerned? Of course, they better understand how their artificial societies work, that is they know better the inner working of their models in technical terms; but they can barely suggest a few insights we laymen might extract from them. They do not pretend to be so intelligent to rationally infer any proper meaning, message or teaching from their mathematical fables. Are they themselves rational in their inductive judgements, and under what meaning of rationality? What does rationality mean when scholars deal with loose reasoning by analogy, or loose inductive inference by comparison?

32 (...)I will begin by considering the dynamics of policy in the context of a specific, necessarily very simple, general equilibrium model. This will occupy most of my time and when I am finished, we will have arrived at a fully understood consensus as to how monetary and fiscal policy ought to be conducted in this artificial society. Then we can turn to the more difficult question of determining how much of this expertise is transferable to the conduct of policy in the world of today. (Lucas R.E. Jr 1986: 1-2).

33 On the strictures of the dominant meanings of rationality in economic theory, see Ingrao 2014.
Sometimes the principle of transparent fairness in being unaccountable for interpretations is applied with naive, but honest strictness. Rubinstein pronounced a fully honest disclaimer. He explicitly declared that he trusts game theoretic models only as far as they provide rigorous reasoning on abstract strategic interaction; he does not trust them at all as providing solutions on issues of economic policy (Rubinstein 2012: 129). The honest disclaimer sheds disturbing light on the social utility of economic theory. One might wonder where do the advantages consist of mathematical language and mathematical modeling, if the constructive addition they provide to our knowledge for improving economic policies is so poor. The honest disclaimer has another corollary: why should mathematical language per se be a canon of rigour or scientific advancement? More than out of scientific soundness, the attachment to mathematisation and mathematical modeling seems to acquire a flavour of magic. Speaking in mathematical language makes the magical trick...the magical words, more than the substantive knowledge.

In the daily practice of the profession, the exceptions to the transparent disclaimer are many, and repeated. The disclaimer seems to be intended more as a defensive tool, against accusations of building models on patently on false assumptions, or fancying about incredible clockwork economies; but it is most often not respected in its purity, and notably so by the scholars theorizing real business cycle models34 (De Vroey 2016: 304 ff.). In his Nobel Lecture Edward Prescott violated it repeatedly. All along the presentation, he underlined primarily the immediate significance for policy issues of the single sector, neoclassical growth model on which real business cycle models are based, and he even suggested that his theory helped avoid monetary instability35 (Prescott 2004). Lucas was more affirmative on the disclaimer, but not always cautious in proposing interpretations. In his Nobel Lecture, he illustrated the very models, which at the close he dismissed for their unsatisfactory explanatory power, with an ample discourse making reference to currents of ideas in the history of economic thought, much as to statistical evidence on the quantity theory of money, and the related controversies on economic policies. To be understood and assessed, the toy models with their fictional worlds require the narration in the shared, stratified language of economic culture: a non mathematical, specialist language, with philosophical roots and ideological echoes, that has been built through controversies on interpretation and historical reconstruction. In other passages Lucas pointed out to the messages that even patently "false" models may convey, arguing that the ultimate justification for exploring them is how far they might shed light on pressing monetary issues. In reviewing Lucas's monetary essays, Sargent underlined the point: "A striking part of this passage is Lucas’s faith that “false”models can teach."(Sargent 2015: ...). The reader may judge the overall coherence of these contradictory, methodological stances.

According to M. Morgan and other scholars, narrative sequences attached to the fictional worlds of models are essential to account for the critical passage to loose inferences that could bridge the gap between the imaginary worlds of theoretical economics and the reality of economic events. Without imposing strict criteria of realism on the mathematical skeleton, it is argued that the fictional worlds of models might acquire real life, and suggest connections to the sphere of real events, thanks to the narrative reading in common language going beyond the strict, formal properties and evoking similarity to some sphere of real economic phenomena36 (Gibbard, Varian

34 M. De Vroey signalled many passages in Prescott's published writings, where he was interpreting the results from the fictional models as talking plain truths about reality (De Vroey 2016: 304 ff.).
35 «The increased stability of the economy and the improved performance of the payment and credit system may be due in part to the diffusion of findings of Finn’s and my “Rules Rather than Discretion” paper. People now recognize much better the importance of having good macroeconomic institutions such as an independent central bank.” (Prescott 2004: 375). It was a pity that such a strong assertion was affirmed three years before the global financial crises.
36 "We will argue that the game structure and the model narrative together constitute a model of an economic situation. We call this the game model. (...) While there is widespread agreement that game theorists engage in the practice of telling stories when teaching or presenting new game models, it is more controversial whether this narrative is part of the game model in the more substantial sense claimed above. In this section, we discuss the different functions that narratives have when using game theory to model economic situations. We argue that each of these functions is
1978, Morgan 2007, Grüne-Yanoff, Schweinzer 2008). Grüne-Yanoff and Schweinzer, developing Morgan's approach, attribute a primary role to the informal "narrative account of events" that is attached to game theoretic models, permitting the articulation of questions and answers (Grüne-Yanoff, Schweinzer 2008: 140).

Thus, although a model’s structure may be its most visible part, its story has at least three crucial roles. It provides an interpretation of the structure as the representation of some economic situation, it allows connecting a question one is interested in with specific manipulations of the model structure, and, finally, it incorporates the results from the manipulation, thus yielding a narrative account of events that is backed up with the exact and rigorous power of a formal structure. Using an economic model, therefore, never means only using a formal structure. Rather, it means using a composite object consisting of a formal structure and a story, both of which play crucial parts in the scientific use of models. (Grüne-Yanoff, Schweinzer 2008: 136).

It is not at all clear where do these narratives come from, and for which reason one should be preferred to another. If they are intended as stories strictly speaking only about the fictional worlds portrayed in the models, then their interest is questionable, as regards the discipline of economics properly; if they are covertly or explicitly predicated as being credible stories, somewhat connected to real events in the economy, from which sources is their credibility established? Which is the lexicon they use? How are they inspired by the heritage of shared culture in wider fields of economics outside theoretical economics properly? How are they influenced by the knowledge of economic history, or of the history of economic thought? Here comes to light the complexity of building inferences, involving diversity of languages, ideas, values, or background in historical knowledge, and the perception of borders and exchanges with other fields of culture. Here come to full light the complexity of what theory is in economics, if theory is to be understood as a body of substantial hypotheses and explanations, which improve the understanding of economic realities, not just the understanding of self-contained fictional worlds.

We cannot go back to the 19th century search for truth in mathematical modeling; but we cannot accept the final dissolution of economic theory into disperse fragments of arbitrary Wonderlands, whose contact with reality are so loose and ill defined that theoretical economists end up refusing to assume responsibility for what they might suggest in terms of explanation and policies. Economic theory, with its complex, controversial, evolving core, cannot be defined by the exclusive dominance of mathematical models; it cannot stand up on the exclusive leg of economic fables disconnected from both evidence and intelligence of the real world. Thus, we should rebalance the primacy of mathematical language in economic theory; to dare cross the controversial bridge, we should acquire, and assess, knowledge by multiple languages, sources, inquiries. This is the only rational strategy to address the complexity of historical events in changing societies, whose intelligence is the aim of economic culture to enlighten. To check by inference the credibility of fictional economic stories, to propose substantive interpretations, or to creatively assess and device economic policies, all tasks the economic profession has to face, sound economic theory is and should be rebuilt on the joint support of diverse cognitive instruments and languages of culture, including the essential legs of economic history and economic thought, much as new, or old, legs from exchanges with psychology and philosophy, or other social sciences.

References

of economic methodology (pp. 45-48). Cheltenham: Edward Elgar.