

Qualitative Simulation Modelling*

Marco Valente
Università dell'Aquila
mv@business.auc.dk

ver. 1.2

EARLY DRAFT.

COMMENTS VERY WELCOME. PLEASE, CIRCULATE.

Abstract

This work presents a proposal for formalising the use of simulation models as a tool for abstract studies in Economics. We advance our proposal by means of three independent steps. Firstly, we claim that social phenomena lack, in general, the characteristics necessary to use the traditional methodology of science as developed in natural sciences. Secondly, we sustain that a weaker, but more general, format of knowledge is embodied into chains of explanations for observed events. Thirdly, we discuss how simulation models can be used to extract and embody this format of knowledge, noting, in particular, the differences with quantitative simulation models.

Keywords: Methodology for Social Sciences, Simulation Models

*The present version is a very early draft meant to provoke comments and feedbacks. The shameless scarcity of references is solely due to the early stage of this revision, and by no means implies the originality of all the ideas presented. The author wishes to thank E.S.Andersen, G.Dosi, M.Ferretti, W.Fontana, K.Frenken, K.Laursen, A.Lawson, F.Malerba (and CESPRI), L.Marengo, G.Silverberg, the few users of simulations models and the vast number of their doubters that provided the motivations and useful insights for this work. Perhaps it has never been truer that the opinions expressed here are exclusive responsibility of the author.

Contents

1	Why Social Sciences Differ From Natural Sciences	4
1.1	Why natural sciences succeeded ...	4
1.2	... and why social sciences cannot follow the same road	5
1.3	Is then Economics a science?	7
2	Knowledge as causes	8
2.1	Classifying types of knowledge	9
2.2	Why has knowledge this format?	11
2.3	Knowledge in Economics	12
3	Simulation modelling for the study of causal links	13
3.1	Qualitative simulation modelling	13
3.2	Empirical evidence and economic theory	16
3.3	Evaluation of qualitative simulation results	18
4	Conclusions	21

“Bobby Fischer, in 1972, played chess differently from Paul Morphy, in 1861. [...] Economics, like chess, is inevitably culture-bound and history-bound. [...] Economics is one of the sciences of artificial. It is a *description and explanation* of human institutions, whose theory is no more likely to remain invariant over time than the theory of bridge design. Decision processes, like all other aspects of economic institutions, exist inside human heads. They are subject to change with every change in what human beings know, and with every change in their means of calculations.” (emphasis added)

Simon (1972).

Introduction

The goal of this paper is to suggest how to run simulation models in Economics rigorously and effectively to produce “*descriptions and explanations*” of economic phenomena. This objective is felt as increasingly important because of the large number of works in the fields making use of such method lacking an accepted system of verification and validation of the results. This negligence generates frequently skepticism in the audience of these works. But, on the other hand, in many cases researchers feel the impossibility to follow the paradigms of scientific methodology, though reputing their efforts worth as contribution to their field. Thus, we have a set of works that, on the one hand, adopt a “sloppy” methodology and, on the other, generate interesting results.

The approach of this work is to focus not much on the methodology required to generate scientific results, but on the analysis of the types of results. “Science” is identified in general with natural sciences, namely Physics, which has developed a methodological approach that, at least in its main guidelines, is universally accepted and spectacularly successful. In section 1 we sustain that Economics, and social sciences in general, can hardly follow the same path as Physics because of major differences in the nature of the phenomena the two disciplines are interested into. In particular, we will sustain that natural sciences focus on phenomena taking place in relatively isolated systems, with minor and controlled influences from external factors, and concerning entities that remain structurally constant, without changing radically their nature. Where these assumptions can reasonably hold, the “traditional” methodology based on studying quantitative relations among numerical variables can be applied. On the contrary, phenomena studied in social sciences generally take place in environments in which factors outside the domain of the discipline are generally of major importance, generating “noise” which is as strong as (and strictly entangled with) the “signal” that the theory tries to capture. Moreover, the entities involved in many cases undergo structural changes that prevent the use of a constant set of variables for their representation. In these cases quantitative records are blurred by historical accidents, and a quantitative analysis is either impossible or meaningless.

Accepting these assumptions, one may conclude (and many actually do) that the scientific methodology cannot be applied and therefore it is impossible to perform scientific analysis on these fields. This position is correct if we consider that the only format of scientific knowledge is quantitative relations among variables, that can be measured in the real world. In section 2 we question such position. We sustain that knowledge is defined, in general, as causal links (or explanations) between states of the world. Such causes can be precise quantitative relations, but can also be of a “softer” type of concepts. In case the questions we pose are quantitative, then the universally accepted as only scientific paradigm should be applied. But we can also aim at solving less universal problems, with less rigid requirements, and we can still claim to make science, that is to improve our

knowledge of the world by rigorous and shared means.

Simulation modelling is a tool that allows a wide range of flexibility, from rigorously quantitative relations, to more *ad hoc* specifications. Given our definition of knowledge as causal link, in section 3 we discuss how simulation model can be used to generate scientifically valid results. The core of this discussion is that the successful modeller should generate with the model “interesting” (non obvious and realistic) chains of causal links which can be useful to interpret real world events. Simulation models built for this purpose differ from models meant to estimate numerical results, and we propose the name *qualitative simulation modelling* to mark such differences. Admittedly, our proposal leaves wide room for subjectivity, generally shunned by science. In part, this subjectivity can be reduced by using adequate technical tools. But in general, we conclude, this limitation is not a weakness of the method, but is imposed by the nature of the goals pursued.

1 Why Social Sciences Differ From Natural Sciences

In this section we contrast the typical problems faced by social sciences with those tackled generally in natural sciences, in order to show the differences of required methodologies. Indeed, the discussion should concern classes of phenomena: those that can be described by quantitative measures only, and those that necessitate a more diverse language. Many natural sciences study phenomena similar to those in Economics (e.g. biology) and some social science concerns squarely quantitative facts (e.g. demography). However, for the sake of readability we will refer to Physics to represent the class of quantitative phenomena and Economics for the other class. In both cases, our depiction of the disciplines will be rather naive and impressionistic. There are, indeed, phenomena in social sciences that can be captured by quantitative relationships, as well as natural phenomena eluding universal quantitative laws. The goal of this section is not to provide an assessment of the state of the art for science. Rather, we want to emphasize why the path of research historically followed in natural sciences cannot be replicated in fields like Economics.

1.1 Why natural sciences succeeded ...

It is universally accepted that the successes in natural sciences stem from the sound methodology established by the founding fathers of the scientific method, prominently physicists. Any school textbook dates the starting of scientific research when people gave up subjective and non-transferable knowledge of quasi-unique phenomena (as in Alchemy) and pursued objective knowledge of phenomena that can be universally observed or replicated.

Although there are still discussions about the precise nature of scientific methodology (details that we are not interested here), it is possible to identify universally accepted steps required for sound scientific researches:

1. Determine elements the theory is supposed to consider, and (at least implicitly) the elements that are not part of it.
2. Represent the elements by means of one or more numerical variables.
3. Provide mathematical representations linking values of the variables.
4. Produce experimental or observational data of the elements concerned.
5. Compare the theoretical results with those produced in the experiment.
6. In case of competing and non-compatible theories, support the one with better and more general data fitting.

The paradigmatic example of such a theory is the Newtonian model¹ of gravitation. This model can be defined as follows:

1. Consider planets in the vacuum, with no other disturbance.
2. Represent the planets by their masses, (relative) positions, and movements (directions and speeds).
3. Define the concept of gravitational force as a combination of other variables. Define movements of the planets as a function of their gravitational force.
4. Measure the position, movements and masses of observable planets.
5. Compare the available data with the models' predictions.
6. The model is so highly reputed, that when the observation does not conform to the theoretical results, it is the observation that is "adjusted" rather than revising the theory, as in the case of the discovery of new, distant planets.

The power of this method relies on the abstraction from any characteristics of the reality other than quantitative measurable variables². A planet is referred to only by means of a vector of numbers, and scientists manipulate only numbers. Obviously, since this approach gained momentum, the progress in physical theories and mathematics proceeded in parallel.

It is frequently sustained that the successes of physical sciences relies on the possibility to carry on experiments to test proposed theories. In fact, the actual reason for the success of the methodology in physical sciences lies on a different aspect, a pre-condition that, as a consequence, provides the opportunity for experiments. The pre-condition is that physicist manage to identify closed systems where the phenomena of interest are not influenced by uncontrolled, external factors. The model of planets' movements would have been proven wrong in a universe where the solar system had been continuously traversed by, say, large dark comets with dimensions comparable with those of the planets but non observable. In this case, though the Newton's law would still have been valid, any observation of the planets that ignored passing comets would have rejected the theory on the basis of the non conformity to the observed data.

All physical sciences adopted this successful method: replace real elements with numerical variables and focus on closed systems. I suggest that the history of failures and successes in any branch of science can be traced to the ability of identifying the correct elements to include into (and those to exclude from) the systems considered, and to determine the correct variables for their representation. Unfortunately, this method cannot work in social sciences.

1.2 ... and why social sciences cannot follow the same road

In order to replicate the successes of Physics in, say, Economics³, researchers attempted to replicate the same methodology: replace elements with numerical variables and produce

¹I am not interested here in differentiating the concepts of models and theories. I will use indifferently the two terms as synonyms, preferring to use the term model when referring to a symbolic representation of a reality, and theory when referring to a more general body of formal knowledge representing a large number of phenomena.

²Historically, this approach marked a decisive change in respect of previous ways (at least in the European tradition) to observe natural phenomena, where physical elements were associated to unmeasurable qualities.

³From now on I will use Economics as a representative for all social, or *soft*, sciences. The reason is that Economics, besides being my own field, is perhaps the most extreme example in the attempt to apply the acknowledged scientific methodology in non natural sciences.

mathematical relations among them accounting for observed phenomena. In this case, for example, a consumer is replaced by the quantity of a good demanded, a firm by the quantity produced, a market by the price and quantities traded. Suitable functions, like the demand and supply function, provide a neat representation of an economic system by means of a set of simple formulas linking the variables of interest.

However, the application of the scientific methodology inherited by physical sciences necessitates two major assumptions, limiting the scope for its application. The assumptions concern the possibility to represent the objects of study by means of a stable vector of variables, and the possibility to identify isolated, closed systems without external interferences.

Replacing real entities with quantitative variables is correct only for the most basic of the economic interactions. As soon as we move to consider slightly more complex economic phenomena, allowing, for example, for heterogeneity, innovations or growth, any mathematical representation becomes so complex to be totally intractable by the usual means of mathematical tools. The problem is that a real economic system does not change only quantitatively, but also undergoes structural changes. New entities enter in the system, and existing entities require different sets of variables to express their actual nature, and therefore their behaviour. Consequently, we have a dynamical system where the very number of variables is itself an endogenous, varying aspect of the model. This is not only a characteristic of social sciences only, but is a concern also in other disciplines. For example, two authors whose interest focuses on the borderline between chemistry and biology, write:

”Conventional dynamical systems, then, are well-suited to treat changes in the magnitudes of quantitative properties of fixed object species, but ill-suited to address interactions that change the objects themselves. The latter is challenging in the dynamical systems context. The relevant “variables” would have to hold objects, rather than the familiar numerical values. But if the objects become the variables of the system, we would need a “calculus of objects” like we have a differential calculus for numerical values.”

Fontana and Buss (1996).

The second aspect preventing the possibility to apply the methodology developed in physical sciences concerns the possibility to define closed systems for economic phenomena. “Natural” economic systems do not, in fact, exist. It is the observers who arbitrarily decide that a set of phenomena enter in the domain of economics, while others do not. For example, the 1929 Wall Street crash is an economic phenomenon, but the world wars are not. The global economic growth of the 60’s and “stagflation” of the 70’s are economic phenomena, but the social unrest of ’68 is not. Though everybody agrees on the interdependencies between economic and other social factors, the discipline insists in keeping artificially distinct what is inside and outside its domain. Though this approach is justified by the obvious need to delimit the kind skills requested, one needs not to forget that the boundaries between economics and psychology, sociology, history, geography etc. are artificial, arbitrary and, in the real world, simply non-existent⁴

⁴One may rightly sustain that any phenomenon, including natural ones, implies interactions among any element, and that close systems do not actually exist: the whole universe, Big Bang and the rest, is an open system. Though this is formally correct, our point can be translated in terms of intensity: social systems suffer from structural changes and external interferences much more dramatically than most natural systems.

Researchers in economics apply statistical tools to “clean” historical data series from exogenous shocks, attempting to obtain observations where the noise does not influence the signal. But the ratio of noise in respect of signal is huge in most cases, and the more so the more we move into micro-events. Worse still, the noise and the signal do actually interact, contributing to each other dynamics. Under such circumstances any support from the data to a model is, at best, weak. In the worst cases many different rationalizations of the same events can find some degree of support.

1.3 Is then Economics a science?

The above considerations seem to suggest that Economics cannot qualify as a scientific discipline, and many people share this view. Let’s see how Economics define itself to try to answer this question. We proceed analytically considering whether economic knowledge is of any interest and whether it can be obtained and shared by a reasonable number of people.

The domain of economics can be defined as the study of people pursuing goals under constraints and the effect of their interactions. Interestingly, Marshall himself insisted in qualifying which goals people are pursuing should be the object of economic attention, explicitly asserting that many human activities do not concern economics, though affecting and being affected by economic ones. That is, a man in love for a woman is not an economic concern, but the same person buying a rose does, as well as the relation between the average number people in love in a given year and the total supply of roses.

A. Smith founded the field by traveling through Europe and making acute speculations on what he observed. He did not search for equations and universal laws, but did provide qualified generalizations of the specific cases he observed. In his case, technological innovation and geography were crucial to make his assessments on economic advancements.

The above considerations show that the awareness of the “artificiality” of economics has always been acknowledged, but that does not help us to answer the question on whether Economics is a science. The chronicle of everyday life shows that there are relevant problems concerning economic facts. For example, consider the sustainability of social security systems in advanced countries, or the effects of the diffusion of Internet on existing industries. These problems are, both, relevant and hard to solve, but still the demand for expertise does not imply that such knowledge can actually be obtained. That is, there are many questions that people would beg for an answer, but that clearly cannot be the object of scientific research. For example, millions of people would be interested in a method to know in advance the winning number of a lottery, but nobody can (scientifically) claim that such goal can be obtained. There is therefore a third test that needs to be passed for Economics: is there some way to gain insight on economic phenomena? In fact, economic problems show this characteristic: that once enough time is passed by, it is (at least partially) clear why the result have been that specific one.

The comparison between the uncertainty *ex-ante* and the simplicity *ex-post* of many economically relevant problems, makes the study of economic mechanisms not only worth (since somebody has, after all, to make decisions), but also intriguing from a purely speculative point. In fact, in most cases it is easy to understand *ex-post* why certain results have been produced, however difficult has been to forecast the development in advance. For an example of this, take any old issue, say one year old, of an economic newspaper, and compare the discussions at the time with the developments that actually followed. The knowledge of future events is not only embodied in the actual realizations of random events (as would be the case of the lottery), or unexpected events (as the attack to the

New York's twin towers). The insight of future provides also the pattern linking past events with the present ones. Debates between parties holding opposite views, however reasonable appeared the respective positions at the time, appear almost pathetic from the perspective of future witnesses. From the privileged perspective of the future we can easily assess which party held the correct representation of the problem, and therefore the more correct expectations.

We can now state the goal of Economics: it should provide *ex-ante* the kind of explanations that, *ex-post*, are used to make sense of observed events. Whether this is sufficient to award the label of Science to Economics is not, after all, of major importance⁵. It is, instead, relevant that economic knowledge can indeed be generated. If the only *ex-post* explanations were the knowledge of the realizations of random events, than there would be no room for economic research, apart trying to assess the probability distributions of relevant random events. Instead, in many cases *ex-post* explanations are based on deterministic mechanisms driving the unfolding of events. The effects of these mechanisms can be marred by unique accidents and heavily subject to Knightian uncertainty. Still, knowledge of such mechanisms is economic knowledge available for grasp and worth to be pursued. What we need is a suitable definition of scientific knowledge, encompassing both universal laws linking quantitative measures on a stable set of variables and “softer” formats, as for economic phenomena.

2 Knowledge as causes

We have suggested that, whether deserving the title of “Science” or not, economic knowledge can, and is worth, be pursued. Since we have seen that the methodology developed in physical sciences cannot be applied, at least to large part of the domain of Economics, we need to look deeper at the nature knowledge we can cumulate, in order to develop a rigorous method for its collection, assessment and revision. In this section we suggest a quite bold proposal: a universal definition of the nature of knowledge.

We suggest to define knowledge as causal links. That is, we claim we have a piece of knowledge if we can explain the way by which a given state of the world, say A, can produce another state B:

$$A \xrightarrow{\mu} B$$

where μ represents some explanatory mechanism.

Mentioning causality and science is very daring, given the risk to enter an epistemological debate spanning from Aristotle to Bertrand Russel. We avoid to take position on whether causality is, or is not, *the* format to express scientific statements. We limit our claim to the fact that there exist phenomena begging for explanations, and that explanations are nothing else than causal mechanisms at work. Possibly (and likely), in many cases the results of science take other forms, more universal and less ambiguous than causality. In any case, we claim, whatever is the format of representing a theory, you need to be able to produce a derived expression that can be used to *explain* observed facts, that is a causal link.

Admittedly, we are threading a pretty convoluted path. We try to skip the debate on causality and science saying that one thing is objective truth (how the world really works) and another is what we can know. Of course, the former is universal and given once and

⁵We tend to support the position that “science” should concern (at least partially) objective and shared knowledge of some phenomenon, and therefore economists are, in the event, scientific researchers.

for all (no room for causality, there), while the second is moving and shifting. Our claim is that human’s way of making sense of the world necessarily can take also the form of causal links, besides, possibly, other formats. And that economic knowledge, given the characteristics discussed above, may well be constituted of only (or mainly) causal links.

Given the subjective role of the causal mechanism, we can give a very extended definition of “cause”. For example, mathematical expressions, like functions or theorems, can always be expressed in such a way, even though this format may not be the most efficient, or elegant, one. Consider, for example, the Pythagorean theorem. Given the two sides of a triangle forming a right angle (A) taking the square root of the sum of the squares (μ) we obtain the length of the hypotenuse (B). Going back to the example of Newton’s gravitational law, we can state the law as A one of all the possible combination of planets, masses, positions in the space and relative speeds, μ as the Newton’s equation, and B the position and speeds of planets in an interval of time. For sure the causal format may not be the most elegant, nor efficient, but it is always available.

As noted above, we do not claim that, in the real world, the length of two sides of a triangle does cause the length of the third one, or that the value of an independent variable causes the value of a function. But we claim that we generate this forms of explanations, and that we store knowledge as causal links. Any student of mathematics can testify that a numerical example greatly helps to understand the actual meaning of a mathematical statement, even if the proof has been clearly understood. Numerical examples clearly lose the generality of the statement, but allow a far greater understanding of the implicit consequences and of the mechanism at work.

Besides representing any mathematical statement, less formalized forms of knowledge can embody less assertive forms of causes, for example including qualifications like “sometimes”, “frequently”, etc. In any case, they always express a form of knowledge, though not necessarily a quantitatively precise ones. As it should be clear by now, the concept of explanation that we use is wide enough to entail every kind of knowledge used by people, being it formal scientific knowledge from textbooks to popular beliefs culturally transmitted.

Note also that the proposed format for knowledge embeds the possibility of infinite recursion. You can always refine an explanation by digging into more and more detailed explanations of the mechanism μ . Initially, for example, you can begin by assessing a correlation between two data series describing A and B . Then you discover that the correlation (general explanatory mechanism) has actually a specific functional form, so you revise your μ following an increase of your knowledge. Furthermore, you can still discover that the functional relationship is the aggregate statical result of an undergoing process, as the relation between the pressure of a gas in a container, heat and Brownian motions of molecules.

In a similar sense, but in opposite direction, one can link different pieces of knowledge together to form a larger, more comprehensive body of knowledge. Suppose, for example, that you can show that the initial state of an explanation/piece of knowledge $A \xrightarrow{\mu} B$ is actually the final result of another explanation $A' \xrightarrow{\mu'} A$. Thus, you can claim the explanation $A' \xrightarrow{\nu} B$, where $\nu \equiv \mu' \xrightarrow{\mu}$. Forming long chains of explanations is a frequent way to extend knowledge, applying to different fields knowledge obtained in others.

2.1 Classifying types of knowledge

As for know, even admitting that no counterexample to the definition of knowledge as cause can be found, we still need to provide a reason for using such definition. We begin

by asserting a taxonomy, or classification, of different types of causal links that can be used. Such classification is not necessary for the purposes we are pursuing, but will help in the following when assessing the potential of simulations to gain and represent knowledge.

As suggested above, knowledge can use any kind of explanation. However, there are only two classes of explanations which are conceptually distinct (conceptually because, in practice, the causal links used to make sense of real events are generally composed by a mixture of the two classes). The two classes are:

- Temporal causes: a mechanism that, with more or less detail, generates through time the final state
- Aggregative causes: the whole as “explained” by its components

For a temporal cause we mean an explanation that, provided some condition, relies on the simple flowing of time to explain the final state. As mentioned above, there may be cases where more detailed explanations of what happens during the time may be interesting, however, at some level, we accept that A can be turned into B by simply waiting long enough.

While the class of temporal explanations are pretty obvious, we need to focus a bit more on the meaning of “aggregative” causes. These refer to the explanation of properties of high level objects based on the properties of their components. The explanation can also flow the opposite way: properties of the components explained by the system they are part of.

For example, suppose you want to explain some property of a wall. You will likely need to consider the properties of the bricks composing the wall. The structured set of bricks and the wall are one and the same thing, and you will likely refer to a brick or to the wall depending on which aggregative level you want to make reference to. Notice that the a bunch of bricks do not constitute necessarily a wall. Aggregation causes may also include, besides the list of the components, also the way these are assembled, and any resulting properties.

In most cases, world events require an explanation composed by the concurrent actions of causes from the two classes. Some mechanism makes change through time one or more elements of a system, and these changes modify also the aggregate structure the elements are part of. Consider, for example, how we can express the knowledge about a group of firms with certain properties, like being competitive at international level. We will need to underline some crucial historical moments, where major changes took place, and the points of major system modifications.

Figure 1 depicts a possible explanation of a phenomenon B from an initial state A . The explanation consists of a set of intermediate steps. Firstly, the aggregative mechanism $M1$ shows how the the initial state, for example the technical skills of a group of workers, generate a higher level entity, for example a firm, referred to as $A1$. Then, $M2$ explains how, through time, $A1$ evolves in $A2$, as the entities undergo some change, for example the initial firm spawns a number of similar firms evolving some special type of expertise. $M3$ explains the formation of a further, higher level, type of entities $A3$, like an industrial cluster coordinating the initial firms. $M4$ explains the change affecting the highest level entity that turns, through time, into $A4$, e.g. how the cluster evolves specific properties through interaction with the external environment. $M5$ tells how the specific properties of the cluster are reflected in the composing firm. Eventually, $M6$ describes the time evolution of these firms.

From the example above, it is worth to note that the nature of causal links can be the most diversified. Verbal explanation, historical examples, mathematical relations,

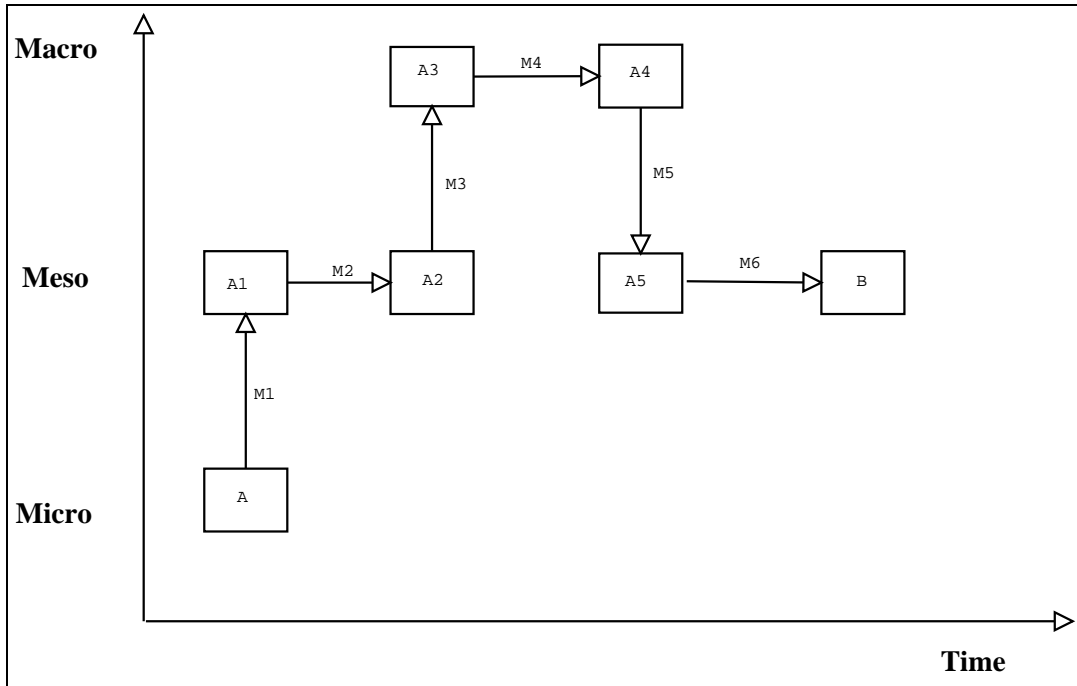


Figure 1: Example of a possible chain of causes to explain the transformation of A into B . The vertical scale represents different aggregation levels, with higher values referring to larger entities, composed by lower values, smaller entities. The explanation mechanisms $M1$, $M3$ and $M5$ are aggregative, while $M2$, $M4$ and $M6$ are temporal.

anything can be used, although, of course, each has different degrees of reliability and of scopes. This diversity simply reflects the different degrees of knowledge one has into a specific phenomenon.

2.2 Why has knowledge this format?

The format of knowledge as causes, or chain of causes, is claimed here to be the most general one, comprising every possible bit of what we can define as knowledge. Though this is not the place where to discuss the subject (also because we definitely lack the competences), it is worth to hint some supporting evidence for why people store knowledge in such a way.

Evidence from psychology, and from our own introspection, shows that people find much easier to remember things that are rooted in a chain of events, either a logical or a temporal one. For example, you are more likely to remember a telephone number if you manage to establish a functional link among its digits, however meaningless the relations may be. Whenever you cannot remember some detail, the natural way to fetch it back from memory is to try to reconstruct the context in which the detail is embedded, so that eventually the searched bit of information emerges from its connection with other elements, or flow of events, that contained it.

The physiology of the brain, as far as the writer can say, does not contradict such representation. Memories in a brain are not stored in specific locations, as bits in a hard-disk, but reside as configurations of neurons and synapses such that flows of electric shocks follow specific patterns. Again, memory is represented as connections among a flowing stream, rather than as static bits.

From the perspective of evolutionary advantage, the fact that the human species owes its superiority to its capacity to gain, store and share knowledge is unquestionable. In the natural context in which this advantage was developed, our forebears needed to make associations between early signals and consequences. The more elaborated where these associations, the more likely one could survive attacks, or find resources.

We have already noticed that our proposed definition of knowledge can be scaled in several degrees, depending on the precision by which the elements are defined. An implication of this is that knowledge can be easily revised by incorporating new evidence which increases its reliability. This means to revise the definition of the states A and B and of the mechanism μ under the pressure of new evidence, without the costs of building new knowledge from scratch.

From a speculative viewpoint, it is worth to note that representing knowledge as causal links implies to view pieces of knowledge as solutions to problems. That is, it seems that pure knowledge meant as objective, self-sufficient truth, cannot be conceived, and that we can only store in our brains solutions to problems. Of course, what constitute a problem worth to be solved is a subjective matter, depending on the circumstances and the preferences of the subject. What is not questionable is the format such knowledge assumes. An example of this is the different interpretation that different people provide when exposed to the same, identical, experience. Different people with different perspectives would place the same experience in a different context, and so would consider its explanation as made of a different chain of causes.

As a last point in this paragraph, it is worth to repeat that representing knowledge as a causal link does not limit its application. Any logical and mathematical expression can be reduced to this format, which also accommodates for fuzzier, but more diffused, forms of knowledge. That is, knowledge as causal links provides the most general definition.

2.3 Knowledge in Economics

In section 1 we suggested that Economics concerns the study of open systems where entities undergo structural changes. Therefore, from the methodological point of view we face a double challenge. On the one hand, available empirical evidence of economic phenomena is blurred by many aspects that do not concern primarily the domain of economics. On the other hand, the representation of economic phenomena cannot be based on a reliable and constant set of variables to represent economic entities, since these undergo structural changes. Therefore, we have very limited scope for the application of a research methodology based on quantitative relations to be validated by empirical measurements.

In the previous paragraphs of this section we sustained that knowledge is represented as causal links between states of the world. Mathematical, quantitative, causes are a powerful format of knowledge because, among other advantages, of its compactness and generality. But, lacking the opportunity to apply quantitative-based methodology, we do not need to conclude that rigorous science is not possible, but we simply need to scale back the format of knowledge to a “weaker” format.

Stating that in general economic phenomena cannot be explained by quantitative relations does not mean to deny that some specific portions of economic knowledge can be effectively represented by quantitative means. Whereas we identify an economic phenomenon where external influences are weak, and where entities can robustly be represented by a constant set of variables, then we can well aim at providing a quantitative model for it. However, we should not ignore that many (most?) of economically relevant phenomena do not respects these requirements, even as first approximation. Worse still,

a purely quantitative representation of an economic phenomenon is useless, unless the author can sustain the economic rationalization behind the numbers.

In the next section we will propose an analytical tool able to represent knowledge in its basic format of causal links, and that even allows upgrades to more quantitatively rigorous formats when possible: computer simulations.

3 Simulation modelling for the study of causal links

3.1 Qualitative simulation modelling

In order to make science, to gain knowledge on a given phenomenon, we need to identify constant elements, invariant in all the historical evidence concerning the phenomenon. The problem is that many kind of phenomena in Economics are represented historically by erratic (quantitative) results, with hugely different values. Consider, for example: the emergence and development of new technologies and markets; the effects of macro-economic policies; the impact of advertisement campaigns (but also, outside the domain of Economics, the evolution of a new species of organism, or the mapping between genotypes and phenotypes). Practically every example of such events is unique, with many special aspects that make a direct comparison based on quantitative descriptors impossible. However, all the examples of these events do have many things in common: forces that are in play are always the same, although with different impacts, and therefore producing different measurements. The invariant part is made of these forces, and these can be scientifically analysed.

In other words, we claim that many events in history take place in many different shapes, producing quantitative records with huge differences. However, all these events are part of the same class, composed by similar elements generating the same type of forces. A scientific analysis does not care, if not for purely instrumental purposes, to produce a theory generating exactly identical quantitative representations of historical evidence, but needs to provide an as detailed as possible analysis of the invariant set of forces, and their interactions.

Simulation models can be used for many different purposes. Actually, we may claim that computers have been originally invented to solve otherwise intractable mathematical problems by means of massive numerical estimations, the so-called *Montecarlo* method. In such exercises we have a set of unknown variables and a set of conditions among these variables that needs to be respected. The problem consists in finding values for the variables such that all conditions are respected. Analytical methods allow to identify the vector of solutions only if the conditions have a particular shape, for example, they are linear equations. For general conditions we don't have analytical solutions, so computers become a handy way to compute numerically solutions by simulating the intractable systems.

Another use of computer simulation tries to solve specific quantitative problems, such as forecasting future values of stocks or inflation rates. For example, suppose we have the data series for past values of a set of variables. We are interested in forecasting future values of some of these variables. We can generate a model linking the values of the variables, and search for the parameters in the equations that minimize the distance between theoretical values (i.e. those generated by the equations) and the actually observed ones. Future values can then be obtained by plugging new values for independent variables in the equations for the dependent ones, using the parameters estimated on the available set.

Finally, consider a function whose shape is highly irregular, for example a fractal or chaotic function. These functions are notoriously difficult to study, that is to appreciate

their global properties (e.g. maximum values) by means of analytical tools. Computers can be used to generate the values of the functions for a subset of its domain.

In all the above examples of simulations the primary interest is not to understand real world phenomena, but in obtaining specific quantitative results. They are numerical problems whose solution can be evaluated by well-defined error functions. The actual program can be *black-boxed*, ignoring its internal working, and evaluated only on the base of the distance between desired and actual properties of the solutions proposed. Whether the best results are provided by a realistic and finely tuned model, or by means of tea-leaves reading, it is not important: we ask for an answer that minimize the error function, wherever it can come from.

The type of simulations we are concerned here, for scientific research, is totally different. We are not interested in the quantitative consistency of the model's values with observed evidence, or minimization of an error function, but we aim at a *qualitative* understanding of some real world phenomenon, and our simulations should help on this purpose. *Qualitative simulation modelling* consists in replicating a portion of a real system used to investigate causal relationships in simulated events and transferring these causes (i.e. knowledge) from the simulated to the real systems.

In Economics, as well as in social sciences (and increasingly in many natural sciences), many phenomena, apparently complex, are interpreted as the result of simple, elementary operations by interacting entities. The problem is that when the elementary operations and the interaction takes some simple (non linear) forms, the overall result is unpredictable, apparently irregular, but not completely so. Lane (1993) proposed the term *emergent properties* to indicate phenomena that can be appreciated at a larger scale of that of the elementary elements. The interaction between high level entities and low level ones runs both ways: high level entities depend on low level ones because the latter are their components; but low level entities depend on high level ones because they constitute their environment, therefore setting constraints on their behaviour. In these systems changes at one level of the system can trigger a complex sequence of events reverberating in all levels, and whose eventual outcomes are unpredictable.

As example of an emergent property consider the behaviour of the groups of those species of fishes swimming in a semi-coordinated way. Each fish has been found following simple rules, which involves some random part and some systemic part. The systemic part of the individual fish consists in keeping an average distance from all the surrounding fishes, and changing direction when the neighbors do. The whole group of fishes shows amazing properties: when a predator attack the group the individuals on the boundaries run away. Even the members of the group that did not actually see the predator begin effectively to run away just replicating the behaviour of their neighbours. The group of fishes as a whole is extremely effective in protecting its members, for example opening "tunnels" around the predator, which is confounded by all fishes swimming simultaneously in all directions.

Researchers interested in the properties of these system cannot limit to observe in isolation only the high level entities or the low level ones, without missing crucial parts of the phenomenon. The observation of economic emergent properties in the real world is generally very difficult or impossible, because evidence on high level properties and low level behaviour is generally, at best, fragmented. To investigate the properties of these systems we need to reproduce a computational model of this reality and studying its properties. The aim is to understand how high level properties depend on lower level ones. The possibility to replicate on a computer a similar system as in the reality allows to observe details that are out of reach in real system.

Simulating systems of emergent properties is a quite different exercise from simulating

a model replicating a quantitatively specific phenomenon. The elements of the model are necessarily a very sketchy representation of the real world counterparts. Moreover, available evidence is frequently impaired by historical and accidental influences making each record unique. Therefore, the verbatim reproduction of historical records is extremely difficult and, even if possible, totally useless. In fact, a model reproducing one instance of the phenomenon would require the introduction in the model of many exogenous, *ad hoc* elements, that would need to differ in order to reproduce another instance of the same class. Instead, understanding (at least some) of the chain of causes and effects potentially available in a system is a general kind of knowledge. In some real world examples the understood mechanism can be more relevant, while in other less so, but still the mechanism is part of the invariant elements of the system, although its quantitative impact is not. Moreover, and crucially, causation mechanisms are systemic properties that do not need an exact quantitative measure to be replicated in an artificial system. Real world mechanisms are based on a logical sequence of steps that can be translated with adequate symbols, and, viceversa, simulated mechanisms can be interpreted back in real world events.

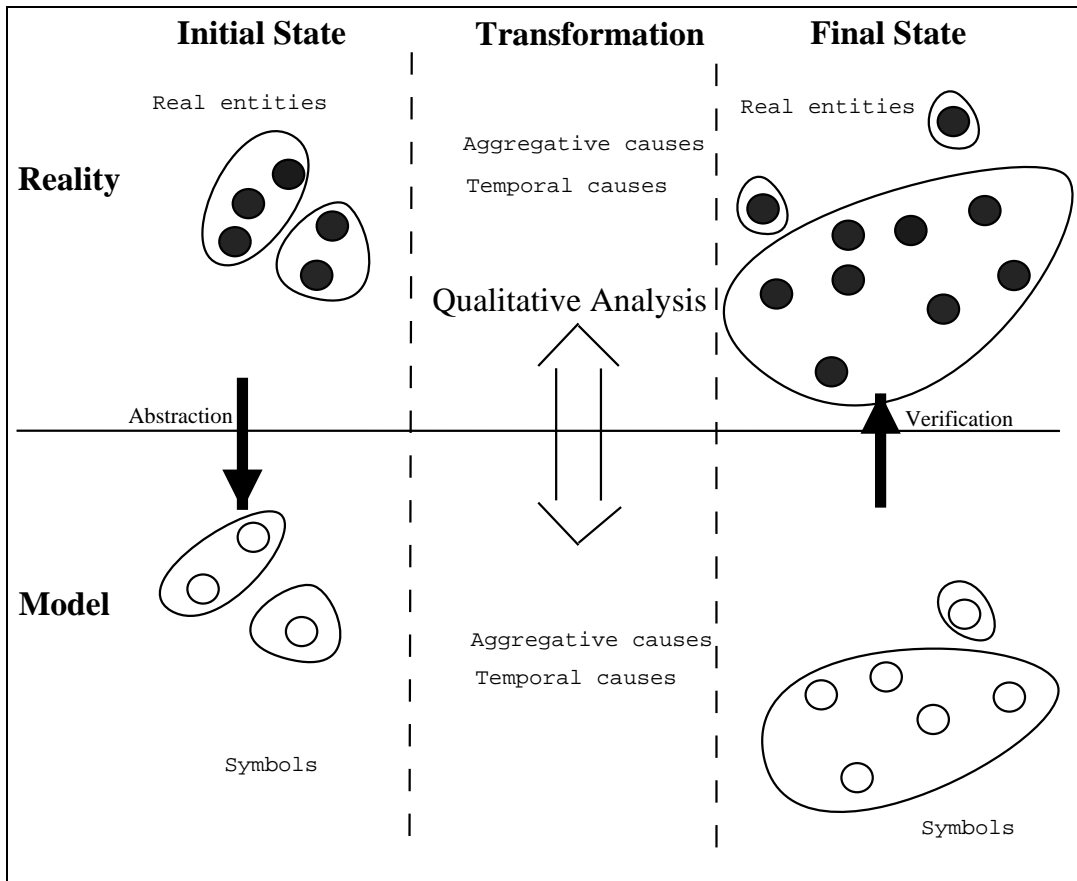


Figure 2: A model is a symbolic representation that starts with the abstraction of a selected reality, elaborate the symbols, and return a final state of the model, to be compared with reality. The validity of the exercise does not depend on the capacity of the simulation to replicate closely quantitative aspects of the reality, but in the possibility of understanding causal relationships in the simulated world that, hopefully, help to clarify their real world counterparts.

Figure 2 illustrates the methodological steps of qualitative simulation modelling, with an ideal path going anti-clockwise from the upper left corner. We start by observing real

world entities, and operate a selection on which we consider relevant for the phenomenon we intend to study. The model is built introducing a symbolic representation of some of the elements from the real world, in a process that we may label as *abstraction*. Running simulations generates a chain of (simulated) events, composed by aggregative and temporal causes. These events can be crystallised in specific states of the world that have a real world counterparts. We compare the simulated and the real world states to verify that our model does not degenerate in a process completely unrelated with the reality. But we do not need to have a precise quantitative adherence between simulated and empirical records, nor we would be satisfied by the mere reproduction of similar data sets. Rather, it is the knowledge we gain on the simulated phenomena, that is the *explanation* we can provide for those, that needs to be evaluated for their capacity to explain real world events.

The methodology we suggest apparently clashes with the pillars of what is considered the scientific method, promoting a return to qualitative (and therefore subjective and unreliable) evaluation of real world events, typical of the pre-scientific thought. In a way, this is true, but it is not a consequence of the method proposed, but of the phenomena we are concerned with. It is a property of the phenomena of emergent properties that history heavily matters, that quantitative indicators vary hugely. Compared with the universal laws ruling the physical world, the wisest statement on social phenomena is doomed to apply only locally, with a validity strictly limited to few temporal and geographical contexts. Yet, knowledge on these phenomena can be cumulated; history never repeats twice in identical ways, but a deep understanding of past events greatly helps to interpret present events and to forecast future ones. Well designed simulation models can increase the amount of knowledge by providing a controlled environment where complex interactions can be observed, their effects measured, counterfactual conjectures tested. Qualitative knowledge may not be the best, but still, it is better than no knowledge.

The scope of a methodology is defined by the nature of the research in which it can be applied. Here we imagine that the research questions to which qualitative simulations can provide useful insight concern the relations between micro- and macro-behaviour. Whenever the issue of interest concerns the product of many interacting elements, then simulations are likely the only viable tool of analysis⁶. In the following we support this claim discussing the relation between empirical evidence and theory implicated in the proposed methodology, and few guidelines on how to practically carry on research using qualitative simulations.

3.2 Empirical evidence and economic theory

The methodology for empirically based studies mandates a precise process of validation of the results based on the correspondence between observed data and those produced by the models. Basically, it requests model's data and the real ones to pass the test as being two samples from the same distribution. We already suggested that quantitative criteria cannot be applied (at least in general) for economic models. Let's see more in detail why it is so, and what alternative criterion can be used.

We suggested that economic phenomena take place in open systems and concern structurally changing entities. In this circumstances any quantitative measurement is forced

⁶It is worth to note that computer simulations can be extended to cover purely mathematical problems, and statistical analysis, either descriptive or inferential. A model developed for purely theoretical purposes can be fed with real world data series in order for simulated agents to face real problems. Mathematical results can be inserted as an endowment to the toolkit of simulated agents in order to transform the simulation in a decision support tool. However, we will not deal with these issues, that go beyond the scope of this work.

to concern only few aspects of the phenomenon of interest, and are likely to be heavily influenced by external events, that we don't want to include in the analysis. This implies that the little quantitative evidence we may have can be reproduced by many different processes. If we adopted the validation process of mere quantitative criterion, we would not be able to assess which model, among many different one, is "correct".

Consider, for example, a well known empirical evidence. In general (but not always), the distribution of firms' size on a market takes a skewed form, with few large firms, and larger numbers of smaller firms. Let's assume also that the evidence shows that the relation between size and frequency of firms is known and stable through all the markets observed. The theory should provide a model of firms' growth such to explain the evidence, but we don't have reliable and systematic evidence on all firms past dynamics, so we have to propose a theoretical model for firms' dynamics that matches the only available evidence at aggregate level. Unfortunately, this is impossible. In mathematical terms, the problem may be defined as underspecified: there are many different functions representing firms' growth that generate the same aggregate distribution. We cannot, based on quantitative criteria only, assess the validity of a model of firms' growth against other ones.

Other problems emerge when applying blindly purely quantitative criteria to assess economic phenomena. Assume we are analysing the distribution of firms' size and a measure of productivity, for example profits on capital. Whatever is the economic philosophy we may adopt, the very minimal assumption an economist must adopt is that more profitable firms grow more than less profitable ones, at least on average over a long enough time. There is no economic approach that assumes poor performance to be positive for growth, but, very likely, this is exactly what a statistical test would suggest. That is, "data show" that the smaller a firm, the higher its profits: any economists from A.Smith onward is proven wrong, and with very high significance.

Fortunately, a closer look at the method would save the foundations of economic theory. When we collect data on economic phenomena we need to pay great attention to what the data actually mean. Grouping together firms based on the category of their product only risks to mix non comparable evidence. For example, a data set concerning the market for cars would contain series for Ferrari, Porsche, BMW, Renault, Ford, etc. Though all these company do produce goods with similar content, they clearly serve different markets. Relative profits of luxurious brands cannot be compared with those for firms serving lower segments of the market. The results obtained ignoring this is likely to produce absurd results, since the dynamics of firms selling cars at 300,000 euros is not comparable that of firms selling products in the 20,000 euros range. Clearly, smaller firms (selling luxurious cars) would show higher relative profits.

The two examples above suggests that economic data require a different consideration in respect of those used in natural sciences. The unit of measures in natural sciences are perfectly defined, and, if any, questions about the quality of the data concerns only problems of measurement. Instead, in most of cases data from economic phenomena are indirect indicators, and different observations entail necessarily some difference of the very unit of measures. Neglecting such differences means to simply ignore the reality represented by the numbers, and is likely to produce totally wrong results.

Our claim that economic values are difficult to manage do not imply that they are completely useless. Rather, they need to be interpreted, to be placed in their context and used only within the meaning they have concerning the phenomenon of interest. Actually, quantitative evidence may even suggest research path that economic theory, on its own, has ignored. Consider for example the most frequent explanations for firms' growth, either absolute or relative. Explanations put forward by economists vary depending on the case

considered and on their theoretical approach, but mostly would refer to differential of costs, prices and/or technological innovation, either that concerning the production processes or embedded in products. But even a casual look at highly aggregated accounts of firms would suggest a totally different explanation. In fact, for the majority of firms marketing is the single highest item of expenses, ahead of production costs, R&D and administrative overhead, and the few cases where it is not the case it is very likely it ranks second. For example, in the recent years the South Korean company Samsung moved (in about a decade) from being a humble producer of low-end television sets and electric ovens to be the leading firm in practically all advanced consumer electronic markets, with total sales larger than Japanese Sony. Such technology oriented firm, which is also by far the leader in production of advanced memory chips is reported⁷ spending 8% of revenues in R&D, or \$2.9 billion, and \$3.0 billion on marketing. The only highest item of expenses is capital spending (\$5 billion) due mainly to the introduction of new plants for the next generation of flat screens, though this is a one-off expenditure likely to disappear in the coming years. Similar figures can be obtained for other high-tech as low-tech companies, but economic theory simply ignores the existence of such expenses, not to mention trying to provide an explanation for it. An economic analysis trying to explain growth would need to, at least, cite the main costs of firms.

Empirical evidence is a crucial part for building a theoretical explanation of economic events, but that traditional methods to manage data cannot be unquestionably transferred from natural sciences to Economics. Economic figures lack the reliability of the unit of measure necessary to apply statistical analysis, but they can provide hints and testing for theoretical conjectures. This is not the place to discuss why economic theory fails so blatantly in accounting for the empirical evidence concerning marketing expenses, or even if this evidence is actually relevant. It is, however, worth to mention that the importance of marketing is very likely to differ greatly in different systems through time and social realities. This is a remind that whatever model we want to build is bound to apply only to few specific cases, and that universal laws accounting for general evidence is impossible, and even uninteresting. Still, there are regularities common to all markets in any time period, and they concern the forces at work in the economic interaction among firms, consumers, workers, public authorities etc.. Though no set of data would perfectly represent these regularities, because of relevant differences in historical, technical, political etc. spheres, still we may investigate them in abstract terms in order to build a body of knowledge, that (we suggest) take the form of explanations. Such pieces of knowledge can be used for different purposes in different contexts, for normative as well for descriptive goals, and even modified to account for novel events. As we already noted, the fact that no more robust claim (in quantitative terms) can be put forward in Economics does not depend on the scientific method adopted, but on the very nature of the subject.

3.3 Evaluation of qualitative simulation results

We have suggested that there are no reliable objective criteria to assess the scientific truth of models for many economic phenomena. But we also suggested that economic knowledge has the form of causal links, and here we discuss how simulation models can help in discovering and supporting this format of knowledge.

Simulation models implementing a relatively large number of interacting elements, however simple can be the actions available for those elements, generate easily pretty complex results. However, it is also very likely that the interactions forces some form of

⁷ *The Economist*, vol. 374 n. 8409, January 15th 2005.

coordination among the elements. The objective of the researchers is to study the (loosely) coordinated masses of elements and their behaviour, either spontaneous or responding to events in the environment.

Analysing these kinds of simulation results can be, admittedly, pretty difficult. In fact, one has to surf thousands of data series trying to find some form of coordination (or lack thereof), which can become a technical challenge. However, the very fact that the computer generates these series means that we already have the tools to deal with this problem, and increasingly sophisticated programs allow a simple management of such huge data sets. In any case, the goal of the simulation analysis consists in finding out how observed dynamics depend on the assumptions used to build the model, namely the variables and the code implementing their dynamics. The source of knowledge available in simulations are the explanations for unexpected simulated events based on the initial conditions.

Notice that the mere similarity, even when statistically sanctioned, between data produced in a simulation run and those from a real world example cannot, in any case, support the model as valid. As we already discussed, empirical evidence is, in general, so vague that many different, and opposing, explanations can be provided. The capacity of the model to generate patterns similar to those observed in real cases must be a pre-condition to the actual analysis of the model. Using the notation we introduced above, showing that the initial state A generates state B in the model as well as in the real world allows to start the knowledge-increasing activity of discovering what explanation chain μ is at work in the simulation, with the hope that this provides insights for the real world cases.

Clearly, there are several degrees in the validity of the explanations provided. Phenomena directly and obviously descending from the assumptions of the model will not provide surprising results. At the opposite, if the analysis of the simulation runs manages to explain phenomena apparently contradictory to the assumptions of the model, and similar to many real world examples, then they can be considered highly relevant.

Frequently simulation models make use of random events, and therefore results vary depending on the random sequence used. Analysis of the results based on the study of the frequencies cannot substitute the analysis of the causes of the simulated events, though the frequencies of different results may be relevant for some specific purposes. Suppose, for example, that a model is simulated many times with identical initialization and different random sequences, and that 90% of times a given result is produced, while 10% of times we observe a different behaviour. If we had to bet on the most likely result of the model, or of the reality simulated, we would be justified in going for the most frequently observed result. But for scientific purposes we are interested in understanding the mechanisms generated during a simulation run, not in forecasting its result. We need to explain the differences appearing during the simulation runs that produce the bifurcation, and it is this explanation that makes a model more or less useful for scientific purposes. And, in the process, providing the ability to early understand in which direction the system is going, which may be an interesting capacity on its own since would allow to win our gamble 100% of times instead of 90% only.

How to provide support for the conclusions one reaches with the proposed method of using simulations is a difficult task, and we can only suggest few tricks. For example, if the claim we want to put forward concerns a necessary condition, then we may test the model running a simulation run under identical conditions but removing the supposedly necessary event. Of course, if the phenomenon still appears, then our condition was not so necessary after all, otherwise, the claim still holds.

The use of simulations we are supporting is to consider simulation runs as virtual histo-

ries, that supposedly replicate a class of real world events on which we have few examples with scarce data. Instead, simulation runs can be executed as many times as we need, and each and every detail can be observed. Moreover, contrary to real world cases, we can intervene during a simulation run to introduce shocks, and compare results with and without those shocks. As programmers, we have total control of the simulation model content, but the sheer complexity stemming from many interacting entities makes impossible to understand the consequences of apparently innocent implementation decisions. Indeed, the goal of simulation analysis should be precisely to clarify these consequences, and the less obvious and intuitive are the consequences, the more relevant is the model in order to increase our knowledge, provided that we can explain the link between changes and consequences.

Whoever had experience of using simulation model can testify the resilience of the model to generate some result. Sometimes this is a positive thing, when the models behave along with our expectations, while other times this is apparently negative, since the model rejects the intuition of its creator. However, even in this latter case the overall balance in the use of simulation model can be turned to be positive. In fact, when the model behaves contrary to the expectations we can (actually, we must) understand where our intuition departed with the model. It may be that the model does not, in fact, implement correctly the assumptions we thought it should. Or it may be that our expectations did not consider some relevant factor. In both cases we have improved our knowledge and we can carry on our research, either by fixing the bug, or by moving the analysis to a further level.

It is worth to spend few words on the most frequent criticism raised against simulation models. They come in three formats: has the model proved to be robust against random variations? did the modeller explore all the possible combinations of parameters' values? did the model passed a validation test with real world evidence? The discussion above has already answered all the three issues, but a quick remind may be useful.

The three questions assume implicitly that the model results take the form of quantitative claims. That is: that the model average values fall in a specific range, but for a random factor; that the results claimed are consistent for any parameterization; that the simulated data series replicate observed evidence. If the modeller actually presents this sort of claims, then he has the burden of the proof along the methodological lines borrowed from natural sciences, and implicit in the above criticism. Conversely, we have sustained that simulation models can be used in a different way, as a testing to understand the chain of consequences from the assumptions embodied in the model structure and code. In this latter case the support for the model result should be more similar to the method used by historians, a chain of explanations supported by logically consistent analysis of examples. In this latter sense, it may well be possible that rare and exceptional cases are the keys to illuminate a generally confused phenomenon, opposing the physical sciences assumptions that the norm only should be considered, and the exceptions discarded. Physiology is another example where the understanding of the general cases proceeds mainly through the study of the pathologies, that is the rare exceptions. The connection among History, Economics and Physiology (but also Biology and possibly many other scientific fields) is that we deal with complex systems, where the consequences of one event are difficult to forecast while they are transmitted through the rest of the system. The simulation methodology we propose as *qualitative* is meant to highlight how the consequences are transmitted, and not, for example, whether they are more or less commonly observed.

4 Conclusions

This work is composed by three different arguments that, though forming a unique line of thought, can also be discussed separately, and the refusal of one does not necessarily disrupt the relevance of the other parts.

We have initially discussed the nature of social phenomena, as those studied in Economics, and of natural phenomena along which the scientific methodology has been developed. We concluded that the paramount importance accorded to quantitative aspects in natural sciences cannot be shared, in general, by social sciences. Two arguments have been used to support the claim that social phenomena radically differ from natural ones: firstly, they pertain open systems, so that no observation of social systems can be considered free of interferences from interacting but external events; secondly, social systems involve structurally changing entities, preventing the very identification of the relevant variables, not to mention their measurement.

The second part discusses what kind of knowledge, then, can be envisaged without roots in objective measurements. We proposed to return to a definition of knowledge based on the concept of explanation, or causal links among events. We have sustained that this is a weaker format of knowledge in respect of quantitative relations, but it is also a “rawer” version, so that, in fact, it is more comprehensive, including any other format. Resuming to use this definition of knowledge is based on the acknowledgment of the nature of social phenomena, and does not prevent, where possible, to upgrade to higher, more objective, formats of knowledge.

The last part discusses the use of what we proposed to call *qualitative simulation modelling*. In respect of simulation models built to explore quantitative properties by numerical means, qualitative simulations aim at the analysis of the chain of causes linking the simulated events. The proposed use of simulations is based on the comparison between the explanations of simulated events with the explanations of real world events.

As mentioned, the reader may well disagree with any one of the above issues while finding others more convincing. However, the main issue we consider is that simulation modelling is a tool able to embody all the knowledge we can gain on social phenomena, even including portions of theoretical knowledge that can take a more traditional form of quantitative relations. In fact, we implicitly suggested that, for example, mathematical theorems can be translated into simulation models. An extension of the present work may aim at showing with greater detail how the standard toolkit of Economics, as for example that contained in a undergraduate text book, can be implemented as sets of simulation models.

A topic that we did not discuss in the body of this work concerns the technical requirements that a programming language should satisfy to perform the role requested by our proposal. In fact, we require the researcher to get directly involved in the process of writing the code of the model, analyse the results, perform tests etc. Such activities require a certain degree of programming and statistical skills that cannot be avoided by outsourcing the technical work to experts. In fact, we sustain that theoretical advancements are strictly depending on these technical activities and delegating contractors to solve technical problems would prevent the very possibility to gain knowledge with simulations. However, forcing an economist to write and analyse simulation programs does not require he or she to become an expert computer scientists. By using the appropriate tools one can be exposed to all the theoretically relevant aspects of modelling without the necessity to solve merely technical problems. For example, controls on the consistency of the code, documentation of the theoretical content of the model, management of the initial-

ization and results, management of the problems emerging when merging different models are all operations that can be largely solved by specific programs. Details on the currently available tools can be requested to the author, and their improvements constitutes the natural prosecution of the proposal presented here.

References

- FONTANA, W. and BUSS, L. W. (1996), “The Barrier of Objects: from Dynamical Systems to Bounded Organizations”, in J. CASTI and A. KARLQVIST, eds., “Boundaries and Barriers”, Addison-Wesley, Massachusetts.
- LANE, D. (1993), “Artificial Worlds and Economics. Part 1 and 2”, *Journal of Evolutionary Economics*, **3**, pp. 89–108, 177–197.
- SIMON, H. A. (1972), “From Substantive to Procedural Rationality”, in C. MCGUIRE and R. RADNER, eds., “Decision and Organization”, Amsterdam, North-Holland.