Revue Internationale de
systémique

ISSN 0980-1472

Vol. 9, N° 5, 1995

afcet

DUNOD

AFSCET

Revue Internationale de systémique

Revue
Internationale
de Sytémique

volume 09, numéro 5, pages 461 - 489, 1995

An epistemological framework for system dynamics modelling

Magarita Vásquez, Manuel Liz, Javier Aracil

Numérisation Afscet, août 2017.

REVUE INTERNATIONALE DE SYSTÉMIQUE
Vol. 9, N° 5, 1995, pp. 461 à 489

AN EPISTEMOLOGICAL FRAMEWORK FOR SYSTEM DYNAMICS MODELLING

Margarita VÁZQUEZ [1], Manuel LIZ [2], Javier ARACIL [2]

Abstract

Among the various techniques of modelling and simulation developed in the context of systems research, System Dynamics occupies a central place. This paper analyzes some epistemological problems concerning modelling and simulation through System Dynamics, mainly the sense in which System Dynamics models endeavour to grasp or represent relevant aspects of reality. Two main perspectives have being advanced with regard to this topic: the naive realism linked to philosophies of science such as those of logical positivism and critical rationalism, and the relativism which emerged from the crisis of said philosophies. We examine these perspectives by giving a general overview of the philosophy of science in this century. Then, we try to show that some recent epistemological proposals, such as the "internal realism" of Hilary Putnam, are able to offer a new third way which lies between naive realism and relativism. The "internal realism" of Hilary Putnam should be able to clarify many of the features of System Dynamics modelling, especially the important role mental models play in it.

Résumé

Cet article étudie quelques-uns des problèmes épistémologiques soulevés par la modélisation et la simulation à partir de la Dynamique des Systèmes. Il s'agit de savoir en quoi les modèles de Dynamique des Systèmes saisissent ou représentent les aspects pertinents de la réalité. Deux points de vue ont été développés à ce propos : le réalisme naïf associé au positivisme logique et au rationalisme critique d'un côté ; le relativisme, qui émerge d'une crise de ces philosophies de l'autre.

1. Facultad de Filosofía, Dept. de Lógica y Filosofía de la Ciencia, Universidad de La Laguna, 38203 Canary Islands, Spain. E-mail: mvazquez@ull.es; aliz@ull.es
2. Escuela Superior de Ingenieros, Universidad de Sevilla, Avda. Reina Mercedes s/n, 41012 Sevilla, Spain, E-mail: aracil@esi.us.es

Rev. intern. systémique. 0980-1472 Vol. 9/95/05/ $ 4.00/© Afcet Gauthier-Villars

Nous examinons ces approches à partir d'une vision générale de la philosophie des sciences contemporaine. Nous tentons de montrer que certaines propositions épistémologiques récentes, comme le « réalisme interne » d'Hilary Putnam, sont en mesure de constituer une troisième voie intermédiaire entre le réalisme naïf et le relativisme. Le « réalisme interne » de Putnam devrait permettre d'éclairer bon nombre des caractéristiques de la modélisation par la Dynamique des Systèmes, notamment le rôle important qu'y jouent les modèles mentaux.

I. INTRODUCTION

In the wide context of systems research, the building of computer models of simulation has recently become one of the areas of major interest and growth. Among the various representational and computational tools for modelling and simulation developed in that context, System Dynamics (SD) has had an increasing importance and applicability since the 60s, especially in the fields of social sciences and economy. SD is a complex scientific and technological activity deserving of a serious epistemological analysis. The epistemological analysis of modelling and simulation through SD is important both to practitioners and theorists of SD. At the same time, this analysis also suggests some new and interesting perspectives in the general context of systems research.

In this paper, we will focus on the sense in which SD models can grasp or represent certain relevant aspects of reality. This will offer a special kind of genuine explanation and deep understanding, even though we can always have a plurality of models and some of them can be empirically equivalent. We will pay particular attention to new developments in recent philosophy of science.

We want SD models to have the most realistic content possible, as their intention is to grasp or represent certain relevant aspects of reality. There is a great difference between, on the one hand, purely correlational or statistical models and, on the other hand, SD models. SD models are intended to be devices useful for forecasting and control. However, SD models also try to offer explanation and understanding, even some sort of causal explanation and causal understanding. In relation to this, one of the main epistemological problems we must confront is that it is very difficult to arrive at formal (*i.e.*, logical or mathematical) restrictions that enable us to select and justify, among all the possible empirically equivalent SD models that could be built, the more realistic ones.

We know that, formally, the same quantitative data contained in our reference modes or temporal series, the same empirical behaviours, can always be generated by many different structures. Furthermore, there is no formal way to choose among them, or to obtain the ones with the most realistic representational content (from very different perspectives, this has been pointed out by, among others, Aracil 1986; Aracil, Vázquez and Liz, 1990; Putnam, 1981, 1983, 1987, 1990; Searle, 1980, 1984; Vázquez and Liz, 1989; and Zeigler, 1976, 1984; and is very close to some theses of Quine, 1968). The only possible strategy to decide among different empirically equivalent SD models appears to be not formal, but rather, linked to the non-formal ways in which real systems can be in touch with our knowledge of them.

Authors related with SD have brought up various reflections with regard to this issue. Some have suggested that the epistemology of critical rationalism, that is, a falsationism along the lines of Popper, could fit SD procedures (Bell and Bell, 1980; Bell and Senge, 1980). However, critical rationalism has often been criticized because it provides an idealistic view of the scientific enterprise. Critical rationalism is based on the premise that it is possible to establish a general logic of scientific research, a logic of conjectures and refutations able to assure a progressive approach to an objective truth. There is no doubt that critical rationalism marks a deviation from the rigid inductivist epistemologies of positivism, mainly from the verificationism of the logical positivists, but its insistence on the demands of a single method for all scientific knowledge has the same negative effects as those of logical positivism. In the images of science provided by both perspectives, knowledge is understood as the discovery of the structures possessed by reality itself, independently of our entire epistemic contribution. In order to know, it would be enough to decide be rational and to follow the general method of science.

But, if we want to analyze the sense in which SD models may have explanatory power and may help us understand complex real systems and manage our actions through them, we need to go back to the real and concrete subjects of knowledge. It is necessary to bring into the process of knowledge all the elements overlooked both by positivism and also by critical rationalism. And, at this point, we must give consideration also to the insights of all the philosophies derived from Kuhn, of the various pragmatisms and holisms, of the structuralist philosophies of science, and of other traditions which, although falling outside the strict area of philosophy of science, as is the case of phenomenology, hermeneutics, and so on, are nonetheless able to detect very well many of the failures and shortcomings of the so-called "myth of science".

In this vein, other authors related with SD have thought that a relativistic epistemology would offer an adequate framework for the justification of the claims of SD models (*see*, for instance, Barlas and Carpenter, 1990). By relativism, these authors mean some sort of moderate or practical form of relativism in which all epistemic justification is contextual and relative to specific interests and purposes, not an extreme relativism. Indeed, extreme relativism can say nothing about justification, whereas moderate relativism can. Extreme relativism is not an alternative epistemology and philosophy of science, but the rejection of all epistemologies and all philosophies of science.

Moderate and practical forms of relativism, such as those defended by Barlas and Carpenter, are able to make sense of many aspects of SD modelling, for instance a valid SD model is one that proves to be useful with respect to a set of particular interests and purposes. The difficulty with this epistemological point of view is that it cannot say much more. And even though usefulness and success are properties which must be considered of importance to consider a SD model to be valid, in themselves, with respect to our interests and purposes, usefulness and success cannot be the only properties which define the validity of these models. As we have said before, we want, for our valid SD models, some kind of real explanation and understanding, and these are things that do not fall within the narrow scope of moderate relativism.

Consequently, we cannot rely on any of these perspectives (logical positivism, critical rationalism or falsationism, or relativism) in our attempts to find a solution for the epistemological and methodological demands of real explanation and understanding in SD. Fortunately, there exist nowadays other philosophical perspectives that can help us. In this paper we propose the "internal realism" of Hilary Putnam (*see*, for instance, Putnam, 1981, 1983, 1987, 1990) as an useful philosophical orientation to clarify some of these conceptual problems. The perspective offered by Putnam allows us to make sense of how some kind of realism, explanation and understanding are possible even though there is no privileged single model, or set of models, able to grasp every single aspect of a real system, and even though there are no formal restrictions that allow us to select and justify the specific structure posited by certain models ahead of all the alternative structures able to generate the same empirical behaviours. The "internal realism" of Putnam helps us to clarify these points by preserving a central place for aspects such as the interactive character of the modelling process and the role which mental models can play in that process as an unavoidable source of knowledge (about that, *see* Meadows, 1980).

II. MENTAL MODELS, REAL SYSTEMS, AND SYSTEM DYNAMICS MODELS

In SD modelling, three main kinds of knowledge converge: 1) the structural knowledge of the systems usually provided by mental models, 2) the quantitative knowledge related to the so called "reference modes", "temporal series" or "empirical behaviours", and to the initial conditions in which real systems are placed, and 3) the operational knowledge involved in the specific strategies for building SD models and extracting from them dynamic consequences and certain policy actions.

It is essential to have these three kinds of knowledge included coherently in our SD models. While empirical behaviours give the quantitative data and anchor the quantitative results and predictions of SD models in reality, mental models give information which is not so much quantitative as structural. Mental models are sometimes supplemented with particular applications of available theories but, of none are available, they constitute the only source of structural knowledge concerning the systems being modelled (Forrester, 1986). In any case, and even with the help of theories, mental models are fundamental both to postulate a certain structure and to steer our action with the help of SD models.

Lastly, SD operational knowledge gives the basis from which it is possible to articulate all the information, ideas and hypotheses. It is constituted by the specific SD skills and practical knowledge that the modeller uses when the other two kinds of knowledge are integrated obtaining a very special formal representation: the SD model. SD models are a kind of computer model. The tool provided by SD operational knowledge is an adaptation of the mathematical theory of dynamic systems to the representation of some structural and dynamical aspects of reality. The end product of the modelling is a formal or abstract representation, a model in SD format, which, with the help of computers, enables us to broaden and make clear and operative both the previous more or less intuitive knowledge provided by mental models and the quantitative knowledge obtainable from reality.

Of the three kinds of knowledge ennumerated above, mental models seem to be of particular importance. We can assume that mental models are a sort of psychological construction with an intended representational content. Mental models lead to certain descriptions of reality that are usually expressed by a set of sentences in ordinary language, describing both the interactions among the elements within the system and their external influences. These sentences describe the qualitative way in which a change in one magnitude leads to variations in another.

In many fields it is usual not to have any other structural knowledge available except for the one coming from mental models, with practically no theoretical component. The modelling of complex social and economical systems is a perfect example of this. In these cases, when no other more specific theoretical knowledge is available, mental models become the main source of knowledge that enable us to articulate and express our ideas with regard to the structure we hope to find in the real system, especially our ideas concerning the basic structural components and relationships that could generate its behaviour. Mental models are a basic and indispensable source of knowledge in the SD models building process. In a nutshell, SD models must be guided by mental models (*see* Meadows, 1980; for a general analysis of the various concepts of model in SD, *see* Liz and Vázquez, 1992).

However, mental models are very poor with respect to obtaining dynamic consequences or providing precise knowledge of behaviour. From mental models, and with the help of the two other kinds of knowledge (2 and 3 above), we build the formal SD models. SD models are a kind of computer model in which a given mathematical structure is able, over time, to generate the possible behaviours of all the variables considered. SD models refine mental models and let us identify their dynamic properties in a much more precise way. In particular, SD models enable us to explain and understand how the relationships among the structural elements suggested by mental models are able to give rise to certain relevant behaviours. In consequence, SD models are guided by mental models but mental models must be also clarified and improved by SD models. And this is so in two senses: 1) with respect to the possible dynamic consequences of the structures assumed in reality by our mental models, and 2) with respect to the decisions and policy actions to be adopted.

At this point, it is necessary to note three important attributes of mental models:

1. Mental models are not fixed; they change with experience, action and discussion, and also through the SD model building process.

2. Mental models are not simple; they contain rich and relevant information about the basic components and structural relationships of the systems in which subjects are involved.

3. The structural information that mental models provide about certain systems, those which are the result of human decisions and actions guided by these same mental models, is usually highly reliable, for example, socioeconomic ones.

In other words, mental models can be said to be strongly interactive and to have a very rich and relevant representational content regarding the structure of the systems; moreover, in some cases, this structural information is highly reliable.

SD models are rational structures that generate a formal behaviour which must fit the empirical behaviour of the system being modelled. Thus, for a model to be accepted as valid, in the first place, it is necessary that the hypotheses used to build the model be compatible with the available scientific or heuristic knowledge. And, secondly, these hypotheses must be able to be captured adequately with the representational tools of SD language, and all this information must be properly processed to reach conclusions that fit the empirical behaviour. So far, we have some sort of empirical adequacy able to offer forecasting and control. However, the validation of our SD models requires something further. It also requires an explanation and understanding of the structures that really work in the systems. SD models must provide explanation and understanding. That is a third element necessary for the validation of SD models in addition to their empirical adequacy. Only in this way is it possible to clarify and render operative the previous mental models upon which SD models are based.

A SD model is the final result of the progressive refinement and formalization of a mental model or set of mental models, and this refinement and formalization is not merely quantitative. One important aim is to obtain a formal model that can generate the adequate empirical behaviours. But it is also very important to obtain a formal model able to explain and render comprehensible these behaviours. The formal model must allow us to understand and explain how behaviours are generated from the assumed structures. Thus, the role of our SD models is not only to generate a certain behaviour, but also, to a certain extent, to explain and understand how this behaviour is generated. And this directly involves connecting up with the way in which mental models focus on real systems.

As we have noted above, mental models are highly interactive, have a rich and relevant representational content, and this content sometimes provides highly reliable structural information. With the help of other quantitative empirical types of knowledge, coming from reference modes, initial conditions and so on, SD models are able to formalize, improve and extract precise and quantitative dynamic consequences from mental models. Perhaps the same quantitative results could be obtained by means of correlational or statistical strategies. There are other possible modelling techniques that, although totally unlike our use of mental models, can achieve more or less the same forecasting and control power. But, if SD models require explanation and understanding, and not merely forecasting and control, they must link up with and be in constant touch with mental models. This is one of the most distinctive and important characteristics of SD modelling.

The difficulty encountered here is that it is impossible to find formal (logical or mathematical) restrictions that enable us to select a single SD model, or set of SD models, as having the intended most realistic representational content. Sometimes, we have empirically equivalent SD models, that is, several alternative SD models able to reproduce the same empirical behaviours. On other occasions, we have to choose between a large and a small SD model, etc. These problems undermine considerably the claim that our SD models grasp or represent relevant aspects of real systems, and that their epistemological justification can assure these realistic claims. What is meant when it is said that a given SD model represents an aspect of reality? What is the scope of our claim that we represent faithfully, through our SD models, several aspects of real systems? What does the SD model capture from reality, or, to put it in another way, what does the SD model reproduce of reality?

We will analyze these difficulties in greater detail, although first, we wish to give an overview of the general problem of the justification of knowledge through some of the more recent developments in the philosophy of science.

III. AN OUTLINE OF THE PRESENT PHILOSOPHY OF SCIENCE

There has been a good deal of discussion in the philosophy of science since the beginning of this century that can help us better understand some of the conceptual problems involved in the elaboration and justification of our SD models. The importance of these philosophical reflections for the theoretical development of SD has been underscored on several occasions (*see*, for instance, Barlas and Carpenter, 1990; Bell and Bell, 1980; and Bell and Senge, 1980).

III.1. The Origins

In the second part of our century, philosophy of science is in marked contrast with the conception of it which prevailed in the first part. The latter conception meant the genesis of the philosophy of science, in the way we understand it today. The main components were the logical atomism of Bertrand Russell and Ludwig Wittgenstein; the logical positivism, or neopositivism, of the Vienna Circle and of the Philosophical Society of Berlin, which grew up around Schlick and Reichenbach, respectively; and developments in logic introduced by Frege, Russell and by the Polish School of Logic, with Lesniewski, Lukasiewicz and Tarski, among others. (The following references are useful: Achistein and Barker, 1969; Ayer, 1959; Dummett, 1973; Griffin, 1964; Kraft, 1950; Russell, 1956; Tarski, 1941, 1956.)

All these authors and groups maintained strong links among themselves. They defended positions close to empirism, on the one hand, and, on the other, to rationalism. They asserted most of the philosophical views of classical empirism (Francis Bacon, Locke, Newton) and of Comte's positivism. But at the same time they used the formal tools of symbolic logic, looking for the clarity and the precision that classic rationalism (Descartes or Leibniz) had sought with the individual use of reason.

In this conception, experience was the basis of all knowledge. For some authors, like Carnap or Russell, this empirical basis was made up of sensory data; for others, the majority (including Carnap in another period; *see* Schilpp, 1963), by the physical world. In any case, a point of contact between knowledge and reality was considered necessary. This compromise with reality was essential to confer content on all our knowledge.

However, although experience was the basis for all knowledge, it was necessary to distinguish between the genesis of knowledge and its justification. The genesis of knowledge could be subjected to changes in history, sociology, economy, biography, etc., and so resist our rational analysis. However, the justification of knowledge was a rational affair. Knowledge, science, was basically understood as being a language with an empirical content and with a logically analyzable formal structure. Empirical and conceptual controls of knowledge had to be in tune with a certain logic. To discover that logic was the main goal of this philosophy of science, and an important part of such a logic consisted in a rigourous systematization of the relationships established between the singular statements of that empirical basis of science and the general statements of its theoretical elaborations. These relationships were thought to be mainly of an inductive and verificationist type.

Due to the existence of that method, science became the paradigm of theoretical rationality and the master of all objectivity. From here came its great cultural value. Science was considered to be the only cultural pattern able to guarantee objectivity and rationality. Only science could be rational and discover rationally the very reality of things.

That objectivity and rationality would explain both the dynamics of scientific development and, at any moment in time, its relative success and failure regarding explanation, forecasting, and technological application. Science progresses and enjoys success when it discovers the reality of things. And that discovery is achieved by adjustment, through the historical, sociological, economical or biographical opportunities, to the criteria of rationality imposed by its own scientific method. Thus, it should be possible to speak of rational reconstructions of science.

It was believed that the objectivity and rationality of science should be extended to other cultural fields, and particularly to the social and technological fields. Social and technological organizations should be conducted following exclusively scientific criteria, and the theoretical disciplines that analyze them should follow the general method of science. Social and human sciences should follow natural sciences. At the same time, every technological activity should be applied science. In general, the theoretical objectivity and rationality of science should influence every practical objectivity and rationality.

This positive valuation of science led to the idea that it was necessary to differentiate science from pseudo-science as well as from any other cultural manifestation. Where the rationality and objectivity of science could not be used, for example in religion or art, one could find only things like imperative norms, feelings or emotions. With respect to this, the viewpoints of the Vienna Circle were particularly radical. Only verifiable statements could be scientific. To be more precise, only those statements with respect to which we had a procedure to test, more or less directly, their truth or falsehood could be scientific. Statements that are not verifiable are statements without meaning. They can only express normative facts, feelings or emotions. Nothing more. Outside science, there are only pseudo-statements, statements without any meaning by which they can be considered true or false.

III.2. The End of a Myth

The image of science that we have described above is considered to be a myth in the recent philosophy of science. The crisis of such an image has several fronts. We are going to mention briefly four of particular importance: 1) the Popperian front of critical rationalism or falsationism, 2) the front opened by Kuhn, 3) the front of holist and pragmatist philosophies, and 4) the front of the structuralist conceptions of science.

The first front was opened with Popper's book "Logic of Scientific Discovery" (Popper, 1959; *see* also Popper, 1965 and 1972; for a wider contextualization, *see* Lakatos and Musgrave, 1970). Without breaking completely with the previous image of science, Popper was always highly critical of inductive tendencies of empirism, particularly with the verificationist proposals of logical positivism. For Popper no scientific theory, no general statement is completely verifiable. All our knowledge is, by nature, provisional.

Popper finds verificationism not a suitable methodology for science either. To look for highly verifiated hypotheses is a poor methodological strategy. Science, for Popper, should not try to find verifiable hypotheses, but rather ones which, being refutable, are full of content. In other words, for Popper, science should dry to find hypotheses that 1) although not false at the moment, may become so, and that, at the same time, 2) are as compromising, risky and ambitious as possible. Science develops through conjectures and refutations, through trial and error, not through inductions, generalizations, and verifications. We accept a hypothesis not because we think that is true forever, but because, in saying more about the world than alternative hypotheses, it has not yet been refuted.

With respect to the problem of distinguishing science from non-science, Popper maintains that, because total verification is always impossible, verifiability cannot be useful as a criterion to separate them. What would be necessary to distinguish science from non-science is the existence of methods to refute scientific statements and the inexistence of methods to refute non-scientific ones.

Popper's view is more dynamic than those offered by previous philosophies of science. At the same time, it is more global. It places scientific development in the wider context of the cultural development of our civilization. However, beyond their different perspectives, both logical positivists and Popper believe that it is possible to establish a logic of science, an inductive logic or some sort of logic of conjectures and refutations, able to assure the progressive approach to objective truth. The falsationism of Popper entails some change with respect to previous philosophies of science. But the rupture was not complete until the appearance of Kuhn's "The Structure of Scientific Revolutions" (Kuhn, 1962).

Kuhn generalized the idea that it is not possible to analyze science from an exclusively logical point of view, but rather that it is necessary to study its true history, the development of scientific institutions and the biography of scientists using empirical methods (the empirical methods used in history, sociology, psychology, etc.).

Kuhn begins by distinguishing between normal and revolutionary science. Normal science is what scientists usually engage in and consists of using a paradigm to solve typical problems. Revolutionary science consists of creating new paradigms. Despite the considerable ambiguity of the term "paradigm" in Kuhn's work, paradigm could be described as the set of theories, concepts, uses, methods and scientific traditions that confer unity to the scientific activity of a community during a relevant period of time. The important point here is that there is no rational way to choose between different paradigms. Paradigms are, between them, incommensurable. Any criterium, empirical or conceptual, to compare paradigms must be developed within one paradigm or another. There is no neutral "logic of science".

Kuhn attaches great importance to the very dynamic of institutions, scientific communities and real people in the genesis, propagation and death of paradigms. The introduction of a new paradigm looks like the imposition of a new ideology, so much so that one cannot speak of a rational and critical acceptation. Neither is it possible to attempt rational reconstructions of science. These issues, and the thesis regarding the incommensurability between different paradigms, undermined the internal rationality of scientific development which had been emphasized by previous philosophies of science.

In this situation, some philosophers, like Feyerabend (Feyerabend, 1970, 1981), accepted all the relativism entailed in Kuhn's point of view. On the one hand, every observation, every piece of data, is loaded with theory. It is not possible to separate theory and experience completely, and theories come from incommensurable global world conceptions. Empirical confrontation is never definite or decisive. Reality is always seen through our theories and world conceptions. On the other hand, conceptual criticism cannot be wholly distinguished from ideological imposition either. The only epistemological and methodological advice that philosophy of science can give us is that of an absolute pluralism. According to Feyerabend, the hope of a fixed method, of a logic of science or, more generally, of a theory of rationality, comes from a naive view of human beings and their social environments. There is only one normative principle for all circumstances which are part of the human development: "Anything goes".

We would like now to turn briefly to an examination of the holist and pragmatist philosophies of science. One of the former exponents of holism is the French scientist and philosopher Pierre Duhem, who developed his view towards the end of the last century and at the beginning of the present

one. He insists in the idea that any datum which contradicts our theories can always be reformulated through some theoretical readjustments and thus fail to be a problem. According to Duhem, on the one hand, theories are artificial constructions designed to resume and logically systematize experience. On the other hand, however, there is no experience without theories. Theories resume and systematize an experience which is always guided by other theories (Duhem, 1989).

Pragmatism is another of the philosophical approaches that contributed to the crisis of the previous philosophies of science. Pragmatism originated at the end of the last century in Great Britain and the United States (on the history of pragmatism *see*, in general, West, 1989). Pragmatism emphasizes the practical function of belief. The truth of our beliefs, statements or theories is considered to be a function of their utility in human development. Truth cannot have a correspondence with reality because reality is not static or fixed independently of our preconceptions and points of view. Truth is not discovered, but it is not merely invented either (unlike, for instance, a simple fiction). Truth is constructed through our actions. This view runs into a pluralistic perspective. Truth is always relative to the epistemic subjects. It makes no sense to think of truth as a universal value ("truth for everybody"). Dewey and James are, perhaps, the pragmatist philosophers who have most insisted on the pluralist consequences of pragmatism (*see*, Dewey, 1978, 1981; and James, 1978).

All these holist and pragmatist intuitions were subsequently taken up and developed by philosophers such as Quine, Davidson, Putnam, Goodman or Rorty.

Quine rejects what he calls the two basic dogmas of empirism: 1) the analytic/synthetic distinction; that is, the distinction between logical truths relative only to the meanings of the terms of a language and factual truths that depend on the way the empirical world is, and 2) the existence of an incontrovertible empirical basis able to solve all empirical problems through a reduction (Quine, 1951).

Quine also develops the ideas of Duhem and arrives at what is currently known as the "Duhem-Quine thesis": scientific theories must be globally understood and they can never be definitively confirmed or falsified (*see* Hahn and Schilpp, 1986). We can also find in Quine the so-called thesis of the radical opacity or inexcrutability of the reference: terms in our languages and theories always admit alternative interpretations of their references which are able to preserve the truth of what is said, and there is no logical or empirical way to decide among them to solve this indetermination (Quine, 1968).

To the dogmas criticized by Quine, Davidson adds another very interesting and more general one which he also rejects. This is the dogma of the distinction between scheme and content, between the conceptual schemes with which we wish to describe something, and what is described through those conceptual schemes (Davidson, 1974).

Putnam, who had previously defended and argued in favour of a classic philosophy of science, is now the main exponent of the trend known as "internalism", which we will examine in detail in the next section. He takes up the criticism of empirism made by Quine and Davidson and the holistic and pragmatist traditions, and insists that there is no sense in pretending that our knowledge represents reality as it is in itself, independent of our languages, conceptual schemes, concerns, viewpoints, values, goals, and so on (Putnam, 1981, 1983, 1987, 1990).

This front leads also, like Kunh's, to philosophies of science which are quite close to relativism. A clear example is that of Richard Rorty (Rorty, 1979, 1982). The pluralism of Nelson Goodman is a further example (Goodman, 1978). What all these views have in common is the rejection of the impartiality of both the tribunal of experience and the tribunal of reason; in other words, the view that there are no ultimate and unchallengeable criteria that can justify our knowledge.

The last front we shall refer to is that of the structuralist philosophers of science. Their analyses are elaborated mainly in opposition to the views of logical positivism, which they denote as the "standard or received view". Some particularly important structuralist philosophers are Balzer, Moulines, Sneed, Suppe, and Stegmüller (*see* Balzer, Moulines and Sneed, 1987; Sneed, 1971, 1983; Suppe, 1974; and Stegmüller, 1970, 1973, 1979).

According to structuralism, theories are not logically organized sets of statements, but rather mathematical structures that are applied to reality through certain models, chiefly models that have been historically proposed as their paradigmatic applications. Why are these models so privileged? Any theory admits formally (in a logical and mathematical sense) a large number of unintended models or interpretations. This multiplicity of models and interpretations would undetermine what reality the theory is about. This is the same problem we have seen in Quine, with respect to the determination of the references of the therms of a language. And it is also the same problem that we find in authors as different as Searle and Zeigler, with respect to the possibility of generating the same behaviour through different structures. It is

something we have pointed out with respect to SD and that we shall see again in Putnam (the main references here are Quine, 1968; Putnam, 1981, 1983, 1987, 1990; Searle, 1980, 1984; Zeigler, 1976, 1984). For structuralism, the only way to get away from this formal indetermination is through contextual criteria. The intended models or interpretations of a theory, the slides of reality that we want to talk about, are in the first place those which have been historical examples of its application.

Another important point in structuralism is that it maintains that the classical distinction between theoretical and observational terms is not clear. Two distinctions exist: theoretical and non-theoretical, and observational and non-observational. The second distinction poses few problems. What is or what is not observational depends on our sensory apparatus. Structuralists focus on the first distinction. For them, the distinction between theoretical and non-theoretical terms must always be made in relation to a certain theory. What is non-theoretical regarding a certain theory, is theoretical with regard to some other. The empirical basis of science becomes, therefore, something which is always viewed through our theories.

III.3. New Insights

What, then, is the present situation in philosophy of science? First of all, there has been a clear shift in aspirations. While philosophy of science at the beginning of the century was proud of its results, recent philosophy of science offers more questions than solutions. Instead of the logical reconstructions of scientific knowledge and their justification procedures, we are faced nowadays with the impossibility of making clear in formal (logical or mathematical) terms most of the peculiarities of scientific knowledge. We always need contextual elements: psychological, sociological, economical, historical, etc.

Linked to this antiformalism, we find an acknowledgement of the holistic and pragmatic character of the scientific enterprise and its interrelations with other aspects of our culture. The complex relationships between science and technology are a special case of this recognition. That is why the philosophy of technology has recently become an independent field of research. Nowadays, technology is not usually considered to be an applied science. Technology introduces ways of knowing and behaving related to those of science, but different in many ways.

In opposition to the classical search of the Method of Science, with capital letters, for a set of criteria able to define what is the justification of our knowledge and its rationality, recent philosophy of science offers an open and pluralistic view. The criteria for justifying our knowledge are not fixed, but are changeable and very circumstantial. They depend on our interests, decisions and goals. There are no final justifications either. Justification and rationality are more a process than a state. They are a process in which saying nothing, supposing that something has been completely justified, represents a backward step.

The changeable and circumstantial character of the justification criteria entails many implications. There is more than one kind of justification, and the kind of justification required in a given field need not to be the same as that used in other fields. Each program of scientific research and technological development has to define its specific criteria of justification, in a way similar to that of Kuhn's paradigms.

Even outside science there may be justification criteria that do not necessarily overlap with scientific ones. Here, recent philosophy of science links up with epistemological traditions which on other occasions were very far removed from it. We are referring to traditions such as phenomenology, existentialism, dialectics, hermeneutics, recent French philosophy, and so on (for an introduction to these, *see* Bubner, 1981; Descombes, 1979; and Rajchman and West, 1985). For the old philosophies of science, experience was always considered as either being a matter of sensory data or of the physical relationships that a subject can maintain with the environment. These traditions stress another meaning of "experience", the experience we have of the common sense world as the place in which we live. According to these traditions, all the other meanings of "experience" must refer us to this one. Recent philosophy of science is very close to this line of thought.

The realistic perspective with which the old philosophy of science interpreted our knowledge has changed considerably. The value that we attach to science cannot depend on its capacity to grasp or represent reality as it is in itself, the Single Reality, also with capital letters, that is supposed to be independent of all our decisions, conceptual schemes, languages, expectations, goals, etc. Science is not an autonomous entity, independent of its cultural and social contexte. Science is, mainly, a social institution. The value of science is a cultural value among others, and very often it clashes with these other cultural values. Decisions on scientific and technological research and development affect all of us and the whole of society must therefore be involved, to a certain extent, in its control and evaluation.

Realism is not considered nowadays as the only hypothesis able to explain the development and success of science and technology, or, for that matter, the dynamics of scientific and technological communities or individual people either. It is increasingly difficult to understand knowledge as a, more or less direct, representation of reality as it is in itself. It is even difficult to understand action as the last resort in order to keep us in touch with this reality as it is in itself. It is necessary to introduce other elements to understand and explain how we know and act.

There is no pure appeal to experience that could make it possible to determine whether or not we succeed in being adequately in touch with reality. Neither are there any kind of formal (*i.e.*, logical or mathematical) restrictions which able to select and justify a single model from the various alternative models, each positing a different structure, that can generate the same behaviour. To look for some sort of direct confrontation with reality or for some sort of ultimate formal restrictions is merely to repeat an old-fashioned epistemological project, one which presupposes that our knowledge requires some sort of absolute foundation. But no such foundation exists, either in experience or in the formal realm of mathematics or logic.

IV. LOOKING FOR THE EPISTEMOLOGICAL JUSTIFICATION OF SYSTEM DYNAMICS MODELS: THE PERSPECTIVE OF PUTNAM'S "INTERNAL REALISM"

The most important challenge in current philosophy of science is the search for a third way, between the naive realism, such as that of older philosophies of science, including here some of the views of Popper, and the relativist trends of recent philosophy. Naive realism is very well represented in the context of SD by analyses such as those of Bell and Senge (Bell and Bell, 1980; Bell and Senge, 1980). Relativism is also present in this context, and a moderate or practical version has been defended, for instance, by Barlas and Carpenter (Barlas and Carpenter, 1990). However, as we will attempt to show in this section, a third way is possible in philosophy of science in general and, in particular, in the epistemological and methodological reflections on SD modelling. We are referring to the "internal realism" (IR) of Hilary Putnam (*see*, mainly, Putnam, 1981, 1983, 1987, 1990). This perspective can be of great use here to clarify the way in which mental models help to select the structures that, from the SD point of view, must be assumed as working in real systems.

From the perspective of IR, sometimes also called by Putnam "pragmatic realism", or simply "internalism", to think that there must exist a unique adequate description, theory or model for each real system, or for reality as a whole, waiting simply to be discovered, is a myth. Putnam maintains that there is no sense in asking what really exist outside our conceptual schemes. There is no sense in claiming that our descriptions, theories or models can display reality to us as it in itself actually is, independent of any conceptual framework. It makes no sense because that independence would mean knowing reality, but without doing so through our descriptions, theories or models. And that is simply impossible.

Previous to the adoption of any particular point of view, an indefinite pluralism of alternative conceptual schemes may always exist, which we do not know whether or not they finally converge. The existence of several conceptual schemes that structure experience in different ways is always a possibility. Even an ideal epistemic situation of empirical and conceptual control, including here all the relevant observations and empirical tests, all the relevant logical and mathematical analyses, and so on, would not be able to eliminate that pluralism without the intervention of decisions and choices that are reasonable from some point of view.

In spite of these problems, all of us are more realistic with regard to some things than to others. This is a fact concerning how we want to know and act. The success of our predictions and actions may not be all that we intend to obtain from our relationships with reality. It could not be all because we would, at least, need to distinguish between real success, in our prediction and action, and the mere appearance of success. And even at this point, we need to adopt realistic compromises. A simple operationalist or instrumentalist view is really untenable.

An adequate conception of realism must be compatible with these two claims; on the one hand, with the fact that all our knowledge is developed through our descriptions, theories, models, relevance criteria and interests, and, on the other hand, with the fact that we often adopt very realistic commitments with certain objects and properties. Putnam says that any assertion about real existence is relative to a general framework, to a conceptual scheme. However, he maintains at the same time that these conceptual schemes do not lead us to any kind of relativism. It makes sense only to adopt realistic compromises from within our conceptual schemes. Nevertheless, from within our conceptual schemes, relying on our descriptions, theories, models, and so on, decisions about what is real or not are not a business of mere choice or convention. Although our conceptual schemes may be diverse, they may have objective truth criteria inside them. There are no absolute criteria, but this does not mean that there are no criteria.

This is one of the most important points of "internal realism" that is of interest for SD. We were faced with a problem of justification of SD models and their aims of explanation and understanding. Explaining and understanding the behaviour of a system requires the identification of some mechanism or structure that, from the standpoint that guides the SD model building process, may be considered as the one which brings about the behaviour. When this does not happen, we can reject the simulations of the systems, and the SD models, as being non-explanatory and as not providing sufficient understanding of the situation. This is so because forecasting and control are not the only goals of SD modelling. Beyond forecasting and control, SD model builders and users want to achieves a deeper knowledge of the real systems their SD models are describing.

It is true that if we have access to theories concerning the system we are modelling, it is easier to select the structures that may be considered responsible for the behaviour in question. In this case, theories guide the SD model building process, suggesting which ones may really be the relevant causal elements. In SD modelling, however, precise and established theoretical knowledge from which our models are an application is usually absent. Moreover, we tend to rely most of the time on the help of SD to model complex socioeconomic systems because we do not have easily applicable theories here. And in relation to these systems, the problem of being more or less committed with the structures posited by our models is decisive for their utility and effectiveness.

The problem being discussed here appears in all kinds of models, although it is especially important when dealing with models of complex socioeconomic systems in which our action policies are an essential part. These are, as we have indicated, the models usually built with the help of SD "language". These models should be useful for managing actions in a very special sense. It would be radically wrong to view these SD models as simply some sort of "calculator devices" that serve to process and predict with great accuracy quantities of data and, in this way, make it possible to control the behaviour of these systems. We stated at the beginning of our paper that SD models interact very strongly with mental models. They do so in a way which is very similar to that referred to by Seymour Papert in relation to the learning of geometry through experimentation with computer-based procedures. In LOGO, the computer learning system developed by Papert, children discover the principles of geometry by learning how to instruct the LOGO turtle to trace different figures on the computer, and this discovery can be extended to other subjects (on this, *see* Papert, 1980). These ideas need to be taken seriously. SD models are useful for managing actions by interacting with mental models.

This last remark may place us on the right track with respect to our problem. If SD models have to interact strongly with mental models, as they do in order to be useful and effective in decision-making with regard to what policy actions to adopt, then mental models can also interact with SD models in order to identify the sort of structures that, from the point of view that guides the modelling process, must be assumed as being the ones responsible for the behaviours of the real systems. We have also said that mental models offer rich and relevant information about the structural elements of certain special systems, and that, even if mental models on their own cannot obtain all the dynamic consequences of that structural information, this information is in many cases highly reliable.

All this entails two important things with respect to SD models, or at least with respect to SD models that are built without the direct help of relevant theories, and that are designed to steer our policy actions through the systems modelled, as is usually the case of SD models concerning complex socioeconomic systems:

1. SD models must have a realistic character from the point of view of the mental models of the users of these SD models.

2. The restrictions that can enable the most realistic SD models to be distinguished might come, in some cases, from the structural information provided by the mental models of the experts on which the SD models are based.

In which cases, and why, could mental models identify and distinguish the most realistic SD models? The answer to this question is crucial. Mental models can do so when the actions of the subjects that have these mental models are among the actual causes that produce the structure of the concrete systems about which the SD models are built. The structures of many systems are caused intentionally by the beliefs, desires, goals, etc., of agents. These systems are an intentional effect of the mental models of the agents involved in their production. Socioeconomic systems are paradigmatic examples of this. The structure of such systems mirrors the intended structure that is present in the mental models of the subjects responsible for their existence. Certainly, most of the time the dynamics of such systems, their behaviours, are not the ones which are expected or desired. But, from the point of view of the agents, their structures, their real structures, are often very clear. The agents themselves impose these structures on reality creating systems that are

simply not found in nature (on this point *see* Senge, 1990, particularly part I entitled "How our actions create our reality... and how we can change it").

SD models are not only devices designed to achieve forecasting and control. If the internal structure of a SD model is completely different from the way users of the SD model think the real system is structured, the SD model will be of no use in helping to understand and explain the real system, nor will it be a good guide for managing action. Thus, SD models must possess some sort of realistic character from the point of view of the users of the SD models. And the way the most realistic SD models can be selected from among alternative SD models featuring different structures, even though they are empirically equivalent with respect to the behaviours involved, is again through certain mental models, those of the experts in the systems being modelled. SD models must have a realistic character from the point of view both of the users of the SD models and of the experts in the real systems that are modelled. Sometimes, users and experts are one and the same. That is, the users of a SD model can be those from whom SD modellers have obtained the relevant structural knowledge. Furthermore, sometimes users, experts and modellers can be one and the same. In this case, SD operational knowledge is incorporated into our cognitive skills helping us to make our own mental models clear and operative. At other times, users, experts and modellers are different people; this case is more common with regard to complex socioeconomic systems that involve actions and decisions of very different people. Here, the relationships of the users, experts and modellers are more indirect and intricate. Nevertheless, points 1 and 2, mentioned above, continue to be very important, in order to assure the usefulness and epistemological justification of SD models.

It is important to see that being realistic in the sense of IR is not a substitute for another, difficult but nonetheless possible, stronger kind of realism. If we accept the historical development of recent philosophy of science as analysed above, the sense in which IR is realistic may be the only sense in which one can be realistic at all.

It is also important to consider that, even if we assume the pluralism of our conceptual schemes, nothing in IR precludes the convergence of several descriptions, theories or models. It is true that we cannot know in advance whether, in general, two or more alternative conceptual schemes converge or not prior to our attempts to achieve such a convergence. However, we can achieve that convergence by elaborating other descriptions, theories and models. Incommensurability is only a question of fact. Relativism usually understands the phenomenon of incommensurability as an insurmountable problem. That is very often shared by extreme relativism and also by moderate forms of relativism. But, it must be made clear that IR is not a variety of relativism.

What Putnam's IR does is to discard the so-called "God's eye" point of view in science and philosophy. The "God's eye" point of view would offer total, objective, perfect and definitive knowledge. That is the point of view that Putnam calls "metaphysical realism" (MR), or "externalism". From the point of view of MR, reality has a (in Putnam's words) ready-made and fixed structure that our descriptions, theories and models ought to grasp. Moreover only one true description, theory or model for the whole of reality, and for each of its parts exists. Truth consists of some sort of correspondence relation between the structure of reality and the structure posited by our descriptions, theories and models.

MR was the epistemology of almost all the philosophies of science of the first half of this century. However, there are many things which it never manages to explain. The correspondence relationship between reality and our concepts remains a total mystery. MR is also unable to make sense of the existence of structurally different but empirically equivalent descriptions, theories and models. In addition, the ready-made structure that MR supposes in the world contradicts the intuition that perhaps the dividing line between the objective and the subjective is not as clear-cut as was sometimes imagined.

But, MR is not only the epistemology of philosophies of science such as those of logical positivism, falsationism and so on. According to Putnam, part of this MR also belongs to the image of reality that impregnates both absolute and moderate relativism. Relativism of both kinds views reality as something with a ready-made and fixed structure which falls outside the scope of our best attempts to obtain knowledge of it. Relativism also demands the "God's eye" point of view. Relativism is only a pessimistic MR.

Permit to go back for a moment to the problem of the existence of a world with a ready-made and fixed structure. This is the main point that stands behind all the other claims of MR. But, what does it mean? Let us think, for instance, of all our institutions or all our socioeconomic systems, all the systems that are the result of human conceptions, decisions and actions. What are their structures? Are they ready-made and fixed, independent of our conceptions, decisions and actions? Surely not. We impose on reality certain structures which give rise to those systems. And these same structures and their dynamic properties are precisely what we aim to know better, to explain and understand, through our descriptions, theories and models.

At this point there is a strong temptation to say that IR might give a good perspective in relation to artificial systems of the type as those mentioned just above, but not in relation to natural systems such as those studied in physics. It would suffice us to adopt IR only in relation to the first kind of systems. In the end, the analysis of that special type of systems is one of the main concerns of SD. However, for Putnam that would a typical MR temptation. If there is no approach to reality, even no observation, that is not loaded with our conceptual schemes, if there is no access to reality, which is independent of our conceptions, decisions and actions, then what we always achieve and all that we can achieve is a combination of something natural and something artificial, that is, a combination or mixture in which it is impossible to separate the component elements completely. As Putnam says in one of his last books (Putnam, 1987), "The Trail of the Human Serpent is Over All".

This combination of the natural and the artificial that we find in all reality makes it impossible to distinguish between the world of science and the common-sense world. These two worlds overlap in many ways. And this is the reason for the collapse of all kinds of reductionist claims. In particular, there is no sense in thinking that it would be possible, even in principle, to reduce every significant statement to the language of physics. Physics can no longer be the only privileged sort of knowledge that tells us how reality itself is. (With respect to the problems of the distinction between the natural and the artificial, and the primacy of the artificial in all our relationships with reality, *see* the classic and interesting work of Simon, 1973).

The implications of these ideas to the important issue of causality are clear. The search of strict causal relations, deterministic causal relations with the greatest scope and with no relevant restrictions regarding ideal conditions, "ceteris paribus" clauses, and so on, is no longer viewed as the main goal of scientific acitivity. Aside from basic physics, it is very difficult to find strict causal laws. Even within physics, probabilistic considerations and conflicts between our more basic physical theories can undermine the causal sense of many laws. On most occasions, it is more appropriate to speak of relations of partial causal dependence between different magnitudes, and always in the context of a particular explanation, rather than to speak of strict causality.

Every explanation is an answer to a why-question, and a lot of explanations are really causal explanations. Questions like "Why is this move in chess wrong?" do not call for a causal explanation, but a question such as "Why is the inflation rate now rising so fast?" does. In causal explanations we look for sufficient factual conditions to explain why something happens. However, not every causal explanation needs to be expressed in the causal terms of physics. Neither does every causal explanation have to be reducible to some set of strict, general and deterministic physical causal explanations. In order to preserve the ontological unity of the world, it seems enough to imagine that, underlying our causal explanations, there may be physical causal relations of one kind or another. Reducibility to a physicalist language (or to a pure language of sensory data in other versions of logical positivism) is now being substituted by simple compatibility with a supposed particular application of physical knowledge, or even with some supposed particular application of an admissible extension of it. Often it proves impossible to go further and discover the underlying physical basis, or we are simply not interested in it.

The possibility of furnishing useful causal explanations through SD models would be one such case. SD models offer causal explanations. In fact, each SD model is a powerful source of causal explanations. SD models have such great explanatory power because they enable us to answer many why-questions about the sufficient factual conditions which cause what is being simulated. SD models provide the perfect illustration that we can acquire not only forecasting and control capacity, but also genuine causal explanations and deep understanding without having strict physical causal explanations.

These last points are really very important in relation to all modelling techniques inside the field of systems research that, like SD, want to make us of sound causal explanations without depending on their reductibility to other, assumed as more fundamental, causal explanations. Even if reduction is a way to obtain causal explanations outside physics, it is not the only way. Especially with respect to complex socioeconomic systems whose structure is in a very relevant sense the result of human conceptions, decisions and actions, we could achieve sound causal explanation, and all the benefits of a causal modelling, without being involved in the need to offer reductions. (About this, *see* the interesting analysis of causality and causal modelling in economy developed by Paulré, 1985.)

V. CONCLUDING REMARKS

We have seen that in IR there is no point in asking what really exists beyond our conceptual schemes, and that there are no privileged structures in reality waiting to be discovered by us independent of our epistemic contribution. From this point of view, we can say that SD is a clear example of how some epistemic elements, the mental models of the experts involved in real systems

and the mental models of the users of SD models, can select the structures that must be assumed as working in the real systems.

SD operational knowledge offers a survey of archetypes of generic structures (feedback loops and so on) and ways to obtain dynamic behaviours from these structures. These archetypes or generic structures give a survey of basic structures that very often appear in different problems and that have been more or less typified by SD practitioners and theorists (on this, and in regard to the similarities between SD and so-called "institutional economics", *see* Radzicki, 1990; *see* also Senge, 1990, especially appendix 2 entitled "Systems archetypes"). In short, SD could be considered as a sophisticated conceptual scheme and, in a wide sense, as a "language". With the help of this "language", we see and describe reality. This "language" guides our perception of the problems and help to organize our actions, rewriting our mental models. SD offers a new way to organize experience, new tools to know and to act. However, mental models are necessary in order to anchor in reality the archetypes or generic structures that are the basic representational elements of SD "language". Mental models provide realistic content to the structures posited by SD models.

The mental models of experts impose these structures on reality and are part of the structuring cause of the systems in which the experts are involved. Moreover, these same mental models interact with SD models in the search for forecasting, control, and some sort of deeper explanation and understanding of the dynamic properties of the real systems. This way, the mental models of the experts become the mental models of the users of SD models (as we have said, sometimes they are one and the same, but not always). The selection of the SD models with the most realistic representational content made by experts and users has a crucial justification in relation to the structural dependence and sensibility that some real systems have on the human actions developed within them.

SD models are typically constructed upon the intuitive and presystematic knowledge of people who are particularly related to the systems being modelled. SD models may be explanatory and allow us to know these systems better, not just to forecast and control them. However, this explanation and this increase in knowledge are always internal to the conceptual frameworks, to the mental models, of real and concrete subjects. From the externalist philosophies of science (with "God's eye") such as those of logical positivism, Popperian falsationism, and so on, these explanations and understanding are neither completely explanatory nor do they amount to genuine knowledge. From Putnam's internalist view, however, they may be. The reason is that all knowledge and every explanation must be internal to some conceptual scheme. The explanation and understanding provided by SD models may be adequate from within the conceptual scheme of SD itself.

With respect to complex socioeconomic systems, these conceptual schemes involve part of the ordinary common-sense framework, the ordinary common sense applied to the actions undertaken in a socioeconomic environment. Putnam thinks that the common-sense world and the world of science cannot be separated. In the case in which the most important things that need to be known are actions undertaken, possible decisions, etc., as is the case of complex socioeconomic systems modelled through SD, this thesis is revealing. There should be no other way of access to the structure of those systems other than the intuitive representations held by the subjects involved in them. This is because, in the end, their decision and actions, guided by their mental models, perhaps in interaction with SD models, are among the causes of that structure (*see*, again, Senge, 1990, part I).

The questions of pluralism and convergence, as analyzed by Putnam, are also very important for us. Sometimes several SD models may be in an extreme position of incommensurability. That is, they may be empirically equivalents and have the same realistic plausibility from the point of view of mental models. Certainly, pluralism is not unusual in SD. However, this pluralism must be considered only as a matter of fact, and not as something necessary. Convergence of different SD models can be achieved in SD modelling thanks to the strong interactive character of mental models. It could even be achieved through reasonable decisions and choices. SD modelling is a continuous process of revision and adjustment. However, it is one thing that convergence be achieved in this way and another very different thing that we do actually achieve it. Convergence is also a matter of fact that cannot be known in advance.

From the epistemological perspective of IR we could avoid the classical concerns about the ideal aim, pluralistic or convergent, of our knowledge in general and, more particularly, the pluralistic or convergent character of system modelling through techniques like SD. And we could also avoid the classical concerns about aspects such as the reduction of the causal relations presupposed in our SD models to others, assumed as being more basic, causal relations. This last point is very promising in relation to the methodological debates among (mainly neoclassical) economists and SD practitioners and theorists. If the epistemological perspective of IR is adequate, no kind of fundamental micro-economic theory would be needed to explain and give justification to the macro-causal relations posited in SD models of socioeconomic systems (on this particular point, *see* Radzicki, 1990). Reduction can be a kind of causal explanation, but it cannot be the only kind of sound causal explanation outside physics. Outside physics there may be non-reductive but at the same time very sound causal explanations. And reduction, like pluralism and convergence, is only a matter of fact.

Finally, we find in Putnam's IR the rejection of both MR and relativism. IR is in fact a third way between the naive realism linked to the old positivist philosophies of science, such as those held by logical positivism, by falsationism, etc., and the relativism linked to the crisis of such philosophies. The possibility of some kind of moderate relativism that does not entail absolute relativism remains an open question. But, in any case, if this moderate relativism, in the context of SD, only says that usefulness and success with respect to our interests and purposes must be important properties of valid SD models, it would fail to provide a satisfactory and full account of SD modelling. Moderate relativism is unable to make sense of the search for genuine explanations and understanding through our SD models.

Acknowledgements. – This work has been partially supported by CICYT (Spanish Ministry for Education and Science) under grant PB87-0336.

References

P. ACHISTEIN and S. BARKER (eds.), *The Legacy of Logical Positivism*, Baltimore, John Hopkins Press, 1969.

J. ARACIL, Bifurcations and Structural Stability in the Dynamical Systems Modeling Process, *Systems Research*, 1986, *3*, (4), pp. 243-252.

A. AYER, (ed.), *Logical Positivism*, Chicago, Free Press, 1959.

W. BALZER, C. MOULINES and J. SNEED, *An Architectonic for Science, The Structuralist Program*, Dordrecht, Kluwer, 1987.

Y. BARLAS and S. CARPENTER, Philosophical roots of model validation: two paradigms, *System Dynamics Review*, 1990, *6*, (2), pp. 148-166.

J. A. BELL and J. B. BELL, System Dynamics and the Scientific method, in J. RANDERS (ed.), *Elements of the System Dynamics Method*, ch. 3, 3-22. Cambridge, MIT Press, 1980.

J. A. BELL and P. M. SENGE, Methods for enhancing the refutability in System Dynamics Modeling, *TIMS Studies in the Management Sciences*, 1980, *14*, pp. 61-73.

R. BUBNER, *Modern German Philosophy*, Cambridge, Cambridge Univ. Press, 1981.

D. DAVIDSON, On the very idea of a conceptual scheme, *Proceedings and Adresses of the American Philosphical Association*, 1974, *47*, pp. 5-20.

V. DESCOMBES, *La même et l'autre*, Cambridge, Cambridge Univ. Press, 1979.

J. DEWEY, *The Middle Works of John Dewey*, Carbondale, Southern Illinois Univ. Press., 1978; *Later Works*, Carbondale, Southern Illinois Univ. Press, 1981.

P. DUHEM, *La théorie physique*, Paris, Vrin (2*a* ed.), 1989.

M. DUMMETT, *Frege*, London, Duckworth, 1973.

P. FEYERABEND, Against Method, in RANDER and WINOKUR (Eds.), *Minnesota Studies in the Philosophy of Science*, IV, Minneapolis, Univ. of Minnesota Press, 1970; *Philosophical Papers, vols. I and II*, Cambridge, Cambridge Univ. Press, 1981.

J. FORRESTER, Lessons from System Dynamics Modelling, Proceeding of the System Dynamics Conference 1986, Seville, Univ. of Seville, Also in *System Dynamics Review*, 1986, *3*, (2), pp. 136-149.

N. GOODMAN, *Ways of Worldmaking*, Indianapolis, Hackelt, 1978.

N. GRIFFIN, *Wittgenstein's Logical Atomism*, Oxford, Oxford Univ. Press, 1964.

L. HAHN and P. SCHILPP, *The Philosophy of W.V. Quine*, La Salle, Open Court Publishing Company, 1986.

W. JAMES, *Pragmatism*, Cambridge, Harvard Univ. Press., 1978.

V. KRAFT, *Der Wiener Kreis*, Berlin, Springer, 1950.

T. KUHN, *The Structure of Scientific Revolutions*, Chicago, Chicago Univ. Press, 1962.

I. LAKATOS and A. MUSGRAVE (eds.), *Criticism and the Growth of Knowledge*, Cambridge, Cambridge Univ. Press, 1970.

M. LIZ and M. VÁZQUEZ, The System Dynamics Concepts of Model in J. VENNIX *et al.* (Eds.), *System Dynamics 1992. Proceedings*, Utrecht, System Dynamics Society, 1992, pp. 375-384.

S. PAPERT, *Mindstorms: Children, Computers, and Powerful Ideas*, New York, Basic Books, 1980.

B. PAULRÉ, *La Causalité en Économie. Signification et portée de la modélisation structurelle*, Lyon, Presses Universitaires de Lyon, 1985.

K. POPPER, *The Logic of Scientific Discovery*, London, Hutchinson, 1959; *Conjectures and Refutations*, London, Routledge and Kegan Paul, 1965; *Objective Knowledge: an evolutionary approach*, Oxford, Clarendon Press, 1972.

H. PUTNAM, *Reason, Truth and History*, Cambridge, Cambridge Univ. Press, 1981, *Realism and Reason*, Cambridge, Cambridge Univ. Press, 1983; *The many faces of realism*, La Salle, Open Court Publishing Company, 1987; *Realism with a Human Face*, Cambridge, Harvard Univ. Press, 1990.

W. QUINE, 1951, Two Dogmas of Empiricism, in *From a logical point of view*, Harvard University Press, 1961; Ontological relativity, in *Ontological Relativity and Other Essays*, New York, Columbia Univ. Press, 1969.

M. RADZICKI, Methodologia oeconomiae et systematis dynamis, in *System Dynamics Review*, 1990, *6*, (2), pp. 123-147.

J. RAJCHMAN and C. WEST (eds.), *Post-Analytic Philosophy*, New York, Columbia Univ. Press, 1985.

R. RORTY, *Philosophy and the Mirror of Nature*, Princeton, Princeton Univ. Press, 1979 - *Consequences of Pragmatism*, Minneapolis, Univ. of Minnesota Press, 1982.

B. RUSSELL, *Logic and Knowledge*, London, Allen and Unwim, 1956.

P. SCHILPP, *The Philosophy of Rudolf Carnap*, La Salle, Open Court Publishing Company, 1963.

J. SEARLE, Mind, Brains and Programs, *The Behavioral and Brain Sciences*, 1980, *3*, pp. 417-457; *Minds, Brains and Science*, Cambridge, Cambridge Univ. Press, 1984.

P. SENGE, *The Fifth Discipline*, New York, Doubleday, 1990.

H. SIMON, *The Sciences of the Artificial*, Cambridge, MIT Press, 1973.

J. SNEED, *The Logical Structure of Mathematical Physics*, Dordrecht, Reidel, 1971; Structuralism and Scientific Realism, *Erkenntnis*, 1983, *19*, pp. 345-370.

W. STEGMÜLLER, *Theorie und Erfahrung*, Berlin, Springer-Verlag, 1970; *Theorienstrukturen und Theoriendynamik*, Berlin, Springer-Verlag, 1973; *The Strcturalistic View of Theories*, New York-Berlin, Springer-Verlag, 1979.

F. SUPPE, *The Structure of Scientific Theories*, Urbana, Univ. of Illinois Press, 1974.

A. TARSKI, *Introduction to Logic and Methodology of Deductive Sciences*, New York, Oxford Univ. Press, 1941; *Logic, Semantics and Metamathematics*, Oxford, Clarendon Press, 1956.

M. VÁZQUEZ, J. ARACIL and M. LIZ, Some Conceptual problems in the System Dynamics Models Building Process, in D. ANDERSEN, G. RICHARDSON and J. STERMAN (Eds.), *System Dynamics'90*, Massachusetts, The System Dynamics Society, 1990, pp. 1175-1186.

M. VÁZQUEZ and M. LIZ, Systems Formalization and Models Building. An Approach to Zeigler's view through a University/unemployed model in P. MILLING and E. ZAHN (Eds.), *Computer-Based Management of Complex Systems*, Berlin, Springer-Verlag, 1989, pp. 285-292.

B. ZEIGLER, *Theory of Modelling and Simulation*, New York, John Wiley and Sons, 1976; *Multifacetted Modelling and Discrete Event Simulation*, London, Academic Press, 1984.

C. WEST, *The American Evasion of Philosophy: A Genealogy of Pragmatism*, Madison, Univ. of Wisconsin Press, 1989.