A FUNDAMENTAL PHILOSOPHY OF SCIENCE QUESTION AND VALIDATION OF SYSTEM DYNAMICS MODELS

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ABSTRACT

System Dynamics models, being causal simulation models, are in this sense very much like scientific theories. Hence, there is a relationship between validation of such models and verification of scientific theories. In evaluating System Dynamics models, we naturally apply our implicit "norms of scientific inquiry". Most criticisms of such models hold that System Dynamics does not employ formal "objective", quantitative model validation procedures. We show through a historical review of Philosophy of Science, that this type of criticism presupposes the traditional logical-empiricist philosophy of science. This philosophy assumes that knowledge is entirely "objective representation" of reality, and that theory justification can be an entirely objective, formal, "atomistic" process. According to the more recent "relativist" philosophy of science, on the other hand, knowledge is not "entirely objective Truth", but it is relative to a given culture, epoch, and scientific worldview. Theories can not be verified (falsified) by entirely formal, reductionist, "confrontational" methods. Completely objective (theory-free) observation is impossible. The act of observing itself requires an assumed theory. Theory justification is therefore a semi-formal, holistic, social, "conversational" process.
We discover that these two opposing philosophies of science correspond to two opposing philosophies of model validation. Most critics of System Dynamics seem to assume the traditional empiricist philosophy of science, whereas System Dynamicists mostly agree with the recent relativist phiplosophy on the question of model validity. We show that these philosophical results do have practical implications for both the System Dynamicists and their critics. Finally, having shown that the relativist philosophy is consistent with System Dynamics practice, we emphasize that such a philosophy of model validity should not lead to a total rejection of formal quantitative tools of model validation. On the contrary, we argue that such tools, appropriately chosen, are most useful when interpreted with the relativist philosophical perspective.

I- INTRODUCTION

Both in natural sciences and in social sciences, the question of how models should be validated has been a most controversial issue for many years. Especially in social sciences, this controversy has become more and more crucial as new and complex modeling tools have emerged in recent years. System Dynamics (SD) methodology constitutes one such tool, and not surprisingly, SD model validation practices have been subject to close scrutiny.
In the last 20 years, there have been numerous reviews (positive and

negative) of SD models and we have witnessed a heated debate on validation of such models. (For example see Ansoff and Slevin (1968), Forrester (1968), Nordhaus (1973), Forrester et al.(1974), Forrester (1980) and Zellner (1980)). Throughout this long debate, critiques of SD methodology have had one common general theme: SD does not employ formal, "objective", rigorous quantitative model validation procedures (which are supposed to be fundamental to scientific inquiry). The implication of this type of criticism is that SD models are not "quite scientific enough". System Dynamicists have responded to this, by stating that model validity is strongly tied to the nature of the problem, the purpose of the model, the background of the user, the background of the analyst etc. Accordingly, model validation is inherently a social, judgemental, highly "qualitative" process: Models can not be "proven" to be valid but they can be "judged" to be so.
We see that there are some fundamental differences in the worldviews of the two sides in the SD validity debate. The issue is complicated by the fact that certain concepts such as "model", "reality", "truth", "validity" that are central to the debate are understood and used differently by authors of different worldviews. Unless we are explicit and clear about what we mean by these terms, the question "is SD methodology scientific ?" is not meaningful. Furthermore, it is impossible to answer this question without first stating what exactly makes an inquiry "scientific" (or "unscientific"). In this article we will try to clarify the fundamental differences in the two opposing worldviews involved in the validity debate. We will show that the validity debate is strongly tied to a fundamental Philosophy of Science problem. After reviewing this philosophy problem in its historical development, we will derive its implications for SD model validation

II- MODELS AND MODEL VALIDITY

In order to see the connection between Philosophy of Science and SD model validation, we must first define what we mean by "models" and by "SD models". Then, we will see that validation of SD-type of models, by their very nature, involves some fundamental Philosophy of Science questions.
"Models" are used in most disciplines: natural sciences, engineering, architecture, computer science, social sciences, philosophy... It is impossible to give a single and specific definition of "model", because its usage greatly varies across diverse disciplines. Quite broadly though, a model might be defined as "a substitute for some aspects of reality". Thus, whether we have a scale model of a submarine, a collection of balls describing the movement of gas molecules, a set of mathematical equations to predict demand for a product, or even an entirely verbal description of the major factors involved in drug addiction, all these models are "substitutes for some aspects of reality". The models mentioned above are different from one another in many different respects: Physical (eg. model ofsubmarine) vs. conceptual (eg. mathematical equations); dynamic (collection of balls) vs. static (model of submarine); quantitative (mathematical equations) vs. qualitative (verbal model) etc. For our purpose, the category of "conceptual models" is important because SD models belong to this category. "Conceptual models" are comprised of thoughts, expressions, symbols and diagrams, rather than "physical objects". A mathematical model is one type of conceptual model where the model is constructed by

means of mathematical symbols and expressions. SD models are examples of mathematical models.

In this article, we must further distinguish between two fundamentally different types of mathematical models: 1- Causal (theory-like) mathematical models, 2- Non-causal (statistical-correlational) mathematical models. Causal models base their mathematical expressions on postulated causal relationships within the modeled system. They are collections of mathematical statements describing how the modeled system works -in some respects- in real life. Thus, by making causal claims about how certain aspects of a real system function, they become theories about that system. Therefore, such models can be used for both prediction and explanation. Non-causal mathematical models on the other hand, simply express observed associations (in form of statistical correlations) among various elements of a real system. Such models are purely empirical (correlational), their mathematical relationships not being based on any theorized causal mechanism. These models are used only for prediction purposes, on the assumption that "they just work" within a certain range of values of variables. They can not be considered theories since they make no causal claims.

SD models belong to the class of causal mathematical models. A SD model consists of a set of mathematical equations that attempts to describe the causal relationships existing in the real system. Hence, a SD model is at the same time a theory about how a system actually works in certain respects. This is the crucial property of SD models with respect to the problem of validation. Since individual model equations claim to be causal statements about system relationships, every individual statement (equation) must be defended and justified as an indispensable part of model validation. That is probably why SD models are usually very closely scrutinized. If a critic can show that one of the model equations does not make sense (does not agree with an obvious causality), then the model is refuted even if the aggregate model output agrees well with observed data. The same is not true for purely correlational models. In such models, since no claim of causality is made, every equation is not subject to criticism and justification. What matters is only the final output of the model. If the model output matches the observed data with a certain degree of accuracy, the model is validated. For SD models, in addition to individual statement justification, the overall output behavior of the model must also be evaluated against available output data. Hence, there are two conditions for SD model validity, both necessary but neither of them by itself sufficient.

We now turn to the crucial property of SD models that makes them different from some other quantitative models of social systems: This is the principle of causal explanation. A SD model consists of "causal mathematical statements" that must be justified individually for the model to be valid. In this respect, SD models are very much like scientific theories. Thus, whether we are System Dynamicist or critic, we tend to apply our accepted norms of scientific theory testing to SD model validation. This is where one faces fundamental Philosophy of Science questions: "What constitutes justification of a proposition ?" "Is it possible to completely confirm the truth of a statement ?" "How are theories verified in mature (natural) sciences ?" Answering these questions will provide a reference point in discussing the validation of SD models. More specifically, it will set an upper bound on the formalism to be expected from SD validation procedures. It will be an upper bound because SD models have certain properties (uncertainties inherent in human systems, complexity and dimensions of typical SD models, impossibility of controlled experimentation, unavailability of data, too much noise buried in observed data...) which make them more difficult to validate than theories of natural sciences. (We will not discuss these properties in this article. Interested reader may refer to Barlas (1985) Chapters II and IV and Forrester (1961) Chapter 13). In the next section we take a fundamental Philosophy of Science problem, termed "justification of knowledge-claims" or "verification of propositions".

One technical point needs clarification before we start the following discussion : As a philosophical term, "validation" refers to a purely logical problem, dealing with the internal consistency of a set of propositions with respect to a set of logic rules. The philosophical problem of "verification" on the other hand, deals with "justification of knowledge claims" and corresponds to "validation" as used in modeling literature. "Verification" in modeling literature deals with the internal consistency of a computer program. One must be careful in interpreting these two terms, as they "switch" meanings from one literature to the other. We will adopt the usage of "validation" common in modeling literature. Readers with philosophical background should read this to mean "verification".

III- A FUNDAMENTAL PHILOSOPHY OF SCIENCE QUESTION

Once SD models are considered as theories, their validation bears direct relation to the fundamental Philosophy of Science question: "Under what conditions should a scientific theory be regarded as having been confirmed?" Philosophy of Science has emerged as a distinct philosophical discipline in the late nineteenth and early twentieth century, but it is strongly related to a much older philosophical subject: Epistemology (Theory of Knowledge). The purpose of epistemology is to find out the "conditions that make knowledge possible". Since scientific theories consist of knowledge-claims, it is very natural that Philosophy of Science encompasses epistemology. In the following section, we give a brief historical overview of epistemology before we go on to discuss the fundamental Philosophy of Science question.

III.1. Epistemology

The idea of developing a coherent "theory of knowledge" can be traced back to Rene Descartes (1596-1650). Descartes believed that philosophy needed a new method, the deductive reasoning of mathematics, because the only truths that can be accepted without any doubt were the ones revealed by this method. He claimed that such a purely deductive reasoning was possible because the ideas of such reasoning were innate, prior to all experience: He was a pure rationalist. In his famous Meditations on First Philosophy (1641), Descartes uses his "method of doubt" and deductive reasoning in order to find out what we can believe with certainty and what we must doubt. He concludes that the "Mind" ("Thinking Self") exists with certainty ("I think therefore I am"), and that the existence of the "things out there" ("corporeal objects") must be doubted. But Descartes does not claim that the corporeal objects are non-existent. He reasons that external objects "must exist", yet we could never be sure of their existence since our knowledge about them is uncertain. For him, the only true knowledge is the kind revealed by deductive reasoning, from self-evident propositions.

The other important source of modern theories of knowledge is John

Locke's empiricism. Locke (1632-1704) can be considered the founder of the empirical theory of Knowledge. In An Essay Concerning Human Understanding (1748), Locke hopes to discover where the ideas and Knowledge come from, what we are capable of Knowing and how certain Knowledge can be. Locke disagrees sharply with Descartes by believing that none of our ideas are "innate". According to him, our mind is a "blank tablet" ("tabula rasa") when we are born. All Knowledge is the result of experience. Locke believes that external objects do exist, but agrees with Descartes that our Knowledge about them is uncertain. But Locke's doubt comes from his extreme empiricism: When we see an object, we must be satisfied of its existence as long as we look at it. But the moment we stop looking at the object, we have no Knowledge as to whether it still exists. According to Locke, "ideas" are caused directly by the physical world, and Knowledge is a result of the mind's "acquaintance" with the ideas. Although Knowledge acquisition also involves the mind's manipulation of the ideas (termed "description"), "acquaintance" is prior to "description": The ideas are first put in the passive mind, and then the mind starts manipulating them. This model of Knowledge acquisition will be, as we shall see, very influential in the mainstream Philosophy of Science.

In the Eighteenth Century, Immanuel Kant (1724-1804) defined the epistemological problem as a search for the "principles of thinking" (1933). Kant had been influenced by the two most important philosophical schools of his time: Descartes' rationalism and Locke's (and David Hume's) Empiricism. From Descartes, he took the concept of the "active mind", and from Locke the role of sensations (experience) in Knowledge acquisition. According to Kant, ideas are caused by experience, but having ideas does not mean having Knowledge; the latter is not by mere "acquaintance", but it is by "description". The mind does not just receive the Knowledge, but it actively produces it. The ideas are organized according to some "a priori forms of intuitions" and processed according to the "principles of thinking". Thus, the "essence" of Knowledge is not to be found in a special Kind of relationship between the external objects and the mind, but in the necessary "non-empirical rules of understanding". This is the fundamental difference between Kant and Locke. In Kant, the mind is not a "blank tablet". It has certain "ideas of reason" which are "a priori", not warranted by experience. Such a priori ideas regulate the operations of understanding. According to Kant, there are three types of statements: 1- "Analytic a priori", which are warranted by definitions and rules of logic, 2- "Synthetic a posteriori", which are warranted by experience, and 3- "Synthetic a priori", which are warranted by an internal organizing principle of the mind. A crucial characteristic of Kant's philosophy is its acceptance of "synthetic a priori" statements. According to Kant, the general principles of all sciences (such as "every effect has a cause") and "mathematical judgements" ("straight line between two points is the shortest") are synthetic a priori. Kant believed that such statements -synthetic, yet prior to experience- were not only legitimate, but also essential for Knowledge to be possible.

Let us now observe an assumption common to the theories of Knowledge: Knowledge is seen as entirely objective, asocial, acultural, ahistorical "Truth" (rather than "socially justified belief"). It follows that Knowledge acquisition can be understood by "pure" philosophical analysis, an analysis independent of all the social, cultural, historical conditions of particular era. For instance, Kantian philosophy attempts to "ground" all possible Knowledge in a description

of "Mind", a frame independent of all social and historical factors. In his recent book, Philosopher Richard Rorty (1979) calls this ongoing search for "neutral" foundations of knowledge the "foundationalist" philosophy. According to Rorty, this attempt to find the foundations of "Truth" in something permanent, neutral (entirely objective) goes as far back as the ancient Greek philosophy. In Descartes/Kant tradition, the permanence is sought in the "Mind"; in the "linguistic philosophy" of the Twentieth century, "language" replaces the "Mind". But one commitment has persisted for over three hundred years: the effort to construct a timeless, neutral framework of inquiry relevant for all times, for all culture. All mainstream philosophies agreed on one thing: knowledge is a result of some "privileged relationships", and once we understand them, we can tell exactly which statements are "objectively true", independent of all cultural, historical factors. Knowledge is entirely objective representation of reality. Rorty uses the metaphor "Mirror of Nature" to explain this "foundationalist" view: knowledge is the reflection of nature on an "unclouded mirror" (the "Mind", later the "language"). Thus, knowledge is imposed via a privileged relationship. The philosopher's task is to see that the mirror is being used properly, because if it is, it will automatically deliver the "Truth".
An alternative view of knowledge, which emerged in the 1950's is that knowledge is "socially justified belief". It is not a result of "mirroring" the nature. A knowledge-claim is true not because of some "privileged" way it was acquired, but because of the arguments given to support it. Knowledge is socially, culturally and historically dependent. Accordingly, there are no "neutral foundations" of knowledge, and entirely objective verification of knowledge-claims is not possible. Knowledge justification is a relative, social, external process, rather than an absolute, representational, internal one. We shall focus on this recent philosophical trend later in the article. But first, the mainstream ("foundationalist") philosophy of science movements of the Twentieth Century.

III.2. Mainstream Philosophies of Science

In the Twentieth Century, epistemology took in general an anti-Kantian character by rejecting the legitimacy of Kant's "synthetic a priori" statements. Inspite of this anti-Kantian trend, almost all philosophers of science have been attracted to Kant's problematique of discovering the neutral "foundations" of knowledge. Bertrand Russell was one of the first and most influential of such philosophers. Russell explicitly rejected the existence of "innate ideas" and the legitimacy of "synthetic a priori" propositions. He believed that all ideas come from sense experience. He revived the Lockean thesis that knowledge by "acquaintance" is prior to knowledge by "description". In this respect, Russell is anti-Cartesian. Russell's philosophy is an important revision of Kant's epistemological program. The foundations of knowledge are no longer to be found in the mind, but rather in those propositions that come from "direct acquaintance" with objects. Russell argues that statements about physical world could be translated into statements about "sense data", data of immediate experience (Russell (1949)). This reductionist claim that statements can be categorized according to the degree of their empirical content has been very influential in the development of philosphy of science. As we shall see, philosophers have assumed that propositions could be separated into empirical and non-empirical components and the empirical components could then be

isolated and "verified" against empirical data.
Another important work that influenced the Twentieth Century is Ludwig Wittgenstein's early book Tractatus (1922). Like Russell, the young Wittgenstein had strong reductionist and empiricist views (which, he abandoned in his later years). In Tractatus, Wittgenstein attempts to show how a meaningful language system ought to be formulated. He states that an analytic a priori statement, that says "nothing new about the world", is not empirically verifiable. A synthetic statement, on the other hand, does say something new, and must correspond to empirical "atomic facts". Therefore, any synthetic statement that is not empirically verifiable (which Kant called "synthetic a priori") is meaningless. (This category would include value judgements, ethical arguments, most philosophical inquiries). Tractatus argues that people frequently talk nonsense because of the deficiency of the ordinary daily language. An ideal language system ("logical symbolism") would prevent nonsense by excluding those statements that are more than logical deductions and at the same time not empirically verifiable. This thesis has been very influential in the philosophy of science, especially in the development of "logical empiricism" which has been the most widespread philosophy of science until the 1950's.
Logical empiricism (or logical positivism) is the name given to the philosophical movement emanating from the "Vienna Circle", a discussion group of famous philosophers who met between the early 1920's and mid 1930's at the University of Vienna. Originally, the most important topics involved the possibility of reducing all synthetic statements to direct observational statements, setting up a rigorous criterion of meaningfulness and designing an ideal meta-language for philosophical analysis of scientific language systems. As a general philosophical movement, logical positivism became very influential although not all philosphers associated with it agreed on all issues involved. Among the most prominent logical empiricists were Rudolf Carnap, Moritz Schlick, Otto Neurath, Carl Hempel, Richard Von Mises and Ernest Nagel. If logical empiricism is taken in its narrow sense as it originated in the Vienna Circle, some of the above philosophers would not be strictly called logical empiricist. But we will use the term in a wider sense to imply an agreement on the following points at least: 1- Rational discourse can have only two types of statements: Analytic a priori (definitions and purely logical deductions) having no empirical content, and synthetic a posteriori (statement of facts) that must be empirically verifiable. All synthetic statements that are not empirically verifiable must be excluded from rational discourse. 2- Philosophy must reshape the general structure of scientific statements so that they become free from ambiguity, vagueness and inconsistencies. The ideal would be to reduce all scientific languages into one unified canonical form ("unity of science"). 3- The context of scientific discovery can and must be totally separated from the context of scientific justification. Discovery is a historical, social, psychological process and lies completely outside the domain of philosophical analysis. (Justification consists of the verifiability of propositions and deductive validity of the arguments). Logical empiricism, taken in this wider sense, comprises the great majority of the early philosophies of science.
One of the major flaws in logical empiricism was a logical problem involved in the principle of "verification". Karl Popper (1959) analyzed this "problem of induction" and suggested his own solution. To see the problem, consider a theory T and its conclusion C. C is derived from T according to the deduction:

If the theory T is true, then the conclusion C follows.
Now to verify T, according to "verificationism", one tries to observe C. But the verifying argument "C is observed, therefore T is true" is logically incorrect since in reality C may occur as a result of a process different from the one hypothesized in T. Thus, statements of general nature (scientific theories) can never be fully verified by observation. Popper's solution to this problem is the principle of "falsification" (Popper, 1959). Accordingly, the following argument is always logically valid:

If T is true, then C follows
"Not-C" is observed, therefore T is false.

Thus, Popper argues that the requirement of falsifiability must replace verifiability: Scientific theories must be required to be falsifiable. The credibility of a theory increases as more and more non-falsifying observations are found. Thus, theory verification is replaced by a gradual process of "corroboration".
The wide acceptance of this principle of "falsification" can be seen as a sign of "mellowing" for the hard-line logical empiricism. Yet, like verifiability, falsifiability too has strong logical empiricist elements. It assumes too, that theories can be totally separated into their analytic and synthetic components and that for every synthetic component it is possible to find a corresponding observation. Furthermore, falsification assumes that although theories gain credibility gradually, they are thrown away at once, upon a falsifying instance. But in reality, this idealized scenario does not happen because a typical theory is always presented with a set of assumptions:

If assumptions A and T hold, then C follows.

Now if C is not observed, it is not always clear whether it is due to wrong theory or invalid assumptions. It is always possible to keep the theory by stating that "the assumptions did not hold". Another practical problem is that an observation rarely ever comes as either C or "non-C", but mostly as a complex data subject to interpretation. It is largely up to the scientist to organize and interpret the data and to decide whether the observation actually constitutes a "falsifying instance". For these -and other reasons that we shall see later- Popper's original principle of falsifiability was actually a logical empiricist thesis. Another major problem with the early logical empiricism was its insistance on predictive ability as the only criterion for theory justification. Since, according to logical positivism, the content of a scientific theory is irrelevant to the philosophical problem of verification, explanatory power is not a criterion for justification. According to the principle of verifiability (or falsifiability), the only criterion for justification is whether the observations match with the predictions (implications) of the theory. According to this view, explanation may be quite important in other activities such as construction of new theories, but has nothing to do with justification. Stephen Toulmin (1977), reviewing the last fifty years of the philosophy of science, explains the absurdity of relying merely on predictions, by noting that we would then consider "horserace tipsters as scientists" and evolutionary biology as "non-scientific". Faced with this difficulty, many empiricists had to accept the importance of "explanation" as evidence of knowledge. This acceptance, Toulmin observes, "... began to undercut the formalist approach at its very foundations..." because explanation necessitates "... a shift to quite another conceptual level, involving a kind of theoretical reinterpretation whose merits can not be captured in a merely formal algorithm" (Toulmin, 1977).

There were criticisms of traditional epistemology as early as in the Nineteenth Century, but the bulk of consistent criticism came in the second half of the Twentieth Century. Richard Rorty (1979) mentions two very important works that questioned the basic assumptions of epistemology taken granted since Kant. One of these assumptions holds that knowledge acquisition consists of two separate and distinct forms of representations: What is "given" to us from the outside, and what is "added" by our mind. This fundamental distinction between the "given" and the "added" is challenged by Wilfrid Sellars in Science, Perception and Reality (1963). The other crucial assumption of logical empiricism is the claim that propositions can be separated into their analytic (true by meaning) and synthetic (true by virtue of experience) components and that every synthetic statement must correspond to a unique sense experience ("reductionism"). This assumption is challenged by W. V. Quine in "Two Dogmas of Empiricism" (1953). We shall very briefly summarize the main ideas of these two works , as they constitute major steps towards the construction of a new philosophy of science.

Traditional epistemology assumes that two essentially different sorts of ideas ("given from the outside" and "added by the mind") come together to produce knowledge. Wilfrid Sellars (1953) tries to show that this given/added distinction is not an inevitable, "essential" one, but merely a convention of the reductionist, atomistic theories of knowledge. According to Sellars, it is impossible to draw an absolute line between the "given" and the "added". Knowledge acquisition is holistic rather than atomistic. The empiricist's assumption that learning of the "particulars" constitutes the basis of knowledge is misguided. Even awareness of particulars is a linguistic (social) affair. We can not define the "awareness" of a machine, an insect or a new-born baby, because none of these can play our "language game". As we understand awareness, "being aware of things" makes no sense prior to language acquisition ("language" defined in its most general sense of "symbol manipulation"). Thus, according to Sellars, knowledge is socially justified belief. By opposing the "given/added" distinction, Sellars does not try to develop a new theory of how mind works. On the contrary, he claims that such a theory could not possibly account for why knowledge is possible, because the latter is socially justified belief and occurs in a social, conversational domain. Once we acknowledge that knowledge is social and temporal, then we do not need a Kantian theory of how the mind works in order to find the necessary conditions for knowledge. Rorty (1979) observes that, Kant had made the given/added distinction, not because he had discovered something fundamental about how mind acquires knowledge, but because such a distinction was needed for his philosophical program of finding the objective, neutral foundations of knowledge. Once the given/added distinction is abandoned, knowledge acquisition becomes naturally holistic and developing an atomistic theory of how and why knowledge is possible becomes hopeless

In "Two Dogmas of Empiricism" (1953), W. V. Quine attacks two important assumptions of empiricism. First, Quine shows that the analytic/synthetic distinction is not absolute or essential, but it is merely conventional. Quine asserts the impossibility of defining "analyticity" except in extremely unimportant and trivial cases like "no unmarried man is married". Quine shows that, in its more general and frequent usage, it is impossible to define "analyticity" without assuming some "synthetic" (empirical) facts. Thus, it becomes impossible to define an essential "philosophical" analytic/synthetic distinction. (See Quine (1953) for his lengthy argument on the topic). According to Quine, certain statements are appropriately called "analytic", because there is virtually total consensus about the meanings of the terms involved, and given our linguistic rules, it becomes very easy to reach an agreement on the truths of such statements. Quine is not against such a distinction as a useful "convention". Quine's criticism is the way philosophers have been using the distinction in order to construct a "reductionist" theory of verificationism. Thus, the "second dogma" that Quine attacks is the reductionist claim that for every synthetic statement, there must be a unique set of observations the occurences of which would help confirm that statement, an a unique set of observations the occurences of which would decrease the likelihood of its truth. Quine shows the problems involved in trying to test individual statements in isolation from the accompanying ones. According to him, statements can only be tested as a corporate body. Quine argues that in a scientific theory, the analytic and synthetic components can not be entirely separated. Furthermore, he claims that science is like a "... field of force, like a fabric which impinges on experience only along the edges", but "... no particular experiences are really linked with any statement in the field except indirectly through considerations of equilibrium affecting the field as a whole" (1953). Accordingly, there are many ways of accomodating a theory to an "abnormal experience". We choose a particular way of doing it, not not due to some absolute scientific principle, but because it is convenient, causing small disturbance in the existing theory. Thus, Quine's view of justification is holistic and conversational as opposed to reductionist and confrontational.

Quine's and Sellars' criticisms of the two fundamental assumptions of logical empiricism were important steps towards the formation of an anti-positivist philosophy of science. In the meantime, Thomas Kuhn published his extremely influential anti-positivist work, The Structure of Scientific Revolutions (1962). Kuhn attempts a historical analysis of how science progresses. He argues that, at any given epoch, the rules to be followed by science are dictated by the "ruling paradigm". During the periods of "normal science", the paradigm is accepted without any questioning of the underlying assumptions: "In its normal state, then, a scientific community is an immensely efficient instrument for solving the problems or puzzles that its paradigms define" (1970, p.40). Eventually comes a period when the "ruling paradigm" can not solve certain problems, and scientists start questioning the paradigm's fundamental assumptions. When enough scientists become convinced that it is impossible to solve the "anomaly" within the framework of the ruling paradigm, and only if an alternative paradigm is already available, then a "scientific revolution" takes place. The old assumptions are abandoned and replaced by new ones. Kuhn shows by historical examples that a scientific revolution involves a fundamental shift in the scientific worldview so that new problems are defined by the new paradigm. The perspective, the methods and rules to be followed, and even the "norms of rationality" are restated. What is rational in one epoch may be considered irrational in another epoch. In short, it is as if the scientist's world has totally changed. After the revolutionary paradigm establishes itself, it becomes the ruling paradigm for next generations to come, and the process repeats itself. Kuhn sees this process as "scientific progress". Kuhnian progress is not directed towards an objective and absolute "Truth", it is simply "successful creative work".

A scientific theory is accepted not because it is true in any absolute sense, but because it proves to be useful for the advancement of science in a particular era. The crucial anti-positivist element in Kuhn's thesis is that everything a scientist does depends on the dominant scientific worldview. Accordingly, "theory-free observation" is simply not possible. Everything from the initial formulation of the problem to the interpretation of the results is shaped by the dominant worldview. Richard Rorty illustrates this idea by stating that Newton did not necessarily give "right answers to the questions to which Aristotle had given wrong answers", because they were not necessarily asking the same questions (1979, p.266). According to Kuhn, ruling paradigms of different epochs are "incommensurable" because they do not even deal with the same problems. Kuhn's thesis that there can be no "neutral observations" has done considerable damage to logical empiricism, because the entire verification (falsification) theory assumes the possibility of neutral observations.

After the 1960's, faced with the kinds of criticisms illustrated in the previous paragraphs, logical empiricism has had to acknowledge the impossibility of purely formal, ahistorical, acultural analysis of scientific inquiry. Karl Popper recognized the importance of understanding the "internal history" of science, though he still tried to exclude sharply the "external" factors influencing scientific inquiry. His view of history of science was a "rational reconstruction" of history under the principles of "scientific rationality". His student Imre Lakatos holds an even less positivist view of science. In his view, the history and psychology of science are important in understanding how science progresses. Lakatos also acknowledges that entirely rational reconstruction of history is impossible, that studies of both internal and external histories are necessary. He rejects "naive falsificationism", having observed that "no experiment, experimental report ... alone can lead to falsification" (Lakatos 1970).

In the 1970's, philosophers and scientists have increasingly acknowledged the inadequacies of logical empiricism. Today, logical empiricism has lost its prestigious place it held in the first half of this century. The purely formal, algorithmic, abstract "organon" of logical empiricism has proven inadequate for the practhcal questions facing the studies of science. Many Philosophers now hold that it is impossible to explain the scientific change as an entirely objective process. Stephen Toulmin describes this tendency as "From Form to Function" (1977). Thus, the "doors of history, psychology and sociology" have opened one by one to the philosophy of science (Toulmin 1977). Toulmin observes that after the 1960's, terms like "historicism", "relativism" or "psychologism" were not anymore being used to discredit those who "mixed" history, sociology or psychology in their philosophical works. As a consequence of this, Toulmin notes, "These days, we are all prepared to be 'interdisciplinary'" (1977). The pursuit of timeless and absolute truths has become out of fashion. "Practical use" has taken the place of formal rigor, "truth" and "excellence". In short, "formal" was being replaced by "functional" (Toulmin 1977).

This brief historical review of epistemology and philosophy of science shows that there exists two opposing philosophies: The traditional formalist/absolutist camp and the new functional/relativist camp. In the following sections, we shall see the implications of both philosophical positions for model validation controversy.

IV- IMPLICATIONS FOR SD MODEL VALIDATION

If one adopts a logical empiricist, reductionist, formalist philosophy of model validation, then validation is seen as a strictly formal, algorithmic, "atomistic" and "confrontational" process. Since the model is assumed to be an objective and absolute representation of the real system, it can be either true or false. And given that the analyst uses the proper validation algorithms (and (s)he is honest), once the model "confronts" the empirical facts, its "truth" (or falsehood) is automatically revealed. Validity becomes a matter of "formal accuracy", rather than practical use.

If one takes a relativist, holistic, functional philosophical approach to the validity problem, then validation becomes a semi-formal, conversational process. A valid model is assumed to be only one of many possible ways of describing a real situtation. No particular representation is superior to others in any <u>absolute</u> sense. No model can be entirely objective, for every model carries in it the modeler's worldview. Models are not either true or false, but lie on the continuum of <u>usefulness</u>. Model validation is a process of building confidence in the usefulness of the model. Such a process is inherently gradual and at best partly algorithmic. Validity does not reveal itself automatically as a result of some formal tests, but it builds gradually as a result of a social process. Validation is a matter of social conversation, because establishing model usefulness is a conversational matter. This is especially true when the model user is not the model builder, in which case the user must be <u>convinced</u> about the usefulness of the model.

Thus, we see that the two opposing schools of philosophy of science imply two opposing philosophies of model validation. In the following sections, we shall illustrate this observation by referring to specific articles. Although our main topic is SD model validation, we shall also present examples of non-SD articles addressing some fundamental issues of model validation. (Our intent is by no means to give an extensive literature review. For a quite complete review of validation literature, the reader is referred to Wright and Shahin (1980)).

IV.1. Relationships with Non-SD Modeling Literature

One of the early and important non-SD articles dealing with philosophical aspects of validation is Naylor and Finger's "Verification of Computer Simulation Models" (1968). The authors discuss some basic philosophical positions in validation controversy: 1- "Rationalism", 2- "Empiricism" and 3- Milton Friedman's "positive economics" which asserts that assumptions of a hypothesis should <u>not</u> be required to be verified, that the only criterion of confirmation is the model's predictive ability. Naylor and Finger argue that in practice, these three views need not be mutually exclusive, and try to combine the three in a "multi-stage" verification program. Although Naylor and Finger take an eclectic approach, their fundamental asumption is actually empiricist: "... a simulation model the validity of which has not been ascertained by empirical observation, may prove to be of interest for expository or pedagogical purposes (eg. to illustrate a particular simulation technique), such a model contributes nothing to the understanding of the system being simulated" (1968). The article also holds the view that a model is either true or false, rather than viewing validity as a 'degree of usefulness'.

Another article, published about the same time, but closer to the opposite philosophical view is Mitroff's "Fundamental Issues in the

Simulation of Human Behavior" (1969). Mitroff argues for C. W. Churchmann's "experimentalism". This view holds that reality can not be known as an isolated object; it is not a "starting point", but a "process" of going back and forth between the world and the model. According to experimentalism, knowledge is holistic and social, and both model building and model validation are inevitably subjective, by being aspects of one's theory of scientific inquiry. Mitroff (1969) notes that those elements we choose as "essential" and include in our model are probably also chosen as "essential" in validating the model.
Milton Friedman's "positive economics" discussed briefly in Naylor and Finger (1968) is analyzed by Cyert and Grunberg (1963) at more length. According to Friedman, the assumptions of a hypothesis need <u>not</u> be realistic. A hypothesis is confirmed only by its predictive success. Given that such a success is achieved, the validity of the assumptions is irrelevant. (This sounds very much like early logical empiricism). In Friedman's example of the "expert billiard player", the hypothesis he considers is: "the player solves the formal mathematical problem of the path of the balls required for success". Now, this hypothesis is based on the assumption that the player has the mathematical knowledge to solve such a complex mathematical problem. It is easy to disconfirm this assumption by testing the player for his mathematical skills. But for Friedman, such disconfirmation is irrelevant to the verification of the hypothesis! If the latter predicts that the player will make certain shots on certain situations, and if the player does make the predicted shots in all those situations, then the hypothesis is confirmed. Cyert and Grunberg criticize this view. They point out that Friedman's first mistake is his belief that conclusive empirical confirmation is possible. They take the Popperian view that hypotheses can only be disconfirmed. The second -and fundamental- problem with Friedman's theory is that, followed literally, it would lead to the acceptance of hypotheses without any critical appraisal or discussion. His theory implies that "explanatory power" has no role in hypothesis confirmation. Cyert and Grunberg propose that we give much more emphasis to the explanatory ability of models. They make the important observation that acceptance of "billiard player's knowledge of advanced math" comes from an unwillingness to study his actual decision-making process. If we take the alternative approach of trying to model his decision making process and incorporate it in our hypothesis, then, the authors state, "we can not only join our knowledge with that of other disciplines studying similar behavior, but we will gain explanatory value for our models as well as predictive ability" (1963).
A very good overview of the problem of validating "large scale models" is provided by House and McLeod (1977). The authors approach the problem of validity from a very practical perspective, by considering what a "businessman would be willing to spend" for a model: "The businessman can not afford to discount a 'hoped-for' infinite return as the result of an unknown expenditure for a near-perfect model today. Our business world exists in the present, so the businessman will be satisfied to buy a somewhat less than a perfect model for a known cost" (1977). 'Perfect validity' is an unrealizable, ideal concept which implies that a model is an exact duplicate of the real system. Interestingly, the authors reject the desirability of 'perfect models' even as an ideal concept, because understanding them would be as difficult as understanding the real system!
This brief review of literature on validation illustrates how different views of model validation assume different philosophies of scientific

inquiry.

IV.2. Relationships with SD Validation Literature

The first exposition of the views of SD paradigm on the question of model validity was given in chapter 13 of Industrial Dynamics by Jay Forrester (1961). Forrester argues that validity of a model can not be discussed without reference to a specific purpose: Model validity is a relative concept. He makes the stronger claim that "the validity of a model should not be separated from the validity and the feasibility of the goals themselves". Since reaching an agreement on the feasibility of the goals can not be achieved through an entirely formal algorithmic process, validation becomes very much a matter of social discussion. According to Forrester, "any 'objective' model validation procedure rests eventually at some lower level on a judgement or faith that either the procedure or its goals are acceptable without any objective proof" (1961). Forrester also criticizes the illusion that using fixed statistical 'significance levels' brings objectivity to the validation procedure. His point is that the selection of the significance level must ultimately be tied to our goals. Another non-traditional view of Forrester is his willingness to accept non-quantitative model validation. He argues that a negative attitude towards 'qualitative' validation procedures is not justifiable, since "... a preponderant amount of human knowledge is in non-quantitative form" (1961). Finally, Forrester sees explanatory power as important as predictive power in model validation. Forrester's views on model validity correspond to the relativist, holistic philosophy of science. We shall see in the following sections, that this is true for System Dynamicists in general. Seven years after its publication, one of the most well-known and representative reviews of Industrial Dynamics was given by Ansoff and Slevin (1968). Ansoff and Slevin criticize -among other ideas of Industrial Dynamics- Forrester's views on model validation. First, they object to Forrester's claim that model validation need not be entirely quantitative. They quote from another critic of Industrial Dynamics, Harvey M. Wagner: "Does Industrial Dynamics represent a truly scientific approach? Or does it represent the judgemental approach of a particular scientist"? (1968). The authors admit that such a criticism should be directed not only to SD, but to the 'management science' in general. This implies that management science is not "truly scientific", because it is "qualitative and judgemental". This view assumes a utopic concept of science. Like logical empiricism, it assumes that there can be an entirely objective, "non-judgemental" method of inquiry. Ansoff and Slevin point out that Forrester is not as much concerned with the quantitative predictive validity as an econometrician is. In Industrial Dynamics, the authors state, emphasis is placed on "making models 'true to life' the first time, on observing carefully, on testing boundaries, on testing the internal logic of the model, on obtaining parameters from real-life situations" (1968). The authors complain that neither a clear criterion of validity, nor the degree of "correspondance sought" is specified by Forrester, rendering the validation process not only qualitative but also subjective. They add that seeking "objective validity" does not necessarily mean seeking "absolute accuracy". According to this view, "absolute truth" is unattainable due to the imperfections of the inductive method, but not due to the subjective elements inherent in all inquiry. According to this "naively realist" view, scientific method has its limitations, yet it can be entirely objective. Ansoff & Slevin overemphasize "quantitative", "formal" validation. Towards the end of the article, they state the first condition a theory must meet: "It should embrace a well-defined body of <u>observable</u> variables (emphasis added)" (1968). Overall, Ansoff & Slevin defend a philosophy of model validation that has strong logical empiricist elements

In his response to Ansoff and Slevin, Forrester (1968) articulates his relativist ideas of model validity presented in <u>Industrial Dynamics</u>. He reemphasizes the role of 'explanation' in model validation by stating that a model may well replicate the observed behavior "for the wrong reasons". Forrester also asserts that validation is ultimately an "agreement" and not a proof. Thus, although the question of validity has no definite answer "in the abstract", he states he has "never personally encountered a situation where, in the context of a specific system, a particular model and a clear purpose, there was a continuing disagreement about validity" (1968). Once again, Forrester argues for a "conversationalist", "functional" philosophy of model validation.
Another strong criticism of SD method is given by Nordhaus (1973). Nordhaus' paper mostly consists of specific technical criticisms of a specific SD model, namely Forrester's <u>World Dynamics</u>. The technical criticisms are naturally beyond the scope of our article (a detailed technical response is provided by Forrester, Low and Mass 1974). But a few general assertions made by Nordhaus on the question of model validity are pertinent to our discussion. The author states that "the treatement of empirical relations in <u>World Dynamics</u> can be summarized as <u>measurement without data</u>", "... as not a single relationship is drawn from empirical studies". To what extent these criticisms are valid depends on what the author means by "empirical studies", on the <u>purpose</u> and intended <u>use</u> of the model, none of which specified in the article. But beyond the technicalities, the author does hold an empiricist philosophy of science quite incommensurable with that of System Dynamics. Quoting from Naylor and Finger, he claims that a model not subjected to empirical validation is "void of meaning". Such a "criterion of meaning" is reminiscent of the extreme logical empiricism of the 1930's.
An important philosophically oriented SD article is Donella Meadow's "The Unavoidable A Priori" (1980). The central idea of the article is the Kuhnian thesis that every modeling school inevitably has biases that influence the selection of problems, solution methods and evaluation criteria. Meadows compares the major assumptions of two specific modeling schools: System Dynamics and Econometrics. Accordingly, the major assumption of SD is that the behavior of a complex system arises from its causal structure, that people do things for some reason (whether known or not). The process of modeling consists of writing causal equations that in some way describe the system's structure. To be able to explain the behavior by the system's internal structure (rather than by external influences), the modeling approach must be extremely 'holistic' and 'interdisciplinary'. The approach is non-empirical in its classical sense, not requiring strict numerical empirical validation. Many of the equations may be derived by "conversations with people involved". Meadows next takes the Econometric modeling and states that in such models causality is not a major concern. The model equations, mostly dictated by data, do not make an explicit claim of causality. The crucial criterion is that the model predicts; 'causal explanation' is not sought for. The approach is empiricist, highly "atomistic" and "non-interdisciplinary". Next, comparing the SD and Econometrics paradigms, Meadows asks "Will one competing paradigm eventually eliminate the other completely (as a Kuhnian position would imply)"?

Meadows, while admitting that the two disciplines can not be mixed, states that the two can co-exist because they do not compete to solve the same type of problems (long-term perspective vs. short-term forecasting).

Finally, the most complete discussion of model validation in SD is given by Forrester (1973) in an unpublished research paper. In an attempt to clarify the issues underlying the model validity debate, Forrester asks how and why the concept of validity is interpreted differently by different groups of people. He observes that most professionals (managers, engineers, doctors) take validity as "relative usefulness", whereas most literature on social systems modeling sees it as a "formal logical concept rather than a pragmatic issue". Forrester calls the two groups the "operators" and the "observers" respectively. How "operators" see validity is very similar to House and McLeod's description of how businessmen see validity. An operator sees a model as an incomplete, imperfect theory about his reality, which is valid if it proves to be a useful tool in making decisions. Forrester stresses that an operator "seeks shared confidence" because he is "seldom a secure and absolute dictator. He must persuade, he must explain, he must lead" (1973). For an operator, model validation is very much a "public process". To illustrate the viewpoint of an "observer", Forrester refers to the notion of "logical validity of an inference" (described earlier in this article as the "philosophical problem of validity of an argument"). Forrester claims that many "observers" have such a concept of "validity" when they seek absolute and objective model validity tests. Now such tests will tell us whether a logical mistake is made in deriving model implications from its assumptions, but nothing about the relevance of the model to a real-life problem. Such tests are necessary, but insufficient to establish model credibility. Forrester seems to suggest that many "observers", not having to make real-life decisions, are confused about the two aspects of model validity . According to Forrester, such "observers" fail to see the impossibility of model justification by entirely formal objective tests: For them, "the appropriateness of the assumptions is not a part of the validity issue". (M. Friedman's "positive economics" described earlier is an illustration of this view). Thus, Forrester argues that models built by such observers become "collector's items", having no purpose of practical use. And he concludes that since for most observers practical use is not important, rather than seeking "shared confidence and consensus", observers would seek debate: "The observer aims not to create public constituency, but instead to display individual effort, diligence and virtuosity" (1973).

This brief survey of literature shows that the views of System Dynamicists on validation are in the direction of the "relativist" philosophy of science. SD practitioners see the validation problem much the same way the new philosophy of science sees the problem of "theory verification". Accordingly, validation ("verification") is inevitably relative. It is a matter of social conversation, rather than objective confrontation. It is holistic, rather than reductionist, practical, rather than formal. Having seen the connections between model validation and the two opposing philosophies of science, we can repeat the question posed at the beginning of this article: "Is System Dynamics method truly scientific?" The answer is obvious: "It depends on one's philosophy of science". If one adopts the traditional formalist, empiricist philosophy, then SD method does not sound entirely scientific. We showed in this article that this type of philosophy underlies most of the empiricist criticisms of SD methodology. If on the other hand, one adopts the recent relativist philosophy of science, then there is nothing "unscientific" in the way SD treats the question of model validity.

V- CONCLUSIONS

In this paper, we started by stating that since System Dynamics type of (causal) models are very much like scientific theories, we tend to ask that validation of such models conform with our norms of "scientific inquiry". But then we showed that the philosophy of science does not present a unique view about the nature of scientific inquiry. We described two fundamental and opposing philosophies of science: The traditional logical empiricist view which holds that scientific theories can be verified (falsified) by entirely objective formal methods. The natural implication of this view is that model validation can be carried out by entirely objective formal, "confrontational" methods: validity means "truth". The modern, relativist philosophy of science, on the other hand, holds that scientific theories can only be verified by gradual, conversational, semi-formal methods. This means that model validation too can only be carried out by semi-formal, holistic, conversational methods: validity means relative usefulness. Then, we showed that the System Dynamics paradigm sides with this recent relativist philosopy of science on the issue of model validity. Similarly, most criticisms of System Dynamics methodology are based on the opposing logical empiricist philosophy of science. These conceptual links between the philosophies of science and the views of model validation have important practical implications for both the System Dynamicists and their critics.
First, the implications for the critic's position. The critic who accuses SD for being "unscientific" because SD validation procedures are not "objective, formal and quantitative enough" should know that his/her view of "scientific objectivity" and "formalism" represents only one side of the fundamental philosophy of science debate. S(he) should take into account the fact that there is an alternative widely held philosophy of science that is in agreement with how System Dynamicists view the model validity question. This being so, critics should try to avoid criticizing SD model validation based on such general characterizations as "not objective", "not empirical", "not formal enough". Such criticisms will never be persuasive for the System Dynamicist who happens to hold just the opposite philosophical view on such issues. To be constructive, critics should take specific SD model validation techniques and applications and explain why they think these are weak tools of model validation. Critics must be able to say "the following specific validation tools you are using are not convincing for the following reasons". Then, they must suggest alternative more "objective and formal" methods, and state why these alternative methods would help increase the validity of the model.
Our conclusions have some practical implications for the System Dynamicist's position as well. Real-life experience has taught most SD (or other causal) modeling practitioners that models are inherently incomplete, relative and partly subjective and that model validity is really usefulness with respect to a specific purpose. But at the same time, most practitioners unaware of the recent relativist philosophical developments, would think that their own view of model validity is not

really "quite scientific". These practitioners have been influenced by the established traditional philosophy of science that requires a utopic objectivity and formalism for an inquiry to be "scientific". Thus, many practitioners, while experiencing that validation is bound to be a relative, semi-formal and conversational process, at the same time see this as a weakness of their modeling fields. We show in this article that the recent relativist philosophy of science claims just the opposite: Accordingly, even the scientific theories of natural sciences are justified much the same way as models of social systems are validated. There is no qualitative difference between the two: They are both semi-formal, relative, holistic, social processes. SD-type modelers owe no apology for not meeting an outmoded and utopic criterion of scientific inquiry.

Finally, we must point out that the relativist philosophy of model validation does not imply that pursuit of formal quantitative validation tools be abandoned. On the contrary, such tools are most useful when they are used with the relativist philosophical perspective. Accordingly, formal tools can not be complete tests of model validity and they can not turn the overall validation problem into a purely objective formal, algorithmic process. But, these tools are very effective ways of organizing, summarizing and communicating information. Formal tests can not automatically determine the validity of a model, but they can provide valuable information in judging and communicating the usefulness of a model. Since the relativist philosophy emphasizes that validation is a matter of social conversation, System Dynamicists should be the first to appreciate the role of formal quantitative tools in summarizing the information pertinent to model validity and communicating it to the interested community. (See Sterman 1984 and Barlas 1985). The challenge is to design quantitative measures that capture information pertinent to the model's usefulness with respect to its purpose.

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