Philosophical roots of model validation: two paradigms

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System dynamics models, as causal models, are much like scientific theories. Hence, in evaluating such models, we assume certain norms of scientific inquiry. Most critics hold that the system dynamics approach does not employ formal, objective, quantitative model validation tests. This article argues that this type of criticism presupposes the traditional logical empiricist philosophy of science, which assumes that knowledge is an objective representation of reality and that theory justification can be an objective, formal process. According to the more recent relativist philosophy of science, knowledge is relative to a given society, epoch, and scientific world view. Theory justification is therefore a semiformal, relative social process. We show that relativist philosophy is consistent with the system dynamics paradigm and discuss the practical implications of the two philosophies of science for system dynamics modelers and their critics.

In the natural and social sciences the question of how models should be validated has been a controversial issue for many years. Especially in the social sciences, this controversy has become more crucial as new and complex modeling tools have emerged in recent years. System dynamics methodology constitutes one such tool, and not surprisingly, system dynamics model validation practices have been the subject of close scrutiny.

In the last 25 years there have been numerous reviews of the system dynamics approach. We have witnessed a heated debate on model validation (see, for example, Ansoff and Slevin 1968; Forrester 1968; Nordhaus 1973; Forrester, Low, and Mass 1974; Forrester and Senge 1980; Forrester 1980; Zellner 1980; Richardson and Pugh 1981, ch. 5, 6). Throughout this long debate, criticisms of system dynamics methodology have had one common general theme: system dynamics does not employ formal, objective, quantitative model validation procedures, which are supposed to be fundamental to scientific inquiry. The implication of this type of criticism is that system dynamics models are not "truly scientific." System dynamicists have responded by stating that model validity is strongly tied to the nature and context of the problem, the purpose of the model, the background of the user, the background of the analyst, and other considerations. Accordingly, model validation is inherently a social, judgmental, qualitative process: models cannot be proved valid but can be judged to be so.

Clearly, there are fundamental differences between the world views of the two sides of this debate. In this article, we discuss these philosophical differences. We show that the validity controversy is strongly tied to a fundamental problem in the philosophy of science: What is scientific knowledge, and what constitutes confirmation of a knowledge claim? Different philosophical schools have proposed radically different answers to this question. After reviewing the historical development of this major philosophical debate, we point out its practical implications for system dynamics model validation.

Models and model validity

We distinguish between two fundamentally different types of mathematical models: causal (theory-like) models and noncausal (statistical/correlational) models. Causal models base their mathematical expressions on postulated causal relations within the modeled system. By making causal claims about how certain aspects of a real system function, they constitute theories about that system. Therefore, such models can be used for both prediction and explanation. Noncausal mathematical models, on the other hand, simply express observed associations (in the form of statistical correlations) among various elements of a real system. Such models are purely empirical (correlational); their mathematical relations are not based on a theorized causal mechanism. These models should be used only for prediction purposes, on the assumption that they work only within a certain range of values of variables.

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System dynamics models belong to the class of causal mathematical models. Hence, a system dynamics model embodies a theory about how a system actually works in some respect. Since individual model equations claim to be causal statements about system relations, each individual equation must be defended and justified. For validation, this causal claim is the crucial property of system dynamics models, and it is the reason that system dynamics 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 matches the observed data. The same is not true for a purely correlational model, which is deemed valid if the model output matches the observed data to a certain degree of accuracy.

Since system dynamics models are much like scientific theories, scholars tend to apply accepted norms of scientific theory testing to validate them. This is where one faces fundamental philosophical questions: What constitutes justification of a knowledge claim? Is it possible to completely confirm the truth of a statement? What constitutes confirmation of a scientific theory? For centuries philosophers have discussed these questions in the context of the natural sciences and proposed differing answers, which are relevant for the system dynamics validity debate.[1]

The following section describes the historical development of two major philosophies of science: the logical empiricist/reductionist school and the relativist/holistic school. In the concluding section, we discuss the implications of these two philosophies for the system dynamics validity debate.[2]

The historical development of a major philosophical debate

Philosophy of science is a relatively new discipline that emerged in the late nineteenth and early twentieth centuries, but it is closely related to a much older philosophical subject, epistemology (the theory of knowledge). Epistemologists seek the conditions that make knowledge possible. To understand the philosophy of science debates of the twentieth century, it helps to know the major epistemological theories of the sixteenth through the nineteenth centuries. (Figure 1 portrays the philosophical developments discussed here.)

Epistemology

RENÉ DESCARTES' RATIONALISM. The idea of developing a coherent theory of knowledge can be traced to René Descartes (1596–1650). Descartes believed that philosophy needed a new method, the deductive reasoning of mathematics, because the only truths that could be accepted without any doubt were ones revealed by this method. He claimed that 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 can doubt. He concludes that the mind ("Thinking Self") exists with certainty

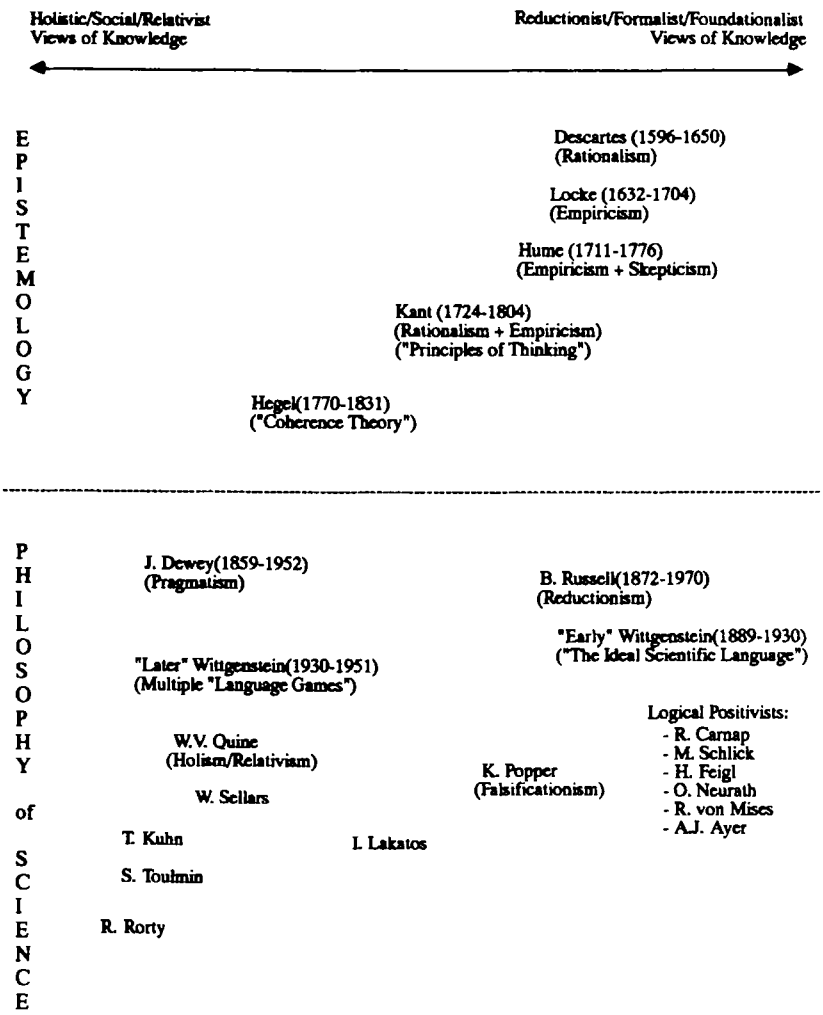

Fig. 1. Historical development of the different theories of knowledge leading to two opposing philosophies of science. For a comment on Rorty's place in this scheme, see note 3.

("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 corporeal objects are nonexistent. He reasons that external objects must exist, yet we can never be sure of their existence because our knowledge about them is uncertain. For Descartes, the only true knowledge is the kind revealed by deductive reasoning, inferred from self-evident propositions.

JOHN LOCKE'S EMPIRICISM. The other important source of modern theories of knowledge is the works of John Locke (1632–1704), the founder of the empirical theory of knowledge. In *An Essay Concerning Human Understanding* (1690), Locke hopes to discover where ideas and knowledge come from, what we are capable of knowing, and how certain knowledge can be. Locke disagrees sharply with Descartes, believing that none of our ideas are innate. According to him, the 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. Acquaintance is prior to description: the ideas are first put into the passive mind, and then the mind starts manipulating them.

KANT'S SYNTHESIS OF RATIONALISM AND EMPIRICISM. Immanuel Kant (1724–1804) defined the epistemological problem as a search for the principles of thinking (1781). Kant had been influenced by the two most important philosophical schools of his time: Descartes' rationalism and Locke'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; knowledge comes not from mere acquaintance with ideas but is developed by "description." The mind does not just receive knowledge but actively produces it. 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 relation between the external objects and the mind but in the necessary "nonempirical rules of understanding." This is the fundamental difference between Kant and Locke. For 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 key 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 judgments ("a straight line is the shortest distance between two points") are synthetic a priori. Kant believed that such statements are not only legitimate but also essential for knowledge to be possible.

At this point, it is important to note an assumption common to all mainstream theories of knowledge: knowledge is entirely objective, asocial, acultural, ahistorical "Truth" rather than socially justified belief. It follows that knowledge acquisition can be understood by pure philosophical analysis, independent of all the social, cultural, and historical conditions of a 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. Philosopher Richard Rorty (1979) calls this ongoing search for neutral foundations of knowledge the foundationalist philosophy. According to Rorty, the attempt to find the foundations of truth in something permanent and neutral has historically manifested itself in different philosophies. Yet all foundationalist philosophies, he observes, share a unifying assumption: knowledge results from our "privileged relationships" with the world, and once we understand them, we can tell exactly which statements are "objectively" true. Rorty uses the

metaphor "mirror of nature" to describe this privileged relationship: the foundationalists' concept of knowledge is the reflection of nature on an "unclouded mirror."

A radically different theory of knowledge, on the other hand, claims that knowledge is socially justified belief rather than a product of mirroring nature. Such a theory was first formulated by Georg Hegel and later articulated by the pragmatist philosopher John Dewey (see, for instance, Dewey 1929). Accordingly, 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. 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. As discussed in the next section, this social/holistic theory of knowledge formed the basis of the relativist philosophy of science of the twentieth century. Similarly, the traditional (foundationalist) theory of knowledge gave birth to the logical empiricist philosophies of the twentieth century.

Mainstream philosophies of science

In the twentieth century, epistemology took an anti-Kantian turn by rejecting the legitimacy of Kant's synthetic a priori statements. Yet this was not a rejection of Kant's attempt to characterize the neutral foundations of knowledge; it was merely a rejection of Kant's way of doing it. 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 rejects the Cartesian (rationalist) component of Kant's epistemology. According to Russell, the foundations of knowledge are not to be found in the mind but rather in those propositions that come from "direct acquaintance" with objects. Russell (1949) argues that statements about the physical world can be translated into statements about "sense data," data of immediate experience. This reductionist claim that statements can be categorized according to the degree of their empirical content has been very influential in the development of the empiricist philosophy of science. As we discuss later, philosophers have assumed that propositions could be separated into empirical and nonempirical components and that the empirical components could then be isolated and verified against empirical data.

Like Russell, Ludwig Wittgenstein held strong reductionist and empiricist views (which he abandoned in his later years). In *Tractatus* (1922), 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 judgments, ethical arguments, most philosophical inquiries.) Wittgenstein argues that people frequently talk nonsense because of the

deficiencies of ordinary daily language. An ideal language system ("logical symbolism") would prevent the expression of nonsense by excluding those statements that are neither logically deducible nor empirically verifiable. Wittgenstein's early thinking was influential in the development of logical empiricism, the most widespread philosophy of science until the 1960s.

LOGICAL EMPIRICISM. 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 in the 1920s and 1930s 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 metalanguage for philosophical analysis of scientific language systems. Understood in its widest sense, logical empiricism comprises a majority of the early philosophies of science. Most logical positivists, including Rudolf Carnap, Moritz Schlick, Otto Neurath, Herbert Feigl, Alfred J. Ayer, C. G. Hempel, and Richard von Mises, would tend to agree with the following statements:

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 (statements of fact), which 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 is to reduce all scientific languages into one unified canonical form (the 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, and psychological process and lies completely outside the domain of philosophical analysis.

SOME PROBLEMS WITH EARLY LOGICAL EMPIRICISM. One of the major flaws of early logical empiricism was a logical problem involved in the principle of verification. Consider a theory T and its conclusion C. C is derived from T according to the following 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 a general nature (scientific theories) can never be fully verified by observation. Karl Popper (1959) analyzed this "problem of induction" and suggested his own solution, the principle of falsification. 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, 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 nonfalsifying 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 hard-line logical empiricism. Yet, like verifiability, the falsifiability principle has strong logical empiricist elements and is not without problems. It too assumes 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 hold, because a typical theory is always presented together with a set of assumptions:

If assumptions A and theory T hold, then C follows.

Now, if C is not observed, it is not always clear whether it is because of a wrong theory or invalid assumptions. It is thus possible to save the theory by stating that the assumptions were violated. Another practical problem is that an observation rarely ever comes as either C or non-C but mostly as complex data. It is largely up to the scientist to organize and interpret the data and to decide whether the observation actually constitutes a falsifying instance.

A second major problem with early logical empiricism was its insistence 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. 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 the construction of new theories, but has nothing to do with justification. Stephen Toulmin (1977), reviewing the last 50 years of philosophy of science, argues the absurdity of relying merely on predictions, noting that we would then consider horse race tipsters scientists and evolutionary biology nonscientific. Faced with this difficulty, many empiricists had to accept the importance of explanation as evidence of knowledge. This acceptance, Toulmin observed, "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 cannot be captured in a merely formal algorithm" (Toulmin 1977).

MODERN CHALLENGES TO LOGICAL EMPIRICISM. Foundationalist epistemologies were criticized as early as the eighteenth and nineteenth centuries (for instance, by the philosophies of Hegel and Dewey), but the bulk of consistent criticism has come in the second half of the twentieth century. Richard Rorty (1979) mentions two very important works that challenged the basic assumptions of epistemology taken for 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 the mind. This fundamental distinction between the given and the added is questioned by Wilfrid Sellars in *Science, Perception and Reality* (1963). Another basic assumption of logical empiricism is 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).

QUINE'S CRITIQUE OF THE ANALYTIC/SYNTHETIC DISTINCTION. In "Two Dogmas of Empiricism," Quine first shows the impossibility of drawing unambiguous and absolute distinctions between analytic and synthetic statements. According to Quine, the analytic/synthetic distinction must be seen simply as a useful convention. He shows the impossibility of giving an absolute definition of *analyticity* except in extremely trivial cases like "no unmarried man is married." He argues that in its more general and frequent use the word is impossible to define without assuming some synthetic (empirical) facts. Thus, it becomes impossible to make an essential philosophical distinction between the analytic and the synthetic.

The second "dogma" that Quine attacks is the reductionist claim that for every synthetic statement there must be a unique set of observations the occurrences of which would help confirm that statement, and a unique set of observations the occurrences of which would decrease the likelihood of its truth (confrontational theory of knowledge confirmation). He argues that there are many ways of accommodating a theory to an "abnormal experience." We choose a particular way of doing it not because of some absolute scientific principle but because it is convenient, causing minimal disturbance in the existing theory. Thus, Quine's view of justification is holistic and conversational as opposed to reductionist and confrontational.

SELLARS CHALLENGES THE GIVEN/ADDED DISTINCTION. 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 (1963) 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. Sellars argues that 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. According to Sellars, knowledge is possible only with some kind of "shared language." Thus, knowledge (which is a linguistic product) is socially justified belief. Rorty (1979) observes that Kant had made the given/added distinction not because he had discovered something fundamental about how the mind acquires knowledge but because such a distinction was necessary for his philosophical program of finding the objective, neutral foundations of knowledge. Once the given/added distinction is abandoned, knowledge acquisition becomes naturally holistic; developing an atomistic theory of how and why knowledge is possible becomes hopeless.

KUHN CHALLENGES THE TRADITIONAL CONCEPT OF OBJECTIVITY. Another extremely important work moving toward the formation of an antipositivist philosophy of science is Thomas Kuhn's *The Structure of Scientific Revolutions* (1962). Kuhn presents a historical analysis of how science progresses and 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 with minimal 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, 40). Eventually comes a period when the ruling paradigm cannot solve certain problems, and scientists start questioning the paradigm's fundamental assumptions. When enough scientists become convinced that it is impossible to solve the anomalies accumulating within the framework of the ruling paradigm, and only if an alternative paradigm is 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 world view so that new problems come to be defined by the new paradigm. The perspective, the methods and rules to be followed, and even the norms of rationality are restated. A prestigious scientific theory of one epoch may be considered meaningless in another epoch. 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 toward an objective and absolute "truth" but simply toward "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. No theory is objective in the sense of "corresponding only to what is out there." (According to the relativist philosophy, objectivity is a "property of theories chosen by a consensus of rationalist discussants"; see Rorty 1979, 333–342.) The crucial antipositivist element in Kuhn's thesis is that everything a scientist does depends on the dominant scientific world view. Accordingly, theory-free observation is simply not possible. This thesis attacks logical empiricism at its roots, because the entire verification (falsification) theory is based on the possibility of neutral observations.

THE EMERGENCE OF RELATIVIST PHILOSOPHIES OF SCIENCE. After the 1960s, faced with these criticisms, many logical empiricists acknowledged the impossibility of purely formal, ahistorical, acultural analysis of scientific inquiry. Karl Popper recognized the importance of understanding the "internal history" of science, although he still tried to exclude sharply the external factors influencing scientific inquiry. His view of the 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 an 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 1970s, philosophers and scientists have increasingly acknowledged the inadequacies of logical empiricism. Today, logical empiricism has lost the prestigious place it held in the first half of this century. The purely formal, algorithmic, abstract "organon" of logical empiricism has proved inadequate for the practical questions facing science. Many philosophers now hold that it is impossible to explain scientific change as an entirely objective process. Thus, as Toulmin (1977) asserts, the "doors of history, psychology, and sociology" have opened one by one to the philosophy of science. He observes that after the 1960s terms like *historicism, relativism,* or *psychologism* were no longer 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.'" The pursuit of timeless and absolute truths has gone out of fashion. The criterion of practical use has taken the place of formal rigor, "truth," and "excellence." In short, the formal is being replaced by the functional (Toulmin 1977).

Our brief review of epistemology and philosophy of science shows that the reductionist/foundationalist philosophy, which had remained basically unchallenged since the seventeenth century, now faces a viable opposition: the holistic/relativist philosophy. As we have mentioned, Quine challenged the reductionist theory of knowledge justification and proposed a holistic/conversational theory. Sellars questioned the empiricist assumption that learning of the particulars constitutes the basis of knowledge; he argued that knowledge is a linguistic product, socially justified belief, and that its acquisition is holistic. Kuhn challenged the foundationalist concept of objectivity and showed the impossibility of neutral observations. Such criticisms led to the decline of the foundationalist/reductionist philosophy of science and to the emergence of the relativist philosophies.

Implications for model validation

If one adopts a logical empiricist, foundationalist philosophy of model validation, then validation is seen as a strictly formal, algorithmic, reductionist, 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, 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, functional, holistic philosophical approach, then validation becomes a semiformal, conversational process. A valid model is assumed to be only one of many possible ways of describing a real situation. No particular representation is superior to all others in any absolute sense, although one could prove to be more effective. No model can claim absolute objectivity, for every model carries in it the modeler's world view. Models are not true or false but lie on a continuum of usefulness. Model validation is a gradual process of building confidence in the usefulness of a model; validity cannot reveal itself mechanically as a result of some formal algorithms.[4] Validation is a matter of social conversation, because establishing model usefulness is a conversational matter.

Thus, we see that the two opposing schools of philosophy of science imply two opposing philosophies of model validation. In the following sections, we illustrate these implications by using selected articles from the modeling literature. Although our main topic is system dynamics model validation, we also present examples from management science and economics literature that address some philosophical issues of model validation.[5]

Connections with management science literature

One of the early and influential modeling 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 the validation controversy: rationalism, empiricism, and Milton Friedman's positive economics. Naylor and Finger argue that in practice these three views are not mutually exclusive; they combine them in a multistage verification program. Although Naylor and Finger take an eclectic approach, their fundamental assumption 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, [but] such a model contributes nothing to the understanding of the system being simulated" (1968). Furthermore, the article holds the view that a model is either true or false rather than viewing validity as a degree of usefulness.

Closer to the relativist philosophical school 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 cannot 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 relative to the modeler'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 for validating the model.

An overview of the problem of validating large-scale models from a relativist point of view 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 cannot 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." Perfect validity is an unrealizable, ideal concept that implies 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!

Connections with economics literature

The dichotomy between empiricist and relativist philosophies of science can also be seen in views of model validity in the economics literature. An excellent illustration

of this dichotomy is Milton Friedman's positive economics and its critique by Cyert and Grunberg (1963). According to Friedman, the assumptions of a hypothesis need not be verified; a hypothesis is confirmed only by its predictive success. Given that such success is achieved, the validity of the assumptions is irrelevant. In Friedman's example of the expert billiard player, the hypothesis he examines 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 problem. It is easy to refute this assumption by testing the player for his mathematical skills. But for Friedman, such refutation would be irrelevant to the verification of the hypothesis! If the latter predicts that the player will make certain shots in 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 by suggesting that Friedman's first mistake is his belief that conclusive empirical confirmation is possible. They defend the Popperian view that hypotheses can only be disconfirmed. Furthermore, Friedman's thesis would lead to acceptance of hypotheses without any critical appraisal or discussion. His thesis implies that explanatory power has no role in hypothesis confirmation. Cyert and Grunberg (1963) propose that we give much more emphasis to the explanatory ability of models. They make the important observation that acceptance of the 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 into our hypothesis, the authors state, "we cannot only join our knowledge with that of other disciplines studying similar behavior, but we will gain explanatory value for our model as well as predictive ability." Cyert and Grunberg advocate an interdisciplinary and practical view of model validity.

Differences between the logical empiricist and relativist/holistic approaches to economics are discussed by Radzicki (1988; 1990). He contrasts neoclassical and institutional economics, finding that neoclassical economics assumes a logical empiricist philosophy of science and institutional economics adopts a relativist/holistic philosophy. Comparing institutional economics with the system dynamics paradigm, he argues that the two possess strikingly similar (relativist/holistic) world views and proposes a marriage between the two disciplines with the purpose of strengthening and extending both.

A comprehensive overview of validation of econometric models is provided by Dhrymes et al. (1972). For our purposes, the interesting feature of the article is that it adopts a hybrid philosophy comprising both empiricist and relativist elements. The authors divide the general model evaluation problem into two broad categories: parametric evaluation and nonparametric evaluation. A parametric evaluation consists of formal statistical tests of hypotheses. A nonparametric evaluation consists of less formal descriptive procedures that do not rely on statistical axioms. Parametric statistical tests, whenever available, are the authors' first choice. The underlying assumption is that these tests render the evaluation process objective, while nonparametric tests are considered ad hoc. This is the empiricist component of the authors' philosophy of validation. However, the authors do acknowledge the futility of

proving or disproving the "truth" of an econometric model, at least in the current state of our knowledge. Thus, they state, nonparametric informal/descriptive procedures can be "important and valid under many different axiomatizations." They further argue that "in this context, validation becomes a problem-dependent or decision-dependent process, differing from case to case as the proposed use of the model under consideration changes." Clearly, on this issue, the authors take a position that is closer to the relativist philosophy of science.

Connections with system dynamics validation literature

System dynamics literature is rather rich in articles that deal with various philosophical aspects of the model validation problem (see, for instance, Andersen 1980; Bell and Bell 1980; Bell and Senge 1980; Forrester and Senge 1980; Richardson and Pugh 1981, ch. 5, 6; Sterman 1985). In this section, we review only a few major articles that are most directly related to the philosophy of science dichotomies already discussed.

The first exposition of the system dynamics paradigm as it relates to model validity was given in Chapter 13 of *Industrial Dynamics* (1961) by Jay Forrester. Forrester argues that the validity of a model cannot 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 cannot be achieved through a formal algorithmic process, validation becomes very much a matter of social discussion. According to Forrester (1961, 123), "Any 'objective' model validation procedure rests eventually at some lower level on a judgment or faith that either the procedure or its goals are acceptable without any objective proof." 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 nontraditional view of Forrester is his willingness to accept "qualitative" model validation. He argues that a negative attitude toward qualitative validation procedures is not justifiable, since "a preponderant amount of human knowledge is in nonquantitative form" (1961, 128). Finally, Forrester sees explanatory power as being at least as important as predictive power in model validation. Forrester's views on model validity correspond to the relativist/holistic philosophy of science. We show in the following sections that this is true for system dynamicists in general.

Seven years after the publication of *Industrial Dynamics*, a well-known and representative critique of the book was given by Ansoff and Slevin (1968). A good part of their article is devoted to criticism of Forrester's views on model validation. First, Ansoff and Slevin 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 judgmental approach of a particular scientist?" The authors admit that such criticism should be directed not only to system dynamics but to management science in general. (This view implies that a discipline that has judgmental aspects cannot be truly scientific, which represents a utopic concept of science: like logical

empiricism, it assumes that there can be an entirely objective, nonjudgmental method of inquiry.) Ansoff and Slevin point out that Forrester is not as much concerned with quantitative predictive validity as an econometrician would be. They state that in *Industrial Dynamics* 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." Ansoff and Slevin complain that neither a clear criterion of validity nor the degree of correspondence 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 (rather popular) view, absolute accuracy is unattainable (because of the well-known deficiency of the inductive method), but objective validity *is* attainable. (Objectivity of science in this foundationalist sense is seriously challenged by Kuhn and Rorty.) Ansoff and Slevin emphasize quantitative, formal validation throughout the article. Toward the end of it, they state the first condition a theory must meet: "It should embrace a well-defined body of observable variables." Overall, the authors defend a philosophy of model validation that has strong logical empiricist elements.

In his response to Ansoff and Slevin, Forrester (1968) articulates the relativist ideas of model validity presented in *Industrial Dynamics*. He re-emphasizes 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." Once again, Forrester argues for a conversationalist, functional philosophy of model validation.

Another strong criticism of the system dynamics method is given by Nordhaus (1973). His paper consists mostly of specific technical criticisms of Forrester's *World Dynamics* (1971). The technical criticisms are beyond the scope of our article (a detailed response is provided by Forrester, Low, and Mass 1974), but the general character of the assertions made by Nordhaus on the question of model validity are pertinent to our discussion. Nordhaus states that "the treatment of empirical relations in *World Dynamics* can be summarized as measurement without data, ... as not a single relationship is drawn from empirical studies." From a relativist point of view, the validity of Nordhaus's criticisms depends on what he means by empirical studies and on the purpose and intended use of the model, neither of which is specified in his article. It is evident that Nordhaus holds an empiricist philosophy of science quite incompatible 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 1930s.

An important philosophically oriented system dynamics article is "The Unavoidable A Priori" (1980) by Donella Meadows. Her central idea 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. The system dynamics paradigm assumes causality and is holistic and interdisciplinary in nature. The approach is nonempirical in the classical sense, not requiring strict numerical empirical validation. Many of the equations may be derived by "conversations with people involved." In econometric modeling, Meadows says, causal explanation 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 should predict. The approach is empiricist, highly atomistic, and noninterdisciplinary. Comparing the system dynamics and the econometrics paradigms, Meadows asks, "Will one competing paradigm eventually eliminate the other completely (as a Kuhnian position would imply)?" While arguing that the two disciplines cannot be mixed, Meadows states that the two can coexist because they do not compete to solve the same type of problems.

Finally, the most complete discussion of the philosophy of system dynamics model validation is given by Forrester (1973) in an unpublished research paper, portions of which were revised and published by Forrester and Senge (1980). 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, respectively, the operators and the observers. How operators see validity is very similar to House and McLeod's (1977) description of how businessmen see validity. An operator sees a model as an incomplete, imperfect theory about reality that is valid if it proves to be a useful tool in making decisions. Forrester (1973) 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." 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" (see note 2). He claims that many observers have such a concept of validity when they seek absolute and objective model validity tests. Such tests will tell 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" (compare, for example, Friedman's positive economics). Thus, Forrester argues, models built by such observers become collector's items, having no purpose or practical use.

Practical implications

This brief survey of the literature shows that the views of system dynamicists on validation parallel the relativist philosophy of science. System dynamics practitioners see

the validation problem much the same way as the new philosophy of science sees the problem of theory confirmation. Accordingly, validation (confirmation) 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 discussed the connections between model validation and the two opposing philosophies of science, we repeat the question posed at the beginning of this article: Is the system dynamics method truly scientific? The answer is now clear: It depends on one's philosophy of science. If one adopts the traditional formalist, logical empiricist philosophy, then the system dynamics method is definitely not truly scientific. We showed in this article that this type of philosophy underlies most of the empiricist criticisms of system dynamics methodology. If, on the other hand, one adopts the more recent relativist philosophy of science, then the way system dynamics treats the problem of model validity is as scientific as the way any science treats the problem of theory confirmation.

These conceptual links between modeling approaches and philosophies of science have important practical implications for both system dynamicists and their critics. Critics who fault system dynamics for being unscientific because its validation procedures are not sufficiently objective, formal, and quantitative should be aware of the fact that the traditional (logical empiricist) view of scientific objectivity and formalism is not the only or unchallenged philosophy of science. They 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 model validity. Criticisms of a general nature, like not empirical, not objective, or not formal enough, should be avoided. Such criticisms will never be persuasive for the system dynamicist who happens to hold the relativist philosophical view of model validation. To be constructive, critics should explain why specific system dynamics model validation techniques and applications are weak. Then they must be able to suggest alternative validation methods and state why these alternative methods would help to increase the credibility of the model.

Our results have some practical implications for the system dynamicist's position as well. Real-life experience has taught most system dynamics practitioners that models are inherently incomplete, relative, and partly subjective, and that model validity means usefulness with respect to a purpose. But, at the same time, many practitioners unaware of the recent relativist philosophical developments would think that their own view of model validity is not truly scientific. Such practitioners have been confused by the historically dominant philosophy of science, which requires utopian objectivity and formalism for an inquiry to be scientific. Thus, many practitioners, while experiencing that validation is bound to be a relative, semiformal, and conversational process, at the same time see this as a weakness of their modeling efforts. But we show that there exists a relativist philosophy of science that is in agreement with system dynamics practice. Accordingly, even theories of the natural sciences are justified in much the same way as models of social systems are validated. There is no qualitative difference between the two: they are both semiformal, relative, holistic, social processes. System dynamics practitioners do not have to be apologetic for not meeting a utopian criterion of scientific inquiry.

Notes

1. Because of the greater uncertainties inherent in social systems, philosophical theses about norms of theory confirmation in the natural sciences set upper bounds on the formalism to be expected from model validation procedures in socioeconomic systems. For a more detailed discussion, see Barlas (1985, ch. 2, 4), Richardson and Pugh (1981, ch. 5, 6), and Forrester (1961, ch. 13).
2. 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 indicated by *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 adopt the use of *validation* common in modeling literature. Readers with a philosophical background should read this to mean *verification*.
3. Although we place Richard Rorty near the relativist extreme in Figure 1, it would be inaccurate to call him a relativist philosopher of science. Rorty is indeed one of the leading critics of all versions of foundationalist philosophy (see Rorty 1979). But he and a number of modern philosophers have increasingly framed their writings as attempts to move beyond relativism. Their main argument is that the very posing of the objectivist/relativist debate as a philosophical problem is a product of foundationalist philosophy. In moving beyond epistemology, Rorty looks to literary criticisms and hermeneutics for clues about possible directions for postfoundationalist philosophies (Rorty 1979, ch. 7, 8). For the purposes of this article, it is appropriate to place Rorty near the relativist extreme, since he has provided one of the most thorough relativist/holistic criticisms of foundationalist philosophies.
4. We must note that the relativist philosophy of model validation does not imply that pursuit of formal quantitative validation tools be abandoned. Formal tests cannot 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. The challenge is to design novel quantitative measures that can achieve this purpose (see Sterman 1984; Barlas 1985; 1989).
5. Our purpose is by no means to give an extensive literature review. For comprehensive reviews of validation literature, see Wright and Shahin (1980) and Balci and Sargent (1984).

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