The Systems Approach: Its Variety of Aspects*

Richard Mattessich
Distinguished Arthur Andersen & Co. Professor, Faculty of Commerce and Business Administration, University of British Columbia, Vancouver, B.C., Canada V6T 1Y8

This article offers a concise survey and glimpse of the vast literature presently available in the field of systems thinking and cybernetics. The first part discusses the areas of systems philosophy, systems analysis (mathematical systems theory), empirical systems research, and systems engineering, while the second part offers some details about the contributions of the following selected scholars: von Bertalanffy, Bogdanov, Ackoff, Churchman, and Herbert A. Simon. A bibliography of some 150 books and papers closes the article.

Introduction

The systems approach has many scintillating facets and reflects different colors when illuminated from different angles. Its originators deemed it to be a potential replacement for traditional science; but these hopes have long been abandoned, and to many people the systems approach remains nothing but a temporary fad, with Berlin- ski (1976) and Lilienfeld (1978) as its two major critics; to others, it is a new scientific discipline or subdiscipline; to some, it is a purely mathematical undertaking, while others regard it as belonging to the empirical sciences; for some, it is an ontology par excellence, while others envision it as a valuable methodology; for some, its application is universal, while others expect it to be especially useful in the life sciences, and yet others regard it as particularly suited to the applied and social sciences. Whether this great variety is the strength or weakness of systems thinking is difficult to assess, but it certainly is a source of bewilderment to the uninitiated. Although an article of this limited size cannot exhaust such a multitude of interpretations, I shall try to bring some order into this apparent chaos, and shall then proceed to discuss the contributions of a few authors whom I deem especially important, though occasionally neglected.*

The International Encyclopedia of the Social Sciences (see Vol. 15, pp. 452-495, 1965) classifies “systems analysis” into the following five categories: (1) general systems theory, (2) social systems, (3) political systems, (4) international systems, and (5) psychological systems. For our purpose, this disciplinary classification is hardly satisfactory, and I prefer to employ a more methodologically oriented scheme of categorization:

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**Systems Philosophy**

From the very outset (cf. Bogdanov 1912/1922, von Bertalanffy 1945, 1950, 1968), the systems approach has had a strongly philosophic flavor, not so much because of its programmatic nature, but because it embodied many aspects of holistic paradigms expressed in the philosophies of Lao-tse, Heraclitus, Leibniz, Vico, Hegel, Marx, Whitehead, Driesch, and others. But its pioneers, Bogdanov and von Bertalanffy, regarded systems thinking (or “tektoologia” as Bogdanov called it) as a scientific approach and not as a mere philosophy. Although the writings of those founders of the systems approach contain many more philosophic passages than falsifiable or verifiable scientific propositions, systems thinking as a deliberate philosophic enterprise is of relatively recent vintage. Although it fermented long before the 1960s (cf. Blauberger, Sadovsky, and Yudin, 1977), it found its literary precipitation in the late 1960s and the 1970s in some of the books by Churchman (1968a, 1968b, 1971, 1979; see also my section on the contributions of Ackoff and Churchman), Laszlo (1969, 1972a), Sutherland (1973), and Ackoff and Emery (1972). Whereas the first two of the books by Churchman (1968a, 1968b) could be described as popular formulations of systems ethics, oriented toward the management and social sciences, his other works are written in a professional philosophic and historical vein. Laszlo presents us with an *Introduction to Systems Philosophy* (1972a) strongly influenced by von Bertalanffy and his General Systems Theory. Along the same line is Sutherland’s (1973) attempt to apply general systems philosophy to the social sciences. Ackoff and Emery’s (1972) work is much more rigorous than the latter two and offers a great deal of conceptual constructions and clarification; whether one regards it as an exercise in philosophy or as a foundation work for empirical science is a matter of personal taste. This constitutes what might be regarded as the ethical and introductory phase of systems philosophy.

The next phase, developing in the late 1970s, is marked by the methodology, epistemology, and ontology emerging from the systems approach. I agree with Klir (1969, 1972) and others that systems thinking is first and foremost a point of view and a methodology arising out of this viewpoint. It is for this reason that I undertook (in Mattessich, 1978) a comprehensive epistemological examination of the systems approach as a methodological tool. There, I examined systems thinking in its relation to deductive and inductive inference, to management science, economics and the modern decision methods, as well as to the applied and social sciences in general. To my mind, it seems that the systems approach, with its strong emphasis on input–output features and its purpose-orientation, is especially suited to the applied sciences which have traditionally lacked an epistemological and methodological foundation. The traditional epistemology of pure science seems to be insufficient for the applied sciences (and probably for many aspects of the social sciences) because the latter deal not only with cognitive hypotheses but, above all, with means–end relationships, or, in our terminology, instrumental hypotheses.* Most significantly, two eminent scholars within the area of professional philosophy have recently adopted the systems approach. A purely epistemological application was developed by Rescher (1979), and the systems approach as an ontology par excellence is presented by Bunge (1979); this book is part of a comprehensive philosophic system that ultimately will comprise seven volumes. (A discussion of Bunge’s systems ontology as it compares with my own systems methodology is provided in Mattessich, 1982a.)

**Systems Analysis (Mathematical Systems Theory)**

Although the meaning behind “systems analysis” may not be unequivocal, we prefer this term to “mathematical systems theory” since it is questionable whether something like a single, uniform and undisputed systems theory, mathematical or nonmathematical, exists at all—perhaps “mathematical systems analysis” would be the clearest expression. Some of the mathematical system theorists (e.g., Mortazavian, 1982) seem to be tempted to restrict the entire systems approach to the analytical, model-building aspects. We have pointed out elsewhere (cf. Mattessich, 1982b, 1982c) that such a restriction is neither feasible nor advisable; but there can be little doubt that the analytical part of the systems approach is most promising and fertile. Undeniably, this analytical part emerged in the areas of cybernetics† and communication theory with such mathematics as Wiener (1948/1961), Shannon and Weaver (1949), and others. But what complicates the situation is the fact that major contributions to this kind of systems analysis were made by scholars from empirical fields (psychology and other behavioral sciences, economics, management science, and engineering) like Ashby (1956), Rapoport (1966), Lange (1962/1965), Iberall (1972), Bowler (1981), and others. However, the program for a general mathematical systems theory seems to have been conceived first by Mesarović (1960). For over two decades, he and other applied mathematicians (e.g., Kalman, Falb, and Arbib,

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*My terms, “instrumental hypothesis” and “instrumental reasoning,” are not related to Dewey’s *instrumental or pragmatic philosophy, but rather to Parsons’ (1951) “instrumental orientation” and Parsons and Shils’ (1951) “instrumental action,” as well as to Lowe’s (1965) and Machlup’s (1969/1978) “instrumental inference” and “instrumental analysis.” I regard, in agreement with Machlup, instrumental inference or reasoning as leading to normative (i.e., prescriptive) statements, and do not envisage instrumental analysis as a third category beyond or between positive and normative science (as Lowe seems to suggest).

†To my knowledge, only Mesarović and Takahara (1975) lay claim to a “general mathematical systems theory” in the above sense; Bunge’s (1979) more global axiomatic system is only a systems framework (as he explicitly admits) into which various system theories may fit.

‡For the relationship between cybernetics and the systems approach, see Eden (1982) and Mattessich (1982b).
by means of which this conversion process is functions foundation for general systems theory. Beyond this general mathematical foundation, the following two major areas ought to be distinguished:

1) Linear and nonlinear system theories and control theory. This classical version of mathematical system theory pivots on the transform functions expressing the conversion of inputs into outputs and on the feedback functions by means of which this conversion process is controlled (cf. Dorf, 1967; Bellman, 1971; Brogan, 1974; Vidyasagar, 1974; Siljak, 1978; Kailath, 1980; Gabel, 1980; Sethi and Thomson, 1981). Hereby, such features as the Laplace transform, dynamical system characteristics, the identification problem (identifying the nature of the change of system parameters), the output targets, and error signals assume particular importance and elevate the theory to something beyond a mere system of differential and simultaneous equations.

2) Automata theory (cf. Nelson, 1968, Hopcroft and Ullman, 1979). Its basis is the theory of recursive functions (for an explication, see Hofstadter's best seller, 1979, pp. 136-140); but it reaches from Turing machines (1936) over the theories of self-reproducing automata (von Neumann, 1966) and computability to such problems as pattern recognition and neural networks. Berlinks (1976, p. 157) points out that:

Automata theory and the classical theory of linear systems thus offer rivalrous interpretations of systemhood and its essences; in so doing the theories tap different intuitions. The classical tradition abstracts from common engineering experiences, stripping from filters, amplifiers, and electrical circuits their coarse physical properties. . . . Automata theory, on the other hand, has historically arisen (at least in part) as the result of abstractions performed on computational experiences.

Empirical Systems Research

In this area, a distinction between the programmatic formulations and actual empirical research ought to be made. The works of Bogdanov (1912/1922) and von Bertalanffy (1945, 1950, 1968) and of many others will have to be regarded as programmatic, while Herbert Simon (1981) is one of the most eminent authors pleading for and actually doing empirical systems research. Indeed, he and his collaborators have advanced one type of empirical systems research (heuristic research) that leads to the chess-playing behavior and theorem-solving behavior of computers (e.g., Simon and Newell, 1958). Another, but related type of system research, employs computer simulation. In the area of management and economics, the best known research of this type is that of Forrester (1961, 1971), Meadows et al. (1972), and Mesarović and Pestel (1974).* Although such research usually pivots on model building, its ultimate quest is for new insights into the system's behavior and conditional forecasting, both through computer simulation, instead of experimentation with the concrete system itself.

A further type of system research consists of the many behavioral studies of financial, managerial, and other systems conducted by means of questionnaires, empirical observations, and sometimes even through experiments (often with student groups instead of actual executives) in graduate schools of business administration and in various departments of the behavioral sciences. Inevitably, one has to add here a good deal of the "programmatic" and "theoretical" work preparatory to actual behavioral research, in as far as it stresses the systems approach. Typical examples are the contributions by such authors as Parsons (1951, 1960, 1968), Kuhn (1974), and Cavallo (1979) in sociology and social science research; Kaplan (1957), Knorr and Verba (1961), Boulding (1961/1969), McCelland (1961), Rosencrance (1963), Hoos (1972), Krone (1980), and Rosenau (1980) in political science; Köhler (1929), Allport (1960), Rokeach (1960), Harvey (1961), Gochman (1962, 1966), and Piaget (1970) in psychology; Buckley (1967, 1968) in the behavioral sciences in general: Beer (1959, 1967, 1979), Payne et al. (1975), Knight (1979), and Checkland (1981) in the area of management; Boulding (1961/1969) and Aoki (1976) in economics; Bennet et al. (1978), States et al. (1978), Mulholland (1978), DeSouza (1979), and Huggett (1980) in social ecology, geography, and environmental impact analysis; Fuller (1979) in architecture; Metzler (1977) in neuroscience; Johannides (1979) and Reisman (1979) in health care; O'Neil (1979) in education; Mannheim (1979) in transportation; Townley (1978) in information retrieval; and finally, anthologies of a more general nature, such as Emery (1969), Laszlo (1972b, 1973), and Opert (1973). But the major problems of empirical systems research still lie in the relative scarcity of operationally defined concepts (cf. Rapoport, 1980) and in imprecise conceptualizations [as Amey (1980) illustrates, for example, with regard to the notion of "steady state"].

Systems Engineering

This category ought to be understood in fairly broad terms, including not only the systems research undertaken by engineers (e.g., Hall and Fagen, 1962; Wymore, 1967; Zadeh and Polak, 1969), but also by such professional groups as accountants and other information system experts (e.g., Van Gigch, 1974) concerned with the prepartion for or actual construction of concrete systems. Much of this research overlaps with systems analysis

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*Other pioneers in this area are Cohen (1960), Clarkson and Simon (1960), Orecutt (1960, 1961), Mattessich (1961, 1964a Chap. 9, 1964b), Balderton and Hoggatt (1962, 1963), and Evans and Klein (1968, 1969), Klein (1969), Zeigler et al. (1979), and many others; for a survey, see Naylor (1971), and for a general exposition, see Gordon (1978).
Von Bertalanffy's General Systems Theory and Bogdanov's Tektologia*

General Systems Theory (GST) is a movement founded by Ludwig von Bertalanffy and fostered by the Society for General Systems Research. GST aims at a unification and particular reconstitution of most sciences. It also seems to imply a fairly radical holistic notion which, however, is different from vitalism. GST can best be understood as a reaction (perhaps an overreaction) to the dominant position of physics within modern science, to its methodology and its lack of teleological concepts. As a biologist, von Bertalanffy (1968, p. 37) has sought to replace the "lifeless" positivistic foundations of science with a kind of holistic vision, claiming that:

General system theory, therefore, is a general science of 'wholeness' which up till now was considered a vague, hazy, and semi-metaphysical concept. In elaborate form it would be a logico-mathematical discipline, in itself purely formal but applicable to the various empirical sciences. For sciences concerned with "organized wholes," it would be of similar significance to that which probability theory has for sciences concerned with "chance events."

But von Bertalanffy's deeds belie his words, because his own research bears hardly any similarity to that of a mathematico-logical discipline. Any ambitious attempt to reconstruct the entire realm of science, to become a new superscience, is in danger of losing itself in verbosity, ambiguity, and contradiction—at worst, even in mysticism. Indeed, it is the ambiguity of some of its publications more than its actual program that endangers GST.

Von Bertalanffy's first major work (1928) aimed at what we would consider a synthesis between the two opposing biological theories. Out of the clash between "biological mechanism" and "vitalism" emerged his own system-theoretic view, which he christened "organismic biology."* At first, von Bertalanffy was far from generalizing his system ideas, and his first article on general systems theory was not published for more than a decade (1945). Bogdanov, however, developed his theory from the very outset (1912) as a general theory of systems organization, providing a broad range of illustrations—from atomic, chemical, and biological "complexes" (a term he uses interchangeably with "systems") to man and human organizations. Above all, Bogdanov (1922, p. 82, quoted from Gorelik, 1975, p. 3) developed a fairly comprehensive conceptual apparatus for dealing with the very problems later explored by the disciples of GST and cybernetics. His main concern was to clarify and generalize the organizational modes encountered in nature and society. He asserted that:

Tektology deals with organizational experiences not of this or that specialized field, but of all these fields together. In other words, tektology embraces the subject matter of all the other sciences and of all the human experience giving rise to these sciences, but only from the aspect of method; that is, it is interested only in the modes of organization of this subject matter.

The vocabulary and many of the notions found in Tektologia are, of course, different from those used in GST; but the basic thrust of both is surprisingly similar. For Bogdanov is concerned with the organizing and disorganizing forces of systems, with their differentiation, growth, and duplication, with the interdependencies between system and environment, with the control and variiances, types of stability, the selection, vulnerability, crises of systems, and so forth. As Setrov (1970, p. 59) claims, it even seems that:

... many generally theoretical problems of the system approach are elaborated by A. Bogdanov more fully and rigorously than is the case in the contemporary theory of systems and cybernetics.

*Its main feature is "the establishment of laws of biological systems in contrast to mechanism which neither saw nor wished to see this fundamental characteristic of life," and vitalism which "put a philosophical construction in the place of natural scientific investigation." (Von Bertalanffy, 1934/1962, p. 188).

*This and the subsequent two sections are based on Mattessich (1978, pp. 276-301).
A complaint brought against von Bertalanffy, rather than against GST, is the former’s persistent silence concerning Alexander A. Bogdanov (1873–1928), the originator of a truly generalized systems theory. The latter’s fundamental and highly original work appeared in Russia under the title Tektologia, Vols. 1–3 (1912–1927), hence several years before von Bertalanffy’s first system-theoretic notions—at the time restricted to biology—were published (1928). Furthermore, a German version of the decisive first two volumes of Bogdanov’s much more comprehensive presentation of systems organization was published in Bertalanffy’s own mother tongue as early as 1926–1928, and was subsequently reviewed in the pertinent German literature.* We are certainly not accusing Bertalanffy of plagiarism, and do not dispute his many genuine contributions; the history of science has shown repeatedly that ideas frequently mature in the minds of two or more scholars simultaneously. Von Bertalanffy may well have conceived his system ideas without any influence from Bogdanov. Yet it seems highly unlikely that such a widely read scholar as von Bertalanffy, who concerned himself with research in the general systems field for nearly four decades, never encountered the German translation of Tektologia or the name of its author—a name famous enough to occupy a separate section in the Encyclopedia of Philosophy (1967). Whatever the reason, we can merely state the fact that in none of von Bergalanffy’s more than 50 publications on this subject does Bogdanov’s name appear. In von Bertalanffy’s defense, it should be mentioned that, in North America, neither the Russian original nor the German translation of Tektologia were widely known. For example, the Encyclopedia of Philosophy mentions Bogdanov’s interest in organization theory, but does not list his Tektologia. The first English translation (with commentary) of one of Bogdanov’s works on systems theory was only recently completed by Gorelik (1980).

The Contributions of Ackoff and Churchman

Three scholars deserve particular attention: Russell L. Ackoff, C. West Churchman, and Herbert A. Simon, all of whom have contributed significantly to methodological or other philosophic as well as empirical concerns of systems thinking.

Russell Ackoff combines a pragmatic attitude with a clear understanding of the need for sound methodological research. For many years, he has been one of the most perceptive contributors to General Systems Research, and his long-standing experience in the research, teaching, and application of systems has significantly helped to shape modern systems thinking. Of special importance are his article “Towards a system of systems concepts” (1971) and his book, Scientific Method (1962)—see also Ackoff and Emery (1972). From the outset, the article distinguishes between an abstract system (all the elements of which are concepts) and a concrete system (at least two elements of which are factual objects) and emphasizes its exclusive concern for the latter. This is a clear indication that, for Ackoff, systems research is an empirical discipline (or at least part of one). However, his disregard of abstract systems could easily be misinterpreted: Ackoff is, of course, interested in the conceptual picture of an empirical system (which itself must be regarded as an abstract system), but his brand of Systems Research is not concerned with systems encountered in pure logic and mathematics. Another decisive feature of this scheme is the classification of systems, according to their goal-oriented behavior, into state-maintaining, goal-seeking, purposive, and purposeful systems. While the first merely reacts (e.g., a simple thermostat system), the second responds by exercising choices of strategies but not of goals (e.g., an automatic pilot); the third pursues different goals, without, however, selecting the goal to be pursued (e.g., a computer programmed to play checkers); finally, the fourth displays will, and thus chooses its own goal (e.g., and organization). Ackoff adds a fifth type, the ideal-seeking system, which, after attaining its goal, proceeds to select another goal even closer to its standard of perfection. These distinctions are important to an understanding of systems and their evolution. Ackoff makes similar distinctions for the components or elements of the system, and finally concludes with the conceptual difference between an organism and an organization: While both are purposeful systems, organisms do not possess “purposeful” elements.

The major concern of the fundamental work by Ackoff and Emery (1972) is to derive the notions of function and purpose from the mechanistic notions of traditional science (without falling back on behavioral positivism). The authors of this exciting attempt advance from a set of generic mathematical and physical concepts and proceed, step by step, to construct a long series of further concepts. Among these, the producer-product concept (x is a producer of y) is regarded by Ackoff and Emery as most critical, because from this idea one arrives at the notion of functional class—and various structural and functional relations between actions and outcomes (uni-uni, uni-multi, multi-multi) are made. However, we consider the most crucial steps to be the ones from functional sets to goal-seeking and purposeful systems. Nevertheless, these big leaps leave several questions open; above all, they do not reveal clearly enough what we consider to be the critical point in the relation between mechanistic and teleological concepts. While the authors seem to represent the notions of “goal” and “purpose” as fairly unproblematic extensions of physical concepts, we believe that mentalistic or reflective aspects (e.g., beliefs, intentions, norms, goals, purposes) and material aspects (e.g., events, properties, points of location and time, mass, acceleration) belong to different epistemic categories.

*The first volume of Tektologia’s German translation, Allgemeine Organisationslehre (1926), was reviewed by J. Plenge in 1927.
Although these two sets of notions are as inevitably bound to each other as two sides of the same coin, they nevertheless represent two different aspects of our universe which must not be confused with each other. To see the connection between them (e.g., between purpose and event), the "coin" must be flipped—and it is this flipping of the coin (i.e., the revelation that a leap to a different epistemic category has been made) which is not brought out in Ackoff and Emery's otherwise excellent presentation. This is not to say that the authors neglect such notions as awareness, consciousness, and mental states. On the contrary, they assure us that the "definition of mental states is the preoccupation of their book" (1972, p. 72), yet they pay too little attention to the crucial distinction between the reflective and the material aspects. For me, a system has a goal or purpose either (1) because the inner or mentalistic aspect of the system is developed highly enough so that norms emerge from this very system (e.g., in the form of new holistic properties), or (2) because some norms are imposed, in one form or another, from outside upon the system. In the latter case, the pertinent system becomes an instrumental extension of some other system, and the goal of the former is reducible to the purpose of the latter. However, Ackoff and Emery apparently believe that the major criterion of purposeful action is the proper (active multifunctional and, of course, environmentally independent) relationship between structure of action and function of outcome.

While Churchman shares many interests and views with Ackoff, his temperament and his contributions to systems thinking follow an entirely different course. Churchman's philosophic training and natural inclination lead him to focus his attention on the existence or nonexistence of a hierarchy of systems and their interdependencies; above all, he dwells on the reconciliation between the goals of the subsystems with the goal of the supersystem. This inevitably leads him to the intricate problem of systems improvement. The following passages are characteristic of Churchman's outlook and aim (1968b, p. 20):

Now, the management scientist's argument against "efficiency" is that it is always conceived in relation to a small segment of the social organization. Mere attention to cost reduction by itself, he says, may do the very opposite of what the manager intends. In fact, cost reduction in many instances may actually increase the system's total cost.

Or in a broader context (1968a, p. 5):

Now of course the discussion of individual behavior does properly belong under the theme described here as the ethics of large-scale systems. It is perfectly proper to talk about a given person within a system in terms of the quality of his behavior, just as we have talked about the quality of behavior of education and health. We could therefore sensibly ask whether an individual might have lived his life in a better way than he did, given the resources made available to him by the whole system. The point, however, is that we cannot judge improvement in an individual unless we have some understanding of the nature of the whole system in which this individual lives. The theme we are exploring is one that claims that a concept of ethical conduct depends at least in part on the concept of system improvement. The ethics of whole systems incorporates the concepts of the ethics of individual behavior and places the problem of the ethics of individual behavior within the context of the whole system.

In order to solve the problem of system improvement, one needs criteria for and measures of effectiveness. Once such measures are created, one can proceed to explore the difficult question: Which system or system structure is satisfactory, or optimal, for a given purpose? This seemingly modest question conceals a highly complex issue for which no solution can be expected overnight. All one can do at this stage is to analyze the problem on several levels, and to have begun this task of clarification is one of Churchman's merits. His major concern is to decide whether the scientist charged with the design or improvement of a system can or should accept the goals and value judgments of his client (cf. Mattessich, 1962); this basic issue is the recurrent theme of much of his writing (1968b, pp. 11-12; see also 1961, 1968a):

In the past the economist or engineer could say to himself, "These are the goals that my client wishes to attain, and I will study how he can best attain them. It is none of my business whether these goals are the correct ones." But when the scientist begins to work hand in hand with the top administrator, he can no longer take such a detached viewpoint toward his client's goals. He must ask himself whether the goals specified by the client are the correct ones in terms of the client's interests, and he often discovers that they are incorrectly stated or dangerously narrow from the point of view of the improvement of the entire system.

Here the problems of "suboptimization" converge with the question of whether science can be free of values. But Churchman does not simply intimate that the management scientist's value judgements be substituted for those of the manager; rather, he suggests that the scientist's task is to help the manager clarify his own goals. But is there not a danger that in this process of advice and "purification" the manager may be influenced by the value judgments of the scientist? Such concerns conceal a more fundamental problem: To what extent can broad value judgments be "substituted" by scientific analysis or, rather, reduced to value judgments of a more basic or narrow type?
Most decisive, however, is Churchman's view about the essence of the systems approach (1968a, pp. 10-11):

All along, our effort will be to expand our capability of thinking about systems. Consequently, this is not a book on the subject of how each individual should learn to think; it is more concerned with the resources that a society has at its disposal for thinking better about its systems. . . . A book of this sort is an extension of the old-fashioned logic and rhetoric in which the student is trained adequately to think about the world. . . . But today our expanding technology provides us with all kinds of additional resources beyond the basic types of logic which were taught by Aristotle, Spinoza, Dewey, and others. There is a plethora of additional resources that we will want to explore as we develop some basic ideas on how to think in our century.

Here again, we encounter the conviction that systems thinking may represent the indispensable methodological tool for our era. In Churchman's view, this approach will close the chasm between the extreme realist who thinks in terms of the short-run and narrow subsystems, and the extreme idealist who might choose the entire universe and the infinite long run as his bases of vision. The essence of systems thinking lies in constantly relating subsystems back and forth to the pertinent supersystem or environment, and in trying to reconcile their often conflicting goals. He points at the many difficulties to be encountered in this endeavor, but warns the reader not to expect "like in a detective story, that in the end we will propose solutions to the perplexing problems. . . ." (Churchman, 1968b, p. 82).

From a purely epistemological standpoint, Churchman's major contribution to the systems area is his book, The Design of Inquiring Systems (1971). In this work he made the fascinating attempt to illustrate, from a systems point of view, the epistemic theories of such major philosophers as Leibniz (together with Descartes and Spinoza), Locke (together with Hume and Bishop Berkeley), Kant, and Hegel—as well as E. A. Singer to whom Churchman, as his former student, feels particularly attached. The notion of interpreting science and knowledge-creation as a kind of information system has far-reaching consequences. Not only does it impart a novel perspective to epistemology, it also invites the reader to contemplate the relationships between various information systems. In the light of this intriguing idea, I suggest that it is quite reasonable to speak of a common ground between such a venerable and exalted area as epistemology and such a prosaic and applied field as accounting. Both are important information systems which have more in common than either philosophers or accountants might suspect. For the details of this fascinating concept, the reader may wish to refer to Churchman's book (1971) and to the review articles by Mitroff (1973) and Mitroff, Betz, and Mason (1970).

In his latest work, Churchman (1979) continues the search for generality and for a design of social systems. Here, the major themes, with many variations, are the "environmental fallacy" and "the enemies of the systems approach." The latter expression is not meant in the personal sense and does not refer directly to such opponents of systems thinking as Berlinski (1976) or Lilienfeld (1978), but refers to the traditional approaches to politics, morality, religion, and even aesthetics. Of course, this could easily be misunderstood and its full comprehension is hardly possible without reading Churchman's entire treatise.

Herbert Simon's Science of Design and Artificial Intelligence

In contrast to Churchman, H. A. Simon exhibits a strongly positivistic slant in his approach to systems; he is less geared toward the loosely knit macrosystems of society than toward the much tighter microsystems of administration with their subsystems. Simon's approach may be studied in many of his writings (e.g., Simon, 1977), but is best summarized in The Sciences of the Artificial (1981). For Simon, system thinking focuses on the problem of how to develop a science of design, i.e., a scientific basis for designing and testing systems. In contrast to von Bertalanffy, he does not stress biological organisms and naturally occurring social systems (e.g., a society of ants), but is concerned with artificial, or man-made systems (as regards human behavior, Simon would remark that as long as this behavior is adapted to goals, it also is artificial (cf. Simon, 1981, p. 95). This obviously implies a strong teleological emphasis (Simon, 1981, pp. 6-7):

If science is to encompass these objects and phenomena in which human purpose as well as natural law are embodied, it must have means for relating these two disparate components. The character of these means and their implications for certain areas of knowledge—economics, psychology and design in particular—are the central concern of this book.

The engineer, and more generally the designer, is concerned with how things ought to be—how they ought to be in order to attain goals, and to function. Hence a science of the artificial will be closely akin to a science of engineering—but very different, as we shall see in my fifth chapter, from what goes currently by the name of "engineering science."

In accord with other authors, Simon emphasizes the relation among purpose, elements, and environment of

*The above remark reveals the semantical trap into which Simon might step if he were to identify "adaptation" of a system as an artificial process; then the natural sciences, too, especially biology, would become sciences of the artificial.
the system; but he utilizes Claude Bernard's (1878) distinction between inner and outer environment, underscoring the fact that a system is a meeting place between inner and outer environment (Simon, 1981, pp. 15-16):

Central to their [the artifacts] description are the goals that link the inner to the outer system. The inner system is an organization of natural phenomena capable of attaining the goals in some range of environments, but ordinarily there will be many functionally equivalent natural systems capable of doing this.

The outer environment determines the conditions for goal attainment. If the inner system is properly designed, it will be adapted to the outer environment, so that its behavior will be determined in large part by the behavior of the latter. The behavior of the system will only partly respond to the task environment; partly, it will respond to the limiting properties of the inner system.

Since it is often difficult to predict the behavior of a complex system, Simon recommends vicarious system experimentation through simulation, pointing out that this technique may even create new knowledge about system behavior. He is especially keen to demonstrate that system behavior can be predicted even in ignorance of (or with a minimal knowledge of) the system's structure. In connection with this, he speaks in favor of what are called black box theories (Simon, 1981, p. 20):*

We knew a great deal about the gross physical and chemical behavior of matter before we had a knowledge of molecules, a great deal about molecular chemistry before we had an atomic theory, and a great deal about atoms before we had any theory of elementary particles—if indeed we have such a theory today.

This skyhook-skyscraper construction of science from the roof down to the yet unconstructed foundations was possible because the behavior of the system at each level depended on only a very approximate, simplified, abstracted characterization of the system at the level next beneath.

Simon also refers to John von Neumann's research in computer reliability and the problem of organizing a system in such a way that as a whole, it becomes relatively reliable in spite of the possible unreliability of its components. This he accepts as evidence for the possibility of constructing a mathematical theory of systems independent of a corresponding factual microtheory of the pertinent system. But the reader must not misinterpret this as Simon's rejection of empirical research in computer science; rather, it supports his contention that a distinction must be made between the factual knowledge about individual system components (e.g., the solid-state physics of transistors) and behavioral research of the entire system. This kind of research forms the core of what Simon envisions as systems research in a truly empirical sense, and this idea might turn out to be one of the major insights of empirical systems research.

Reflections of this nature and a series of experiments lead Simon to postulate the hypothesis that even man, as a behavioral system, is structured simply, and that his apparent complexity stems from the complexity of his environment. Man's brain operates serially, limited by the capacity of its memory, and by its ability to digest only a few signals simultaneously. If sufficiently confirmed, such an hypothesis could become of far reaching consequence for psychology as well as for systems science. As far as the hierarchy of systems is concerned, he approaches the problem from the viewpoint of decomposition. Unlike Churchman, who looks passionately at specific existing systems, noting their inadequacy or goal-conflict with the supersystem, Simon begins with the supersystem and considers its potential decompositions into subsystems.

There is no logical end to this examination of the systems approach. Its variety of aspects can be pursued by exploring areas of special interest through the suggested readings that follow.

Suggested Readings


Balderston, F. E.; Hoggatt, A. “Simulation models: Analytical variety and the problem of model reduction.”

*“A black box theory treats its object or subject matter as if it were a system devoid of internal structure; it focuses on the system’s behavior and handles the system as a single unit” (Bunge Vol. 1, p. 509, 1967).


Mattessich, R. *Instrumental Reasoning and Systems Methodology—An Epistemology of the Applied and


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