A REVIEW OF *SYSTEMS: NEW PARADIGMS FOR THE HUMAN SCIENCES*

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This essay is a selective review of *Systems: New Paradigms for the Human Sciences*, edited by Gabriel Altmann and Walter A. Koch (Berlin: Walter de Gryter, 1998). It is selective because it is impossible to engage such a varied collection of systems-theoretic essays in a review of reasonable length. To invoke a relevant dialectical idea: the characteristic strength of any system is often also its characteristic weakness. One strength and weakness of the systems field is its great diversity, and this diversity is reflected in this volume by the range of subjects addressed in its 27 articles. I will not attempt what the editors themselves have declined to undertake, namely an integrating overview, nor will I offer brief remarks on many articles. Instead, I want to comment in detail on just three articles which bear on my own interests. I do not mean to suggest that these articles are more valuable or central to systems theory than the others.

After discussing the three articles, I will conclude by adding a few general remarks and by listing the authors and titles of the essays in the book, so that readers might be alerted to items of potential interest. From my study of the three articles I discuss and from my skimming of several other articles, I strongly recommend this book to systems researchers, especially researchers interested in the human sciences.

George Klir, "From Crisp Systems to Fuzzy Systems" (pp. 79-103)

Klir argues that science is shifting towards the acceptance of "uncertainty" as an attribute of our knowledge of the physical world and towards the development of methods by which this uncertainty can be rigorously examined. Uncertainty offers a necessary counterbalance to the traditional emphasis in science on lawfulness -- and predictability, which until the discovery of chaos was assumed to follow from determinism. As Klir points out, uncertainty is unavoidable in descriptions of "organized complexity" (Weaver, 1948) a domain intermediate between "organized simplicity," where analytic methods suffice, and "disorganized complexity," where statistical methods suffice. In this intermediate domain neither analytic nor statistical methods are effective, and problems of computational complexity are encountered.

Klir's next proposition is the critical one: set theory and probability theory are inadequate frameworks to capture the full scope of the concept of "uncertainty." Uncertainty in set theory means nonspecificity; in probability theory it derives from conflicting likelihood claims. Generalizations of set and probability theories, for example, fuzzy set theory and fuzzy measure theory, expand the concept of uncertainty while encompassing these standard connotations; they are thus potentially of great value for science. Klir briefly presents the main ideas of these generalizing theories, noting where they extend the more familiar set theory and probability theory. For example, while the probability of the union of disjoint sets is additive (equal to the sum of the probabilities of the separate sets), fuzzy measures can be either superadditive or subadditive.

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Klir goes on to discuss the general framework of systems modeling, which he and his colleagues have developed, known as GSPS, the "general systems problem solver." The main categories of this epistemological hierarchy of system types are quickly reviewed in the article; for further detail on this deep and comprehensive epistemological framework, one must consult Klir's major work in this area (1985). Fuzzy set theory and fuzzy measure theory enlarge the categories of this framework, and Klir illustrates by discussing the fuzzy set approach to linguistic variables defined in terms of continuous base variables. Klir notes that even nominal variables can be fuzzified; there are, for example, finite-state fuzzy automata. This is an important point, because nominal variables are the *most general* kind of variable possible, though most explanations of fuzzy set theory rarely discuss such variables.

The demonstration that fuzzy mathematics generalizes set theory and probability theory is presented in a comprehensible and convincing way. From my own perspective, however, I wonder if these developments represent, as Klir argues, a paradigm shift in science *per se,* as opposed to an expansion of the modeling methods used in science. One could view fuzzy set theory as a high level modeling language which bridges the precision of mathematics and the flexibility of natural language. One might characterize the emergence of such a new modeling language as a paradigm shift. Or, one might instead insist that a new paradigm in science should impact not only methodology, but also ontology. Can fuzzy mathematics be interpreted as a different view of what exists in the world? Here I raise a controversial issue about which people have different perspectives. Constructivists focus on methodology and epistemology and typically abstain from speaking about ontology. To the extent that fuzzy mathematics emphasizes issues of "belief," "degrees of plausibility," "degrees of evidence," and the like, it reflects this particular orientation. I prefer a more realist position: models link observers to realities, and should be capable of being regarded *either* as epistemological (or methodological) statements or as ontological ones. Klir's presentation reflects the constructivist position, but he also offers an interesting quote from Smuts (1926), which seems to envisage fuzzy scientific models which can be viewed in both ways. Smuts notes that in nineteenth century science, "Vagueness, indefinite and blurred outlines ... was abhorrent ... This logical precision immediately had the effect of making it impossible to understand ... actual causation ... We have to return to the fluidity and plasticity of nature and experience in order to find concepts of reality."

To make the point concretely: until recently most applications of fuzzy mathematics have been in engineering, particularly in the design of control systems, and not in scientific theory itself. This might be contrasted, for example, with the mathematics of non-linear dynamics. While models of chaos are used *both* in design *and* to describe phenomena in natural and social systems, the mathematics of fuzziness has so far been applied mainly to design. Of course, designed (e.g., fuzzy control) systems are important objects of scientific study if their behavior is not fully explicit in their specifications. Still, I would argue that one can speak of a paradigm shift *in science* centered in concepts of fuzziness only if scientific -- as distinct from engineering -- uses of fuzzy mathematics become extensive. Some promising uses of this sort are apparently occurring (Klir, 2000), and if notions of uncertainty from fuzzy set theory, fuzzy measure theory, and related formalisms, do become as central to new scientific theories as probabilistically-defined uncertainty is to quantum theory and nonlinear dynamics, then this would indeed constitute a dramatic development. It is too early to tell whether or to what extent this will occur. The mathematics of fuzziness has encountered strong opposition, and Klir provides references for and a brief overview of the controversy.

The characterization, above, of fuzzy theories as high level tools for modeling does not actually do justice to their value as links between mathematics and natural language. These links may have deeper significance for the systems project, which, following Bunge (1973), might be described as an attempt to construct an "exact and scientific metaphysics." "Metaphysics" here does not refer to questions of God, free will, and the like, but to the study of general features of the world, such as order and disorder, system-environment interactions, self-organization and self-maintenance, information processing, regulation and control, morphogenesis, adaptation, learning, competition and cooperation, etc. A metaphysics is "scientific" if it draws from and contributes to the sciences and is thus both fertile and (indirectly) open to empirical test. A metaphysics is "exact" if it is expressed mathematically, if not now, then as a future goal. (Bunge actually includes exactness within a notion of a "scientific metaphysics," but I find it useful to separate it out as a third factor.) By being scientific and exact, a metaphysics might satisfy both correspondence and coherence criteria for truth. To borrow a term from Nicolescu's article in this volume, the dual requirement of being scientific and exact constitute necessary *resistances* for a metaphysics; they ground it and keep it honest.

A metaphysics is necessarily expressed in natural language. This allows it to be the common heritage of humanity and not restricted to technical specialists, and enables it to serve as a bridge to other aspects of philosophy, to religion, and to the arts and humanities. So the linkage of natural language and exact mathematics is important to the systems project. Fuzzy mathematics is one way of establishing such a linkage. Fuzzy notions bear directly on distinguishing between system and environment, between one element within the system and another, and between different attributes of the various elements. Fuzzy notions address the fundamental issue of the ontological status of the discrete versus the continuous. (This is treated, for example, by the use of linguistic variables in fuzzy set theory and by "granulation" -- fuzzy membership functions --as an alternative to "discretization.") One finds recognition of the importance of this issue in Smuts or, earlier, in Hegel's dialectics, which also raised the issue of fuzziness. For many centuries, in the notion of the "Great Chain of Being" which dominated western philosophical thought, the continuity -- and by implication fuzziness -- of all *types* of being was a central premise (Lovejoy, 1936).

The notions of fuzziness thus bear on systems in general. They also bear on the particular class of systems of special interest to us, namely that class of beings, generated by evolution, with developed faculties for modeling their environments, themselves, the interaction between the two, and, self-referentially, these very "modeling subsystems" themselves. The highest forms of these faculties for modeling and for communication emerged with the development of natural language. From this perspective, mathematics was only a later enhancement and specialization of language, applicable only to limited aspects of reality and usable only by a small sub-population. While we have had the idea, since Pythagoras, that the laws of the universe are mathematical, and feel awe and puzzlement, as Wigner put it (1960), about the "unreasonable effectiveness of mathematics" in the natural sciences, the optimum human means for describing the world in its full richness continues to be natural language.

The bridge offered by fuzzy theories between mathematics and natural language thus cannot be dismissed as a mere "higher level computer language." How much interaction this bridge promotes between natural language and mathematics is, however, another issue. Whether the mathematics of fuzziness will afford deep insights into reality and thus constitute a paradigm shift in science remains uncertain. Still, it is plain that this mathematics is important to Systems Science. Klir's presentation, in its compactness, rigor, and clarity, distills the essence of this development.

Solomon Marcus, "No system can be improved in all respects" (pp. 143-164)

The essay of Solomon Marcus, "No system can be improved in all respects," reflects an orientation I have also articulated (Zwick, 1984). The article mainly addresses the "conflictual pairs" which characterize many mental representations, and his discussion of these pairs is subtle and illuminating. The notion of a conflictual pair is introduced using Gödel's findings about the conflict between consistency and completeness. Marcus identifies the unexpectedness, the *a posteriori* character, of the {consistency, completeness} conflict as a distinctive and defining characteristic of these pairs. A similar non-*a priori* "conflict" characterizes the complementarity in quantum mechanics between knowledge of position and of momentum. It does *not*, however, characterize the [particle, wave] pair, in which the separate terms are inherently and explicitly opposed. (Note: Marcus uses ordinary parentheses for all of his pairs, but I will use curly brackets for conflictual pairs and ordinary brackets for explicit oppositions.)

Explicit and *a priori* oppositions are relatively straightforward, the terms being related essentially through negation. Conflictual pairs, where oppositions are implicit and *a posteriori*, are intrinsically more interesting. Marcus offers a number of conflictual pairs in this paper, some of which bear a family resemblance to one another, but he does not actually seek to identify an underlying commonality between all these pairs or between very different pairs, other than the non-obvious nature of the conflict between the terms. Since some of these pairs arise in mathematical domains, one might conceivably hope to identify some similarity in the origins or character of the conflicts within different formalisms (e.g., one might try to identify some relation between the property of a conjugate pair of variables in quantum theory and the property of consistency and completeness in formal mathematical systems).

A related philosophical remark and some questions. It seems possible to view some pairs as "dyads" which arise from a distinction made within some "monad." For example, the dyad, {position, momentum}, differentiates the monad, {state}. A monad, {quantum object}, might be considered to unite -- or be the source of -- the pair, [particle, wave]. One might inquire: Can a monad be identified for all or only some conflictual pairs?

What is the difference in the monad-to-dyad transformation for conflictual versus explicitly-opposing pairs?

{Precision, truth} is another conflictual pair. If a claim is made that some variable has a very precise value, the claim is unlikely to be true. This is partially the motivation for fuzzy mathematics (see the quotation of Smuts cited by Klir, mentioned above). A similar pair might be seen in {exactness, truth}, especially in the aesthetic realm, where exaggeration is often a vehicle for conveying artistic truth not achievable by exact representation. Related to this is {certainty, reality}, where the second term refers to some empirical reality. Here again we encounter a justification for fuzzy logic, and fuzziness is now offered as ontology. Similar also is {rigor, meaning} and its correlate {syntax, semantics}, where rigor is syntactic success and meaning is semantic success. (One might ask about the 3rd -- the pragmatic -- dimension of communication.)

With respect to the last of these pairs, Marcus notes that syntactics is often seen as “dominating” semantics, but in Gödel’s proof semantics dominates syntactics in that “any statement that can be proved is true, but the converse is not valid.” This recalls the post-modernist position (from Derrida) that in every polarity, one term is “marked,” i.e., favored. Yet by “translating” a dyad into an alternative but nearly equivalent terminology, the *other* term of the dyad may come to dominate. For example, in the dyad of [order, disorder] as understood in physics order is marked. (This is not a conflictual pair because the opposition is explicit and *a priori*.) But if [order, disorder] is translated into [constraint, variety], it is variety which becomes the favored term. As Ashby (1976) notes, variety is not inherently different from noise (disorder). So marked has the term “chaos” become in contemporary scientific culture despite its disfavored past, that Kauffman was induced (1991) to call order “anti-chaos”!

As noted, many pairs resemble other pairs. Marcus mentions Hjelmslev’s {coherence, exhaustiveness} which resembles Gödel’s {consistency, completeness}. A quote cited from Braque suggests {artistic relevance, clarity}, which resembles {reality, certainty} and also {meaning, rigor}. Marcus also offers conflictual pairs from mathematics (from numerical approximations, algorithmic complexity, formal systems, etc.), including pairs related to randomness and negligible sets and to local vs. global properties. For example, while it can be demonstrated that most strings are random, it is impossible to prove that a specifically given string is random.

Towards the end of the article, conflictual sets of more than two terms are introduced. For example, a conflictual triad based on the Arrow Impossibility Theorem is given. (Blair and Pollak (1983) offer a more accessible formulation of Arrow’s result: no voting system which aggregates ordinal preferences among three or more alternatives can be rational, decisive, and egalitarian.) Marcus suggests possibilities of conflictual sets containing more than three terms, and there seems to be no reason to preclude conflictual sets of still higher ordinality.

It may be possible to gain insight and a more unified understanding of conflictual pairs by “clustering” similar oppositions, trying to understand the commonalities within clusters, and then trying to relate clusters to one another. Marcus presents these conflictual sets as applying to *representations*, e.g., models (abstractions) of empirical phenomena or purely conceptual (e.g., mathematical) systems. However, to return to an earlier theme, from a different point of view some of these conflictual sets might be regarded as being ontological in character. One might view the {position, momentum} pair of quantum theory as reflecting not a limitation in our current representation, but an opposition inherent in reality, which no alternative representation can ever remedy. Similarly, the Arrow result has actual implications for democratic decision-making and is not merely about representations of decision procedures. Corresponding to Gödel's result is the Halting Problem in automata theory, and while Gödel's proof may seem to concern only formal mathematical systems, the relevance of undecidability to automata, dynamics, games, etc., suggests that more than mental representations may be involved. But this debate should not distract us from the finding convincingly argued by Marcus: the mysterious ubiquity and inter-relatedness of these conflictual pairs.

Basarab Nicolescu, "Gödelian aspects of nature and knowledge" (pp. 385-401)

It is refreshing to read in Basarab Nicolescu's essay the plain assertion that "Reality is not *only* [italics added] a social construction, the consensus of a collectivity, or some intersubjective agreement. It also has a trans-subjective dimension, to the extent that one simple experimental fact can ruin the most beautiful scientific theory." This, to be sure, is an idealization of the scientific enterprise, but Nicolescu offers an important insight when he designates "reality" as "that which *resists* our expectations, experiences, representations, descriptions, images or mathematical formalization." If sociologists and philosophers of science who advocate the strong constructivist position (that "reality" is social constructed) did laboratory experiments or computer simulations, the personal experience of such resistance might lead them to appreciate better the argument for realism. Nicolescu also regards simple chaotic equations which generate infinities of images as exemplifying such resistance; "resistance" thus is tied to inexhaustible fecundity, a linkage which merits further elaboration.

A central thesis of Nicolescu's article is that apparent contradictions, such as the wave-particle duality, which are associated with one particular "level of reality," may be resolved via a third term at another level of reality where levels of *reality*, as distinct from levels of mere *organization*, are characterized by discontinuities in fundamental laws. Nicolescu argues that the classical and quantum domains constitutes two such levels of reality. It is true that interpretations of quantum phenomena in terms of waves and particles are contradictory from a classical perspective, and are united without contradiction in quantum formalism, but more justification is needed for asserting that classical and quantum domains are utterly discontinuous and radically different from one another. There is today in fact extensive research on the interface between the classical (especially the chaotic) and the quantum domains, and older links exist in physics and chemistry between classical and quantum explanations. Nicolescu's article is short, so he can give only limited space to his assertion of the utter discontinuity between classical and quantum mechanics, but since much in his article rests on this assertion, it needs a more detailed argument. One related and minor terminological point: the quantum-classical distinction should not be characterized as a microscopic-macroscopic distinction, as quantum phenomena can occur at macroscopic scales as in superfluidity and superconductivity.

Nicolescu implies that science supports the postulation of *other* levels beyond the classical and quantum. Unfortunately no examples are given (except a passing reference to Husserl's phenomenology, a philosophical project outside of science), so it is hard to grasp what Nicolescu is actually advocating. One does not need to multiply *realities* or kinds of "materiality" to indicate differences other than merely compositional; one can speak about *function* and its different levels.

Nicolescu's assertion that the classical-quantum distinction "can lead us to reconsider our individual and social life" is intriguing but left undeveloped. It is not obvious that quantum theory is relevant to more than physics and the adjacent field of chemistry, or, more specifically (in the context of this particular book) that it carries any implications for systems theory, which *a priori* is concerned with descriptions of reality common to more than one scientific discipline. Since Nicolescu does not offer any examples of different levels of "reality" (or "materiality") other than the quantum-classical distinction, the importance he sees in quantum theory seems to favor a reductionist viewpoint (i.e., the usual hierarchy of chemistry reducing to physics, biology reducing to chemistry, and so on). This is hardly the systems position, and it contradicts the author's later denial that any level of reality should be taken as privileged.

Quantum mechanics is important for Nicolescu beyond its implications concerning levels of reality. Nicolescu advocates a "logic of included middles," specifically the 3-valued logic of Lupasco (1987), and illustrates his position by pointing to the wave-particle duality, in which a contradiction at a classical level is reconciled at the quantum level. If A is a wave and non-A a particle, then the [A, non-A] contradiction at the classical (lower) level is resolved by T, the system at the quantum (higher) level. Nicolescu argues that this logical structure is open-ended, that new contradictions emerge at the upper level which can be resolved by a still higher level. This implies the existence of a "level of reality" higher than quantum theory, but no suggestion is given about what this higher level might be. Contradictory pairs abound in quantum theory, but examples from domains other than physics are unfortunately not offered.

For systems theorists, the view that classical 2-valued logic may not be adequate to describe reality needs no justification from quantum theory, as it is well-articulated in the systems -- especially the fuzzy -- literature, though this is not mentioned in the essay. Nicolescu credits Lupasco with being the first to formalize a logic of the included middle. It would have been helpful if he had explained the place of Lupasco's work in the literature of multivalued logic, which goes back at least to Lukasiewicz (1930) and includes the systems-oriented non-Aristotelian logic of Varela (1975, 1979). There are many non-classical logics, and the unique importance of Lupasco's logic is not explained. For a discussion of some of these issues, which also shows how Varela's work can be used to axiomatize a 3-valued logic proposed earlier by S.C. Kleene, see Schwartz (1981).

In Marcus' terms, Nicolescu is addressing *explicit* oppositions. It would be interesting if Nicolescu would take up also the *implicit* oppositions which Marcus writes about. There are many kinds of oppositions and the resolution of oppositions is a big subject. Resolution at a higher level is one conceivable approach, but there are oppositions resolved at the same or even at a lower level.

For Nicolescu, quantum theory has significance beyond physics by virtue of a connection to Gödel's proof, which applies to all formal systems of sufficient complexity. The proposal is a radical one. Although the connection between a Gödelian level/meta-level distinction and a classical/quantum distinction is not actually explained, the distinction between levels in the mathematical instance does indeed have the quality of sharpness which Nicolescu asserts for "levels of reality." One might argue that classical description, especially Bohr's view of the necessity of a macroscopic description, relates to quantum description as meta-number theory relates to number theory. This cannot serve as an example for Nicolescu, however, because he wants quantum, not classical, reality to constitute the higher level.

The temptation to link the quantum revolution in physics to Gödel's revolution in mathematics is understandable. (I have made a similar attempt (Zwick, 1978).) These were profound discoveries, and if they can be rigorously connected, the significance of the linkage would be considerable. The issue is the relevance of Gödelian undecidability to science. It is unlikely that the *specific formula* that Gödel constructed to be undecidable is important for science, and simply to say that science uses arithmetic which is incomplete is also not a compelling answer to this question. One wants to know if significant assertions *about the world* are undecidable. Nicolescu mentions Pauli and Laurikainen but does not himself discuss this question or the relevant literature (e.g., in dynamic systems theory, automata theory, algorithmic information theory) that addresses the broader significance of undecidability.

It is not obvious how Gödel's results involve a logic of included middle or how the level distinction between number theory and meta-number theory in Gödel's proof have the form of an [A, non-A] contradiction resolved by T at a higher level. It is hard to see a 3-valued logic in operation here because the base (number theory) level is not actually afflicted by contradiction, only by incompleteness. There is also the issue of *self-reference*. It is not apparent if Nicolescu views self-reference as involved in the quantum-classical dichotomy. Self-reference is certainly critical to Gödel's construction and use of a level/meta-level mapping. It is also not clear from Nicolescu's account if self-reference is involved in Lupasco's logic. Even if one interprets a 3-valued logic (Lupasco's or Varela's) as encompassing a kind of self-reference, I wonder if it is a self-reference as rich as the kind of self-reference found in Gödel's work, which involves only classical 2-valued logic.

Nicolescu's attempt to connect the classical-quantum dichotomy, multivalued logic, and undecidability is only sketched in the article, and may be more developed in his other publications. This conceptualization is then the foundation for a wide-ranging discourse on philosophical topics, which Nicolescu calls "the transdisciplinary approach." I find much of his discussion on this obscure (it needs some grounding *resistance*), but it does offer interesting insights. For example, Nicolescu rightly observes that we need but no longer have a coherent and rich sense of Nature. “Nature is dead, but complexity remains.” This is a perceptive, pithy, and ironic assessment of why the “sciences of complexity” have captured public attention. But still, I think his nearly exclusive focus on physics has caused Nicolescu to miss the cultural potential of these sciences, which constitute a renaissance of the systems research program. In my opinion, it is from the systems theory/complex systems project, which addresses systems of every conceivable type, rather than from theories in physics, that a new philosophy of nature can -- and will -- ultimately be constructed.

More about this volume

Table 1 provides a list of the articles in this book. The diversity of topics covered is actually not as extreme as it might be. Beyond the articles classified as “general,” the primary topic areas in this book focus on knowledge, language, and communication, as opposed to the subject matter of anthropology, sociology, political science, and economics. Perhaps this is implied in the use of the phrase “human sciences” as opposed to “social sciences” in the title. But there is also considerable material drawing upon and relevant to the social sciences in this volume. This book has many riches which can be mined by readers, and I anticipate returning to it to explore articles I have not yet studied.

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To reiterate a point made at the outset of this review, such a wealth of material calls for a coherent framework which might organize it. It is unfortunate that although one of the prime motivations of the systems movement is the integration of knowledge from a variety of disciplines, the field itself desperately lacks integration of its own ideas and methods. Collections such as this volume are valuable resources, but in addition to such collections and expositions reflecting the viewpoints of particular researchers, we need synthetic works, better yet, textbooks, aimed at the development of a systems science canon.

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